



## Responses to Plenary Session Papers, NECMT 2000

Allen Forte



REFERENCE: <http://www.mtosmt.org/retrofit/mto.00.6.3/mto.00.6.3.dubiel.php>

REFERENCE: <http://www.mtosmt.org/classic/mto.00.6.3/mto.00.6.3.hisama.php>

REFERENCE: <http://www.mtosmt.org/classic/mto.00.6.3/mto.00.6.3.kaminsky.php>

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ABSTRACT: At the NECMT 2000 meeting five formal papers were presented, covering new approaches to music theory and analysis (Dubiel and McCreless), the advantages of opening new repertoires to study (Hisama), a retrospective and prognostic of music theory pedagogy (Karpinski), and a discussion-demonstration of an analytical approach to rock music (Kaminsky). The responses by the present author address issues and special features he perceived in this array of interesting papers.

Editorial Note: If an author has elected to reply to Prof. Forte's response, that reply appears directly after the appropriate section of Forte's response, and a second response by Forte follows that. Sections are numbered consecutively, and paragraphs within sections bear secondary numbers (e.g., [1.3]). Those who post comments to mto-talk about this exchange are asked to note carefully which author is writing when. References in square brackets, e.g., [4.2] are paragraphs within this document, unless accompanied by an author's name, in which case the reference is to that author's paper.

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### Joseph Dubiel, "Analysis, Description, and What Really Happens"

#### [1] Response by Allen Forte

[1.1] I find Professor Dubiel's paper original and thought-provoking. I only wish I could say that I agree with his idea of what I will call concept formulation, but I am sure he will acknowledge that in this instance, as in many others in life, there is, as they say, room for disagreement, and I hope that he will not be offended if I express mine rather directly, given the restrictions on time under which we are all operating this afternoon.

[1.2] Let me begin by saying that I, personally, have never thought that the purpose of what Prof. Dubiel calls received theory was to tell anyone how to hear music. His anonymous quotation, however, demonstrates that others do—in particular, those who partake of a teleological viewpoint.

[1.3] Prof. Dubiel describes teleological analysis as analysis that purports to explain how a musical event should be heard, why it occurs, and why its prior history leads inevitably to its later history. I'm not sure that all "received analysis" does this, but perhaps many analysts have been guilty of unquestioningly accepting a teleological concept of this kind. Without intending to dwell on this subject, it seems to me that a teleology is indigenous to tonal music—for example, the concept of harmonic progression, which is goal-directed, especially in its purest form, the circle of fifths. And even Prof. Dubiel's description of his experience of the key succession in the Schumann movement involves a teleology, namely, the notion of syntactic harmonic succession.

[1.4] Nevertheless, in Prof. Dubiel's view, teleological analysis does not qualify as a basis for "sharp, attractive, useful concepts for grasping our experience of music" [Dubiel 17]. I can certainly see that a teleological approach to the analysis of music may be completely inappropriate in some repertoires, especially in music of the rapidly disappearing 20th century, and to that extent I agree with his view.

[1.5] I come now to my two main objections to the propositions Prof. Dubiel puts forth in his paper. The first concerns his interpretation of received theory. Music theory, and American music theory in particular, seems to me to concern itself primarily with the explanation of and speculation about musical structures and with analytical applications, with analysis broadly construed, to a variety of repertoires. In my view, it is not basically a didactic endeavor.

[1.6] Second, I find the comparison of the concept-formulation idea with the wide range of achievements of American music theory—much of which is (forgive me) formal in nature—at best inapt. If we were to accept Prof. Dubiel's recommendation ". . . to give a very prominent place to the kind of invention, communication, and understanding represented in my stories" [Dubiel 11], the character of the music theory endeavor would change radically. Conference papers would tend to exclude rigorous analyses based upon new theoretical ideas, there would be no room for speculative work in the area of musical systems, and the discussion of issues in history of theory would become irrelevant, or orthogonal, to borrow one of Prof. Dubiel's adjectives. I cannot see what future that holds for scholarly research in our field, nor do I see significant pedagogical applications at any level.

[1.7] Probably this dire prediction, which was intended to raise the level of rhetoric in order to accentuate what I regard as important issues—probably that prediction is exaggerated—but not, I believe, completely unreasonable. Is there, then, a way of accommodating Prof. Dubiel's strongly articulated recommendation? Yes. According to Prof. Dubiel, it is possible that received methods of analysis might lead to "a distinct and interesting conception of how a piece goes" [Dubiel 17]. Thus, his final recommendation: "If we theorists are as smart as we say we are, then we ought to make our characteristic concern with musical structure, or whatever we call it, recognizable as a source of stimulus for the invention and articulation of such concepts" [Dubiel 17]. By such concepts he refers of course to the exemplary phrases rendered by Brodbeck and Hasty. Still, if I read him correctly, he recommends the creation of concepts as the central task of music theory, and I cannot agree with that.

[1.8] Finally, despite the negative tone of my response, I want to say that Prof. Dubiel has given us a good deal to think about, and I hope I have not seemed to reject his idea lightly.

## [2] Joseph Dubiel replies:

[2.1] I appreciate the conviction and frankness of Professor Forte's response, yet I find that it confronts my ideas only obliquely. One reason for this, I suspect, is a difference of values between him and me that is considerably more extensive than the specific matter of my talk. Another reason, certainly, is a pattern of misunderstanding that occurs at several levels: I propose that theorists' present range of operation be *expanded*, through our adoption of ideas or methods *in addition to* those that are now predominant, and he responds as though I have proposed to *eliminate* what is familiar and *replace* it with something new. When he misreads me in this way, he naturally finds my suggestions disturbing—so would I! I can't promise that he will be any less upset by what I actually say; but, for what it's worth, I will try to clarify.

[2.2] The most easily identified (and rectified) instance of this pattern is Professor Forte's claim that, "in Prof. Dubiel's view, teleological analysis does not qualify as a basis for 'sharp, attractive, useful concepts for grasping our experience of music'" [1.4, in part quoting Dubiel 17]. No: in my view, teleology already *is* a sharp, useful, attractive concept for grasping our experience of music—when our experience happens to be of teleology. Whoever first thought of saying, of a suitable passage of music, that it sounded as though various events in the passage led to and followed from other events, had a great idea, one that has obviously caught on and proven susceptible to further application and elaboration. But of course it isn't the only

such idea we need; and we'll need to keep devising more, as long as we keep hearing more different pieces going more different ways.

[2.3] It is surprising that this is such a big point for Professor Forte, because, as far as I can tell, we agree completely on the (hardly radical) proposition that some musical passages proceed teleologically and some don't. For some reason, he devotes half of his response to warding off the assumption that he might be an inflexible teleologist [1.2], defending the attribution of teleology to some music [1.3], and mistakenly attributing an obviously silly view to me [1.4]; he needn't do any of this on my account.

[2.4] If anything, Professor Forte ought to be on my side in trying to help the people I'm trying to help, namely those who find the teleological model *so* impressive that they see a fundamental *methodological* difference between accounts of pieces in which the model is applied and ones in which it isn't. Look again at the quotation in my talk that defines analysis (as opposed to description) by reference to teleology [Dubiel 15]; it amounts to this: to analyze a piece *means* to show how its events are "made necessary" by earlier events and "lead to" later events. In that case, if you give an account of a piece as going that way, you are analyzing it; but if you give an account of a piece as *not* going that way (say, because you don't hear it going that way!), then you are *not* analyzing it (although you may be doing something less, such as "merely describing" it). The main point of my talk is that I don't find this distinction altogether rational. But (like many not altogether rational ideas) it's not uninfluential: people really do go around talking about minimalist music, or Morton Feldman's music, as "unanalyzable"; and, probably more insidiously, people bend their analytical models of less obviously refractory music in the direction of what they have learned to think are the methodologically most impressive claims they can make, namely claims of implication and inevitability, when they could recognize more possibilities and exercise freer choice. (An adequate treatment of the analysis-explanation-teleology series would require an extensive discussion of explanation; but since this is not an issue between Professor Forte and me, I will not go into it.)

[2.5] Since I have just summarized it, I should point out that this particular account of this particular issue—that is, the differentiation of analysis from description on the basis of explanation, which is most characteristically teleological—is what I call the "received view" in my talk, and it is the *only* thing that I ever call "received." There is no such thing as "what Prof. Dubiel calls received theory" [1.2]; what Professor Forte calls "received theory" [1.2, 1.5] or, at times, "received analysis" [1.3, 1.7] is *his* issue. I take him to mean by this the music-theoretical status quo, broadly understood, perhaps with an emphasis on the "formal," a characteristic he seems to feel he has to defend against me [1.6]. In my talk, this isn't even a subject—let alone one toward which I take a hostile attitude. If anything, my attitude is one of indifference.

[2.6] And not even indifference, exactly: it's rather that I don't have any attitude at all toward formal, systematic analysis as such—I don't think that this is even the right sort of thing to have an attitude toward. I don't care whether a claim about music is presented formally or informally, systematically or unsystematically, as long as I can tell what experiences of what music are supposed to correlate with it. I value formality and systematicity when they help to clarify the implications of a theoretical claim for our hearing; I value informality and unsystematicity when they help to clarify the implications of a theoretical claim for our hearing. (I would cite my writings as evidence that I am committed to mobility along the methodological continuum, not to any fixed point on it.)

[2.7] The two stories in my talk give accounts of pieces that are unusually, even exaggeratedly, informal and unsystematic (especially in the compressed forms in which I present them). It is of course part of my point that these are not the characteristics of style and method that theorists are in the habit of esteeming, at least in professional contexts. But (obviously, I thought) it is no part of my point to recommend these "untheoretical" characteristics *as against* their opposites; my point is to demonstrate that these characteristics are no impediment to getting the job done—the job being to help us get a better grasp of a wider range of musical experience. It is this conception of the job that I want to emphasize above everything else in my talk. Understanding what we hear is the reason to do music theory— isn't it? And if so, then it should set our standards of methodological adequacy, and of empirical and interpretive accomplishment—what else could?

[2.8] Professor Forte's response to this misunderstands it on the pattern to which I have already referred. I say "Look: besides what we already recognize as valuable, here's something else that can work—it's happened to me"; he reacts as though I have made some threat against what is recognized. In his zero-sum nightmare, "conference papers would tend to exclude rigorous analyses based upon new theoretical ideas, there would be no room for speculative work in the area of musical systems," and so on, if a "prominent place" were given to "the kind of invention, communication, and understanding represented in my stories" [1.6, in part quoting Dubiel 11]. The invention of models of pieces that *might or might not be* teleological; communication of them that *might or might not be* formal; and understanding of them that is concerned above all

with their relation to musical experience—exactly *what* would this leave “no room” for?

[2.9] The only thing I can think of would be analysis that could not be connected to specific ways of hearing specific passages. And, now that I think of it, good riddance (although getting rid of things is not my project, and in real life I would be against discarding anything until a strenuous sympathetic effort had been made to work out any audible significance that its author may have failed to articulate). But in any event I see no reason to expect success and failure by this standard to be anything but randomly distributed with respect to (in)formality or (un)systematicity. If I wanted to tweet Professor Forte, I could accuse *him* of a pessimism about the auditory relevance of his “received analysis” that I would never dream of entertaining. This might not be accurate as a representation of his thinking (he offers no alternate account of the relation of analysis to hearing, if he in fact doesn’t like mine), but we are invited to imagine the worst by his horror story. He concedes this scenario to be “probably . . . exaggerated” [1.7]; but the concession only relieves him of the responsibility to defend this fantasy in the terms in which he presents it. We are still left with an image contrived to alarm us.

[2.10] I cannot even tell whether Professor Forte thinks of himself as disclosing a perverse intention of mine, or merely pointing out the unintended implications of an innocent but ill-conceived fancy. (Am I a pyromaniac, or just playing with fire?) Since he has opened this larger question, I shall conclude by saying what I think would be the consequences for the music theory of the methodologically inclusive, experience oriented view of analysis expressed in my talk. I should specify that I am reluctant to claim it as *my* view, because I know I am not alone in holding it, or even in asserting it; accordingly, its influence is not all in the future—but for simplicity’s sake I shall write as though it were.

[2.11] In that golden age, we theorists will hold ourselves responsible to tell our readers, as well as we can, the audible significance of our representations—what ways of hearing what music are to count as confirming (or disconfirming) our theoretical claims. Theoretical work will be considered ill-formed when it elides or bungles this crucial step—ill-formed in the only relevant sense, namely not formed so as to facilitate experiential evaluation. The evasion of such evaluation is the only trait that will be stigmatized as a lack of “rigor” (although unbendingness will not rank as high as flexibility, responsiveness, imagination, and solicitude toward experience among the qualities for which we will commend one another). Older theoretical work, which may or may not have been executed with the ideal of auditory relevance uppermost, may have to be reread and reevaluated—as normally happens when the interests of a disciplinary community evolve. Some of this work will reward such reading, richly and immediately, and some, initially more obscure, will be found to have musical significance beyond what its authors managed to place in the foreground. Some older work may resist such explication, but even then this work will have something to teach us, as little as we may wish to emulate it: we will have at least the historical project of figuring out what its authors were trying to do and why. If our predecessors turn out ever to have maintained something other than musical experience as their topic of investigation, or something other than efficacy at rendering and explaining experience as a methodological mandate, it will be mind-stretching to find out what it was. But our main business as theorists of music will be elaborating, exchanging, and trying out vividly imagined, diversely formulated accounts of what and how we hear. What’s scary about that?

### [3] Allen Forte responds again

[3.1] Despite the occasional gestures of bonhomie and the expressions of generosity in support of a broad view of music theory and analysis, one that would include more traditional activities, in the concluding statement of his reply to my response Prof. Dubiel is unable to relinquish the narrow conception of our field that was so evident in his original NECMT paper but that has been modified by numerous additions and new formulations in his reply. I shall return to this major issue at the end of my response.

[3.2] Indeed, many of Prof. Dubiel’s clarifications, amplifications, and especially criticisms of my response are written as though they are based upon statements made in his original NECMT paper. If only they had been there. Right at the outset of his reply he asserts that I have misread him, and that I have responded “as though I have proposed to *eliminate* what is familiar and *replace* it with something new” [2.1].

[3.3] Denying this intention, Prof. Dubiel writes “I propose that theorists’ present range of operation be *expanded*, through our adoption of ideas or methods *in addition to* those that are now predominant . . .” [2.1]. But where does he say this in the NECMT paper? On the contrary, he makes a number of statements that support my original interpretation, forming a consistent and contradictory thread, for example, in the following categorical statement: “The received theory [by which he I assume he designates currently standard music theory in general] isn’t antithetical to these experiences [related in the preceding two ‘stories,’ which demonstrate epiphanic concepts], it’s orthogonal to them” [Dubiel 12]. Now, the mathematical

term “orthogonal” means “right-angled” or “statistically independent,” which, construed metaphorically, signifies something like “tangential,” “incidental,” or “peripheral.” It certainly does not support the idea of “*in addition to*,” which Prof. Dubiel accuses me of misreading.

[3.4] Here is another instance: “I want to show you how a certain way of thinking about analysis and certain familiar ways of learning about music are mutually irrelevant; and, faced with that situation, I want to recommend keeping faith with our actual experience and practices, and letting an imposed methodological scheme go by the boards” [Dubiel 2]. Doesn’t the phrase “letting . . . go by the boards” signify rejection? If this statement implies rejection of “methodology,” as I believe it does, then it excludes much significant music theoretic- analytical work, such as Schenker’s, and, more recently, neo-Riemannian harmonic theory, which I would say embeds a “methodology.” From these and other statements, which are not contradicted elsewhere in the NECMT paper, I conclude that Dubiel’s complaint in [2.1] concerning my misreading is not supported. I read what he wrote and not what he didn’t write.

[3.5] Yet another strand of the rejection thread occurs at the end of his discussion of the two pieces, the anomalies (my word) of which had been of concern to him, but were laid to rest by the concepts Hasty and Brodbeck provided. Dubiel then considers the possibility that someone might “tell me how these pieces actually did proceed teleologically. . . .” In that case, he declaims: “. . . I am convinced that any meta-analytical framework that would even suggest that we say this is a framework that we’d be better off without” [Dubiel 15]. I submit that this categorical statement and others like it contradict the idea of broad inclusion that he expressed in [2.1]. And in [2.11], at the end of his reply to my response, we see what the final determination is with respect to those older, “received” views of music theory and analysis.

[3.6] Moreover, Prof. Dubiel disputes my statement that in his view “teleological analysis does not qualify as a basis for ‘sharp, attractive, useful concepts for grasping our experience of music;’” whereas in [Dubiel 15], after introducing “teleological” as the primary characteristic of analysis through an anonymous quotation, he rejects the teleological approach, stating, “And of course that’s exactly the view that, in these two cases, I got over. . . .” From this and the following statement I conclude that teleological analysis did not engender the kind of concept (which in [Dubiel 17] he characterizes as “sharp, attractive, useful”) represented by the Hasty and Brodbeck extracts.

[3.7] I regret the defensive posture I have taken at the beginning of my response, but I felt that it was necessary in order to remedy flaws in the discourse, perhaps engendered by Prof. Dubiel’s haste in preparing a reply to my NECMT response and consequent detachment from his original paper. Or perhaps he wished to correct by amplification certain omissions in the original paper that were due to the time restrictions imposed by the NECMT format. In that case he should have informed the reader that there are significant differences between what he actually said at NECMT 2000 and what he implies he said there.

[3.8] In [2.2] Dubiel also upbraids me for devoting (“for some reason”) a significant portion of my response to the issue of teleology, a term that he introduced. The “reason” was the central role he accorded it in his NECMT paper, whereupon I took the occasion to ruminate, and to include reflections on my own position, since I had not given much thought to this issue, and had been inspired by that feature of the Dubiel paper—and by others, as I indicated at the end of my NECMT response.

[3.9] Again, in [2.4] Dubiel brings up the teleological attribute of analysis, amplifying his NECMT paper to assert that if an account of a piece shows how its events “made necessary” by earlier events and “lead to” later events, then it qualifies as analysis. Otherwise not. Thus, he places in motion yet another dualism of which there are many in his reply, analysis and non-analysis (my terms), a dichotomy that was fundamental to his NECMT presentation. In his MTO response he writes: “The main point of my talk is that I don’t find this distinction altogether rational.” Frankly, I don’t know how to interpret this, especially since this is but one of what he has called his main points. Perhaps he is leaving the door open for teleological analysis, but only if it fulfills the requirements set forth in [2.11]. In the NECMT paper, however, he takes a dim, if not a completely negative view of analysis, preferring what in my response I dubbed “concept formulation.”

[3.10] Prof. Dubiel is fond of the term “received,” using it more often than he indicates in his reply (“received account [Dubiel 1],” “received distinction [Dubiel 11],” “received view” [Dubiel 2, 12, 15] and “received theory” [Dubiel 12], which he so adamantly denies using.<sup>(1)</sup> He does not, however, use the term “received analysis,” and I apologize for imputing that usage to him.<sup>(2)</sup>

[3.11] Still on the issue of what is “received” and what is not, Dubiel writes: “what Professor Forte calls ‘received theory’

[Forte 1.2, 1.5] or, at times, ‘received analysis’ [Forte 1.3, 1.7] is *his* issue. I take him to mean by this the music-theoretical status quo, . . . , perhaps with an emphasis on the ‘formal,’ a characteristic he seems to feel he has to defend against me [Forte 1.6]” [2.5]. I acknowledge that I am defensive about the formal aspect of music theory and that this is not an issue Dubiel brought up *explicitly* in his paper. It is, however, relevant to this discourse, and at this point I wish to pose a question, one that Dubiel would probably regard as heretical: Is the sole purpose of music theory to explain “pieces”? I say “heretical” because it violates what I would call his axiomatic criterion of audition, which is that the purpose of music theory is to explain what we hear. I leave aside the questions of how “what we hear” is determined, as well as how to account for individual variations in that regard, in order to make an observation based upon simple fact: many of the venerable figures in the history of music theory—Boethius, for example—would be disqualified as music theorists were Dubiel’s axiomatic criterion of audition to be applied indiscriminately.

[3.12] Further, on the topic of audition as it relates to music theory, Dubiel reports that he transformed Hasty’s concept of “a constantly evanescent beauty” into immediate audibility, without Hasty (or Brodbeck) “letting me in on some kind of structure that underlay what was audible” [Dubiel 13]. I find the reality of this occurrence difficult to grasp, that is, the proceeding instantaneously from a verbal description to an auditory image. And, once more, the throw-away phrase “some kind of structure that underlay” strongly and negatively refers to one of those “ideas or methods . . . that are now predominant” [Dubiel 13], namely, I assume, Schenkerian theory and analysis.

[3.13] In [2.7] Prof. Dubiel again introduces something that was absent from his NECMT paper, namely, his willingness to accommodate formal or systematic music theory if it gets the job done, “the job being to help us get a better grasp of a wider range of musical experience.” He continues: “It is this conception of the job that I want to emphasize above everything else in my talk.” Sounds good to me, as long as the “range of musical experience” is broad enough to include intellectual experience that may not relate directly to the audition of individual pieces.

[3.14] In [2.8] Dubiel again complains that I misread him. But as I have set forth in my earlier comments, his ecumenical inclusion of analysis, presumably of the traditional type, was not made explicit, giving rise to the “nightmare” scenario I concocted in order to frighten readers. Surely, Prof. Dubiel, you can forgive a little hyperbolic rhetoric. After all, you have made effective use of it in both of your contributions to this forum.

[3.15] There is still more on my “horror story” in [2.9]. In large part I have answered this in my comments above, which suggest a partial answer to Dubiel’s comment here to the effect that I offer “no alternate account of the relation of analysis to hearing. . . .” This is of course a very large question, and it would be foolish even to attempt to answer it in this context, particularly since there are others far better qualified to cope with the attendant complexities than I.

[3.16] In Prof. Dubiel’s [2.10] we encounter the synthesis that was lacking in his NECMT paper, as he says that he will conclude by setting forth “the consequences for the music theory of the methodologically inclusive, experience-oriented view of analysis expressed in his talk.” I failed and still fail to find the “methodologically inclusive” part in Dubiel’s NECMT paper, but perhaps I am misunderstanding the syntax here. Is “methodologically” an adverb that modifies the adjective “inclusive”?

[3.17] The golden age! What should we call what is described in [2.11] of Dubiel’s reply? The New Music Theory? Whatever it is called, we must all remove our hats or, for those who do not wear hats, figurative hats, as the brave young theorists march past bearing trophies of “older theoretical work” that did not measure up to “Prof. Dubiel’s ideal of auditory relevance.” Who will have determined that failure? Why, “the interests of the disciplinary community,” of course. (Here I pause to savor the double meaning of “disciplinary” in this context.) Why have these older theories been subjected to disciplinary rejection? Because they were “not formed so as to facilitate experiential valuation.” Who determines whether they were not so formed? Those who understand what “experiential evaluation” is. The reader will kindly excuse this catechism, but I have found it necessary in order to avoid yet another accusation from Prof. Dubiel to the effect that I have misread him.

[3.18] To close in a more serious vein, my own vision of the future of music theory is not as neat as Dubiel’s. I am not as certain as he is about what “our main business as theorists of music will be.” At the present time there are many intellectual activities that impinge upon our small field, and many of these will exert influence. This is not so new, of course. Of those that were influential in the recent past, although they lack visibility at the present time, I recall the information theory of the sixties, the phenomenology work of my former student, the late Thomas Clifton, and the structural linguistics of the seventies. In Dubiel’s vision of the future, one of these (Clifton’s phenomenology) might be regarded as well formed—I’m

not sure—while the other two would certainly be subjected to “figuring out what [their] authors were trying to do and why” [2.11], which is presumably the last stage before final interment. In any event, the world of music theory, according to Dubiel, is to consist of two communities: those whose work facilitates “experiential evaluation” (I love that term) and those whose work does not. None of his persuasive language and occasional expressions of generosity toward those who might not agree with him conceal this basically authoritarian point of view. And I do not say this to frighten anyone. Prof. Dubiel has convinced me that that was a bad idea. It is just how I see it, following, consistently, I hope, upon my charge from the folks at NECMT to respond to the NECMT 2000 papers.

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**Ellie M. Hisama, “Life Outside the Canon? A Walk on the Wild Side”**

**[4] Response by Allen Forte**

[4.1] During my banquet address in Rochester at the 1987 celebration of the tenth anniversary of the Society for Music Theory I said: “What is most striking to me is that the programs of the Society now exhibit a breadth of interests that probably could not have been predicted when it began, but which is to be encouraged.”<sup>(3)</sup>

[4.2] Prof. Hisama now tells us that a decade later a significant increase in activities related to music theory began, activities that involved research and publishing of various kinds centering upon music “outside of the Western European art music canon as well as the standard tonal and post-tonal repertoire” [Hisama 2]. From this she predicts career possibilities based upon analytical involvement with this non-canonical music. All this is very exciting, and I was impressed with the array of names Prof. Hisama invoked when introducing the topic of professional potential (possibly leading to that summit of academic achievement, tenure). Impressed is hardly adequate to describe my reaction, which was momentarily vertiginous. And I must admit that out of the list of names she presented as possible subjects of research on the non-canonic side, I recognized only three, those of Sting, Mary Lou Williams, and Adam Guettel. I am definitely running with the wrong crowd.

[4.3] Before I move along to a concluding and general comment on Prof. Hisama’s thoughtful and challenging paper, I would like to consider the term “canon” that is tossed around so blithely these days. As a dedicated sesquipedalian, I went to the Oxford English Dictionary to locate the entry “Canon.” Here is what I found. Of the 14 definitions of the word in the OED, number 4 seems closest to what I perceive as current usage: “The collection or list of books of the Bible accepted by the Christian Church as genuine and inspired.”<sup>(4)</sup> Thus, the word is of religious origin. It is doctrinal in nature. I found myself trying to imagine just how this information would be received by the musicologists in Bob Jones University’s Department of Music.

[4.4] I believe that the term canon is a recent arrival in the musicological literature, perhaps introduced by some disgruntled scholar, possibly with negative intent, since it implies permanence, immutability, and of course a reverential attitude, not exactly attractive qualities to your average post-modernist. At any event, it seems to be here to stay. When we use it as a keyword in scholarly discourse, however, we would do well to be watchful. Indeed, we might well pay heed to another meaning of canon, number 2c in the OED: “A standard of judgment or authority; a test, criterion, means of discrimination.”<sup>(5)</sup> This would certainly apply were one to allow an extension of the canonization process to include new canons, if that is not a contradiction in terms. Allow me to digress for a moment, however, to avoid possible misunderstanding. Just as Prof. Hisama says she does not recommend “that research on canonical composers should [be] dispensed with” [Hisama 4], I am not suggesting that research involving non-canonical music be suspended, a suggestion that, in any event, would be Chutzpah of the highest order. Quite the contrary. As I have indicated, we should recognize that the canonization process is ongoing. We are all familiar with instances, among the most visible of which is the music of the Beatles, now fully transcribed and available in score form in a high-class binding. And of course another, perhaps even more visible canonic corpus of music is the classic American popular song repertoire, which comes from musical theater and movies during the twenties, thirties, and forties.

[4.5] Following the second definition of canon, it is clear that the formation of new canons involves selection, which, in turn, requires criteria of discrimination. Of course complex processes of selection are always in operation in a vital art form such as music, and that is especially true in our often bewilderingly complicated world of information dissemination. Clearly, as Prof. Hisama indicates throughout her paper, a large part of the burden of selection, from the scholarly vantage point at least, falls upon younger scholars, and I hope, sincerely hope, that they bear in mind that second definition of canon. I would also hope that job attainment leading to tenure is not a major motivation for exploring music outside the canon, taking precedence over the intrinsic interest of the musical artifact.

## [5] Ellie M. Hisama replies

[5.1] After mentioning his 1987 SMT banquet address which noted the diversity of interests then exhibited by the Society, Prof. Forte writes: “Prof. Hisama now tells us that a decade later a significant increase in activities related to music theory began, activities that involved research and publishing of various kinds centering upon music ‘outside of the Western European art music canon as well as the standard tonal and post-tonal repertoire’” [4.2]. As the introduction to my paper states, my survey of developments in music theory takes 1997 as its starting point simply to avoid duplicating the efforts of papers presented at the 1997 SMT plenary session. It does not identify 1997 as a *beginning* in research activity on music outside of the Western art music canon. On the contrary, I would agree with Prof. Forte that the foundations of the discipline’s present diversity are rooted in earlier work.

[5.2] Prof. Forte continues: “From this she predicts career possibilities based upon analytical involvement with this non-canonical music. All this is very exciting, and I was impressed with the array of names Prof. Hisama invoked when introducing the topic of professional potential (possibly leading to that summit of academic achievement, tenure)” [4.2]. One of the purposes of my paper was to offer the perspective of someone who recently sought and obtained a tenure-track position in music theory. While I do not wish to present an unduly optimistic view of a job market that frequently requires graduate students to piece together adjunct teaching from multiple institutions to make ends meet or that sometimes forces newer scholars to leave the field altogether, I do believe that analytical work outside the Western art music canon is now becoming more widely accepted than it was ten years ago.

[5.3] The discipline’s increased interest in non-canonical music is underscored in Joelle Welling and Cynthia I. Gonzales’s recent study of the music theory job market. Surveying advertisements for music theory jobs published in the College Music Society’s Music Vacancy List from 1985 to 1998, their article shows that the demand for applicants who could teach world music, jazz, and popular music in addition to theory courses rose sharply during the period 1994–1998. During this four-year period, world music was listed as a desired additional teaching area 12 times as compared to five times over the nine-year period 1985–1994; jazz was listed as a desired teaching area 14 times over this four-year period in comparison to 18 times for the previous nine-year period; and popular music was listed as a desired teaching area seven times over the same four-year period in comparison to only two times during the previous nine-year period.<sup>(6)</sup>

[5.4] The choice of topic alone of course does not guarantee one a job interview, an offer, or quality analytical work. Prof. Forte raises the concern that the canons of the future may not be based upon discriminating criteria but upon the career possibilities they present. The current offerings of analytical work outside the canon eases this anxiety. I do not advocate choosing a topic based on what’s currently marketable, and I sincerely hope that shoddy scholarship on bad music is not the wave of the future. Rather, I would encourage theorists who are attracted to repertoires that remain little discussed and analyzed to pursue their interest and to bring to this music the same care with which they might approach a piece by Beethoven.

[5.5] As most ABDs who have recently sought an academic position know all too well, the job market for music theorists is bleak. Welling and Gonzales’s study showed that the average number of junior-level tenure-track jobs advertised in the Music Vacancy List from 1985 to 1998 was 10.5 per academic year, with a low of 4 in 1988–89 and a high of 20 in 1989–90.<sup>(7)</sup> With more than 10.5 applicants on the market in any given year, these statistics indicate that there are clearly not enough tenure-track jobs in music theory to go around.

[5.6] Among the memorabilia preserved from my job search are several rejection letters that apologetically mentioned the number of applicants, which averaged 150 in the stack I collected. One of the lowest numbers of applicants for a job to which I applied was 107—these odds were positively heartening when compared to those of getting a one-year, non-renewable position at a liberal arts college in the South, which received a whopping 200 applications.<sup>(8)</sup>

[5.7] Prof. Forte concludes by voicing the hope that “job attainment leading to tenure is not a major motivation for exploring music outside the canon, taking precedence over the intrinsic interest of the musical artifact” [4.5]. Like the apprehension he expressed at the 1987 SMT meeting about “overdiversification” (“By fostering diversity, . . . the Society does run the risk of excessive fragmentation”), his concern is misplaced.<sup>(9)</sup> Because Prof. Forte has not recently experienced first-hand the intense competition for junior-level tenure-track jobs, he may not realize that these days it is a practical rather than a crassly careerist measure to assess the relationship between an area of research and one’s chances of getting a job with training in that area. Such matters do and should weigh heavily on the minds of young scholars in the current market. Selecting an area of study in a music doctoral program without considering its professional implications is simply not an option unless one has

independent means of support. We need to recognize the present realities of the job market while selecting research topics of intrinsic interest.

[5.8] Prof. Forte's invocation of tenure as "that summit of academic achievement" [4.2] led me to Cary Nelson and Stephen Watt's *Academic Keywords: A Devil's Dictionary for Higher Education*, which defines tenure as "the expectation that a faculty member who has been through a probationary period and has passed a tenure review will remain employed until retirement, voluntary separation, or removal for cause."<sup>(10)</sup> To graduate students who hope to obtain a position more permanent than a teaching assistantship or an appointment as an adjunct, being employed for life may indeed seem like the summit of academic achievement. Whether one's research topic is Neo-Riemannian analysis of Wagner or timbre in *Bikini Kill*, tenure guarantees us the freedom to explore such topics and is not to be dismissed lightly, particularly by those who already have it.

#### [6] Allen Forte responds again

[6.1] I would like to begin by clarifying a minor issue: Prof. Hisama misreads my observation concerning the "significant increase in activities related to music theory" [4.2, referred to in 5.1] a decade after my 1987 SMT banquet address. I did not say that these activities began *only* a decade later, but simply wished to connect the content of her present-day paper to my earlier forecast as a verification and as a device for beginning my response. At any event, we are in agreement as regards chronology, and I regret any misunderstanding that may have ensued from the lack of precision in my prose.

[6.2] I do not dispute the growing interest in music outside the "canon," a term to the exegesis of which I devoted an inordinate amount of time in my original response to Prof. Hisama's paper. At the same time, I am gratified to see Prof. Hisama at least touch upon the issue of quality that I raised, when she writes: "We need to recognize the present realities of the job market while selecting research topics of intrinsic interest" [5.7]. While my apprehension concerning "excessive fragmentation" has decreased in the past thirteen years since the 1987 SMT meeting, I still feel that as professionals we would do well to remain watchful in that direction. By its traditions American music theory is a small field, one that may not be able to accommodate unlimited diversification without sacrificing some of its basic characteristics: primarily, the cultivation of abstract theoretical concepts designed to illuminate specific repertoires of music. Large-scale diversification in terms of identifiable repertoires probably belongs to our mother discipline, musicology, or to related disciplines, such as ethnomusicology.

[6.3] With respect to the topic of diversification and the focus of research activities, the Welling and Gonzales study, cited by Prof. Hisama, is relevant because it dramatizes what many of us have known for a long time, namely, that music theory still has a long way to go to achieve broad recognition in academia, especially in smaller institutions where members of the music department faculty, including those hired to teach "theory," typically teach in a wide variety of areas. In this regard I have had not first-hand, but second-hand experience in connection with the four NEH Summer Seminars for College Teachers I have directed, all of which were attended by extraordinarily talented and, of necessity, diversified individuals. Prof. Hisama is right, however, when she points out that I have "not recently experienced first-hand the intense competition for junior-level, tenure-track jobs" [5.7], a rite of passage that for me exists, blessedly, only in the remote halls of memory. Nevertheless, I continue to experience that competition second-hand, through the many position-seeking Ph.D. recipients, with each of whom I have spent several congenial years in a pedagogical and advisory capacity. Therefore I am sympathetic to Prof. Hisama's sensitive statement that "these days it is a practical rather than a crassly careerist measure to assess the relationship between an area of research and one's chances of getting a job with training in that area" [5.7], even though I am not convinced that young scholars always make that assessment or even that they should, given the high rate of burn-out in professions where the selection of training and research areas and job attainment are so closely interlocked as to leave little room for responsible long-range personal choice.

[6.4] Finally, although I should perhaps regret having made that final statement in my response to Prof. Hisama's paper in which I expressed the hope that "job attainment leading to tenure is not a major motivation for exploring music outside the canon, taking precedence over the intrinsic interest of the musical artifact" [4.5], I do not. Nor do I withdraw my description of tenure as "that summit of academic achievement" [4.2], which, in making its point, inadvertently exemplified the kind of institutionalized wit to which I have been exposed for so many years. Thus, although that quip was intended to be mildly sarcastic, I do believe quite seriously that intellectual achievement and industry should take precedence over academic security in the psyche of the burgeoning scholar. At the same time, I certainly value tenure, for the reason so astutely formulated by Prof. Hisama at the end of her response, but also for larger reasons having to do with the sanctity of the academic environment and the need to protect it from anti-intellectual and other negative forces that may infringe upon the freedom of thought that is so essential to the development of many, if not all, academic fields, including ours.

[7] Response by Allen Forte

[7.1] Before I say a few words about Peter Kaminsky’s paper I want to make clear that my knowledge of rock is quite limited—some deriving from Mark Spicer and a couple of other renegade graduate students at Yale, the remainder from desultory reading and listening. Thus, I approached Prof. Kaminsky’s paper with a clear mind, as it were. If I were running for public office, however, this would be the moment when I would tearfully reveal my own dalliance with non-classical or non-concert music, that happy portion of my innocent youth in which I was involved with the popular music of a different era and a different repertoire. Thus, although my qualifications for responding to this paper are meager, my intentions are good.

[7.2] Much of my selective knowledge of rock derives from the anthology edited by John Covach and Graeme Boone entitled *Understanding Rock*,<sup>(11)</sup> which contains seven studies, all by academics with substantial music theory credentials. All the essays involve (forgive me) structural analysis. I should also point out that my name is inscribed in the Acknowledgments, for reasons that are not altogether clear to me, except that John Covach is a friend of mine and I may have uttered words of encouragement in an unguarded moment.

[7.3] When I sat down to make notes on the many highlights of Peter Kaminsky’s paper, the first thing that came to mind was the enthusiasm that drives it! This is also evident in the analysis, which must have been written before Part I of the paper; it is so beautifully polished and focused.

[7.4] In the non-analytical portion of the paper, I found the discussion of British authors Philip Tagg and Richard Middleton reasonably fair and broadly informative, although Prof. Kaminsky stops just short of rejecting Tagg’s intertextuality idea, and comments only briefly on his approach to an *Affektenlehre* for popular music—which seems not such a bad idea, after all. Middleton’s confusion (my interpretation) of analytical approaches and his evident need to satisfy the pressures of the new musicologists is documented, including yet one more derogatory reference by Middleton to “formalist music theory,” a tiresome, but apparently obligatory gesture.

[7.5] On the other side of the coin, I like Walter Everett’s statement and Peter Kaminsky’s elucidation of it, especially the point that the music must have a certain “degree of structural integrity” [Kaminsky 9]. This involves the question of “quality,” which I feel is so important in the study and evaluation of non-canonic music.

[7.6] Part II of Professor Kaminsky’s paper, his analysis of “Lithium Sunset,” is an excellent piece of work. Both the analysis and the tape of the performance directed my attention to the simple, but very eclectic harmonic progression. A general study of this aspect of the music, spanning the various styles that have evolved in the rock genre seems worthwhile, if, indeed, it has not already been achieved.

[7.7] Since it would be wholly inappropriate to end my laudatory response to this paper on a positive note, I would like express my opinion that [Table 1: Projects and Publications](#), seems not altogether useful, since many listings do not include publication data. Also I am unable to infer anything about analytical approach. I assume that Prof. Kaminsky knows all of the individuals involved and has some idea of the quality of their work.

[8] Peter Kaminsky replies

[8.1] First, my thanks to Professor Forte for his sensitive and thoughtful response to my contribution for the plenary session. With respect to [Table 1](#), Forte correctly notes that my omission of publication data (where appropriate) and the analytical approach taken in these works clearly limits its usefulness as a bibliographic tool. Given that my original file including such details runs over twenty pages, I chose to omit this information. My principal motivation in providing Table 1 was to give a snapshot of the variety and sheer volume of ongoing, not-yet-published work by theorists and musicologists on rock and popular music. However, in order to optimize its usefulness, I would be happy to post for retrieval by interested readers the complete file of survey data from which Table 1 is drawn.

[8.2] Professor Forte also notes a certain incompleteness in my citation of Philip Tagg’s seminal work, “Analyzing Popular Music: Theory, Method and Practice,” from 1982. Tagg’s work continues to resonate because of its central tenets: the study of popular music is of necessity interdisciplinary, and, therefore, the exclusive use of “the traditional tools of musicology [and

by extension music theory]”—i.e., analysis—is inadequate. I will first respond with an anecdote. After hearing my run-through for this paper, a colleague noted, “I can understand your use of traditional analysis with Sting. But take some of the popular music from my formative years, like the Carpenters for example. Is scholarly study of their music possible?” My response was yes, it is possible. However, here the use of traditional tools of structural analysis and the central focus on “the music itself” may not be very illuminating. Obviously the perspective and choice of methodology must be appropriate to the music, and should best explain what are perceived as salient aspects of that music. (I shall leave the proper study of the Carpenters’ music as a challenge for some enterprising theorist or musicologist.)

[8.3] There is also the matter of interdisciplinarity itself: one’s notion of what constitutes interdisciplinarity depends in part on how one views the relation between methodology and the resultant scholarly work. Clearly a number of scholars view certain analytical tools, especially formal methodologies like Schenkerian theory, set theory, etc., as constraints and even blinders which, notwithstanding their explanatory power, also possess the unfortunate side effect of liquidating any musical elements outside their purview. If, however, one views an analytical method as enabling rather than enslaving, as filtering and coloring rather than blinding, then the relation between method and scholarship changes and becomes symbiotic and catalytic rather than coextensive. Under this conception, most scholarly work, on popular music as well as other music theoretical topics, is interdisciplinary to some degree. Hence to debate what constitutes an appropriate degree of interdisciplinarity is quite a different matter than merely to question the employment of purely “formalist” methods. Moreover, a degree of interdisciplinarity—or, closer to home, a rapprochement with musicology (*pace* McCreless in his [contribution](#) to this plenary session)—need not and should not result in a weakening of theoretical/analytical rigor.

[8.4] In conclusion, I shall turn once more to Tagg, again in response to a perceptive observation by Professor Forte: “Professor Kaminsky stops just short of rejecting Tagg’s intertextuality idea, and comments only briefly on his approach to an *Affektenlehre* for popular music—which seems not such a bad idea, after all” [7.4]. In reconsidering Sting’s “Lithium Sunset” it is only now apparent to me how relevant Tagg’s intertextuality and his incipient *Affektenlehre* have been in the formulation of my analytical approach. Specifically, the markedness (following Robert Hatten in *Musical Meaning in Beethoven*) of cross relations carries with it the potential for expression and representation. This potential is creatively exploited by Sting against the (ironic) stylistic backdrop of country music, and a listener’s response may be affectively linked to the dual recognition of musical dissonance and stylistic dissonance. Clearly the genesis of such a network of musical relations and affects presumes an intertextual framework, which I construe as one of Tagg’s central (and significant) points.

[8.5] In place of a cool and catchy ending appropriate to a popular music essay, I offer a riddle and a question. Riddle: What is the source of song #2, “Love of My Life” (guest artist and co-writer Dave Matthews), on Santana’s recent Grammy-winning album *Supernatural*? Question: What difference(s), if any, does this make in a prospective analysis of this song?

#### [9] Allen Forte responds again

[9.1] Peter Kaminsky’s informative reaction to my NECMT response does not elicit very much in the way of a reply, since it essentially enlarges upon points he made in his original paper—with the exception, perhaps obviously, of his brief introduction of “interdisciplinarity” [8.3] (sesquipedalian alert!), a topic that underlay some of the papers on that fateful day at Brandeis, notably Patrick McCreless’s, and that was lurking in others, including my responses. Here is material aplenty for debate, for example, in the reference to the view that Prof. Kaminsky cites concerning the limiting effects of certain formalistic analytical methods held by a number of scholars. Clearly, this is a topic to be set aside for another day, with ample time allowed for sharpening rhetorical weaponry. Nevertheless, I suspect that a number of scholars would dispute Prof. Kaminsky’s conclusion that “. . . a degree of interdisciplinarity . . . need not and should not result in a weakening of theoretical/analytical rigor” [8.3], perhaps rewriting to read, “A degree of theoretical/analytical rigor should not result in a weakening of interdisciplinarity.”

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#### Gary S. Karpinski, “Lessons from the Past: Music Theory Pedagogy and the Future”

#### [10] Response by Allen Forte

[10.1] I am sure that many of us relate to the issues discussed so cogently in Prof. Karpinski’s fine essay. I know I can, having spent many happy hours with eager and not so eager undergraduates who are discovering some of the intricacies of tonal music theory. In my response I will deal with only four of the six topics covered by Prof. Karpinski, namely, counterpoint, figured bass, harmony, and curriculum design.

[10.2] Styled or Renaissance counterpoint became widely taught in the U.S. after the publication of the translation of Knud Jeppesen's *The Style of Palestrina and the Dissonance* in 1927,<sup>(12)</sup> followed by his 1939 textbook on 16th century counterpoint.<sup>(13)</sup> Many composers teaching theory, which was the norm in those days, adopted this approach and taught species counterpoint with reference to the vocal repertoire of the high Renaissance, as formalized in Jeppesen's rules, which corresponded closely to those of Fux. Counterpoint courses, justified as an essential and traditional discipline, became standard in many music departments. There was always the question of student interest, however, inside as well as outside the academy. George Gershwin became bored with the exercises that Henry Cowell assigned him, but whether that was Henry's fault or George's remains moot, although George was known to have a short attention span. Cole Porter, on the other hand, preserved his extensive French species-like counterpoint exercises, which are now at Yale.

[10.3] The list of eminent composers who studied species counterpoint is long. Prof. Karpinski has mentioned Mozart and Beethoven, but there was also Haydn and Brahms and of course many others. But it was Heinrich Schenker who understood the basic reason for the importance of counterpoint study: it forges a link between the fundamental models of musical motion embedded in species exercises and the often large-scale expansions and elaborations of those models in free composition. This beautiful idea of course creates a bridge between high-level music theory and elementary instruction, serving as a model for music theoretic studies in general.<sup>(14)</sup>

[10.4] Why, then, is counterpoint instruction in decline? I suggest the lack of qualified teachers as a major reason, teachers who themselves can do counterpoint and teachers who can make the study interesting and meaningful to students. Harmony seems so much more accessible, so much easier for the teacher equipped with the right textbook, that is, the one that tells you what to do next, and of course students who do not expect too much from theory are unaware of what they are missing.

[10.5] When it comes to figured bass, I am very old fashioned. As everyone here knows, this notational system was intended originally for performers, but gradually became a standard way of introducing students to music as nascent composers. It is a wonderful way of obtaining immediate and tactile access to counterpoint, construed as tonal voice-leading, and to harmony, construed as constellations of intervals arranged vertically.

[10.6] As in the case of counterpoint, why is figured bass not taught as a standard course? Once again, there is the matter of teacher competence. How many teachers can actually play from figured bass, let alone improvise over a given bass? Also, mass instruction in figured bass is out of the question. In my experience, individual lessons are the best way of experiencing this basic discipline, unless, of course, one of Gary Karpinski's machines, schooled in Artificial Intelligence, comes along to solve that problem!

[10.7] Since my own harmony textbook has been out of print for some time, my remarks in this area may be somewhat out of control. I will say, however, that most harmony books are deplorable, beginning with the absence of any stated purpose for publication of the book in the first place. With Prof. Karpinski, I would also emphasize that in many textbooks, excluding of course Aldwell & Schachter, there is a failure to distinguish between harmony and voice leading, a failure that in the worst case leads to the dismal preparation for advanced analysis that those of us teaching graduate students routinely experience. As Prof. Karpinski points out, this is not a new problem. And in this connection I might cite Brahms's statement in a letter to Hugo Riemann: "You would not believe what I had to put up with from incompetent textbooks and how I had to unlearn everything."<sup>(15)</sup>

[10.8] Prof. Karpinski correctly identifies the movement of largest scale concerned with curriculum design in this country: the Comprehensive Musicianship Program of the 1960s, funded by the Ford Foundation. In its defense I should say that many dedicated and idealistic musicians were deeply involved in the program, which, thanks to Ford money, was able to sponsor workshops that disseminated CMP philosophy across the country. However, the problems Prof. Karpinski outlines were and are real. His integrated curriculum sounds wonderful, although I am concerned about its practicality, simply in terms of available faculty in a small music department such as we have at Yale.

[10.9] As my final word on this topic, I would recall Prof. Karpinski's rueful close: ". . . but none of this [referring to the adoption of new methods, new technologies, etc.] will matter very much at all if we have nothing of real value to impart" [Karpinski 8.1]. In reflecting upon this statement, it occurred to me that perhaps the best way of measuring the real value of our teaching is to try to imagine the degree to which it will have a positive effect upon the musical activities of our students in the future, which is after all not such a vague undertaking, because it is supported by our own experience as students and, increasingly over time, by our experience as teachers.

[11] Response by Allen Forte

[11.1] Before I say a few words inspired by Patrick McCreless’s paper, I would like to introduce an excerpt from a relevant article published in the recent past. The topic of the article is high-level, academic music criticism:

Criticism does not exist yet on the American music-academic scene, but something does exist which may feel rather like it: theory and analysis. The shadowy, fluid state of the field; its problematic relation to analysis; the sporadic nature of the published material; the small number of practitioners—these features certainly make it hard to get hold of and discuss with confidence. However, I think we realize that those practitioners, if they ever paid the least attention to traditional musicology, might fairly claim achievements ranking in rigor and importance with those of the historians. Theory and analysis are not equivalent to criticism, then, but they are pursuing techniques of vital importance to criticism. They represent a force and a positive one in the academic climate of music, and tactful efforts should be made (on both sides, let us sincerely hope) to arrange a rapprochement with musicology.<sup>(16)</sup>

[11.2] This remarkable view of the field of American music theory as it was then emerging has of course changed quite radically, and one wonders exactly what the author read in those early signs of vitality, which had not yet peaked on that glorious fall day in 1977 at Northwestern University, when a few of us proclaimed our independence from Musicology and led the way to the founding of the Society for Music Theory. At any event, according to Prof. McCreless, we ought to pursue the rapprochement so strongly advocated by the author of my quotation, and, in certain respects, I must agree with him.

[11.3] A major question concerning the pursuit of a “rapprochement,” however, is the extent to which we then weaken one of the major characteristics of American music theory that has drawn international attention. I refer to the rigor, precision, and logic of the more abstract studies in the field, many of which have been published in *Journal of Music Theory* since its inception in 1957 and, later, in the society’s journal, *Music Theory Spectrum*. I am especially concerned here with the nature of the field as it will be determined by present-day graduate students in future years, with the perception of music theory as the handmaiden of musicology (its role in European musicology), especially if it is radically reshaped by the new musicologists. I know that this is a dangerously exaggerated statement of course, and I only make it to reinforce my point, which is that music theory needs to preserve its essential independence, no matter what Leo Treitler, Lawrence Kramer, or other eminent scholarly figures say!

[11.4] Except for omitting this important feature of American music theory, Prof. McCreless’s description of the distinction between the interests of the musicologists and those of the theorists around, say, 1960, is apt and concise. In particular, the question of repertoire, which he raises, is an important one. Upon reflection it occurred to me that many of the individuals interested in music theory in its formative days had a background in musical composition, hence were involved in the problems of 20th-century music and its structural immediacies. It is also true that the horizons of music theorists have been considerably uplifted, both with respect to target repertoires for music theoretical and analytical applications as well as in relation to music-historical events that pertain to music theory. The recent movement called Neo-Riemannian is a case in point, and the exegesis and reinterpretation of Rameauian thought is another.

[11.5] The crux of Prof. McCreless’s paper, concerns the point at which musicology and music theory most profitably meet. I quote his eloquent description of that confluence: “. . . at the joint where analysis shades into interpretation, where structure shades into hermeneutics” [McCreless 5]. A little further on he writes: “It is . . . at this juncture of analysis and interpretation that historical awareness can best deepen and enrich the analyses that we do as theorists, and that analysis can best deepen and enrich the critical interpretations that we do as historians” [McCreless 5].

[11.6] As an instance of historical awareness, we have heard Prof. McCreless’s sensitive and perceptive reading of the opening portion of Shostakovich’s *First Quartet*, and I need not comment upon it, except to suggest that it may not be what the new musicologists would have in mind as a contribution to a rapprochement. There is no literary criticism, no narrativity, no deep hermeneutics, and no sex.

[11.7] Finally, it seems to me that when we consider the juncture of analysis and interpretation or structure and hermeneutics the elemental question remains: how much does each contribute toward illumination of music? The answer to this question should perhaps be left open until the rapprochement is achieved. But because the new musicologists have left no doubt about their scorn for formalized music theory and structural analysis, they have created a substantial crevasse that would

have to be traversed as part of any serious effort toward rapprochement, unless of course music theorists simply jump without looking.

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## Footnotes

1. There is a discrepancy here between Dubiel's NECMT paper, and that paper as published in MTO 6.3. In [Dubiel 12] we read "The received view isn't even *antithetical* to these experiences . . ." whereas in the corresponding location in the NECMT paper he writes: "The received theory isn't antithetical to these experiences . . ." I would not mention this rather trivial discrepancy were it not that Prof. Dubiel so vehemently denies having used the expression "received theory," asserting that ". . . what I call the 'received view' in my talk . . . is the *only* thing I ever call 'received'" [2.5].

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2. The first definition of "received" given by the *Oxford English Dictionary* is: "Generally adopted, accepted, approved as true or good." I believe that Prof. Dubiel uses the term in a mildly sardonic way, but I could be wrong.

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3. Allen Forte, "Banquet address: SMT, Rochester, 1987," *Music Theory Spectrum* 11/1 (Spring 1989): 95–99.

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4. *The Oxford English Dictionary*, 2nd ed. Oxford: Clarendon Press, 1991. Vol. 2, p. 838.

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5. Ibid.

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6. Joelle Welling and Cynthia I. Gonzales, "The Music Theory Job Market from 1985–86 through 1997–98," *College Music Symposium* 39 (1999), 119–21.

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7. Welling and Gonzales, "The Music Theory Job Market," 111.

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8. *On the Market: Surviving the Academic Job Search*, ed. Christina Boufis and Victoria C. Olsen (New York: Riverhead Books, 1997) documents the grim condition of the academic job market in general through recent Ph.D. recipients' personal accounts.

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9. Allen Forte, "Banquet Address: SMT, Rochester, 1987," *Music Theory Spectrum* 11.1 (1989): 98.

[Return to text](#)

10. Cary Nelson and Stephen Watt, *Academic Keywords: A Devil's Dictionary for Higher Education* (New York: Routledge, 1999), 292.

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11. John Covach and Graeme Boone, eds. *Understanding Rock: Essays in Musical Analysis* (New York and Oxford: Oxford University Press, 1997).

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12. Knud Jeppesen, *The Style of Palestrina and the Dissonance*, with an introduction by Edward J. Dent; trans. by Margaret W.

Hamerik (Oxford: Oxford University Press, 1927).

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13. Knud Jeppesen, *Counterpoint: The Polyphonic Vocal Style of the Sixteenth Century*, trans., with an introduction, by Glen Haydon (New York: Prentice-Hall, 1939).

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14. Heinrich Schenker, *Counterpoint*, trans. John Rothgeb and Jurgen Thym, ed. by John Rothgeb (New York: Schirmer Books, 1987).

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15. Michael Musgrave, *A Brahms Reader* (New Haven: Yale University Press, 2000), 80.

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16. Joseph Kerman, "A Profile for American Musicology," *Journal of the American Musicology Society* 18 (Spring, 1965): 61–69. Delivered at a plenary session of the 30th annual meeting of the AMS, Washington, D.C., Dec. 27, 1964. I was there and remember the furor that ensued.

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