a journal of the Society for Music Theory

Volume 16, Number 4, December 2010

Commentary

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Received November 2010

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[1] The five essays in this issue are noteworthy not only for their focus on Non-Western music, but also for the fact that they involve detailed accounts and analysis of performances and performance technique, both qualitative (Locke, Burns, Scherzinger) as well as quantitative (Polak, Butterfield). This focus on performances, and in many cases the particulars of drumming technique, foregrounds the problems of trying to forge a one-to-one mapping between a series of actions, the sound(s) those actions create, and analytic representations of those sounds. For example, Polak makes useful observations on the timing distinctions between two separate rhythmic events articulated by two hands on a drum, versus a flam that essentially is one "musical object"—and notes that there is a gray area when the flam vs. two-note articulation is not at all clear. Polak, Burns, and Locke also pay close attention to the ways in which timbre plays a crucial role in the definition of particular rhythmic patterns. Thus these five essays cut along many edges.

[2] While I would have enjoyed commenting on all five papers, I will limit my remarks to just two, those by Butterfield and Polak, as they most directly engage my own work and make substantial use of empirical methods of analysis with which I am familiar.

[3] In "Variant Timekeeping Patterns and Their Effects in Jazz Drumming" Matt Butterfield gives a detailed analysis of Steve Davis's performance of "Tune Up," taken from a Jamey Abersold recording. Butterfield's analysis exhaustively catalogs all of the ride cymbal figures Davis uses in this recording and notes their relative distribution against a family of variants of the basic "ding-CHICK-a-ding" pattern. Butterfield's taxonomy of basic patterns is itself of interest, for he both lays out the range of rhythmic possibilities for the ride pattern and analyzes the different motional qualities of each variant. In so doing he is able to do more than just note the relative distribution of each pattern (and pattern families, as per his Table 1); he is also able to discuss how Davis uses different patterns, with their different propulsive qualities, at different points of the composition.

[4] This approach is a good marriage of traditional musical analysis (i.e., our intuitive assessment of the phenomenal qualities of a pattern explained in terms of its material basis) with empirical methodologies that track the distribution and deployment of each pattern. Butterfield's larger point is that previous characterizations of the drummer's role in the ensemble—either as the timekeeper, or as a comping soloist—are guilty of the fallacy of the excluded middle. Jazz drummers can do a bit of both at the same time. As Butterfield shows, by strategically altering the ride patterns themselves, drummers can and do modulate the motional energy of the timekeeping elements themselves.

[5] Butterfield makes an important observation about the sound of the hi-hat that is worth highlighting. As he remarks about his Example 1b, the hi-hat can transform a rhythm from thetic to anacrustic: "the accented quality of the hi-hat, as well as the brevity of its actual sounding duration relative to the longer ride cymbal strike that precedes it, generates anacrusis [/] on the backbeats and thereby directs motional energy to the ensuing downbeat." This is true, but not due to the hi-hat's accent or brevity. Butterfield treats the hi-hat, as music theorists usually do, as an instrument that generates a sound S of some duration D whose onset occurs at some time T. And many sounds are like that. But a hi-hat sound isn't so much an "attack" as it is a modulated envelope of sound—we hear the dynamic shape of its opening and closing, and that is what creates the sense of anacrusis when the hi-hat is present. A hit on the snare, which would also be accented and short, would not have the same effect. To put it another way, the hi-hat sound is not a simple event, but a kind of event-complex. This of course occurs in many other contexts, with various sweeps and swoops of the voice or of instruments that lack fixed pitches. Orchestration can produce similar effects, as can filtering and other effects applied to a live or recorded sound. Butterfield's

analysis here reminds us both of the importance of timbre (here in its ASDR aspects) as well as how important it is to analyze the sounds as we hear them, and not their representations.

[6] Butterfield's analysis also points out a kind of epistemic/ontological problem with the analysis of polyrhythm. I use the combined "epistemic/ontological" descriptive here because I am not sure quite where the problem lies—that is, whether it is with our knowledge of how things are produced, or with the basic categories into which those things are to be understood. A look at Butterfield's Example 4a will make this philosophical problem clearer. In this example Butterfield notes that when a common three-beat Jazz waltz figure is played in the context of a regular 4/4 tune, the result is a high level 4x3 polyrhythm. But as Example 4a points out, the resulting rhythm can readily be understood as a succession of three variants (Butterfield's A^2, C^4, and B^3) of the basic ride pattern. So while we can describe this as 4x3 polyrhythm, should we? Even if we recognize it as such, does Davis conceive of and/or produce the figure as a 4x3 pattern, or as the concatenation of three 4-beat variants? Phenomenally, does it give rise to a sense of cross accents in the context of four, or of two simultaneous layers of motion, moving at different rates? These same questions can be asked of Butterfield's other polyrhythmic analyses. While one can find antiphase layers of motion (Example 4b), and/or more rapid layers (4c), it is not clear that these will be heard as such. I do not have any answers here, but I want recognize that there is a basic problem of analytical description at play for any large-scale metrical dissonance or polyrhythm: while they can be accounted for as the combination of separate streams, each of which has its own distinct and characterizing scheme of organization, by the same token certain concatenations of characteristic figures may appear to be instances of an identical polyrhythm even when they are not. One cannot, from morphology alone, tell the difference.

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[7] In reading Rainer Polak's article "Rhythmic Feel as Meter: Non-Isochronous Beat Subdivision in Jembe Music from Mali" I was and am delighted that my work has proven so useful for Polak's study of Jembe rhythm, both in theory and in practice. I am also appreciative of Polak's special combination of insider knowledge, as an enculturated expert performer, tempered with his careful study of performance timings. Most of all, I am especially pleased with the challenges he raises to my hypotheses regarding non-isochronous (NI) meters. Thus while Polak's article has many important observations on performance, didactic tradition, and research methodology (to mention just a few), I will concentrate my remarks on his "SFL Hypothesis": that in the Jembe music of Mali, one can and should describe the rhythmic structure of its beat subdivisions (sub-pulses in Polak's terminology) in terms of three categorically distinct units: a short (S), a medium (M), and a long (L), and that these units may be strung together (as well as polyphonically coordinated) in a variety of ways. In *manjanin*, for example:

> The subpulse is consistently patterned according to an SFL model of ternary subdivision (short, flexible, long). The main pattern of this feel type is SML (short, medium, long); a second variety is SLL (short, long, long). The feel is characteristic not only of specific phrases, particular grouping structures, single instrumental parts, or individual playing styles. Rather, it is inherent in the meter of the piece [96].

[8] Likewise, in his Table 13 Polak notes, relative to meters of different cardinalities, there are a number of basic subpulse patterns, (a) LSLS, (b) *SFLF*, in which the *F* (flexible) element may be realized as either SMLS, SSLS, or SLLL, (c) *SFL*, realized as either SML or SLL, and (d) *LFF*, realized as LMS, LSS, or LSM. While Polak's performance timing data supports this taxonomy, I disagree with (some of) the theoretical and analytical generalizations he draws from it.

[9] In order to understand our disagreement, I must briefly recapitulate my approach to NI meters, laid out in London (2004). I define the component NI beat classes quantitatively: what distinguishes a short (S) from a long (L) beat is that the S is a (potential) duplet while the L is a (potential) triplet, each based on an isochronous substrate of beat subdivisions. I also argue that it is difficult if not impossible to have more than two beat classes, that is, a medium (M) in addition to the S and L, for this would open the door to various ambiguities and contradictions (e.g., a 1:2:3 ratio involves a double ambiguity of S vs. M and S+M vs. L, as 2*(S) = M and 2*(S+M) = L; similar problems obtain for beats in a 2:3:4 ratio). My argument is that such ambiguities and contradictions muddle the distinction between beats versus beat subdivisions, conflating and confusing different levels of the metric hierarchy. Given also that the absolute value of various durations or IOIs is often determinative of the distinction between beats versus beat subdivisions (with a dividing line around 300-400ms), using ratios other than 2:3 to define S:L beat classes (e.g., 3:5) will often force the short to be on one side of the perceptual divide and the long to be on the other.[(1)]

[10] Polak's approach challenges my arguments on both grounds, (a) that one can have 3 distinct subpulse-classes (to adopt his term), and (b) these distinct subpulse-classes may be defined qualitatively rather than quantitatively. I think he is correct on the latter, but not on the former. I am convinced from both Polak's empirical data and from his ethnographic reports that Jembe players and listeners recognize categorical distinctions amongst subpulses, what I would call elements of beat subdivision. And precisely because these elements are *subdivisions*, one cannot define those categories quantitatively, as I have argued one must do for beats. The distinction must be qualitative. I will come back to this important distinction later.

[11] Where we disagree is whether or not one may have three distinct classes of beat subdivision. I believe Polak's data shows that there are two, and that his M category represents expressive timing variants of underlying short (S) or long (L)

subdivision units. To be clear, I think Polak's data clearly indicate that Jembe performers consistently play different subpulses with different durations depending on their position in the metric cycle. But Polak is making a stronger claim: not that that these are simply expressively timed versions of one or two subpulse-classes, but that they manifest three categorically distinct subpulse-classes. When one makes this claim one is saying that they are "different things" versus "different versions of the same thing." To make an analogy in vision, there are different shades of red, and different shades of orange, but we recognize a break between red and orange. In terms of musical rhythm, a duplet is categorically different from a pair of durations (L-S) that invoke a triplet, though as Clarke (1987, 1989) and Clarke and Windsor (1992), as well as Large (2000) have shown, the same durational ratio may be heard as a duplet in one context and as a triplet in another. Thus categorical determinations are not simply "stimulus driven" but a product of the interaction between stimulus and listener (which rings true with Polak's candid description of his struggles as a novice drummer!). This being so, one must be cautious in the interpretation of empirical timing data, as the presence of different timings may or may not indicate different perceptual categories for the durations involved.

[12] Consider the ratios in Polak's Table 3, which compares sub-pulse timings of several manjanin players, expressed as percents of the total beat length.[(2)] In his table Polak properly gives both mean, range (min-max), and standard deviation of each triplet pulse for each performer. Using the means and SDs, I have re-drawn his table, illustrating the typical ranges for most of the durations in each subpulse category (i.e. capturing about 68%, following the statistical rule for normal distribution):

1st Pulse	2nd Pulse	3rd Pulse	Performer
24–28	30–34	40–44	Kone
25–29	30–34	49–43	Kuyate
21–25	30–34	43–47	Keita
23–27	34–38	38–40	Konate

Most of the SDs are 2%, save for Konate's 3rd pulse (1%). Looking at these restricted ranges eliminates the overlaps at their extremes and also gives us a good idea of how distinct each category is. As can be seen, only Keita has three distinct and widely separated durational ranges; his is the strongest empirical case for three separate pulse-classes. Kone and Kuyate have a clear durational distinction between the 2nd and 3d pulses, but less so between the first two. Conversely, Konate has a clear distinction between his 1st and 2nd pulses, but less so between his last two. Of course, this presumes we can perceive these durational distinctions; at very rapid tempos we probably cannot, as Polak rightly notes (see his [86]). Given the problems in maintaining three separate beat classes noted above, and given Polak's data, I would simplify his model, eliminating the M durational category. This means that the *SFL* archetype, for example, can be realized as either SSL or SLL. But it also means that within each category one still can have durational variation—nuanced Ss or Ls. Thus Kone and Kuyate produce a SSL, while Konate produces a SLL.

[13] So far, Polak has claimed there are three categories; I have argued for two. My worry is that the distinctions, especially between the S and M category, are too small to be perceptually salient. Problems of ambiguity and contradiction also arise between the S and L elements (indeed, as Polak notes the L category is transformed into two shorter categories in cases of nesting). Mostly, however, I want to keep the very useful distinction of within versus between category variation (e.g., burgundy-red versus brick-red, as opposed to red versus orange). Happily, Polak's data also points toward how we might adjudicate our argument. For there are well known research methodologies for studying categorical perception; here it would involve presenting a listener with a pair of stimuli, a standard (either a clear S-S-L pattern or a clear S-L-L pattern) and then a comparison, and asking if the latter is the same or different. It is a "forced choice" experiment, and the experimenter knows that some comparisons will match their standards better than others and that some comparisons will produce more variance than others. The pattern of responses one observes—basically, whether one has a gradual vs. a rapid shift in responses against a gradual change in the comparison stimuli—is what indicates the presence or absence of categorical perception. One would also want to study whether a middle category, if it does occur, is present at most tempos, or only at some tempos, if it is sensitive to parametric interactions (i.e., when drumstrokes are distinguished by timbre and/or pitch), and so forth. Most of all, one can show the extent to which enculturation and musical training affect one's categorical perception, for perceptual categories, whether in speech, color perception, or music, are learned.

[14] Returning to my previous concession, as noted above, Polak has shown that S and. L subpulses involve distinct categories that are metrically salient. I agree—and how can I not? For Polak has rightly forced me to grasp the nettle of the Many Meters Hypothesis: if we have not just tempo-metrical types, but tempo-metrical types that are individuated by patterns of expressive timing (London 2004, 153–155), then metrically-distinct types must involve some categorically different elements on at least one level, if not more, as the basis of their distinction. Qualitatively different categories of subdivision can do this. This means we must sort out those timing distinctions on the subpulse level that are truly categorical versus those that are not, realizing that the latter may still contribute to the expressive character of a gesture, be indicative of different personal styles, etc. Moreover, recent research I have conducted with Bruno Repp and Peter Keller (Repp, London, and Keller 2010) shows that relatively complex durational ratios can be performed as readily as simple ratios based on

isochronous subdivision, and indeed, simple ratios are typically distorted away from their isochronous underpinnings; we found that a 1:2 ratio, even when given by a deadpan pacing stimulus, is often performed in something close to a 4:7 ratio. Thus there is plenty of support for Polak's observation regarding the stability of these different timings in performance, and that in and of itself is a strong hint that some of them may be categorically significant.

[15] The larger lesson from Polak's careful study, however, is this: different levels of rhythmic and metrical structure play by different rules. One-size-fits-all WFCs or MWFRs just don't work; metrical structure is messier, more complex, and more interesting than such simplifying theories will allow. While beat-classes may be distinguished quantitatively, subdivision-classes can and must be distinguished qualitatively. This means that isochrony is not required on any level of the metrical hierarchy, though stability and a hierarchically manifest form of maximal evenness are still required. This also means that it is time for me to go revise my set of WFCs.

[16] I thank Rainer Polak deeply for helping to clarify my thoughts on this matter, and I am eager to see what his forthcoming research may show.

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Footnotes

1. Additional problems that arise with having 3 distinct beat classes in relation to the limits on the shortest possible units for beat subdivisions and longest units possible for beats are discussed in London 2004, 109.
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2. Comparisons of timing data always present challenges, as one cannot simply compare the absolute value of each duration; one must find a means of normalizing variations in tempo within and across performances. Polak's approach is fine so long as tempos are relatively commensurate. I have a small worry, given that in some cases the tempo increase over the course of a Jembe performance is great enough to effect a shift of metrical levels–i.e., what were initially beats may become subdivsions.
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