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Shaping Time: Music, The Brain, and Performance,
a response to William Rothstein

RESPONDENT

David Epstein

To the editor:

I write in response to William Rothstein's review of my book, *Shaping Time: Music, the Brain, and Performance* (*Journal of Music Theory*, 42.1, Spring 1998, 125–44). I would like to cover a number of points raised in this review, one of them, by its complex and seemingly contradictory nature, truly inviting discussion. It concerns well-known enigmatic places in the Ninth Symphony of Beethoven. I further discuss in these pages an interesting issue concerned with the nature of ideas themselves that arises from Rothstein's review, though it is not pursued therein. Lastly, the review contains misunderstandings and misrepresentations of major ideas offered in the book that need to be corrected.

A number of Rothstein's critiques contribute to ways in which ideas in *Shaping Time* will evolve as I pursue them further. This is particularly true of some comments with regard to proportional tempo, a concern that occupies much of *Shaping Time*, in which Rothstein seems to find potential riches despite reservations. I appreciate this contribution.

The review fails at the outset, however, to fulfill one critical aspect of a reviewer's obligation—that of *reportage*. Reviews are expected to provide readers with an overview of a book's perspectives, as well as to present a reviewer's assessment of its ideas. That overview is notably incomplete here. Specifically, the review takes virtually no notice of a major component of the book—its concern with the neurobiological basis of musical timing.

Other than a quote of mine (Rothstein, 127) that proportional tempo seems a way of making music “most in resonance . . . with the neurophysiology of time processing and communication ([Epstein] 155)”, and a comment that the book “makes extensive use of insights gleaned from

the cognitive sciences" (Rothstein, 125), readers would have no idea from this review that a significant part *Shaping Time* is concerned with the neurophysiology of timing and time control in music. Further, that the periodicity which characterizes a steady beat in the performance of music seems to rest upon the periodic nature of neuronal firings, the oscillatory function that these neurons represent, the coupled nature of neural oscillators that results in linkages of periodicities covering varying spans of durations. Yet a significant portion of the book deals with these facts. Chapter 6 is devoted to the biological aspect of neural periodicity, its hierarchical and phase relationships, the bases these provide for biological clocks, the neural mechanics of these clocks and their coupling oscillatory functions. Chapter 7 is devoted to tempo measurement techniques as they relate to proportional tempi, to deviations from exact proportions and criteria for judging their significance or lack of it, to issues of temporal order threshold and consequent aspects of temporal perception. Chapter 9 offers case studies of music from seven nonwestern cultures which suggest that proportional tempo may be a universal. Should this be so, the likelihood that these tempo relationships rest upon neural mechanisms basic to all human beings is strengthened.

These chapters, together with their notes and bibliography, occupy almost 100 pages of *Shaping Time*, roughly one sixth of the book. Their ideas further permeate discussions in later chapters. They are a major focus of the study, and they offer a perspective that has begun to work its way into music theory only in recent years. Yet the review does not discuss them at all. Readers not familiar with the book would thus scarcely be aware of these contents or their scope.

Moving to other matters, let me discuss the points mentioned at the outset of this letter:

Discussion: 1) Re the Beethoven Ninth Symphony

Rothstein (135) speaks to conundrums posed by two tempo markings in the Beethoven Ninth, markings that to date, as he rightly notes, have eluded adequate resolution. I discuss these markings and their contexts in *Shaping Time*, but comment further upon them here, for the recent experience of conducting the work yet again has yielded new perspectives. My view of the tempo relations in these places has not changed substantially, but my sense of the contexts that bear upon them has enlarged. The places in question are the Scherzo and Trio of movement II, their tempi and the *stringendo* that links these sections, and the *Alla marcia* section in the last movement and its indicated tempo.

My sense of things regarding the second movement rests upon the question whether Beethoven's marking of half=116 for the Presto tempo that constitutes the Trio is valid. The prevalent alternate view is that

whole=116 was intended. I continue to feel that half=116 is correct. I agree with Rothstein (135) about the *im Volkston* character of the Trio tune—a factor that requires the slower tempo, and further find that manuscript studies (outlined in *Shaping Time*, 234–36) confirm this tempo. As for the rhythmic/thematic relations that I offer and Rothstein (125) disputes, there seems room here for legitimate disagreement.

The salient question about this passage is the one that Rothstein (137) asks—namely, “how can one effect a *stringendo* or *ritardando* between two tempos if those tempos are effectively identical?” My present-day response is that if we accept Beethoven’s printed half=116 tempo for the Trio, the passage can thereby be seen as a species of *accelerando* . . . *a tempo*, a mode of tempo discourse not unknown in eighteenth-century music though developed more within the nineteenth century, to an extent by Beethoven himself. That said, this is nonetheless a curious version of *accelerando* . . . *a tempo*, for almost every detail of the passage is variant from the standard notion of that model. *A tempo*, for example, is not stated in so many words, and the *Presto* that marks the Trio seems to contradict it. Further, Beethoven calls for *stringendo*, not *accelerando*.

There are affinities with the prototype, however. For one, *a tempo*, thus the same beat speed as prior to the passage, is explicit in Beethoven’s metronome marking. (The “Presto” indication at the *alla breve* remains enigmatic and probably cannot be adequately explained. If Beethoven’s metronome designation is accepted, that is, *presto* is best seen as character designation. If on the other hand *presto* is to determine speed [thus the alternative whole=116], the *Volkston* character suffers.) As for the acceleration aspect of the model, Beethoven’s specified *stringendo* is a significant variant. Acceleration can be seen as physics embraces the term, i.e., scalar acceleration, or change in rate of speed. *Stringendo* connotes a more complex event as well as an additional factor: the factor is an inner psychological or affective state, often one of tension; the event that this creates (or motivates) is a driving, or pressing forward, of tempo. Pressing forward also suggests resistance, with the result that the speed ultimately attained via *stringendo* is probably less great than that achieved by acceleration. (The French and English terms for this *modus operandi*—“pressez,” or the “press forward” seen above, convey this sense better than the Italian “stringendo,” whose connotation of “squeezing” suggests more a consequence of the action than its cause, thus a beat “squeezed” to smaller size.)

This subtle set of circumstances works its way in the passage, as does the sense of forward motion established by the phrase structure of these 18 bars (mm. 396–413), wherein the melodic and/or motivic groupings continually shorten by a factor of 2, from 4-2-1-1/2 bar sets. (See Epstein, Example 8.1f, p. 235, and the discussion, 234–35.) Added to this mix is the change to *alla breve* at the close of the passage, which has the

effect of further “confusing” the sense of time, of beat, of activity. In essence there are two calculi at work in the passage, one effecting change via *stringendo il tempo*, the second creating change through the altered meter and its attendant activity within the beat. The result is described in *Shaping Time*—in essence, that “when the Presto theme arrives we are left momentarily ‘beatless’, unaware for at least one beat (if not a bar or more) what the tempo, pacing, motion may be” (234). By the same token, Beethoven sets the stage at this point for the Trio—for its tempo, and for the psychological sense of pace. The details that control that pace—pedal tone, restricted thematic range, “uncomplicated” melodic direction, soft dynamics, light articulations, essentially static harmony primarily consisting of local activity, etc. are discussed on p. 235.

In its entirety the passage effects a transformation of perception more akin perhaps to late nineteenth-century practice or that of our own era than to 1817–1823—a modulation of pacing, tempo, and details of activity achieved with the subtlety of an “unseen hand.” We emerge from change, that is, before we are fully aware of what has changed.

The second conundrum, mentioned earlier, concerns what Rothstein (135) describes as “the oddly slow 84 (to the dotted quarter) for the *alla marcia* variation in the finale.” The perception is apposite. My view of this tempo, as discussed in *Shaping Time* (238–39), has not notably changed; in the main it still seems unviable. The context within which it takes place, however, I now view differently to the extent that aspects of the tempo do in fact appear plausible.

Several factors influence this view—for one, the fact that 84 to the beat at the *alla Marcia* is minimally different from the pulse of 80 that precedes it.¹ The discrepancy is 5%, the Just Noticeable Difference much discussed in *Shaping Time* (though ignored in the review). Rothstein (137) characterizes such a perception, in a different context, as being in “recognizably the same pulse-stream.” His phrase captures this essence.

This similar “pulse-stream” bears upon the segment that precedes the *alla Marcia*. It bears upon the return of the “Freude” theme within the *alla Marcia* (m. 543) as well. Essentially, both themes move at the same pace. Both, too, are of loud dynamic, and both share “marked” articulation—the “Cherub steht vor Gott” explicitly so (the staccato marks), the “Freude” passage implicitly through the staccati (spiccati) in the strings and the continual new attacks upon notes in the winds, brass and timpani. Moreover each of these themes is “set up,” so to speak, in such a way that the “oddly slow” pace of 84 feels viable. The “vor Gott” passage closes with a fermata *molto tenuto*. The cadence thus halts the flow of time. Nor is that flow set anew as the march begins: its first four bars are mostly silence, punctuated solely by notes on the weak beat of alternate bars. The ensuing eight measures increase the attacks to every bar, but again

on their weak beats. Straightforward movement does not occur until 13 bars into the march.

In a somewhat reverse way the flow of time is retarded—psychologically, if not physically—following the F# climax (mm. 525–40). The horn note motive continually begins off the strong beat, obscuring the prevalent pulse, and the entire passage “relaxes” by way of a long and structured diminuendo (*ff* to *pp*).

A consequence of this plan is a tempo of 84 that seems viable for these two passages that encase the march. The main body of the march, however (mm. 343–524 or, minimally, 431–524), remains in my view untenable at the tempo of 84. I know of no performance, live or recorded, that manages that tempo, the Norrington one I discuss (239) included. The music demands greater drive than 84 allows.

Seen in this “revisionist” light, the *alla Marcia* is not so discrepant from Beethoven’s indications. The mystery lies not so much with the explicit tempo marking (84) as with the absence of a signal that the march proper must move forward. The large-scale structure, however, clearly is deliberate, planned to a degree not generally recognized. Its reigning tempo (84) appears not so oddly slow in this perspective.

Discussion: 2) Of unity, rigidity, and the evolution of ideas

Rothstein (128) raises a complex point concerning a major premise of *Shaping Time*. He speaks of “Epstein’s rigid adherence to a Schoenbergian standard of unity” which pertains to relationships, tempo ones included, that organize a work as a unified and coherent whole. “This is an assumption, not an argument”, he says, “and it is never questioned in this book.”

But the idea is indeed questioned. Nor is the adherence rigid, as Rothstein suggests. (Documentation on this is provided here. See Case IV, last two items.) Yet Rothstein’s point bears examination.

I acknowledge the cogency of unity as a compositional and an analytical approach, and am aware that postmodern and deconstructionist perspectives have moved away from this earlier view. These changing perspectives touch upon a bigger issue, one contained in Rothstein’s comment but not pursued by him. It concerns the relevance, and the possible interconnections, among evolving ideas—whether perspectives once vital, highly developed in an earlier time, retain cogency in a newer age.

As we move to new theoretical bases, for instance, do we simply *change* bases, or do we in fact *discard* the older bases, much as we change clothes? Alternatively, are there hierarchic theories with levels upon which antithetical ideas coexist, levels wherein they may in fact connect in complementary ways?

I suspect the latter is true. Certainly it is so in the sciences. Newton's view of space and time, for example, continues to serve adequately for simple computations of highway travel (thus $\text{Distance} = \text{Speed} \times \text{Time}$, "simple" in the sense of presumed constant speed). His worldview fails with respect to astronomical space/time, however, which requires relativity theory. Mendelian genetics are not irrelevant to current theories of reproduction and growth, but they are inadequate; hence the evolution of molecular biology and gene technology. As for music, in particular, tonal music: Tovey's approach to phrase structure, with its descriptions of phrase lengths and surface characteristics, is not incompatible with current theory. It is not sufficient, however. Lerdahl and Jackendoff, Rothstein, Schachter, Schenker and others yield deeper insights. Much the same complementarity may be true with regard to Schoenberg's ideas concerning unity, and the significant concerns about the uniqueness and individuality of perception, cognition, construal of information and the like that have emanated from the critical theories of the postmodern world.²

Misunderstandings and Misrepresentations in the Review

The points discussed above, stimulated by Rothstein's review, contribute to the dialogue that any author might hope a large study would elicit. There are also, however, numerous misunderstandings, misrepresentations, misconstruals and misinterpretations of ideas, concepts and facts in *Shaping Time* that figure in Rothstein's twenty-page review. These must be corrected.

I cannot address all the points of misunderstanding in the limited space of this letter, but I will address some. They concern the studies of acceleration and ritard found in the book, as well as the model of rubato offered there. Additionally, I deal with distortions of context that underlie other issues discussed by Rothstein.

Case I. Re Acceleration and Ritard

I appreciate Rothstein's endorsement (133) of the study of acceleration and ritard in *Shaping Time*. He misses, however, aspects of the cubic curve shape that I and my fellow researcher, Jacob Feldman, found characterizes accelerations and ritards (chapter 12). For example, Rothstein (133) states: "An important point is that, given any two points on a graph—representing a beginning tempo, a goal tempo, and a length of time between them—there is one and only one cubic curve that can connect them. This suggests (and Epstein seems to encourage the suggestion) that there is one and only one 'ideal' ritard, or accelerando, between any two given boundary tempos, but that is not quite true: a different cubic

curve might be found that increases or decreases the length of time between the two stable tempos.”

To say that “a different cubic curve might be found that increases or decreases the length of time between the two stable tempos” contradicts one of the given conditions that Rothstein describes, namely “a length of time between [the two given points on the graph].” For in this context of a given condition, “a length” means just that—a length, a singular, unique, one and only length. Rothstein’s interpretation is thus mathematically false.

Moreover, to say that this set of givens “suggests . . . that there is one and only one ‘ideal’ ritard, or accelerando, between any two given boundary tempos” is itself false. That statement implies the generalization of a proposition that in fact is true only for a specific condition—the unique, given length of time between Rothstein’s given boundary points.

Rothstein confuses here the difference between a cubic curve and a cubic *spline*, the latter described in n.5 (555) of chapter 12 (“The cubic curve in Example 12.2 fits within another mathematical classification, that of a spline—a curve that serves the function of joining two lines. It can thus be described as a cubic spline”). A cubic spline is illustrated in Example 12.2 of the chapter (420), its caption reinforcing this definition: “Cubic Spline—the cubic curve X-Y smoothly joins the horizontal lines W-X and Y-Z.” Further explanation is provided on the same page. Referring to Example 12.2, the text explains that “we are presented with two horizontal lines, W-X and Y-Z, and the charge that we join them by a line that will make a ‘smooth’ joint, a joint whose connection with the straight lines provides a maximally gradual transition, devoid of abruptness.” Implicit in the example, and in the descriptions, are the fixed positions of the lines to be joined. Implicit thereby are the fixed positions of the X and Y points on these lines. It follows that these fixed positions bound a fixed duration between the points.

The confusion is compounded when Rothstein states: “This suggests . . . that there is one and only one ‘ideal’ ritard, or accelerando, between any two given boundary tempos.” Yet further confusion follows from his statement that “Epstein seems to encourage the suggestion.” As just seen, Rothstein’s initial given conditions describe a cubic spline. But cubic splines do not “suggest”; they are mathematical fact.

Nor does Epstein “encourage the suggestion.” To the contrary, Epstein and Feldman recognize and state a number of times within this chapter that cubic *curves* (as distinct from cubic splines) can vary individually while retaining their generic identity. Though we do specify (421–22) that “given the end points of such a [cubic] curve, there is only one trajectory that can be drawn between these points that will fit the mathematical description of a cubic curve,” we further explain (438) that dif-

ferences are found in *specific embodiments* of the curve. Speaking of the acceleration and ritard performances studied in the chapter (438), we state: “While the four acceleration/ritard examples viewed above adhere to the model of a cubic curve with striking fidelity, similarities and differences are also found in the application of the model. Karajan’s curves, with their two potential versions and their bell-shaped kinetic aspects, seem unique, as does the de Falla example, with its segmented sections and step-downs. It is not surprising that diversity might reign within the constraints of a prevalent model. It is also not unexpected, though it is certainly striking, that formal prototypes or, more properly, subprototypes also exist within the confines of the model.”

The point is amplified in n.19 (558) of the chapter, viz.: “It is clear that all these curves, while mathematically cubic curves, have somewhat different shapes. In fact, not all cubic tempo curves look alike. Just as there is a parameter (slope) that allows two straight lines to look different from each other, there are several parameters, each embodying degrees of freedom, within the definition of a cubic curve that allow the cubic shape to vary from one example to another. Cubic curves are a small subset of all curves, but still ‘roomy’ enough to permit some variety among them. The R^2 [correlation], relative to a cubic curve, of a particular data set (performance sample) reflects how well that data set matches the *nearest* cubic curve (i.e., the “best fit”). Different data sets with the same R^2 can be near to different cubics, however. Hence data sets with similar R^2 may not have similar shapes, although all data sets with high cubic-curve- R^2 will have a shape that corresponds to the special family of curves that are cubic curves.”

Aside from these explanations, it is visually obvious from the performance examples, graphed as cubic curves, that those curves embody a variety of shapes. Karajan’s acceleration (436, Example 12.7c) begins by going the wrong way (i.e., retarding) before initiating the increase in speed; it ends in a symmetrical but opposite way, accelerating beyond the end-goal tempo before returning (retarding) to it. The acceleration in the de Falla work (431/432, Examples 12.6b/c) is of markedly different profile. Further, the coefficients of the x , x^2 , and x^3 components of the curves, notably in the Stravinsky and Dvorak ritards (respectively 425, Example 12.4b, and 427, Example 12.5b), are also different, indicating via quantitative means the distinctiveness of the individual curves (which is why this information is provided). The Himou curve (446) differs from all the others, not only in duration but in profile as well. Last but hardly least, the five curves of Example 12.9e (444), capturing the profiles of five ritards in performances of Schumann’s *Träumerei*, are notably different. They have been normalized, in fact, specifically to demonstrate these differences. (Normalizing is explained in n.19, p. 558.)

Case II. Re Rubato

Rothstein's discussion (131–32) of the model for rubato offered in chapter 11 of *Shaping Time* is quite mistaken. He correctly portrays a primary aspect of the model (131), namely the proportional phase fit between the lengths of beats and phrases on the most fundamental level of the model (Level I). The errors begin a step beyond this point.

Item. I establish a $\pm 10\%$ range of deviation from a whole-number phase fit of beat duration-to-phrase duration, recognizing, as Rothstein (131) accurately reports, that absolute precision cannot be expected. Rothstein (same page) fails to provide context, however, when he quotes my comment that these “limits are admittedly punishingly severe and restrictive ([Epstein], 377).” He thus obscures an understanding of what this criterion may represent and why. Moreover, given the tone of Rothstein's discussion (“one would suspect Epstein of scholarly malpractice . . .” [131]; “This is the Looking-Glass school of cognitive science . . .” [132]), and lacking adequate background for this “punishingly severe” comment, the comment can appear trivial. It is not. The reasoning behind this $\pm 10\%$ criterion, ignored by Rothstein, is provided within chapter 11 of the book (376–77), viz.:

“In pursuing the case studies that follow, some criterion has been sought by which to discern acceptable deviations from discrepancies beyond the allowable pale. It is not an easy search; ready-made criteria are hardly available. Most studies concerned with limits of temporal perception have dealt with time periods considerably different from those found here, bounded in the main by the psychological present, that all-important period of ca. 0.5–2 sec., which seems to function as a ‘window’ within which so much information is perceived and processed. It is within this sphere that the 5 percent Weber Fraction regarding time perception functions.” (376f.)³

I continue (377): “Acuity of temporal perception seems to diminish rapidly as the perception period widens. (Recall that Getty [1975] and Mach [1865] found the Weber Fraction to rise steeply after two seconds.) To apply the criteria of small-scale timings to rubato studies is unrealistic . . . [for] the temporal periods within which the period/phase correlations take place range from beats minimally of 281 msec. to entire works that cover a number of minutes, the latter durations well beyond the psychological present.”

“In the absence of available criteria we have constructed an artificial criterion, with limits that are admittedly punishingly severe and restrictive [the $\pm 10\%$ discussed above] . . . This is a tough standard, to be sure, particularly if we recall that Getty's test of the Weber Fraction found this tolerance [10–11.5 percent] in time perceptions limited to 2.8 sec. Our durational periods for phrases on Level I are far beyond these bounds.”⁴

In sum, the phrase durations in the three works studied in this rubato

chapter cover a spectrum from 3.228 sec. to 19.753 sec. The majority of the durations lie within double digits. They thus exceed by many orders of magnitude the 2.5 sec. upper limit of the psychological present. Conversely, the 10% criterion for discrepancy is orders of magnitude *smaller* than the Getty and Mach findings of the JND for time periods beyond the psychological present. Given this context, “severe,” arguably “punishingly severe,” do not seem exaggerated descriptions of the $\pm 10\%$ criterion.

Item. Within his same discussion of phase fit Rothstein (131–32) states that “he [Epstein] finds that some phrases do not meet his ‘punishingly severe and restrictive’ criteria (though many do). And so he redefines what he means by ‘beat.’ He substitutes the notion of ‘ground beat’.”

This is a distortion of both context and meaning. Beat and ground beat are not two different things in this model. Nor is ground beat a redefinition, much less a substitute for the concept of beat. Nor in any way is the term ground beat invoked as a *Deus ex machina*, brought into play, as Rothstein implies, to rescue a failing situation.

A ground beat *is* the metric beat that Rothstein (131) describes as a hypothetically metronomic beat. (“He [Epstein] first describes the phenomenon, sensibly enough, as a dichotomy between the performed beat and a hypothetically continued metronomic beat . . .”) The term ground beat indicates the *function* fulfilled by the metric beat in the rubato paradigm. This functional distinction, and the relationship of the terms beat and ground beat, are explicit in passages within the chapter that lay out the model and its properties, viz.:

“Further implicit in both views of romantic rubato is the fascinating phenomenon of two separate and different time controls operating simultaneously as the phrase or other musical segment progresses. There is the *strict metric control of the beat*—the *hypothetically invariant time unit* that serves as the *ground element* for timing. Moving in parallel with this is the pulse of various segments of the phrase, a pulse distorted by rubato as a means of enhancing musical expression.” (Epstein, 373. Emphasis added.)

I continue: “These two time controls [“metric beat” and “pulse”], really *systems* of time control, rapidly become dissynchronous and thus in conflict”, though there is an “eventual reconciliation of these two systems, their return to phase synchrony . . . This resynchronization of beat and pulse lies within the extreme bounds of the phrase itself. It is at the phrase end . . . that the two systems realign.” Note 4 (549), which concerns this point, further makes the link between beat and ground beat: “The idea that *an underlying ground beat* should return into phase synchrony at the end of a phrase is implicit in much thinking about rubato today . . .” (373. Emphasis added.)

Additionally, on p. 374 of the chapter, is the following: “The terms

[Periodicity/Phase] describe the phenomenon discussed earlier [373], in which two parallel different timing systems run simultaneously. One of these systems, *the metric, establishes the basic, or ground, period* that will underlie the full phrase or, on broader levels, a larger musical segment. On the level of phrase, *this ground period is the beat*. Working against it is the pulse of various phrase segments, whose rhythmic timings are distorted via *rubato*.” (Emphasis added.)

Ground beat and beat, as terms and as function, are thus clearly set forth and explained.

Item. Rothstein (132) claims the following: “It is obvious that, given a single ground beat and an allowance of $\pm 10\%$, any randomly selected phrase length has a 20% chance of meeting Epstein’s requirements for phase fit.”

Rothstein’s language, with its invocation of chance, is the language of probability; indeed he explicitly characterizes this and consequent calculations (132, paragraph 2) as probabilities. Probability does not follow from his claim, however. What Rothstein does is depict a 20% criterion of *range* as a probability, which it is not.

Determination of probability would require clear delineation of the elements whose interrelationships give rise to such a probability. Rothstein’s statement, however, is unclear. What does “any randomly selected phrase length” mean? The possibilities are two: 1) any *phrase* selected randomly, that phrase presumed to have a given length; or 2) any *phrase length* selected randomly. Grammatically, the second statement is correct. But the data of these *rubato* studies do not reflect random phrases, or random phrase lengths; the studies present specific lengths, those of specific performances. Moreover, the formulation of probability would entail computations different from Rothstein’s statement.

Thus Rothstein does not present probability here. As noted above, what his 20% figure describes is *range*—namely, the range of allowed discrepancy from an integer fit of ground beat-to-phrase length. This, however, is redundant; the information is a given of the model. The new contribution is the conversion of $\pm 10\%$ to 20%.

Item. Rothstein (132, paragraph 2) describes a test he performs on the data provided in chapter 11 concerning Novaes’s performance of the Chopin A-minor Mazurka, op. 17, no. 4. In his words: “I took the six possible ground beats as performed by Novaes in the first phrase of the Mazurka—since ground beats are performed lengths, they differ in every phrase—and I chose two extremes, 12.691 and 17.223 seconds, based on multiples of my ground beats. (In the outer sections of the Mazurka, all but one of Novaes’s phrase lengths fall between these extremes.) Then I determined, based on the 10% criterion, which ranges of durations between my extremes would constitute acceptable *phase* fits. The answer was slightly above 75%,” which Rothstein identifies as a probability,

namely: “Thus, if Novaes’s first phrase had measured anywhere between 12.691 and 17.223 seconds, it would have had roughly a 75% chance of meeting Epstein’s ‘punishingly severe’ criteria. . . .”

I find it difficult to follow Rothstein’s calculations, since his descriptions of method and the data he provides are spare. It is unclear, moreover, whether his characterization of his figures as probabilities is mathematically appropriate, in view of the discussion of probability found above.

Rothstein’s approach seems partly based upon his intuitions about possibilities of the data, rather than upon the data of chapter 11 themselves, which are factual. But in terms of this intuitive approach are his derived data “hard,” i.e., real, or are they, so to speak, mathematical “paraphrases”? Further, are his data properly interrelated? For example, Rothstein explains, “I chose two extremes, 12.691 and 17.223 seconds.” Why these choices? Only because “all but one of Novaes’s phrase lengths fall between these extremes”? Or, additionally, because they are “based upon multiples of my ground beats”—i.e., based upon Rothstein’s selected multiples of his selected ground beats?

It is also not clear whether Rothstein simply *derives* these phrase lengths from 11 phrases in the piece (the outer sections), or whether these criteria are *applied* to other phrases, though his basis for this choice, as he explains it, is durations performed only in the *first* phrase. There seems in this last point a potential contradiction. Yet more, is this intuitive choice carried out to the exclusion of other data that directly apply to other phrases?

Is there something of a self-fulfilling process here? Come up with a duration, that is, found partly through an intuitive sense of its “rightness” rather than as a matter of fact; multiply the duration a number of times to achieve another, “governing” duration; then find that the governing duration relates within a certain margin to the first duration. Or do I misrepresent Rothstein? Given his limited explanation of method, and in the absence of other data or of computations, it is hard to know.

Rothstein’s method and my study seem significantly different in one respect: from what he explains, his material seems created from other data, thus one step removed from the facts. My study uses the factual data of actual performance.

The absence of data in Rothstein is critical. He provides nothing but two durations. He offers no calculations, nor the mathematical perspectives that justify observations or conclusions drawn from these calculations. Thus readers must accept his conclusions on faith, though faith is not a relevant *modus operandi*. Absent full presentation of his material, and the opportunity thereby for evaluation, neither his argument nor his conclusions are adequate.

Item. Ground beat determination. This is the most complex and diffi-

cult aspect of this model. I am not fully satisfied with the solutions in chapter 11. In this respect Rothstein and I agree. We draw different conclusions about the matter, however. From his discussion I gather that he would scrap the entire enterprise. I do not see it that way. There is evidence here of a significant phenomenon. It will require more development and, very likely, different and more sophisticated mathematical techniques to pinpoint the phenomenon and demonstrate it convincingly.

Rothstein is aware of the difficulty of identifying the beat that carries a tempo, though he discusses it only minimally (“the definition of ‘firm tempo’ in a rubato phrase is a tricky thing” [132]). I describe it in more detail, noting that “although we may hear a rubato performance with a general sense of a common underlying pulse, that pulse is in fact often in flux. The changes are generally subtle, and heard over the span of phrase time their differences are beclouded by the stresses, pulls, and expressive affect of the phrases themselves” (385). I continue: “The operative unit of pulse is not always clear in a performance that continually shifts its ground. What it may appear to be depends upon one’s perceptions, rhythmic reflexes, personal sense of pulse, and feeling for phrase organization among small distinctions in timing and duration. Often, too, as in this piece [Chopin Mazurka, op. 17, no. 4], the music hovers in an in-between state, not really moving in 3 or fully in 1 either, but oscillating continually between these poles. That is the import of the many phrases here where the ground beat was found to vary” (387).

Those several factors—a pulse not really moving in 3 or fully in 1, an underlying pulse often in flux, an operative pulse in a performance that continually shifts its ground—make it “a tricky thing” to nail down a single unit of beat, even a single location of beat. Thus the major question: with respect to a Chopin Mazurka or Waltz moving partly in 1, partly in 3, is it reasonable to suggest that the bar or the quarter note may alternatively be functioning as the ground beat? A “yes” answer is easy if both durations make an integer fit with the phrase length. But what if one duration does, and the other does not? Do we disallow one or the other, or both, based upon the statistical need for consistency of unit across all such phrases? That decision may satisfy the statistician, but does it reveal or does it mask musical phenomena?

The problem is not limited to bar-or-beat dimensions. Therein lies my sense that this issue requires a different mode of mathematical analysis. I discuss this shortly.

Viewed within the framework of *Shaping Time*, however, certainly there are consistent patterns of events in these performances—consistent to an extent that suggests something significant is going on along the lines of the model. If we grant the first bar, or the first quarter note within that bar, status as a ground beat, for instance, then within the Mazurka as played by Novaes 10 of the 15 phrases—67%—fulfill the ground beat

role.⁵ To this we can add phrase 6 which, unlike other phrases, begins in rubato, its ground beat thus occupying the full second bar of the phrase. (See Example 11.1d, p. 386.) Further, all phrases other than no. 6, as noted on p. 387, are played such that the ground beat is articulated in the beginning part of the phrase.

In the Brahms example (Example 11.3c, p. 411), all 14 phrases (100%) embody the singular entity of a quarter note as ground beat. All phrases likewise place this ground beat at the head of the phrase—either upon its upbeat or upon the following quarter note, the phrase downbeat. This within a piece where, as the text discusses (406–15), the structure itself bears ambiguous dispositions of phrases.⁶

I cannot claim equal success in this respect with regard to the Lipatti/Chopin Waltz. In part this may be a shortcoming of the model in its pilot version, which to me suggests all the more the need for a different, more sophisticated mathematical mode of analysis. In part this may also lie with the complex ways that Lipatti shapes timings and articulations within the Waltz, as discussed in my text (395–401). Yet there are patterns and correlations. The matter is not ready for final judgment.

Are there more promising modes of mathematical analysis? I think so. I, in collaboration with colleagues in neurology and mathematics at the medical school of the University of Düsseldorf, am currently at work on this. We anticipate an article in the near future. Among the results of work already published is an article by Large and Jones (1999) that presents a computer model for detecting periodicities among disparate and complex timing data. Essentially the computer compares and cross-correlates all data, algorithms of the program discerning therein periodic patterns. Phase entrainment and phase attractors, the latter an outgrowth of chaos theory, figure in these algorithms. Equally important is the book by Tass (1999) that presents a new theoretical approach to phase resetting and the stimulation-induced synchronization and desynchronization of a population of oscillators [neurons]. Tass's model parallels, in its scientific mode, much of the phase synchrony/dyssynchrony of ground beat and phrase offered in the rubato model in *Shaping Time*. Tass uses stochastic methods from statistical mechanics in working out his model and in presenting his results, some of the latter evidenced by data obtained by MEG and EEG studies.

Jones and Boltz (1989), incorporating elements of the mathematics of dynamic systems, is another source of similarly oriented studies, as is McAuley and Kidd (1998), with its perspectives of neural periodicity as the product of dynamic, adaptive oscillators. Additionally, Drake and Botte (1993) contribute to the growing awareness that time discrimination and its acuity, that the Just Noticeable Difference and the like, seem to be context dependent.

Case III. Distortions of Context (A): Misconstrual; Exaggeration; The “Fiction Factor”

Rothstein’s criticisms contain numerous distortions of context. Some of these are achieved by misconstrual, relating concepts and arguments that are not related by the author nor intended to be so related. Additionally, Rothstein tends to drive distorted contexts to exaggeration.

A second mode of distortion, drawing upon what I have dubbed the “Fiction Factor,” exacerbates the misconstrual approach: A set of concepts, falsified via misconstrual, is presented as a point of view advocated by the author. This fictional view is then attacked by the reviewer. This resembles the “Straw Man” tactic defined by an earlier generation, whereby one creates a fictive figure only to knock him down.

I sample two such cases here:

Item. Rothstein (126–27) quotes three passages about proportional tempo found in *Shaping Time*, offering them as “passages in which Epstein’s core belief is revealed.” He follows with this statement: “The last quotation is especially characteristic, in that Epstein gives the performer latitude with one hand only to take it away with the other. After all, he sniffs, ‘some of us are more gifted than others’ when it comes to matters of tempo ([Epstein], 362). This is a truism in any aspect of musical performance—or of analysis, for that matter—but it is tasteless to state it so baldly” (Rothstein, 127).

Both misconstrual and the “Fiction Factor” are represented here, conjoined in a four-step process:

Step 1 presents the three passages about proportional tempo—Epstein’s alleged core belief. (Two of these quotations, given in full, are discussed later in this letter. See Case IV, third Item.)

Step 2 presents the quote “‘some of us are more gifted than others’ when it comes to matters of tempo.”

Step 3 conjoins the quotes of Steps 1 and 3 via Rothstein’s statement that “the last quotation is especially characteristic, in that Epstein gives the performer latitude. . . .”

Step 4. This connection established, Rothstein attacks it. (“This is a truism in any aspect of musical performance . . . but it is tasteless to state it so baldly.”)

The process is not a simple one, however. For one thing, two of my three passages concerned with proportional tempo are skewed when quoted in Rothstein’s review. As I show later in this letter (Case IV, third Item), the reviewer distorts their contexts by adding emphases that turn their meanings.

Further, the passage in Step 2 is presented completely out of context with respect to the quotes of Step 1. Nor were they intended to be related. Indeed, Rothstein’s “the last quotation is especially characteristic . . .” quote is separated from the “some of us are more gifted . . .” passage by

over 200 pages. The latter phrase in fact arises from a consideration of ideas of Noam Chomsky—ideas concerning innate competence and “performance,” as he sees these matters with respect to language, and the light they may shed on an aspect of tempo in music.

In demonstrating this point, it is helpful to review the summary of the cross-cultural study of tempo which is found on pp. 359–63 of Epstein, chapter 9—a chapter totally ignored by Rothstein, as pointed out earlier. The summary draws a number of conclusions about the study, among them the fact that “when tempos changed in the various musics [of the seven cultures under study], they changed by low-order integral ratios.” Further, “if the newly arrived-at tempo was not exactly in proportion, the deviation in time from this exact ratio was in the majority of cases so small that it was below both the order threshold of perception (PT) and the Weber Fraction (WF)” (359).

The summary also notes that “proportional tempo is a constant in this study—an unchanging factor among musical examples of widely varying cultures, and of varying affect, emotional states, purposes, and settings. It exists in music of processions, of private entertainment . . . , of ceremonies and large social gatherings, of games, even in the rhythms and tempos accompanying barter and bargain . . . , these musics distributed widely across the globe. This suggests both a musical and an aesthetic *constant*, as well as a musical/aesthetic *constraint*. Finally, this study supports the premise suggested in earlier chapters—that an innate biological function seems to be the basis for this tempo phenomenon. It is difficult to see any other basis. Certainly it strains credibility to propose that such closely correlated tempos across so many cultures are a matter of acculturation alone, or the result of chance.

“This universality suggests a mutual neurophysiology. Thus genetic, biological constraints may underlie musical performance. These constraints may augment, as well as underlie, those other bases of authority, of tradition, and of intuition that have heretofore been the sources of our musical training and musical thinking” (359–62).

A question arises from this discussion. “Why, if proportional tempos are natural, do roughly 20 percent of the performances in this study lie outside this practice—not to mention similar exceptions in the performance of Western art music?” (362).

Among the suggested answers are implications of Chomsky’s theories, viz.: “The notion of competence and performance, as developed by Noam Chomsky [as per listed citations] with regard to linguistics, may be applicable to this area of music. Chomsky has determined that human beings everywhere possess an innate competence for understanding grammar and syntax in language. It is this inborn ability that allows us to learn our native language as part of our ‘natural’ childhood development. Nevertheless, though virtually all of us know our native tongue, we vary

in the ability, complexity, even elegance with which we speak ('perform') that language. Thus performance varies within universal, innate competence.

"So it may well be with music, specifically, in this instance, regarding tempo. We may well possess a universal competence to feel tempos in relation to a pulse, and to relate that pulse itself in moments of changing tempos. The degree of precision (and grace, elegance, fluency) with which we effect these tempo changes, however, may vary with individuals. In effect, some of us are more gifted than others" (362.)

Therein the context for the last sentence above. As noted earlier, Rothstein quotes it completely out of context.

The final stage: Rothstein's connective sentence (Step 3) conjoins the disparate quotations of Steps 1 and 2. He establishes thereby the fictitious argument which he then attacks with language suggestive of one who has been morally offended (Step 4).

Item. Rothstein (128) finds the ideas about proportional tempo in *Shaping Time* "contradicted by both compositional and performance practice in the eighteenth and nineteenth centuries" in that, among other things, "movements of a symphony were often not performed together on a program; the first two movements, in particular, might be separated by other instrumental and vocal pieces. At the premiere of Beethoven's Violin Concerto, the soloist improvised between the first and second movements (with the violin held upside down, apparently). Were such interpolated pieces or improvisations to be performed in tempos proportional to those of the surrounding movements? The very question is absurd. . . ."

Indeed, the last statement is absurd. But it is Rothstein's, not mine. No such claim is made in the book.

The essence of Rothstein's criticism is not absurd, but it is illogical. It conjoins two issues that are essentially separate: the composition of a work, with all the structural, syntactical and other relationships, procedures, processes and such that are involved with composition; and what a society at some period of history opts to do with that work.

Following the premises of his argument certainly gets us into muddy waters. Leaving tempo matters aside, are we to assume that because of concert practices such as Rothstein describes, major composers intended no significant relationships among the movements of a multi-movement work? That all that lies across these movements is *ad hoc*? What of works in which ideas from one movement return in another—or, in opera, among different acts?

How shall we characterize the Beethoven Fifth and Ninth Symphonies, as well as his op. 131 Quartet (see note 7, below), the Brahms Third Symphony, the corner movements of Mozart Divertimento, K.136, (and his symphonies discussed in some detail in *Shaping Time* [173–93]), the Mahler First and Fourth Symphonies, the Schumann Piano Concerto (six-

bar transition, movement II to III) and Piano Quintet, the Tchaikovsky Fourth Symphony, the Dvorak String Serenade and Cello Concerto, to mention but a few examples? By Rothstein's reasoning (129) these are "extreme cases," mostly prevalent in the nineteenth century. What constitutes "extreme"? Number of cases? Nature of the relationships? Era? Does "extreme" encompass the fact that these works, with the arguable exception of the Beethoven Fifth, are not "tightly integrated" in the extensive sense to which Rothstein refers? That they make no more than momentary recalls, "flashback" cameos of memory (the Mahler Fourth in fact a "flash ahead" prediction of the final movement), or share *Grundgestalt* characteristics?

Additionally, what of the returns of the two different chorales in the Bach "Saint Matthew Passion," some of these returns reset, all of them with different text? And if nineteenth century opera is allowable in this discussion, what of the brief returning strains of "Mi chiamano Mimi" (Act 1) as Mimi lies dying in Act 4 of "La Bohème"—not to mention Wagner's ever recurring leitmotives?

What of the numerous *Grundgestalt* interconnections in numerous works of the eighteenth and nineteenth centuries that have been demonstrated by a number of analysts? I refer here not to the conventional key relationships or successions of genre types mentioned by Rothstein (129), but to deep-structure relations of theme, rhythm, dynamics, articulations and the like.⁷

Rothstein (129f), perhaps unintentionally, invokes the "Fiction Factor" when discussing the comparative approach of inter-opus studies, which look at works throughout a genre, and the intra-opus approach which focuses upon a specific work. The validity of either approach is not in question. But the either/or context that underlies Rothstein's discussion is certainly questionable. These approaches are in no way mutually exclusive. If anything, they are complementary. The inter-opus approach generalizes; the intra-opus approach particularizes. Moving from the general to the particular is commonplace thinking. It is formalized in the biological sciences; thus genus, and species.

The point applies to Rothstein's argument, viz.: There existed throughout the eighteenth and nineteenth centuries numerous dance forms (*genres*) marked by characteristics of meter, tempo, articulation, character and the like. Composers drew upon these sources for specific works, at times preserving these characteristics, at other times abstracting the music some distance from its roots. Thus the allemandes, courantes, gigues, minuets, sarabandes and such in J.S. Bach's keyboard partitas, cello suites and the like, the minuets of Haydn and Mozart symphonies, sonatas, string quartets and such.

None of this will be news to readers of this letter. Rothstein's primary point, however, that *genre* and inter-opus considerations outweigh, if not

exclude, internal structural matters (intra-opus concerns), can lead to strange conclusions. Taking the Bach keyboard partitas or the cello suites as cases in point, why should we not freely mix the allemandes, courantes, sarabandes, minuets and such among the different suites, or among the various partitas, respecting the major or minor modes of the works, and transposing the movements so that they fit the proper key?⁸ Most musicians, I suspect, will be skeptical of such a proposition (as am I). But if that scheme is not viable, then internal relationships—considerations of motivic shape, harmonic and motivic patterns, other *Grundgestalt* details, and probably tempo matters—must play a role vis-a-vis coherence.

Similar concerns pertain to tempi as found in minuets among works of Mozart—an issue that Rothstein (128ff.) discusses. Rothstein (129) finds me dismissive of Marty's inter-opus approach to this matter. Not so. I point out, however, that Marty's "study would be yet more valuable . . . had he explored the ramifications of his [inter-opus] observations with respect to tempo connections within entire pieces" (Epstein, 527, n. 12). I continue to think so.

A look among the minuets in various Mozart symphonies, string quartets, and the two piano sonatas with such movements illustrates this point. The tempo terms, and the speed categories to which they presumably apply, are not sharply defined among these movements, and there are overlaps as well. This suggests that compositional calculations beyond those explored by Marty may be at work within individual works.

Consider the following sample, for example. Among the minuets marked *Allegretto*, using as a reference tempos commonly heard in our age which I and, I suspect, many others find plausible, I hear commonalities among the symphonies 28, 39, and 41, and the string quartet in E \flat , K. 428. All of these move within a range of 132-to-ca.138 to the quarter (though the "Jupiter" feels more in quasi-one than the others). Yet the D-major quartets, K. 499 and K. 575, both carrying the same *Allegretto* indication, move notably faster, the quarter at about 152–160 (again, some feeling more in 3, others more in 1 or "in-between"). But the minuet of the quartet in C, K. 465, marked not *Allegretto* but *Allegro*, also moves at ca.160 (possibly a hair faster), and the minuet of K. 458, the quartet in B \flat , marked *Moderato*, moves at the lower range of the *Allegretto* examples (quarter ca. 132, perhaps slightly less).

The categories, in other words, are not exclusive. There may be correlations of key and other aspects of *genre* such as Rothstein and Marty describe, though I'm not convinced they are always clearly to be delineated or in all instances necessarily salient. However that issue may be decided, the perplexities of tempo and tempo designations persist.⁹

The problem is yet more complex, for any of these minuets can and may well have been plausibly experienced at tempos perhaps 5–10% faster or slower—all the more so in our time, when early music perfor-

mances have grown ever more prominent, side by side with traditions from our own century and those inherited from the nineteenth century. Where, then, does “validity,” not to mention “authenticity,” lie?

It is in this context that I find the intra-opus approach significant—and not only with respect to Mozart minuets. Internal correspondences among movements, inclusive of tempi; *Grundgestalt* varieties of coherence—these have been discussed and demonstrated in our era. Nor, I repeat, are they incompatible with the generalizations of inter-opus studies. As for the fact that Bach reworked instrumental pieces for use in the cantatas, or that Beethoven shifted the original finale of the A-major Violin Sonata, op. 30, no. 1, to another work in A, the “Kreutzer” sonata (Rothstein, 128): this does not disprove the intra-opus perspective. The shifts and reworkings denote interesting incidents, not prevalent practice.

Case IV. Distortions of Context (B): Omissions: Further misconstruals

I have discussed some of these already (the quote cited in Case III, first Item; the first two Items concerned with rubato, Case II). Further examples follow here—again, only a sampling.

Item. Rothstein (128): “Certainly Epstein often throws caution to the winds, as when he adduces the mere appearance of markings such as *a tempo* or *Tempo I* as evidence for proportional tempos ([Epstein], 154).”

Rothstein chooses not to mention the term *Doppio movimento*, seemingly an incontestable example of proportional relationship, though it is included in my sentence that he quotes (Epstein, 154, top of page). Moreover, the text on p. 154 is a summary of a two-chapter discussion of biological and historical bases for proportional tempo (Epstein, 109–55). The relationship of 1:1 is seen therein as one of these proportions, viewed in the context of connections among movements. *A tempo*, on the other hand, is supportable, obviously, only if a basic tempo has been departed from and then returns. These contexts are not provided by the reviewer.

The chapter on biology includes extensive discussion, replete with sources, of the periodic nature of neural firings—the basis for our so-called “biological clocks.” It further discusses the neurophysical (neuromuscular) entrainment by which the nervous system adapts to periodicities. I suggest in the course of these discussions that if an established musical tempo can be characterized as periodic, it would seem both caused by neural entrainment and an example of it. Therein lies the logic of my remark about *a tempo*. It make sense in this context, as it denotes a departure from an established tempo and a subsequent return to it, the return linked with this function of neural entrainment.

Nothing about these chapters, their discussions, their sources, the summary of them on p. 154, nor the context they provide for my point, is indicated in Rothstein’s remark.

Item. Rothstein (131): “Epstein is not above playing fast and loose with the data. Thus, in his discussion of Mozart’s G-minor Symphony, K. 550—the best and most extensive of his Mozart analyses—Epstein in one place (185) omits the first two eighth-notes of the first violins, Eb-D, when measuring the larger motive of the ascending sixth; a few pages later (192) he includes the same pair of eighths in measuring the same motive. The reason is simple. In the former case, the proportions between this motivic statement and two later ones ‘work’ only if the upbeat is omitted; in the latter a different tempo plan, also proportional, is used, and now the two eighth-notes become necessary for the relationship to ‘work’—and so they are restored. Epstein is nothing if not pragmatic: if a 36-inch yardstick doesn’t give the desired results, he will use another, of 37 inches, or 34 1/2. But he’ll still call it a yard, and he’ll still ask the reader to take his measurements seriously.”

Again, misconstruals of text, distortions of context, falsification. Rothstein merges arguments that are separate, and distinctly different, as I present them. He then draws unwarranted conclusions. To wit:

Discussion I. Page 192 ends a discussion of one plausible set of tempo relations. Some observations based upon those relations are concluded in the first paragraph of that page.

Discussion II. A second set of plausible relations is then discussed, beginning in paragraph 2, viz.: “In closing, we should note another oft heard tempo interpretation of this symphony that is fully compatible with this discussion. It brings the first and last movements into a 1:1 alignment of tempo by means of a small, but flexible, range of pulse that moves between M.M. ca. 132–138. . . .” I then discuss tempo properties seen within this different plan.

Re Discussion I:

Discussion I includes two examples. Concerning the initial tempo scheme (first example): Rothstein is correct that I omit the first two eighth-notes of the first violins, Eb-D [the phrase upbeat], when measuring the larger motive of the ascending sixth (Epstein, 185, Example 8.4b). Rothstein is wrong to say that “a few pages later (192) he [Epstein] includes the same pair of eighths in measuring the same motive” (Example 8.4i, 191—the second example).

I do not do so. Moreover, *I deal with two completely different things in these two examples.* The first example assesses pitches within a motive. The second is concerned with textures, dynamics, harmonies, instrumentation within passages in two different movements. I note the presence of the same motive in both examples, *but I am not concerned with this motive in connection with any measurements.* Thus my text, regarding the second example:

“Another set of interrelated elements is heard at the closing passages

of the developments in these corner movements. . . . The passages are seen in Example 8.4i. Again their affinities are striking. In each case the half-step motive Eb-D is exploited (the inverted C#-D as well, in movement I) with a full *tutti* sound, *forte* dynamic, and largely comparable harmonies. The instrumentation and instrumental registers have much in common, notably in cellos, basses, and bassoons, and the melodic register is also the same.” (191. Text not relevant has been excised to save space.)

I next lay out criteria by which to measure these passages: “There are various ways to view these passages comparatively, since the prolongation of the dominant in movement I goes well beyond the *forte* phrase indicated here.” (191–92) This done, I note the measurement: “If we compare only the *forte* passages in each movement, which provides a similar base of characteristics, we find the the first-movement phrase, *with a duration of eight bars* [my emphasis], amounts to 16 seconds at half=100. The phrase in movement IV [four bars long, self evident from the example] lasts 4.35 seconds, yielding a proportion between the two phrases of 4:1, with a discrepancy of 8.8 percent (again a figure of questionable significance, given the time span between the two segments)” (192).¹⁰

Clearly, as denoted by my emphasis, it is the *bar* that is measured—not the bar plus upbeat, as Rothstein misunderstands; simply the bar, and collections of bars. (The example in question, Ex. 8.4i, p. 191, is reproduced below.) Thus two different things are discussed in Discussion I: 1) Measurement of the D-Bb motive; 2) A second set of measurements that deals with other matters. Even despite this—despite the fact that these measurements are not related, *the upbeats are not included in either measurement*. Rothstein’s criticism is false.

Re Discussion II:

The first issue concluded, I next discuss the second set of plausible tempo relationships (Discussion II, beginning at paragraph 2, p. 192), ex-

The image shows a musical score excerpt from Example 8.4i, spanning measures 152 and 202. The score is for a symphony, featuring strings (Violins, Violas, Cellos, Basses) and woodwinds (Flutes, Oboes, Bassoons). The notation includes various musical symbols such as notes, rests, and dynamic markings. The score is presented in a standard musical notation format with staves and clefs.

Source: Epstein, *Shaping Time*, 1995.

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ploring different aspects of structural features (motives, accompaniment figures, altered character of themes, paradoxical perceptions of speed, etc.) occasioned by the new relationships.

This new perspective involves new criteria, which apply throughout. Thus, as part of this discussion I observe that: “the motivic span from D to the upper B \flat , structurally prominent in the opening themes of movement I, III, and IV [see Example 8.4b], now occupies four beats in each instance, if we alter our mode of reckoning to include *in all cases* the beat that contains the upbeat [my emphasis]. Because all these movements move at the same beat speed, the duration span of the motives is now the same” (192, paragraph 4).

Here, too, Rothstein is off the mark. As just shown, this is a second, completely different case of measurement. The new tempos, not surprisingly, occasion different degrees of relationship. *In assessing them, the same yardstick (i.e., the upbeat) of measurement is applied across the board.*

In sum, Rothstein confuses and melds two separate and distinctly different discussions of tempo. He misreads their criteria of measurement, and comes up with statements that misrepresent what I have written, what I observe, and what I claim. Readers can assess the manner in which he does so.

Item. As mentioned earlier, Rothstein (128) speaks of “Epstein’s rigid adherence to a Schoenbergian standard of unity.” On p. 126 Rothstein speaks in somewhat the same vein, quoting “three passages in which Epstein’s core belief is revealed.” I offer the first two of these:

“The study also suggests that proportional tempo, tempo united by a common pulse, may be *the* clue to a ‘proper’ sense of tempo, leading to tempos that work in the most successful way in terms of musical affect (195, emphasis added [by Rothstein]).”

“Proportionally related boundary tempos seem *a basic condition* governing accelerandos/ritards . . . these elements set the temporal framework within which an accelerando/ritard *must* be shaped (418, emphasis added [by Rothstein]).”

Rothstein’s added emphases promote an impression of rigidity. The normative reading, provided by my plain text, is the intended one. Had I wanted anything else, I would have added italics.

Item. The impression of “rigid adherence” is furthered by the reviewer’s omissions—by the numerous passages about proportional tempo, that is, which Rothstein chooses not to quote. I offer a sampling, with which I conclude this analysis, as these passages may be seen as representative of the true tone and context of my views:

1) Concluding an analytic discussion of tempo in Beethoven, I say: “It may be impossible to draw ultimate conclusions about specific tempos in the Ninth. Tradition has defined one set that, rightly or otherwise, devi-

ates in part from those of the composer. We are told by contemporary witnesses that faster tempos prevailed in Beethoven's time than in our own era. A dedicated contingent of modern performers successfully re-creates the music as it may have sounded in its early days (though that re-creation snags to an extent on the thorny question of tempo). A slippery terrain prevails regarding tempo, in which no base—historical fact, score editions, personal taste, tradition, instruments old and new, performance demands, acoustics, and many other factors—presents perspectives that are not, in some way, irreconcilable.

"The one factor that emerges as a constant in this musical *bouillabaisse* is proportional tempo. Clearly it affected the way Beethoven perceived tempo. Just as clearly, it arose from deeply embedded features of structure within Beethoven's music, adding thereby a temporal dimension of coherence to works that in many other parameters are extensively unified.

"Many conditions will affect actual tempos in a given performance. Ultimately, pacing will rest upon personal intuition, in some ways affected by matters beyond even the control of musicians. (Acoustics is a case in point.) Ultimately, too, we will find tempos satisfying or not upon equally intuitive grounds. That we may find them coherent, however—binding and unifying—seems a *desideratum* that stems from functional aspects of our nervous system as it guides our perceptions, intuitions, feelings" (Epstein, 239–40).

2) Another sample is the passage following the third Epstein "core belief" offered by Rothstein on p. 127. Thus [Epstein, 155]: "We are not machines, however. We can and do depart from an overriding governing pulse in the course of performance. How and where we do so is critical. Flexibility along the lines of the magnet effect [discussed in Epstein, chapter 6, p. 152]—forward movement, ritard, rubato, and the like—adds excitement to performance. These alterations seem to be controlled, however, by a higher order of time organization, their phase relations coordinated on longer spans."

3) I continue, in an endnote affiliated with this last passage: "We can also override a governing pulse, to the detriment of the music, if our musical focus centers upon elements not germane to pulse or to a global view of a work. It is not uncommon to hear musicians linger over a phrase, or exaggerate a ritard or other temporal nuance for the sake of local expression. This can enrich the music if kept within bounds, with a larger temporal design in mind. It can destroy temporal plan and structure when overdone. It would seem to be a matter of degree and, as mentioned above, of focus—where, that is, one centers one's musical thoughts" (Epstein, 518).

This review raises the workload for interested readers. It will not do to read the book and the review, in whatever order, and leave it at that. Given this letter, readers will now have to shuttle between book, Rothstein review, Epstein response, and the various permutations effected by these comparisons in order to figure out where validity may lie.

Ultimately this is to the good. The review has brought to center stage the issue of tempo, which until now has not been at the forefront of theoretical interest. I shall read with interest any response that Rothstein may offer to this letter—in particular, his response to the specific matters in his review that I have raised here.

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NOTES

1. Half=80 is specified as the governing tempo for the “Freude” theme at m. 92, its first occurrence. Though not specified as such when the theme recurs at m. 237, I (and many others) see the connection as implicit.
2. Robert Morgan’s recent article provides significant insights into this question, as do the ideas of Maurice Merleau-Ponty, which Morgan provides (pp. 2–4). Particularly relevant is the discussion of the evolution of ideas, as contrasted with Thomas kuhn’s perspective of paradigm shifts. See Morgan, Robert. “Symmetrical Form and Common-Practice Tonality,” *Music Theory Spectrum* 20/1 (Spring 1998): 1–47.
3. Weber’s Law, and the Weber Fraction derived from it, also known as the Just Noticeable Difference [JND], describe the minimal degree of change from an established periodicity requisite for that change to be perceived. This is discussed in detail on pp. 166–67 (chapter 7) of the book, which is concerned with criteria for studies of tempo in following chapters. As I have noted, the chapter is ignored by Rothstein.
4. The Getty and Mach studies are detailed in chapter 7 (167, and the extensive n.10, 521–23). Getty found that the JND rises to 13–17% at 3.5 sec., the upper limit in his tests; Mach found 18% at 2.5 sec. and 33% at 15 sec. Creelman (1962) found the JND to lie more in the area of 10% within what is essentially the psychological present—from 0.2 sec–2.0 sec. (Details and citations are provided in the text and bibliography of *Shaping Time*.) None of this is discussed by Rothstein.
5. All but one of these (phrase 12) are within the allowed discrepancy of fit.
6. I should note that, from my present perspective, the appearance of a ground beat at the head of a phrase may be a norm, but is by no means a universal nor an all-pervasive demand. There are no rules about this matter. Rubato decisions reflect ways that an artist views a structure and chooses to shape its expressive elements.
7. The Schumann Piano Quintet straddles both categories. It abounds in *Grund-*

gestalt relationships, but also brings back the opening theme of the first movement at the coda of the last movement. Likewise, Beethoven's op. 131 String Quartet, as has been widely observed, emanates to great degree from a Basic Shape—the four opening notes of the fugue (movement I). Yet it comes close to a varied statement of this opening theme in the last movement (m. 21 ff and thereafter). Thus the later theme has something of the “recall” character noted above. The Mozart symphonies fit more the *Grundgestalt* than the “recall” categories.

8. The issue in this hypothetical exercise is musical content, not practical issues such as whether the transposed music “fits under the hands.”
9. Zaslaw (1989), who has devoted much of his career to these vexing questions, parallels many of my thoughts in this letter and in *Shaping Time*. See his discussion of performance practice and tempo, 489–501.
10. Of the eight bars delineated in the first movement phrase, only the first four are included in the musical example.

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