Numerous composers have explored the modes of the acoustic scale (or ascending melodic minor), including Adams, Bartók, Debussy, de Falla, Fauré, Glass, Janáček, Liszt, Prokofiev, Ravel, Reich, Rzewski, Scriabin, Stravinsky, Szymanowski, Shostakovich, and virtually every contemporary jazz musician. The scale is clearly one of the central collections of twentieth-century tonality—and possibly even more prevalent than the octatonic. Though much discussed by jazz pedagogues, academic theorists have paid it scant attention; there are relatively few articles discussing it, and scholars have consistently misinterpreted musical passages in which it appears. Arnold Whittall, for example, describes the acoustic scale in Debussy’s “Des pas sur la neige” (mm. 16–18) as a combination of diatonic and whole-tone elements, while Pieter van den Toorn describes the acoustic scale at the end of Stravinsky’s “Dance of the Adolescents” (R32) as a combination of octatonic and diatonic elements.¹

As a graduate student, I was surprised to learn that academics could be so blind to one of the most important scales of the twentieth century. (Indeed, I still remember a composition teacher pointing to one of my acoustic-scale melodies and proudly declaring, “Aha! The whole-tone scale!”) How, I wondered, had we reached the point where jazz textbooks, intended to help practical musicians develop their improvisational skills, provided a more reliable guide to basic concepts of twentieth-century art music than university press monographs? And what did it say about the methodology of music theory that respected figures such as Whittall and van den Toorn could go so drastically wrong about such obvious cases, or that eminent historians (such as Taruskin) could fail to notice when they do? Such questions suggest that we often perceive what we expect to perceive, and that the growth of understanding can be surprisingly slow. (Van den Toorn's book was published in 1983, with my critique appearing almost twenty years later; Taruskin’s response now comes almost ten years after that.) And they show that music-theoretical disputes are sometimes motivated not by subtle issues of methodology, but by basic issues of musical literacy. After all, most of us were children when we first encountered the melodic minor scale, and virtually everyone agrees that modality is central to twentieth-century composition. So how could we fail to recognize the modes of the melodic minor when we encounter them?

My work on Stravinsky began with the realization that he made periodic use of the modes of the nondiatonic minor scales, and that this use was at times as explicit as any of his more celebrated examples of octatonicism. I took this to suggest that Stravinsky could be situated within multiple historical traditions—not just the Russian tradition of his teacher Rimsky-Korsakov, but also a broader tradition of scalar exploration linking a much larger number of composers throughout Europe and America.² Intrigued by the fact that van den Toorn and Taruskin had misinterpreted these scales, I began to take a more skeptical look at their (“octatonocentric”) methods. While I did not deny that Stravinsky sometimes used the octatonic scale, I did start to worry that the collection had metastasized into a theoretical idée fixe, as if it were the key to unlocking Stravinsky's cabinet of musical wonders.

There seemed to me to be two general problems here. First, Stravinsky's musical method struck me as pluralistic, broad enough to encompass a range of scales, influences, and compositional techniques. It seemed possible that the single-minded focus on the octatonic might have been motivated by a misguided attempt to align Stravinsky with more-systematic thinkers such as Schoenberg, Schenker, Schillinger, or Reti—all of whom (wrongly, in my view) seemed to associate musical value with systematicity of compositional procedure or result. Second, I had severe doubts about the analytic practice of intuitively segregating musical passages into “octatonic” and “diatonic” components, with this segregation not supported by anything obvious in the musical surface. The problem, as Joseph Straus pointed out before me, is that virtually any passage of music can be decomposed in this way—including passages that manifestly belong to other scales.³ This leaves us with a methodological dilemma: how can we determine if we are uncovering genuine features of the music itself, or merely projecting our own fantasies as a palm reader projects a potential future onto the wrinkles in a person’s hand? If even the melodic minor scale could be read as octatonic, then shouldn’t we conclude that the methodology is fundamentally unreliable?

I also made a number of more-specific claims about Stravinsky’s music. First, I suggested that his material cannot

¹ See Whittall (1975). Van den Toorn (1983) also misidentifies the melodic minor scale in Petrushka and Histoire du Soldat, as well as the harmonic minor in Firebird. In a similar vein, Parks (1989) denies the syntactic significance of the acoustic scale in Debussy’s music, treating it as a mere modification of the diatonic. For more, see Tymoczko (2002); (2003); (2004); and (2011).

² Of course Rimsky-Korsakov might also be said to belong to this larger tradition. Indeed, I wish Taruskin had spent less time looking for the octatonic scale in Rimsky’s music, and more time looking for scales such as the acoustic and harmonic major—which was apparently named by Rimsky.

always be definitively categorized as "diatonic," "octatonic," or "a combination of diatonic and octatonic." In my view, Stravinsky uses material that is variously diatonic, chromatic, octatonic, whole tone, acoustic, or sui generis. Second, I argued that Stravinsky frequently superimposed material with very different harmonic characters, including elements belonging to different keys. The keys in question need not relate by minor third or tritone, and hence may not "refer" to a "background" octatonic collection. Third, I proposed that careful consideration of the surface of Stravinsky's music shows that there is less octatonicism than we had been led to believe: for example, my original article tried to identify the explicit appearances of the scale in both the opening of The Rite of Spring and the third movement of the Symphony of Psalms, finding a surprisingly low proportion of octatonicism. I suggested that this should make us doubt that the octatonic collection is omnipresent in the musical background: it is possible that the octatonic scale appears in Stravinsky exactly where it seems to be, and not elsewhere. Fourth, like others, I felt that both Taruskin and van den Toorn underestimated Stravinsky's connection to predecessors like Debussy and successors like Bartók. Emblematic of this, perhaps, is Taruskin's attempt to connect Stravinsky's whole-tone scales to a Russian bone flute, rather than to the more obvious French sources. Finally, in my response to van den Toorn, I proposed an explicit criterion of "scalehood," explaining exactly why theorists needed to acknowledge the presence of nondiatonic minor scales in Stravinsky's music—even if they insisted on accounting for them, at a deeper structural level, as combinations of diatonic and octatonic.

I find little engagement with these claims in Taruskin's polemic. Taruskin is wrong to imply that I deny Rimsky's octatonicism or its influence on Stravinsky. His comments on history are largely irrelevant, since there is an extensive twentieth-century tonal tradition featuring modal use of the nondiatonic minor scales; thus the issue is not whether Stravinsky had influences, but which influences are apparent in his music. Taruskin's suggestion that theorists should focus on what composers do is entirely consistent with the methodological remarks in §1.4 of A Geometry of Music. At one point, he offers something like an argument when he suggests that I am being "inconsistent" in attributing particular passages to the acoustic rather than octatonic collection. But it is hardly inconsistent to claim that Stravinsky knew more than two scales; and if he did, then we may need to think carefully about which is present at any particular moment. The larger problem here—and it is difficult to put this in a way both polite and accurate—is that Taruskin is not a very patient reader or thinker. He favours grand abstractions and sweeping dichotomies (e.g., "creationism" versus "evolutionism"), writing in a white heat without double-checking his claims. This is surprising, since a casual disregard for accuracy is a genuine liability for a historian: after all, readers might start to wonder whether anyone could be entirely correct about Stravinsky while being utterly wrong about Tymoczko.

So can we glean any general methodological lessons from this discussion? One is that Taruskin, for all his self-presentation as a critic of music theory, is more of a traditional theorist than I am. True, I am relatively comfortable with the ambitions of traditional theory: namely, to devise abstract models that can help us understand what previous composers were up to, to help us think about new ways to put notes together, and to help explain why music history might have progressed as it did. But like my teacher, David Huron, I worry that these ambitions are sometimes poorly realized in practice. Over the last few years I have argued against several verities of contemporary theory, proposing (for example) that the Tonnetz does not represent voice-leading distance, that Neo-Riemannian transformations involve unnecessary dualist baggage and should be jettisoned, that group theory does not provide an appropriate model for musical intervals (or "transformations" more generally), that the traditional notion of a "set" is often too precise for our analytical needs, that traditional pitch-class intervals are inadequate for modeling voice leading, that influential discussions of "hearing a musical relationship" confuse hearing as with hearing plus thinking, that Schenkerian theory oscillates unstably between grammatical and psychological claims, and that van den Toorn's octatonic reductions are fundamentally unreliable. If asked to describe the biggest challenge currently facing the field of music theory, I would probably say something like "in music theory, it is very difficult to figure out what sort of evidence counts in favor of a particular theory, what sort of statement can (or should) be subjected to rigorous testing, when general statistical claims can be supported by particular analytical examples, and how ideas from science, mathematics, statistics, and philosophy—or, for that matter, common sense—can be brought to bear on disputed questions."

Taruskin, by contrast, seems to offer history as a straightforward solution to music theory's methodological difficulties. This is most apparent in the following passage:

The octatonic scale, as we know, may be parsed into four triads (either major or minor), four dominant seventh chords, four minor seventh chords, four half-diminished seventh chords, two diminished seventh chords, two French-sixth chords, and so on. This means that any triad or seventh chord can be referred to an octatonic source, and that consequently any composition by Mozart or

4 Taruskin allows that it is proper to describe the "Petrushcha" chord as being "polytonal," but only insofar as "it is borne in mind that the keys in question were chosen not simply ad libitum but from among the circumscribed and historically sanctioned waves of the octatonic complex" (1996, 749). By contrast, I would argue that Stravinsky needed no historical sanction to superimpose material in different keys, and was quite happy to superimpose C major with C# minor (as at the opening of the Concertino) or C minor with C# minor (as in R94 of Part 2 of The Rite of Spring). Here the conflicting keys cannot be derived from a single octatonic scale.5 See Taruskin (1996, 710).

6 In all of these respects, I have been inspired by Huron, who manages to investigate music-theoretical topics with the rigor of psychologists and cognitive scientists. While I think of myself as being somewhat more humanistic than Huron—more open to speculation, more focused on intrinsically compositional problems—I am nevertheless heavily influenced by his work.
Haydn could be given an octatonic "analysis," as could any composition by Bach or Handel, Wagner or Brahms, even Monteverdi or Josquin des Prez. Why are they not given such analyses? Because we all recognize that there is no connection between octatonic theory and the practice of these composers. Whether we wish it or not, or admit it or not, that is an historical determination, not an inferential one. Anyone who undertakes octatonic analyses based on referability alone without establishing historical connections is condemned to inconsequence.7

This is a place where I wish he had stopped to think through the issues a bit more carefully. Substantively, he is wrong to imply that the octatonic scale contains augmented triads and major seventh chords (cf. "any triad or seventh chord"). Historically, he is wrong to draw a sharp line between octatonicism and the chromaticism of nineteenth-century composers such as Chopin and Wagner.8 Put those issues to one side, however, and focus on the philosophical implications of Taruskin's final sentence: "Anyone who undertakes octatonic analyses based on referability alone without establishing historical connections is condemned to inconsequence." Intuitively, one would think that a convincing octatonic analysis would establish something significant—viz. that a particular composer used the octatonic scale, and was thus engaging with musical ideas familiar to us; whereas Debussy more often uses the scale as a macroharmony, precisely as in Stravinsky's early ballets.11 Having distinguished these two aspects of scalehood, it becomes much easier to see how we might use purely musical evidence to identify instances of octatonicism. After all, there are hardly any passages in Josquin, Mozart, Chopin, or Webern in which the octatonic scale (1) acts as a macroharmony; (2) appears with all of its notes; (3) lasts for a substantial number of measures; and (4) is not the obvious byproduct of some other musical procedure (such as the alternation between two diminished seventh chords). By contrast, there are many such passages in Rimsky, Stravinsky, Messiaen, and Bill Evans. So what exactly is the deep methodological problem here?12

9 For those who do not know: Bill Evans used the scale in a very systematic way—over V7 chords, typically stating the collection completely. I have no idea whether he ever talked explicitly about the scale, and do not think it is particularly important to find out, given the weight of purely musical evidence.

10 For example, it is unclear that Taruskin can draw a sharp distinction between "historical" and "inferential" determinations. Presumably he means to distinguish information deriving from historical records (letters, sketches, etc.) from information acquired by studying scores. But as he himself acknowledges, inference is involved in both cases. (Only a poor historian takes verbal testimony at face value.) Furthermore, our historical sense that Renaissance composers did not use the octatonic scale is supported in part by musical analysis—over the centuries musicians have collectively analyzed thousands of Renaissance pieces and found precisely no instances of Renaissance octatonicism.


12 While it is possible to analyze Josquin octatonically, the analysis would necessarily involve many unmotivated scale shifts and nonharmonic tones, indicating that in fact he did not use this scale. By contrast, the ability to produce a convincing octatonic analysis of Bill Evans's music provides genuine evidence that he did indeed use the scale. Taruskin writes as if we

7 Taruskin (2011, 179).

8 As I argue in Tymoczko (2011, §3.11), nineteenth-century composers often exploited efficient voice leading between proximate seventh chords, with the geometry of four-note chord space ensuring that these chords often belong to the same octatonic collection. Thus, it is not at all implausible that composers might hit upon the octatonic scale by noticing that nearby seventh chords also constitute an interesting scalar collection. From this standpoint, it is a relatively short step from the octatonic scale in mm. 25–28 of Chopin's Etude in F Minor, Op. 10, No. 9 (where voice leading plays a central role) to that in Wagner's Tristan (Act I, Scene 5, m. 137ff.), where the scale occurs over a fixed sonority.

9 Intellectuals who deny our ordinary criteria may gain a temporary sheen of sophistication, the appearance of holding themselves to higher standards than do the rest of us, but the certainty they seek is an illusion that cannot be found in historical documents or anywhere else.10 By rejecting the well-worn tools of careful observation, rigorous argument, and judicious inference, they end up dooming themselves to a peculiarly fastidious sort of inconsequence—spinning their wheels in fruitless self-doubt, while the rest of us trudge forward, make our mistakes, and occasionally learn something about the world.

Taruskin's skepticism, I suspect, results in part from theoretical unclarity about what exactly a "scale" actually is. As I argue in A Geometry of Music, the term "scale" encompasses the very different notions of macroharmony (the total collection of notes used over a small period of time) and musical ruler (which provides a unit of distance, the scale step, distinct from frequency ratios or log-frequency distances). Taruskin sometimes conflates these meanings, as when he identifies Glinka and Rimsky—rather than Debussy—as the source for Stravinsky's whole-tone passages: Taruskin's Russian examples generally use the whole-tone scale melodically, augmented by other notes in other voices, whereas Debussy more often uses the scale as a macroharmony, precisely as in Stravinsky's early ballets.11 Having distinguished these two aspects of scalehood, it becomes much easier to see how we might use purely musical evidence to identify instances of octatonicism. After all, there are hardly any passages in Josquin, Mozart, Chopin, or Webern in which the octatonic scale (1) acts as a macroharmony; (2) appears with all of its notes; (3) lasts for a substantial number of measures; and (4) is not the obvious byproduct of some other musical procedure (such as the alternation between two diminished seventh chords). By contrast, there are many such passages in Rimsky, Stravinsky, Messiaen, and Bill Evans. So what exactly is the deep methodological problem here?12

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In asking this, I want to be clear that I am by no means an enemy of musicology or music history. A central goal of my work, taken up most recently in Chapter 4 of *A Geometry of Music*, is to show that scales such as the acoustic and octatonic are very natural musical objects, optimal for a variety of reasons: both, for example, have one- or two-semitone steps and three- or four-semitone thirds, thus permitting composers to generalize routines of traditional diatonic composition. (Similarly, both contain a large number of extended triadic sonorities, of precisely the sort favored by twentieth-century composers.) This is why we should expect that composers would independently converge upon them—particularly those composers who wanted to expand rather than explode the procedures of traditional tonality. Thus I think it is quite possible that at least some jazz octatonicism is independent of its antecedents in European art music. A similar point may apply to Debussy's octatonicism, which often exploits seven-note octatonic subsets relating to the acoustic scale by single-semitone displacement. (For that matter, Messiaen's interest in the octatonic may have been motivated less by the historical example of Stravinsky than by the scale's transpositional symmetry.) For this reason, charting the scope of twentieth-century octatonicism, and determining the relative balance of borrowings and reinventions, is a genuinely difficult and interesting historical project. But in undertaking it we necessarily involve ourselves in the analytical task of figuring out where the octatonicism actually is.

What is most radical about Taruskin's polemic is the way it seems to deny that there is a two-way street connecting theory and history. No doubt history can provide suggestions as to which analytic techniques are appropriate to a given composer. But is it true that analysis can never reveal surprising things about ways in which composers thought? For example, Bach evidently did not think in terms of root progressions or Roman numerals, yet Roman numerals can be used to provide a surprisingly accurate description of his harmonic practice. This suggests that there is something about traditional harmonic theory that accurately represents some cognitive structures in Bach's mind, even though he himself might not have been able to acknowledge this. (Similarly, contemporary linguists can provide extremely accurate descriptions of the grammar of Latin using concepts that were not available to Julius Caesar, with the fit between data and theory suggesting that *something* in the theory is capturing *something* important about the way the Roman mind worked.) Chopin surely did not think about four-dimensional orbifolds, and yet contemporary geometry can in fact help us understand what is happening in his music.14 No doubt this is because the geometrical structures provide an alternative representation of concepts that Chopin *did* have—concepts such as "seventh chord" or "moving one's fingers by a short distance on the piano." By giving us new ways to understand these old ideas, analysis, or at least good analysis, can allow us to learn something new about earlier composers' musical practice.

The flip side of Taruskin's radical skepticism is a surprising and uncharacteristic gullibility in the presence of even an iota of historical information. (Incidentally, this is the place where I argue that Taruskin is less critical of music theory than I am.) For when it comes to van den Toorn, Taruskin seems almost as methodologically naïve as the eminent theorists he criticizes. He raises no complaint when van den Toorn analyzes familiar minor scales as products of "diatonic/octatonic interpenetration," or tacitly eliminates offending notes from his reductions, or arbitrarily categorizes pitches as "octatonic" and "diatonic." It is as if the mere fact that Stravinsky knew the octatonic scale had persuaded Taruskin to overlook all the subtle methodological issues that might lead us to doubt van den Toorn's particular analytical methods. This is very odd: I find it hard to understand how anyone could be so alive to the problems with Forte's octatonic analyses of Webern, or with Schenker's obsessive pursuit of the Urline, and yet so insensitive to the equally glaring problems in van den Toorn's work. It makes sense only if Taruskin really does despair of his ability to evaluate analyses on their own merits, clinging instead to history as the only reliable guide.

Taruskin ends his article by praising two Chicago-based theorists, Lawrence Zbikowski and Robert Gjerdingen, both of whom incorporate psychology and history in interesting ways. I would like to second this praise, as I find that both are provocative, original, and insightful thinkers. But I also want to temper Taruskin's enthusiasm by noting that Gjerdingen's work, despite its virtues, occasionally exemplifies the dangers of overenthusiastic historicism. For in the course of offering "schemata" as a much-needed corrective to the abstract (grammatical) orientation of traditional music pedagogy, Gjerdingen ends up disparaging one of the central achievements of Western music theory, comparing Roman numeral analysis to art instruction by means of "paint by number" kits.15 This is unfortunate. Questions about the utility of Roman numeral analysis cannot be settled by merely observing that the technique was invented after the phenomenon it attempts to describe. (After all, that same charge could be leveled against all of historical linguistics, or economic history, or Freudian interpretations of nineteenth-century novels, had no reliable grounds, other than history, for distinguishing unconvincing analyses (e.g., Forte's octatonic analyses of Webern) from convincing ones (e.g., someone who points out that Bill Evans used the octatonic scale a lot). This is precisely what leads me to suggest that his historicism is driven by (Postmodern?) skepticism about the powers of rational judgment—in this case directed at our ability to evaluate analyses.

13 Presumably Bach conceptualized principles like "avoid the iii chord" in a more roundabout fashion, using the language of figured-bass theory: "When the bass is on the third scale tone use the chord of the sixth, and when the bass is on the fifth scale tone, use the chord of the fifth." Like many musicians, I think "avoid iii" is a more succinct and intuitive formulation, even though it may be anachronistic. What we have here is an instance of conceptual progress, a more efficient way of thinking about tonal harmony—which is precisely why Rameau was such a great theorist.


15 Gjerdingen (1996, 366). This dismissal lives on in *Music in the Galant Style*'s frugal use of Roman numerals, a feature that makes it difficult to use the book pedagogically.
or a substantial portion of the humanities and social sciences, in which contexts it is simply laughable to suggest that analysts restrict their vocabulary to concepts extant during the time they are studying.) What we have here is a genuinely testable hypothesis as to whether or not a certain abstract vocabulary can be used to provide a perspicuous description of a certain feature of musical practice.

As it happens, I have tried to answer this very question. With the help of several dozen music theorists, I assembled corpora of Roman numeral analyses of seventy Bach chorales and all of Mozart's piano sonatas, a substantial body of data requiring over a hundred hours to assemble. The corpora suggest that traditional Roman numerals can be used to provide a simple but successful description of those composers' harmonic practices, accounting for something like ninety-five to ninety-nine percent of their basic chord progressions. The data also suggests something new about how those composers might have thought, since it reveals a role for the descending chain of thirds that sits at the heart of three-note chord space.¹⁶ (This in turn allows us to understand how a composer might arrive at this grammar, while thinking in figured-bass terminology.) This, of course, is the marvel of music theory: newfangled ideas sometimes help us understand music written decades, if not centuries, earlier. Chopin had no inkling of orbifolds, yet, as I argued above, an understanding of four-dimensional geometry can help us appreciate the structures in some of his most difficult works. Bach had no use for Roman numerals, and yet contemporary harmonic terminology can provide a concise and accurate description of his harmonic vocabulary. And though early twentieth-century composers may not have thought explicitly and systematically about scales, our own systematic thinking can help us understand what they were up to—perhaps even better than they themselves did. To say this is not to denigrate history or composition, but to celebrate the fact that human minds can sometimes look out over the same conceptual territory from many different perspectives.

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¹⁶ See A Geometry of Music, Chapter 7.