



Reply to Brown and Dempster

Author(s): Richard Taruskin

Source: *Journal of Music Theory*, Spring, 1989, Vol. 33, No. 1 (Spring, 1989), pp. 155-164

Published by: Duke University Press on behalf of the Yale University Department of Music

Stable URL: <https://www.jstor.org/stable/843670>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



JSTOR

Duke University Press and are collaborating with JSTOR to digitize, preserve and extend access to *Journal of Music Theory*

REPLY TO BROWN AND DEMPSTER

Richard Taruskin

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

There is an unearthly aura of born-yesterday about this jeremiad. The authors really seem to be convinced that they are telling us all things we’ve never dreamt of. They really seem not to know how much wider an airing the D-N model was given than their citation of a few Princeton dissertations implies. They seem not to know about Arthur Mendel’s famous pitch to the International Musicological Society in 1961 on behalf of Carl

Hempel and his covering laws (it was published as “Evidence and Explanation” and for at least a decade or two served as pabulum for musicology proseminars across the land). They seem not to know about alternative models of scientific theorizing, like Karl Popper’s or Paul Feyerabend’s—or if they do, they are not letting on (confident, perhaps, that *we* don’t). Mainly, they seem not to know that their ideas, far from new to us, have been considered and long since rejected as useless; that, in the opinion of those whom they would instruct, their model of scientific music theory is a music theory from which all that is interesting and relevant has been surgically pruned away.

But first, some basic matters of responsibility and straight-shooting. I dare say there is not one among the many writers Brown and Dempster have taken on whom they have quoted or interpreted accurately. I will leave it to the exponents of D-N to rectify the misrepresentations of their position. In my own case, the misrepresentations have been as follows.

Because I have expressed the view that a composer’s “theoretical environment” offers clues to an appropriate analytical approach to his music, I am saddled willy-nilly with responsibility for the view that sketches, which I never mentioned, invariably provide “neutral tests for theoretical claims.” And this makes me Brown and Dempster’s chief abuser of the intentional fallacy. As one who has had frequent occasion to deplore the mindless and mechanical way in which sketches are described by musicologists who then attempt to pass their descriptions off as an account of a work’s “genesis” or its “compositional process,” I find Brown and Dempster’s characterization of my position somewhat bizarre. In my own work I have occasionally used sketches—but only as a means of testing empirical statements (“observables,” if you will), not theoretical ones. It was Stravinsky’s sketches for *The Rite of Spring* that gave the empirical lie to his claim that only the first melody in that ballet was based on a folk tune.¹ And it was his sketches for many vocal pieces, from the *Japanese Lyrics* to *The Rake’s Progress*, that gave the empirical lie to the numerous critics of his work who have contended that his distortions of prosody in French, Latin, and especially English, were the result of his ineptitude.² Sketches, too, can often be an aid to textual criticism, which is another sort of empirical investigation that musicologists are often called upon to make. Thus the sketches for the first of the *Pribaoutki* reveal that the oboe part as published is missing a key signature of three flats;³ when this is supplied, it turns out that the closing cadenza for that instrument is referable to one of the octatonic collections—a not inconsiderable “theoretical” point, to which I shall return, but one for which the sketches provided merely an empirical corroboration of a theoretical surmise. The latter had its origins not in a fishing expedition among the sketches, but in a carefully controlled study of that “theoretical environment” which to a very limited extent had been “universalized” and made “predictive.” It had nothing to do with what our authors call “intentions.”

Similarly, a sketch by Rimsky-Korsakov that contains explicit verbal annotations showing his awareness of the octatonic frame of reference out of which the “*Petrushka*-chord” emerged, contributes an important historical fact to a “theory” of Stravinsky’s point of stylistic departure.⁴ It is not, nor would I ever claim it to be, in itself a theoretical insight, nor is it a “test for theoretical claims,” except in a trivial definitional sense (facts being by definition what theories make coherent, it follows that theory by definition orders facts, and facts, by definition, test theories).

Since I have never said or implied that “*only* considerations of style, historical background or sketches can constitute corroboration” for analytical inferences (I have italicized the distorting word), perhaps I don’t have to disavow before my present sophisticated readership the childish corollaries our authors attribute to me. As one who has for years been drawing fire precisely by reason of my unwillingness to accept Stravinsky’s testimony about his life and work at its face value, I am rather amused to be instructed that “the composer can be ignorant or confused about the underlying structure of his compositions.” Because I have ventured to demonstrate that a congruence subsists in syntax and methodology between Stravinsky’s earliest attempts at composition and his teacher’s longstanding habits and devices (and why this should have surprised anyone continues to baffle me), I stand now accused of purveying “Rimsky-Korsakov’s view (!) of octatonicism” as the single, self-sufficient standard for evaluating Stravinsky’s music. After insisting for years now that we have to find our own explanations for things, not just rely on fashionable authorities, I am apprised that “what passed for an explanation in earlier ages often does not and should not pass for an explanation today.” (Thank you, gentlemen, but consider: can we sensibly claim our present viewpoint superior unless we know past ones?)

The Rimsky-Korsakov “view” is then (mis)characterized as “the idea that the properties of harmonic systems depend on scale type.” This is neither Rimsky-Korsakov’s view (so far as I know it) nor mine. A great deal of space is devoted, in the article from which Brown and Dempster derive the “view” they impute to me and my Russian forerunner, to demonstrating that a certain harmonic routine (the “circle of minor thirds”) was anterior to, and preconditioned, the scale-type in question (as the circle of major thirds preconditioned the Russian use of the whole-tone scale). And in more than one place by now I have stated explicitly my view that mere “referability” to some scalar construct proves nothing of value about a piece of music, whether the object of investigation is “style” or “structure.” What one hopes to achieve by means of analysis is not merely a taxonomy of musical configurations, but insight into *praxis*: methods, routines, devices of composition. Since one cannot claim to understand a praxis unless one can state the theoretical basis on which it rests, one is going to be interested in historical viewpoints as well as any more efficient ones we may now propose.

Though it may be true that “we can establish a tonic without using all scale tones,” is that not merely because we already know the scale that underlies the cadential progressions Brown and Dempster adduce? Can we imagine a listener who would recognize “the progression I-V-I” who would not also recognize the major and minor scales and understand the relationship between the scales and the chords? And is enunciating a cadential progression the same as “determining the tonality of a piece”? To return to Stravinsky, is it by looking at his cadences—what van den Toorn calls his “terminating conveniences”—that we may arrive at an understanding of his tonal practices?

One more question: is it not possible to “establish” an octatonic tonality, as, according to Brown and Dempster, one can establish a diatonic one, “without using all scale tones”? Is the octatonic identity of the opening of Stravinsky’s “The Drake” (*Quatre chants russes*, no. 1) in doubt simply because only six tones are in use (example 1)? An affirmative answer, in my opinion, can only testify to an unwillingness to build up from individual “observables” a mental model of “octatonicism” analogous to the mental model on which our tonal “hearing” depends. And this unwillingness, in turn, can only stem from the notorious propaganda that the works of certain approved twentieth-century composers are entirely “unique” and *sui generis*. It is time to ask, what is this thing called “particularism,” of which I am taken to be a devotee?

According to Brown and Dempster, I subscribe to an ideology which “holds,” among other things, “that a particular work of art can be understood—indeed, should be understood—in complete isolation from the classes and kinds of which it is a member.” But I don’t hold this at all. And neither do most of Brown and Dempster’s “particularists”; for we are, by and large, rational beings, and the statement attributed to us is not only untenable, it is irrational. The only way such an understanding could be gained would be by one of “mystical and emotive acts” enumerated by our authors at the outset of their disquisition as being outside its scope. One of Brown and Dempster’s arch-particularists has recently come right out and confessed, “You can’t possibly regard a piece as totally ad hoc, and you can’t possibly regard a piece of music as having nothing to do with anything but itself.”⁵

The only “Princeton theorist” whose quoted statement might seem in any way to support the position here attributed to particularists as a breed is Peter Westergaard, who has asserted that the theorists he knows “don’t seem to care much about comparing or grouping pieces,” preferring rather to consider individual cases and attempt to elucidate “the syntactic assumptions we use in understanding such structures.” He made this remark to a gathering of historical musicologists at a session devoted to “Style Criticism,” one of the old, now discarded, shibboleths of that discipline, and his objective, clearly, was *épater les bourgeois*. Restating his position in

The image displays a musical score for a piano piece. The upper system consists of two staves: the top staff is in treble clef with a 7/8 time signature, and the bottom staff is in bass clef with a 7/8 time signature. The piano part in the bass clef is marked with the instruction *(sempre)* and features a steady eighth-note accompaniment. The upper staff contains a melodic line with a long slur spanning across several measures, including a half note with a fermata. The lower system shows a single treble clef staff with a sequence of notes, some of which are marked with a sharp sign (#).

Example 1

tranquillity, I would wager he would acknowledge that “comparing and grouping” is indeed a part of what the theorists he knows do, but that it is done at a stage prior to engagement with the individual piece, and in fact largely conditions the “syntactic assumptions” on which his brand of analysis depends.

A moment’s reflection will confirm that we cannot make any observation at all—indeed, we cannot know anything at all—except in terms of comparison. Without knowing “big” we cannot know “small.” Without knowing “hot” we cannot know “cold.” Without knowing “Romantic” we cannot know “Classic.” And, of course, without knowing “atonal” we cannot know “tonal.” Nor do these strictures apply only to “qualities.” Even quantitative knowledge, the only kind positivists recognize as direct and therefore “real,” depends on comparison with units of measurement.

Therefore, when most music theorists or analysts of historians use the word “unique” to describe the works with which they engage they are speaking metaphorically or hyperbolically. What they are really saying, if I may speak for them, is that what attracts and interests them are “the divine details” (as Nabokov called them), considered in and of themselves (essentially, as Brown and Dempster point out, a mute sort of apperception), as well as insofar as they may compare and contrast with the details of other pieces (whether or not the comparison is made explicit in the eventual discourse), and, finally, as they relate to such larger “grouping” models (i.e., models of sameness) as “structure” (if they are “theorists”) or “genre” (if they are “historians”) or both (if they are human). To construe the word “unique” literally for purposes of characterizing—and, ultimately, discrediting—“particularism” is as dishonest a forensic tactic as construing literally the word “hear” in an effort to turn back the perennial challenge put to the theorists and analysts of serial music: “But can you *hear* it?”

Meanwhile, what I take to be the essential task of theory-cum-analysis is in fact adumbrated, if only dimly and unwittingly, within Brown and Dempster’s senseless definition of particularism, when they speak complacently of “the classes and kinds of which [the individual piece] is a member.” The trouble is, they don’t seem to see this as a problem. And yet for me it is *the* problem. How do we make determinations of “class” and “kind” for the pieces we analyze? All too often such determinations depend on unexpressed biases involving tacit if not covert appeals to received opinion, *parti pris*, or mere convenience. To the extent that I have practiced “analysis” in my own work, it has been in an effort precisely and explicitly to determine—to bring, if you will, to consciousness—what the “classes and kinds” are that a given piece belongs to. For me the really pressing problem in Stravinsky research has always been that of placing his music in a truly relevant context. Hence my emphasis on that “theoretical environment.” I am looking for the precise, yes, the *particular* common practice against which Stravinsky should be measured so that what is truly his own

can best be isolated, and so that it may be understood in a manner that does justice to its particularity.

I certainly do not eschew generalizations, just generalizations that are facile and empty. Nor do I see how a practice based on empirical historical research is any more “subjective and *ad hoc*” than one based on “general laws of music.” It certainly doesn’t amount merely to asserting my opinions on the basis of my vaunted musicality or my superior “aural sensibility and musical judgment.” Yet if it does not surprise me it still pains me to find our lofty generalizers stigmatizing “connoisseurship” with a taunt; for connoisseurship is real. It is based on work and the hardwon acquisition of knowledge, and those who have done the work have indeed got a right to claim superior powers of discrimination over those who have not. To deny that superior knowledge can “privilege” the perceptions of the better informed over those of the uninformed is obscurantism.

The knowledge I seek, and of which I speak, is of course a historical sort of knowledge, which condemns it in the eyes of those who would rather operate by fiat than by research. Works of music are created by the time-bound for the time-bound, and we cannot just decide that we, or the works we study, are outside history—unless, that is, we can propose and defend some equally tenable criterion of relevance. But relevance seems to matter not in some towns, and I see that Rochester is one of them.

History, I should add, is not merely chronology. Synchronic time-slicing is the same kind of cynical non-method as is the kind of vertical texture-slicing employed by analysts who have no principle to guide their segmentation and don’t want to do the work it would take to get one. Measuring Stravinsky against “the common practice of the early twentieth century, a common harmonic practice shared by Stravinsky, Scriabin, Bartók, Webern, Berg, and Schoenberg, among others,”⁶ cannot lead, in my view, to an understanding of Stravinsky’s music, or of anyone else’s; for such an oceanic “common practice” has been merely asserted, not adduced from observation.

So I prefer to go on mapping my little streams rather than attempting to navigate the deep blue sea. And hence I remain more than skeptical of a “scientific music theory” that aspires to promulgating “general laws of music” *ex cathedra* and making assertions of universal validity from the comfort of one’s armchair. For “universal” is invariably at odds with “relevant.” As far as I am concerned, the pursuit of universals is the pursuit of irrelevancies, and universal truths—“predictive” truths, if you will—are practically by definition trivial truths. “Music theory . . . should not be required to do more,” our authors admonish us. Why, then, should we waste our time with it?

To say the very least, I get no sense at all from Brown and Dempster of how their vague proposals will “clarify the nature of music” for me or how it will “guide [my] musical activities.” I don’t even know what such expres-

sions could possibly mean. All I see is an exhortation to abandon the project of explaining or interpreting individual compositions (hence to give up the practice of analysis entirely) without any inkling of a compensatory gain. Pluck up, we are told; we will not be “deprive[d] . . . of all understanding of particular works.” But we will understand them in a properly scientific way, we are also told, “only so far as [they are] instances[s] of some general kind.” This is no better, as far as I am concerned, than the old, indeed antiquated, model of the musicologist as style critic, who was interested “not . . . in the individual case as such, but rather in discerning its typicality.”⁷ (The quaint confidence with which our authors proceed to resurrect the old style-critical ideal of authentication purely on the basis of “objectively” observed internal evidence is something merely to be noted with amusement, not seriously refuted.) Finally, we are instructed that if our theorizing about the “particular works” we love is to hold water, we are “obliged” to give equal time to what disgusts and bores us. Strangely, we are not consoled.

After all, one man’s baby is another man’s bathwater. “Masterpieces” and “genius” are Brown and Dempster’s expendables, as the derisive quotes in their last paragraph announce. All a piece of music is to them, in their professional capacity, is an “entity” to be “manipulated.” Like others with whom I have had occasion to debate, they want to see and to treat musical works as if they were rocks or ferns or subatomic particles—God’s creations. But of course they are not that; they are creations of God’s creatures, products of culture, coded with human values, expressive of human volition, agents of some form of human communication, individually as well as in the aggregate. I willingly forgo the bliss of scientific certainty in treating human documents if the price has to be their dehumanization.

It is a question, as I say, of how one wants to spend one’s time. And in the end it is only doctrine, not reason, that postulates some necessary and overriding virtue in seeking “general laws of music.” Nowhere do Brown and Dempster adduce any reasons to show why such legislation is desirable or why it should be obligatory. (All they can do—and frequently!—is feign ignorance of reasonable argument to the contrary, the oldest trick in the sophist’s book.) For theirs is a mandate of faith. To share the faith one must indeed “become one with” the authors, as they put it in their first paragraph. There is no rational road to such a persuasion. Its foundations are obsessional, possibly megalomaniacal. In any case, they are not logical.

It is not surprising, then, that the one practical demonstration our preceptors deign to offer of their brand of “scientific music theory” in action turns out a hopeless muddle. Their attempt to support Schenkerian analytical propositions by “invok[ing] independent laws,” for all that it purports to demonstrate how circularities may be avoided, is in fact the most excruciating circular argument I have ever seen advanced with ostensible seriousness. Their “empirical tests” rest on all sorts of unreduced metaphors (“tonal

motion,” to begin with), “non-obvious structures” (what gives “mixtures” or “tonicizations” any more secure an empirical standing than “a middle-ground, or an implied dominant, or a nexus set?”), and cultural biases (they will have to be mighty careful just what naive listeners they select for their “broad cross section”).

But beyond all that, the whole argument reduces to one horrendous tautology, because the concept of a “tonal piece” has been accepted as a primitive. Given a tonal piece, the authors bravely assert, we can prove that it is tonal! Their sample cannot even be selected without already knowing everything the exercise sets out to prove. And what will it contain? *Missa Pange lingua*? *Pelléas & Mélisande* (anyone’s)? The *Symphony of Psalms*? If not, then on what basis? On the basis, obviously, of the stipulated definitions that in the left-hand column of Table 5 are masquerading as “fundamental laws.” Yes, our preceptors have been asserting as laws a set of definitions. Just the thing they had avuncularly (and rightly) cautioned us never to do.

Well, enough. In my opinion this paper sets a good example of how not to engage in philosophical and methodological debate: with presumption, with unexamined assumptions, without a grasp of the scope of the problem or its history, without having made fundamental and necessary distinctions, without properly understanding the arguments one has undertaken to rebut, and without advancing a viable alternative.

NOTES

1. R. Taruskin, "Russian Folk Melodies in *The Rite of Spring*," *Journal of the American Musicological Society*, XXXIII (1980), 501-43.
2. Idem, "Stravinsky's 'Rejoicing Discovery' and What It Meant: In Defense of His Notorious Text Setting," in Ethan Haimo and Paul Johnson, eds., *Stravinsky Retrospectives* (Lincoln: University of Nebraska Press, 1987), 162-99.
3. See idem, "Forum: Reply to van den Toorn," *In Theory Only*, X/3 (October 1987), 51.
4. See idem, "*Chez Petrouchka*: Harmony and Tonality *chez* Stravinsky," *19th-Century Music*, X/3 (Spring 1987), 268-69.
5. Milton Babbitt, *Words About Music* (Madison: University of Wisconsin Press, 1987), p. 4.
6. Allen Forte, "A Hymenopteran Response," *Music Analysis*, VI/2-3 (July/October 1986), p. 329.
7. Mark Hoffman, "Reimann, Hugo," *New Grove* XVI:5.

