History and Structure in Richard Cohn's *Audacious Euphony*

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The appearance of Cohn's *Audacious Euphony* provides us the occasion to reflect upon both the remarkable achievements and some of the limitations of the Neo-Riemannian movement, the theoretical tradition that largely orbits around a number of Cohn's writings. Certainly, Cohn has benefitted greatly from his principle interlocutors—initially David Lewin and John Clough (to whose memories the book is dedicated) and more recently Dmitri Tymoczko—but the movement's energy and direction has come from Cohn himself. It would not be inappropriate to view the Neo-Riemannian enterprise as basically Cohn's work and the various responses to it. Accordingly, it is also our great fortune that Cohn's book appears at roughly the same time as the *Oxford Handbook of Neo-Riemannian Music Theories* (Gollin and Rehding 2011), and reading each book in light of the other is a particularly rewarding experience.

Like all great books, *Audacious Euphony* cannot be absorbed all at once or apprehended as a single graspable image. The range of concerns Cohn takes up is much wider than the formulation of a workable technology that addresses certain musical phenomena. The way he develops that technology and the values it embodies, along with his claims for its importance, go to fundamental concerns of the discipline. In other words, along with the task of defining and advocating an analytical approach, the book implicitly outlines what Cohn believes to be the respectable goals and procedures of contemporary music theory. As such, one needs to allow its sentences to interact, internalize their claims, work out the consequences, experience the contradictions that emerge. And then start over again. Alternatively, one can always read the book principally as a series of richly detailed analyses, without concerning oneself with the book's several narratives: to my mind, the analyses work best this way, and their particular musical claims become clearer and more engaging, because they are unburdened from the distracting task of staging one technical demonstration or another.

Cohn's book has both a technological and historical orientation and makes claims in which technology and history are deeply entangled. We can synopsize these claims in the following way. Conventional diatonic theories of tonality are defeated in their attempts to relate harmonies by a hefty number of passages in 19th-century musical literature. Atonal pitch-class theory can help us discover new ways of relating triads. The "pan-triadic" model that emerges from this research provides us with engaging analyses. Moreover, we learn that triads have unique properties that instantiate central, conventional musical-structural values—values derivable by reading relevant 19th-century music theorists and by examining the relevant musical repertoire. The pan-triadic model that emerges is so structurally interesting and powerful that we can read the development of music in what Cohn refers to as "the long nineteenth century" (and perhaps even larger stretches of musical development in the West) in its terms. The model exists comfortably as a companion to conventional theories of tonal syntax rather than as a through-going replacement of them. As a result, we can regard the corresponding repertoire as governed by various kinds of interactions between two grand systems of coherence: one conventionally tonal and one pan-triadic.

Among Cohn's goals in writing the book is to assemble his earlier Neo-Riemannian writings into a comprehensive package accessible to a reasonably wide readership. A second important goal is to answer the various criticisms that have accumulated over the years. The criticisms that interest Cohn most are those lodged against the interactions between the tonal and pan-triadic systems (the final sentence in my synopsis above), and they are largely technological rather than historical in nature, namely, that it is a weakness of his approach that it asks us to switch between two distinct logics within a single piece of music. There are other criticisms, which are largely aimed at the way history operates in Cohn's approach.

1. Cohn's Starting Points

Given the double register of history and technology, it is appropriate that Cohn's book has in fact two starting points: the

first, more historical in orientation, centered suitably enough on a passage by Riemann, a figure who seems simultaneously to intrigue and repel Cohn; and the second, more technical in orientation, focused on a passage from Schubert's B♭ Piano Sonata. Both starting points merge quite quickly into a single trajectory. Despite this convergence, it will be worthwhile to examine each in turn.

To open his book, Cohn reflects on a passage from the entry on tonality in Riemann's *Musiklexikon*. The passage Cohn quotes is well known and has been treated fairly thoroughly by others. Somer (1995) mentions it in passing and disapprovingly; Harrison (1994) and Kopp (2002; 2011) place the passage against related excerpts in Riemann's *Skizze einer neuen Methode der Harmonielehre* and *Katechismus der Harmonielehre*. More recently, Alex Rehding (2011) cites it in his study of Riemann's theory of function, placed in the context of the problematic relationship between theory and its associated repertoire.

In the passage, Riemann presents us with a progression of five major chords: C–E–C–A♭–C. He argues that the progression defeats earlier, scale-based theories, which are obliged by the concept of key (*Tonart*) to take one of two strategies in analyzing the progression: the theory of modulation or some theory of modal mixture (1919, s.v. "Tonalität"; 1880, 67–68). By replacing the older concept of key with a system of relations to the tonic triad, Riemann says we are able not only to comprehend "die zwar kühne, aber kräftige und wohlklingende Folge" but also to assert that the chords are closely related (1919, s.v. "Tonalität").[1]

Riemann's discussion provides Cohn two questions that frame his enterprise (as well as provides the title of his book): Why does Riemann understand the chords as closely related? And if the chords are indeed closely related, why does Riemann call them "audacious" (ix)? The questions are interesting, although I think Cohn poses them rhetorically, at least at first, since Riemann tells us quite explicitly (here and in other passages) why he regards the chords as closely related. In other words, if one actually wanted to know the answer to the two questions, one could consider the

[1] Cohn translates "die zwar kühne, aber kräftige und wohlklingende Folge" as "the admittedly audacious but effective and euphonious progression" (ix).

entire passage and read it, as Kopp and Harrison do, in connection with related passages in Riemann's work. Riemann's point, in short, is that earlier theories of tonality, bound by the system of diatonic scales, cannot accommodate progressions like the one he cooks up, which are not diatonically bound. That constitutes a failure for scale-based theories of tonality: the chords in the progression are not indefinable but are related by what Riemann considers a dead normal relationship, namely, his *Terzschritt*, a triadic transformation isomorphic to the interval of a major third, which, along with the perfect fifth and the octave, is a fundamental building block of the consonant triad. Yet, Cohn's interest in the passage lies elsewhere: in spite of his two questions, Cohn is more interested in Riemann's characterization ("audacious" yet contains "closely-related" chords) of the progression—which Cohn sets as the animating paradox for his work— rather than Riemann's particular explanation of it.

Cohn's second starting point is a passage from Schubert's B♭ Piano Sonata. Cohn draws from mm. 217–53 a progression of triads that represents the sequence of the local key areas: B♭–G♭–f♯–A–B♭. Before we can discuss the harmonic logic of the progression, we need to observe the appearance of an enharmonic translation between G♭ and f♯. The translation is an instance of what Ludwig Bussler calls apparent or external enharmonicism, since it comes about solely for ease of writing and reading ("Bequemlichkeit des Schreibens und Lesens") (Bussler 1886, 118–19). We know this because if we begin the progression a chromatic semitone higher, on B♮ (a technique Bussler recommends in such cases), the same notational principle will produce the sequence B–G–g–B♭–C♭. And that makes more sense harmonically, at least as we travel from chord to chord in the progression. But now there is a notational discrepancy between the first event in the sequence and the last. The problem, as Cohn sees it, is that the progression involves a contradiction that defeats conventional tonal analysis, which will be compelled to label the final chord ♭II even though we know it ought to be labeled I. Cohn writes that "[n]o amount of logical sophistry can dislodge us from the conviction that the final chord of the progression represents the tonic degree, not the doubly flatted second" (3).

Yet where does this conviction come from? From another principle of conventional tonal theory: that local keys work to express the grand global key (2). That principle dictates that there is an identity between the first and last events in the sequence, which our roman numeral analysis must reflect. Approaching the Schubert passage from this perspective, we begin by progressing from the major tonic to a major chord borrowed from the tonic's parallel minor; we then hear the minor form of that chord, followed by its relative major. That chord is transformed into a dominant, which resolves deceptively to ♭VI, and which in turn becomes the tonic. None of this seems terribly odd, especially since the relationships from one state to the next in the sequence are conventional diatonic relationships. Yet when we examine the combination of these conventional relationships, comparing the first and last chords, things go south. We can experience this quite keenly when we attempt to apply the relevant roman numerals: I–♭VI–♭vi–the relative major of ♭vi, which is, what, ♭I? That chord becomes V^7 of either ♭IV or ♭iv, and resolves deceptively in the key of ♭iv, which means we're now at ♭♭II!

At this point it is important to bear in mind that the problem here is entirely technological in nature. It emerges in the course of dutifully applying the regulations of conventional roman numeral analysis. If we are stuck with that style of analysis, we are compelled either to abandon what we might call the principle of Big Diatony, the principle that a grand global key governs an entire piece; or to abandon what we might call the principle of Small Diatony, the idea that conventional diatonic logic governs the relationships from one harmonic state to the next. (We might be a bit more precise in our scaling of Diatony, reserving "small" for conventional chord-to-chord relations, and introduce the term Midsize Diatony for the principle that governs the relationship from one key area to the next). In the current musical context, we abandon the principle of Big Diatony if we accept that the B♭ beginning the progression is fundamentally different from the notated B♭ that concludes it, and that the progression begins with I and ends with ♭♭II (if we are analyzing the passage with roman numerals). That strategy certainly resolves the technological contradiction. Yet it raises new questions that are aesthetic or

cultural-critical in nature. And once the discussion leaves the domain of technology and enters that of aesthetics or cultural-criticism, one can raise and lower the stakes involved as one likes. For instance, we can simply find the results compositionally interesting—even really interesting; or we can regard the passage as, say, a structural enactment of the Hegelian critique of the classical law of Identity. Cohn himself links the results to both historical and current evaluations of Schubert's harmony (in general) as aimless, random, disjunctive, irrational, or arbitrary (4), which some writers judge as bad form, while others—Cohn has post-structuralist musicologists in mind—regard it as heroically transgressive (4).

Before examining Cohn's own resolution of the contradiction, it will be worthwhile to briefly note the views of certain 19th-century theorists on the problem. Rehding's (2011) recent discussion of Riemann's function theory is on point here. He cites Weitzmann's, Riemann's, and Oettingen's evaluations of the opening of the Marcia Funebre in Beethoven's op. 26. The technical problem is similar to the one Cohn discusses in connection with the passage in Schubert's Piano Sonata. When one travels from chord to chord in mm. 1–21 of the Marcia Funebre, applying conventional diatonic logic as one proceeds, one recognizes that A♭ minor (the tonic of the piece) in m. 21 should be notated as B𝄫 minor. Oettingen regards this result as evidence of a compositional error, citing similarly flawed passages in the first movement of Beethoven's op. 2, no. 3 (he has in mind mm. 102–08) and mm. 91–98 of the Egmont Overture (1866, 143–44; 1913, 207). To Oettingen's complete incomprehension (1866, 143), Weitzmann claims that Beethoven, as a "daring enharmonic composer" (*kühne Enharmoniker*), believes the two keys to be identical, and that should be the end of it (1861, 28).[2] Riemann's reactions are similar to Weitzmann's (1880, 82). And we can add to the discussion Max Reger, who seems cheerfully to enjoy the resulting contradictions enough to teach the readers of his *Beiträge*

[2] Der kühne Enharmoniker beginnt demnach seinen Trauermarsch in As-moll und schliesst ihn in b𝄫-moll, indem er beide Tonarten als völlig identisch, und nur der Schreibart nach verschieden betrachtet (Weitzmann 1861, 28).

zur Modulationslehre how to modulate convincingly from C major to B♯ major and from C♯ minor to D♭ minor (1904, 11; 24)—no judgments about poor harmonic control or anxiety about articulating a grand global tonality here.

Cohn's preferred strategy is to hold the problem within the realm of technology. We can maintain the principle of Big Diatony if we replace the principle of Small or Midsize Diatony with a different structural principle. In Cohn's proposed pan-triadic model, embodied in his Hexatonic cycles and Weitzmann regions, chord-to-chord logic (or key-to-key logic) is governed not by conventional diatonic relations but by a different set of structural concerns—concerns rooted in particular voice-leading properties of the triad. In short, Cohn's idea is to replace laws of tonal chord progression—whether defined with respect to scale structure (as carried out by roman numeral analysis) or by way of the theory of tonal functions—with the laws of close-relation determined by certain voice-leading profiles between triads. The best theoretical environment to investigate these determinations, Cohn tells us, is atonal pitch-class theory, "whose great achievement was to develop a systematic approach for exploring the properties, potentials, and interrelations of chords ('sets') within the chromatic universe" (13).

In a sense, Cohn's proposal doesn't seem to flow in a straight line from his discussion of the problems posed by the passage in Schubert's B♭ Piano Sonata. As we have seen, Cohn takes pains to demonstrate that while chord-to-chord relations (representing key-to-key relations) are recognizable instances of the "syntactic principles of classical diatonic tonality," they do not "work together to express the global tonic of B♭" (2), and that the absence of a global tonic is projected by the non-identity of B♭ at the beginning of the excerpt and C𝄫 at its end. If we simply replace the complex musical space of letter-classes-and-accidentals with that of twelve-pitch-classes-under-equal-temperament—which is basically Weitzmann's solution—the problem, as Cohn has carefully defined it, disappears. We will then presumably have a passage in which chord-to-chord relations are recognizable instances of the "syntactic principles of classical tonality," which "work together to express the global tonic" of B♭-or-a-suitable-enharmonic-equivalent. In that case, why then should we be convinced that

"syntactic principles of classical tonality" have failed and that we need an entirely new way of defining chord-to-chord relations? In other words, why invoke the musical space of twelve-pitch-classes-under-equal-temperament to better study new notions of triadic relations when (if Weitzmann is to be believed) invoking that musical space has already rescued the old notions of triadic relations?

The problem in Cohn's analytical exercise is not merely a matter of adopting enharmonic equivalence, although it seems clear that Cohn thinks enharmonic problems are symptoms of an underlying failure: they are the way problems in tonal syntax, or at least the problems that interest him, are expressed. So the question for Cohn is not whether or not to adopt an enharmonic pitch space: we must, on his view, adopt it. The question becomes whether to adopt it in Big Diatony or in Small Diatony. Indeed, this framing of the structural-principle choice guides his notion of "essential enharmonicism," namely, enharmonicism forced on Small Diatony to keep Big Diatony free from enharmonicism (9).

My guess is that Cohn's actual line of reasoning differs from the way he seems to set up the problematic at hand. I think he presents the conflict of Big Diatony and Small Diatony as a hopeless cause, technologically speaking, principally to encourage us to go back to the drawing board, to think afresh about tonality, or at least about certain kinds of passages in tonal pieces. I suspect that at least some of the grief he takes from his critics is that they still wish to debate the problematic of Big and Small Diatony, while Cohn has already accepted the problem (articulated in a particular way) and has simply gone ahead to pursue his project. And in fact, as we find out as we read the book, Cohn's thinking about such contexts in general and about the Schubert passage in particular is more subtle and complicated than his starting point suggests. This situation begs the questions: what, in addition to what we are explicitly told, motivates Cohn's ideas, and what do these ideas entail? On that score, it seems to me that the associated structural model has as much to do with protecting the integrity and continued viability of currently dominant theories of tonality and their associated modes of analysis as it does with proposing a novel analytical technology.

2. The Double Syntax Question

At the beginning, I pointed out that one of Cohn's goals is to answer criticisms leveled at his pan-triadic model, and in particular, against his image of the analysis of some 19th-century music as alternating, where appropriate, between conventional diatonic theory and pan-triadicism. There have been other criticisms, but those leveled against his notion of a double syntax governing 19th-century music seem to affect him the most, and he devotes a section of his last chapter to answering his critics.

Before we think further about these criticisms and Cohn's response to them, we should bear in mind the stakes involved. To begin with, it is important to recognize that all musical technologies and their associated theoretical outlooks are immensely vulnerable to criticism. Their vulnerability emerges from the manner in which they must operate. Any analytical technology begins by asking us to attend to this musical feature or event (and therefore not to some other feature or event) and to attend to it in a particular way (and therefore not some other way). Accordingly, any approach is always susceptible to the claim that it is ignoring the importance of this or that other feature, or that the explanatory narrative is incomplete. It is not just the case that each approach is thereby partial or limited: one can't simply add up all possible technologies and thereby grasp the complete picture, because the approaches overlap in ways that produce contradictions when we attempt to combine them.

That being the case, it is no wonder that Cohn's approach has attracted some negative press from a number of different quarters. It is interesting that the objections, at least as Cohn perceives them, seem to have gravitated around the question of a double syntax. Cohn himself detects two forms of the objection. He claims that one is leveled in the interests of "ontological/aesthetic" principles: a belief in "immanent properties" of pieces and a belief in the unity of the musical work (200); and that the other is leveled from the perspective of "epistemological/cognitive" principles, namely, "the supposition that the musical hearing switches between tonal and non-tonal apprehension during a composition or a phrase would be problematic" (Dahlhaus 1967, 100–1; translated and cited in Cohn

2012, 200).[3] Cohn does not genuinely defend himself against what he calls the "ontological/aesthetic" form of the criticism, rather condemns it as an instance of idealism. He does actively defend himself, however, against what he calls the "epistemological/cognitive" form of the criticism, first, by arguing that there are indeed sound cognitive principles supporting our ability to switch between two systems and, secondly, by claiming that the double syntax accounts for some aspects of musical development from the first to the second Viennese schools.

The apologias don't work very well for a couple of reasons. First, Smith (1986) doesn't actually disagree with Proctor (and therefore Cohn) on either aesthetic or ontological grounds. He disagrees with the idea of Proctor's two syntaxes because he experiences "no grinding of gears as one area is left and the other is entered" (Smith 1986, 109; cited in Cohn 2012, 199). In other words, the idea of double syntax does not conform to his musical experience—nothing about aesthetic or ontological principles. Furthermore, it is unclear how Cohn's own admirable bit of idealist history-making—connecting Mozart and Webern by way of the immanent properties of triads—should overwhelm the cognitive objections of Dahlhaus and Lerdahl.

As far as I am concerned, I see no particular problem in the practice of taking the same piece of music and applying one analytical technology here and another there. But my comfort with doing so is motivated by my own approach as reflected in my libertine remarks above, which may not move Cohn in the least. Yet, I do continue to wonder about the relationship between the two systems Cohn ultimately insists are distinct and incommensurate, governed by very different properties. Cohn does reflect on such possibilities in the eighth chapter of the book. He frames the technological problem as follows: "If the syntaxes [the pan-triadic model and classical tonality] operate independently of each other, then the challenge is to model their intertwining without collapsing them into each other" (169). Because Cohn uses maps to represent his pan-triadic model, the chapter is mostly

[3] Cohn uses Smith (1986) as the spokesman for the former position (even though Smith is critiquing Proctor and not Cohn); and Dahlhaus (1967) and Lerdahl (2001) as the spokesmen for the latter.

concerned with considering the design features of various candidates for the map that can adequately intertwine the two syntaxes without "collapsing them into each other."

Yet that's a project that simply isn't going to work to Cohn's satisfaction. For one thing, maps on their own are inadequate to the task of capturing the workings of classical tonality (though Cohn seems generally satisfied with their ability to capture the workings of his pan-triadic system). Riemann, for instance, cannot rely on maps alone to represent tonality. Indeed, he has three distinct models of harmony (which he often obscured or confused) to address different aspects of tonality when viewed as a system. I have in mind his topological model (the *Tonnetz* and the various tonal topoi of *Klänge* that emerge from it), his transformational model (the so-called S/W system), and his functional model (premised on the putative cognitive law that the presentation of a subdominant and dominant produces the impression of a tonic). These models must all interact in certain ways to set his grand system of tonality in motion. It is when Riemann tries to articulate the features of one model in terms of another—which is what his elaborate system of function labels attempt to carry out—that he often runs into technical difficulties of the sort neatly described by Rehding (2011).

In other words, Riemann's *Tonnetz* (or rather, Euler's and Oettingen's *Tonnetz*) is an entirely different construct than the *Tonnetz* Cohn uses to represent the various features of his system, despite visual similarities between the two. In Riemann's system—and we can extend these remarks to 19th-century German harmonic theory in general—the *Tonnetz* constitutes the fundamental musical space. In addition to supplying the basic metrics of pitch relations, it embodies the explanatory theoretic narratives at the ground floor of the entire system. When the *Tonnetz* is the foundational musical space, the semitone has a different status than the one it enjoys in the chromatic scale under equal temperament, where it is the basic unit of measurement. In contrast, Cohn's *Tonnetz* is not the foundational space of his musical system. His foundational space is the twelve pitch classes under equal temperament. From that space, atonal pitch-class theory constructs a universe of pitch-class set-types. One set type, <037>, particularly interests Cohn. His *Tonnetz* is then deployed primarily to represent the specific properties and

relations he thinks are important. Accordingly, in reading his *Tonnetz*, one needs to import into its visual design a great deal of technical work carried out at earlier theoretical stages. Unlike 19th-century theorists, Cohn feels no pressure to build his system from the ground up: he is able simply to declare his basic space as a given, without concerns about rationalizing its structure in some way. In one sense, Cohn's *Tonnetz* is a richer construct, embedding as it does all the theoretical gravitas generated by pitch-class theory. Moreover, it frees him from reflecting upon foundational theoretical issues, although he does carry out that kind of reflection at the end of constructing his model, so that the results of his system end up explaining its foundational origins (xi; 205–8)(citation of passage?). Yet in another sense, it is a poorer construct, because it has no accompanying explanatory narrative that animates the system at each level of organization.

3. Cohn's Use of History

History appears twice in Cohn's narrative: at the beginning, as a way of framing the technical questions he wishes to address; and again at the end, where he draws historical conclusions from his technical model. As such, Cohn's book ought to be of interest both to historians of 19th-century music theory and to historians of 19th-century music.

Yet, despite Cohn's aspirations to understand "how the nineteenth-century ear understood harmonic relations" (ix), he does not read historical theorists with an eye towards developing a coherent image of 19th-century music theory. Instead, he is more concerned to harvest from his reading of historical theorists individual ideas that can be reformulated and developed along lines he finds useful to his project.

How do we evaluate Cohn's approach to historical theory? Thinking of the field in general, we can detect a binary in the two prominent genres of writing history of theory. The first was initiated in earnest by Riemann's own survey of European music theory from its classical roots, and takes as its task the clarification of concepts used by the theorists in question. It may be helpful to think of this genre as an instance of what Michael Rosen has called

an "author's belief" approach (1982, 15ff). The second genre, which one can detect as early as Rameau in his remarks on Zarlino and Descartes, makes use of concepts or issues associated with a given theorist or theoretical tradition, but develops them beyond, often far beyond, the ideational range determined by the earlier theorist, and usually along purely structural lines. It might be helpful to think of this genre as formalist or rationalist in orientation.

One can perceive the distinction between the two genres at work by contrasting the writings of, say, Alex Rehding and Richard Cohn on Riemann; of Leslie Blasius and Matthew Brown on Schenker; or of David Lewin's writing on Rameau and his writing on Riemann. There are many examples of both genres I admire greatly, including all of the examples just mentioned, and I see no particular reason to prefer one over the other. Moreover, there are different ways of carrying out both styles of reading historical music theory. For instance, one can take the "author's belief" approach but carry it out along the lines suggested by Kant, who argued that we can know the author—who may have "not sufficiently determined his own concepts"— better than he knows himself (quoted in Rosen [1982], 18). Or we can take a formalist or rationalist approach and historicize the technological questions that arise. The two scenarios just described may appear to transform the "author's belief" approach into the formalist or rationalist approach. The key difference between the two approaches lies in the goals involved: the author's belief approach, whether it is concerned with accurately grasping an author's beliefs or concerned with exploring their consequences or patching up problem areas, always orbits around the author's original project. The formalist or rationalist approach does not.

That's one way to position Cohn's work: to argue that his engagement with 19th-century theorists, and with Riemann and Weitzmann in particular, is formalist or rationalist in orientation. Yet there is also a fairly strong sense in which Cohn's work is in fact deeply indifferent to history. Kopp, for instance, claims that Cohn's pan-triadic system is a product of his presentism, which Kopp defines as "among other things, the inclination to focus selectively on aspects of a historical theory or theories which have relevance to a contemporary approach (hence assigning the name

'Neo-Riemannian' to a theory with non-Riemannian attributes), and to find evidence in earlier music or music theory of a concept not articulated until later (nontonality)" (2011, 414n3). I do feel some sympathy with Kopp's position, but I doubt Cohn (for one) would regard such uses of historical theory as much a drawback as Kopp (for one) does. There's no obvious reason why one shouldn't read theorists—contemporary or historical—to get new ideas, which one in turn develops and extends as one sees fit. Accordingly, Cohn will not be much impressed with arguments (like Kopp's about presentism) that take history as an imperative. As a result, however, we get an image in the book of 19th-century musical culture as divided into a community of composers who have structural compositional principles that are clear to themselves (and now to us) and a community of theorists who have either an undeveloped or completely erroneous understanding of those principles. Schenker's own conception of that musical culture is remarkably close to this image, with an added ironic narrative twist that the former community becomes decadent and the latter reaches its culmination in him.

These matters bring us very close to the matters raised recently (again) by Taruskin (2011). In the course of his remarks, Taruskin revisits his old debate with Forte about the various demands of history and structure, and how they relate to the different goals of American music theory and musicology. He reminds us of Forte's claim that "a knowledge of history is totally inadequate for understanding musical documents, including musical scores as well as treatises on music. It is only now, with the development of contemporary modes of musical thought, that scholars are beginning to understand more fully many of the classic documents of music theory" (Forte 1986, 335; cited in Taruskin 2011, 180). Taruskin cites this passage, I think, because he wishes to reveal an aggressive whiggishness in Forte's thinking about historical theorists. In light of my discussion of the two genres of dealing with historical thinking, we might regard Forte's remarks as an instance of the Kantian mode of working to "sufficiently determine" a historical theorist's ideas, which by itself is not a particularly whiggish gesture (although Forte's apparent confidence in the superiority of "contemporary modes of musical thought" makes that interpretation unlikely).

My intent is not to revisit the various sides of the debate about the competing claims of history and structure on the study of music, but rather to see how these competing claims operate in Cohn's model. If we were concerned to answer Kopp's objection along historical lines, we might argue that the musical space of atonal pitch-class theory, the chromatic scale under equal temperament, has been left at our doorstep by 19th-century theory; and further claim, citing Bussler's *Lexikon der Musikalischen Harmonien* (1889) as evidence, that both the idea of set-types and interval vectors are creatures of later 19th-century harmonic thinking. I suspect, however, that those for whom history is an imperative would likely not regard those arguments as instances of proper historicizing.

No matter how one feels about the two sides of the question framed by Taruskin, it is possible to view the various debates within the Neo-Riemannian movement along these lines—as reflections of different views about the claims of structure and history.

4. Concluding Remarks

I'll end with a comment on a methodological issue raised in connection with Cohn's model by Steven Rings, who, to a certain degree, echoes comments made earlier by Fisk (2000), to which Cohn has responded (2000). Rings (2011, 500) wonders whether an analytical technology designed to show rational coherence can adequately accommodate passages that in our experience exhibit what Cohn calls the "X-factor," his term of choice for the property that governs extraordinary harmonic events or passages (Cohn 2012, 15). I can imagine Cohn's frustration here. He feels compelled to address Smith's objection—that double syntax models are suspect because one hears "no grinding of gears as one area is left and the other is entered" (Smith 1986, 109)—and now he is compelled to address the objection that his model imposes coherence on extraordinary harmonic events or passages. There is simply no way to satisfy both of these objections.

Rings's point addresses a fundamental methodological question for all analysis regardless of its technological orientation, not just

analysis using Cohn's model. Perhaps it will be enough here to point out that in the particular case of Cohn's model, one might regard the switch from one coherent system (conventional diatonic theory) to another (Cohn's pan-triadic syntax) as sufficient indication of the appearance of the X-factor. The kinds of harmonic contexts Cohn investigates register their extraordinary nature because they defeat conventional diatonic analysis. Moreover, one might challenge the requirement that analysis reflect our initial impressions, on the grounds that a more respectable task for analysis is to transform those impressions. Finally, it is important to recognize that Rings's question itself is abstract, covering a category of events, rather than particular, concrete cases, so that these issues will be most successfully resolved as we consider actual musical examples rather than classes of examples.

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