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Comments on “Rapid motor adaptations to subliminal frequency shifts during syncopated rhythmic sensorimotor synchronization”
by Michael H. Thaut and Gary P. Kenyon
(Human Movement Science 22 [2003] 321–338)

Bruno H. Repp *

Haskins Laboratories, 270 Crown Street, New Haven, CT 06511-6695, USA

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Abstract

Thaut and Kenyon [Human Movement Sci. 22 (2003) 321] have shown that, in a task requiring tapping in antiphase with a metronome, the response period adapts rapidly to a small ($\pm 2\%$) change in the stimulus period, whereas the relative phase between stimulus and response returns to its pre-change value only very gradually. On the basis of these and earlier findings, Thaut and Kenyon argue that period adaptation is rapid and subconscious, whereas phase adaptation is slow and dependent on awareness of a phase error. This interpretation is at variance with results in the literature suggesting that phase correction is rapid and subconscious, whereas period correction is slow and dependent on awareness of a period mismatch. Although differences in terminology (adaptation versus correction) play a role in this conflict, it primarily reflects different conceptions of sensorimotor synchronization and different interpretations of empirical findings. By excluding from their model a central timekeeper or oscillator with a flexible period, Thaut and Kenyon have omitted an essential component of human timing control that is needed for a proper explanation of their results.

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* Tel.: +1-203-865-6163x236; fax: +1-203-865-8963.

E-mail address: repp@haskins.yale.edu (B.H. Repp).

1. Introduction

In a previous study, Thaut, Miller, and Schauer (1998) asked participants to synchronize finger taps with a metronome whose frequency changed abruptly at some point. When this change was small and presumably not detected by the participants, their tapping period adapted rapidly (within a few taps) to the new stimulus period, whereas the relative phase of taps and metronome beats returned to its pre-change value only very slowly. By contrast, when the change in metronome frequency was large enough to be detected, quick adaptation of the relative phase, together with initial overshoot and subsequent rapid adaptation of the response period, was observed. Thaut et al. concluded that period adaptation was rapid and subconscious, whereas phase adaptation occurred only when phase errors exceeded a certain tolerance limit.

The new study by Thaut and Kenyon (2003) – henceforth, T&K – extends these findings to antiphase (syncopated) tapping. The pattern of response adaptation to a small ($\pm 2\%$) change in metronome frequency during antiphase tapping was found to be very similar to that observed by Thaut et al. (1998) during in-phase tapping. The interpretation of the results given by T&K is also quite similar to that of Thaut et al., despite some differences in terminology and modeling approach. That is, T&K are still arguing that period adaptation is rapid and subconscious, whereas phase adaptation is slow and dependent on awareness of a phase error.

T&K pay little attention to intervening research that has suggested a different interpretation. In Repp (2001b), I first replicated the results of Thaut et al. (1998) – with some small differences that need not concern us here – and then conducted a second experiment that led me to a conclusion that seemed quite the opposite of theirs. In particular, I concluded that phase correction is rapid and subconscious, whereas period correction is slow and depends on awareness of a period mismatch. I am going to argue here that the same interpretation applies to T&K's new antiphase tapping results.

The following comments are organized into five sections. First, to explain the difference between T&K's interpretation and mine, and indeed to establish that there is a real difference, I need to clarify the terminology which can be confusing at times. Second, I will present my interpretation of T&K's findings and its relation to oscillator theory, which T&K also appeal to. Third, I will review empirical evidence supporting my interpretation. Fourth, I will examine critically the assumptions that motivated T&K's study of antiphase tapping. Finally, I will consider their mathematical model and the accuracy of their parameter estimates.

2. Terminology and definitions: Adaptation versus correction

In studying any human behavior, it is of fundamental importance to distinguish external, observable response characteristics from the usually unobservable internal processes that give rise to them. This also applies to the study of sensorimotor synchronization (Mates, 1994a, 1994b).

Finger tapping in synchrony with a metronome of fixed frequency yields two principal observable variables: the *inter-response interval* (IRI), also referred to as inter-tap interval or (local) response period, and the *synchronization error* (SE) between a tap and a metronome beat, also referred to as asynchrony or phase error. The metronome is characterized by the *inter-onset interval* (IOI) between beats, also referred to as (local) stimulus period, which is inversely related to (local) tempo or frequency. The IRI and the SE are not independent because each IRI is equal to the concomitant IOI plus the difference between the SEs of the two taps (Vorberg & Wing, 1996, Eq. (17)).

During 1:1 in-phase tapping with a metronome, an unexpected change in the metronome IOI, also referred to as a *step change* or tempo change, necessarily changes the SE because the metronome beat at the point of change occurs earlier or later than expected (a *phase shift*), whereas the tap occurs at the same time as in the absence of a step change.¹ Thus, the IRI (which is highly similar to the IOI up to that point) does not yet change. However, if the participant intends to maintain synchrony with the metronome, both the SE and the IRI will change in the course of subsequent taps: The former will return to its pre-change value and the latter will approach the new IOI. These observable changes (which may include nonlinearities such as overshoot with subsequent reversal of direction) will be referred to here as *SE adaptation* (also, phase adaptation, phase entrainment, or phase locking) and *IRI adaptation* (also, response period adaptation, period entrainment, period locking, or frequency entrainment), respectively. Note that because of the dependence of the IRI on the SE, noted above, SE adaptation implies IRI adaptation, but not the reverse. In other words, it is impossible to be in phase (except momentarily) when tapping at an incorrect tempo, whereas it is possible to tap at the correct tempo but out of phase.

To distinguish between observable changes in response pattern following a perturbation and internal sensorimotor control processes based on perceptual information (feedback), I suggested that the former be referred to as *adaptation* but the latter as *correction* (Repp, 2001b). This is consistent with much of the literature on sensorimotor synchronization (e.g., Mates, 1994a, 1994b; Pressing, 1998, 1999; Semjen, Vorberg, & Schulze, 1998; Vorberg & Schulze, 2002; Vorberg & Wing, 1996), in which the term *phase correction* has been used to denote an internal process by means of which the exact timing of each individual tap is regulated. Phase correction is the basic process that makes it possible to maintain synchrony with an unperturbed metronome. It is usually assumed to be based on perceptual information about the SE (see below for a possible alternative view) and thus constitutes a negative feedback loop between perception and action. Although the apparent purpose of phase correction is reduction of the SE, it is important to realize that phase correction is manifest in terms of *both* SE adaptation and IRI adaptation, and

¹ For simplicity of argument, I am ignoring quasi-random variability, which is always present. Throughout this discussion, I am referring to *expected* values of internal and external variables, which can be estimated by averaging data over a number of identical trials.

sometimes in terms of IRI adaptation only, but *never* in terms of SE adaptation alone. This follows from the dependency of the IRI on the SE, noted above. It may seem paradoxical that *phase* correction can cause only the IRI (the response *period*) to adapt. Yet this is exactly what seems to occur following a small step change in the metronome (Repp, 2001b). To explain this in more detail, it is necessary also to consider period correction.

Period correction (Mates, 1994a, 1994b; Repp, 2001b; Semjen et al., 1998) refers to an adjustment of the period of a hypothetical internal timekeeper or oscillator, denoted by *P* in the following. Period correction, like phase correction, is assumed to be based on perceptual information, primarily (though perhaps not exclusively) on the perceived difference between *P* and IOI (the *period error*, PE). Thus it constitutes a second-order (interval-based) negative feedback loop between perception and action. Although the apparent purpose of period correction is to reduce the PE by adjusting *P*, period correction *by itself* would always be manifested in terms of *both* SE adaptation and IRI adaptation, because *P* affects the IRI by changing its terminal SE (assuming an unchanged metronome). However, period correction never occurs by itself, only in conjunction with phase correction, which is ever-present. Phase correction and period correction thus *co-determine* both observable variables, the SE and the IRI, and their effects are assumed to be additive. This is the *dual-process model* (DPM) of error correction, originally proposed by Mates (1994a, 1994b) and adopted by me (Repp, 2001a, 2001b; Repp & Keller, 2004). Of course, the DPM needs to be tested empirically and modified, if necessary. So far, however, it has provided a very useful framework for understanding sensorimotor synchronization.²

T&K do not adopt the DPM or any other model in the literature but propose their own model instead (discussed later). They justify their approach by saying that “previous dual-process models entail distinctions between internal and external variables that are based on statistically derived assumptions about biophysical and neural properties of timekeeper systems in the brain, which we prefer to subsume under a noise term until stronger neurological evidence is available” (p. 332). However, the distinction between internal and external variables is a fundamental conceptual one that does not rest on “statistically derived assumptions” or on “neurological evidence” and cannot be swept under the rug by “subsuming [it] under a noise term.” T&K go on to say that they “do not need to postulate an internal clock timer” even though there “is little debate that this timing function exists in the brain” (p. 332). The reasons they give for excluding the concept of an internal timekeeper are not convincing, however. Their claim that “long-term correlations disappear during . . . synchronization” (p. 332), besides being inaccurate (see Chen, Ding, & Kelso, 1997, 2001; Chen, Repp, & Patel, 2002), seems irrelevant to the argument. A second claim of T&K, that their model “has a weighted period error correction. . . , providing evidence for direct frequency entrainment” (p. 332) is illogical: How can a *model*

² The DPM is similar to, but not identical with, a phase correction model including first- and second-order terms, as described in Pressing (1998), Semjen et al. (1998), and Vorberg and Schulze (2002).

provide *evidence* of any kind? In the next section I will argue that T&K's exclusion of an internal timekeeper from their model has prevented them from explaining their own findings.

3. The DPM interpretation and its relation to oscillator theory

The DPM interpretation of T&K's antiphase tapping results is essentially the same as that of the in-phase tapping results of Thaut et al. (1998), which I discussed at length in Repp (2001b). There I showed by means of a simple DPM simulation that, following a step change in the metronome IOI, phase correction without any simultaneous period correction leads to *complete IRI adaptation* (i.e., the IRI approaches the IOI within a few taps), while the SE increases at first and then maintains a constant difference from the pre-change SE (i.e., there is no SE adaptation in the long run). When a small amount of period correction is added, the SE gradually drifts back to its pre-change value, just as observed by Thaut et al. (1998) and by T&K. Therefore, I attribute the results of both studies to phase correction with minimal period correction. The more period correction is added, the more rapidly the SE adapts, whereas the IRI shows increasing overshoot before adapting, as is found following larger step changes (Repp, 2001b; Thaut et al., 1998). I am puzzled why T&K ignored this, to me, very convincing demonstration (but see Section 5 below).

One complication of the DPM is that phase correction and period correction are formally interchangeable (Repp, 2001a, 2001b). Therefore, it is true that, in principle, the results of Thaut and colleagues could just as well reflect period correction with minimal phase correction, which is the interpretation they seem to favor (although they do not adopt the DPM). Just fitting the model to synchronization data thus cannot settle the issue. However, there are several compelling arguments in favor of my interpretation, one of which is a partial analogy with weakly coupled oscillators.

The approach of Thaut and colleagues is inspired by dynamical systems theory, to which I am sympathetic. Sensorimotor synchronization can be viewed as an instance of unidirectional coupling (cf. Wimmers, Beek, & van Wieringen, 1992) between a forcing oscillator (the metronome) and a forced limit-cycle (i.e., self-sustaining) oscillator (the taps). However, it also goes beyond this simple physical analogy in theoretically interesting ways. It is well known that every physical oscillator has a natural frequency or eigenfrequency at which it will vibrate when not forced. The natural frequency is a function of physical properties such as mass and stiffness, which do not normally change as a consequence of oscillation. Thus, the natural frequency is typically a constant, and so is the natural period. In other words, physical oscillators are *incapable of period correction*, because period correction (as it is defined here) would change their "internal" (natural) period.

A physical oscillator can be forced by another oscillator to vibrate at a frequency other than its natural frequency, but only within certain limits. The region within

which *period matching* (frequency locking, frequency entrainment, phase entrainment) with a forcing oscillator occurs depends on the difference between the eigenfrequencies of the two oscillators and on the strength of coupling between them (Pikovsky, Rosenblum, & Kurths, 2001). The weaker the coupling, the smaller the range of eigenfrequency differences within which successful period matching will be observed. Period matching is accompanied by a characteristic shift in the relative phase between the two oscillators, which depends on the direction of the difference in eigenfrequencies: The naturally faster oscillator will exhibit a phase lead over the naturally slower one. This relative phase shift is a consequence of the forced oscillator's inability to change its natural frequency, for if it did change its natural frequency, the phase shift would disappear.

We can see now the extent of the analogy to human behavior. Clearly, the period matching of weakly coupled oscillators is analogous to IRI adaptation in human sensorimotor synchronization, and the accompanying phase shift is analogous to the lack of SE adaptation following a small step change. The forced oscillator's natural period is analogous to the human timekeeper period P . Consequently, the persistence of the perturbed SE following a small step change can be interpreted as reflecting a human inability to change P in that situation. (The fact that the SE does adapt gradually suggests that P does change eventually.)

Given that physical oscillators are incapable of period correction, their adaptive behavior during forcing, even though it is often described as period matching or frequency entrainment, can only be due to a process analogous to *phase correction* in humans. Therefore, it is more accurate to refer to it as phase entrainment, which entails period matching. The period dynamics follows from the phase dynamics, which is the primary process. Indeed, Pressing (1999) has shown that the phase error correction parameter of discrete control models such as the DPM is identical with the coupling constant of dynamical systems models.

T&K may have been misled by descriptive conventions when they argue that physical oscillators “only have frequency adjustment capability by definition” and “on-line phase resettings can only be achieved via frequency modulations” (p. 334). On the contrary, changes in the period of a forced oscillator (assuming its mass and stiffness remain constant) are the consequence of continuous or discrete physical forces that act upon the oscillator and whose effect depends on the oscillator's phase at the time of the force's impact (Pikovsky et al., 2001). The frequency of an oscillator is a measure of the periodic recurrence of its phases, which is derived by a human observer. The oscillator only goes through its phases, which are defined in terms of instantaneous state variables, and any external forcing changes the precise time course of these phases. Because a period is a time interval, it is computationally more complex than the registration of mere event occurrences; it is a derivative of the time series of states or phases. It takes a biological system or man-made machine to calculate this derivative. Period computation, and particularly comparison of two periods (as in detecting a PE during synchronization) requires working memory. That is why simple physical oscillators are incapable of period correction, whereas humans do have this capability. Of course, this capability can be modeled as a dynamical system, but the complexity

of such a system must be commensurate to human achievements. (For recent developments in that direction in the modeling of interlimb coordination, see Beek, Peper, & Daffertshofer, 2002.) The DPM may be seen as a discrete linear approximation of such a model (cf. Pressing, 1999).

The process of phase correction in humans thus is similar in some ways to the passive behavior of a forced oscillator: It is largely automatic and does not require awareness of any SE, contrary to what T&K claim (see also the next section). Moreover, as already pointed out, phase correction is obviously the process that accounts for the human ability to stay in synchrony with an unperturbed metronome, when there is no change in the IOI that would motivate period correction (Vorberg & Wing, 1996). There are limits to the analogy with coupled oscillators, however. For example, humans can reduce phase correction by intending “not to react” to a phase perturbation (Repp, 2002a, 2002b), and they can decide not to synchronize at all. Also, there is evidence from studies by Zelaznik and colleagues (Robertson et al., 1999; Zelaznik, Spencer, & Doffin, 2000; Zelaznik, Spencer, & Ivry, 2002) that finger tapping is different from a truly continuous oscillatory motor activity such as circle drawing. Finger tapping is a repetitive discrete activity that approaches oscillatory behavior only at fast rates. Thus, whereas phase correction in humans shows some similarities with a forced oscillator’s behavior, only humans can exert some control over phase correction, and the oscillator analogy may not be fully appropriate for a discrete activity such as finger tapping.

It is likely that period correction is a specifically human ability. It is a manifestation of the more general ability to set the tempo of a rhythmic activity at will (i.e., period setting). Only humans can decide to tap at a fast, slow, or medium tempo, and at many other tempi in between. Although fingers, wrists, and elbows have natural frequencies that reflect the mass of the limbs and the stiffness of the joints and muscles involved, these eigenfrequencies can easily be overridden by an intention to tap at a different tempo and have only vestigial effects, if any, on tapping performance (e.g., Collyer & Church, 1998). In sensorimotor synchronization, the tapping frequency is prescribed by the metronome, and humans easily adopt the specified frequency. As Peper, Beek, and Wieringen (1995, p. 1122) have noted, this period setting “results in an intentional analogue of the eigenfrequencies.” Although humans can change the eigenfrequency of limbs to some extent by deliberately varying muscle stiffness (Peper et al., 1995), this “peripheral” form of period adjustment seems insignificant compared to the wide range of tapping frequencies that can be adopted. Clearly, this wide range requires some kind of central timekeeper or oscillator with a flexible, cognitively controlled period.

Moreover, it is noteworthy that humans seem to be the only animals who can synchronize motor activity with a metronome of any arbitrary period between roughly 200 and 2000 ms (Fraisse, 1974; Merker, 2000). In the rare cases in which synchronization with an external signal has been demonstrated in other species, such as fireflies (e.g., Ermentrout & Rinzler, 1984), the range of frequencies is severely limited, which suggests that the behavior has an inflexible internal period, so that observable period entrainment occurs through phase entrainment, as in physical oscillators.

4. The role of awareness and attention in phase and period correction

Recent empirical findings support the distinction between phase correction and period correction outlined above. Subliminal phase perturbations in a metronome (i.e., local changes in IOI duration) trigger phase correction, but not (or only minimally) period correction in synchronized tapping (Repp, 2000, 2001a). In these and subsequent studies (Repp, 2002a, 2002b), I showed that SE adaptation magnitude immediately following the perturbation is a linear function of phase perturbation magnitude, as is also predicted by the linear phase correction model of Vorberg and Schulze (2002). This suggests that phase correction occurs regardless of whether or not phase perturbations are detected. Still, it could be that phase correction depends on the conscious detection of SEs, rather than of the perturbations in the metronome IOI. This is what T&K (as well as other authors, e.g., Mates, 1994a) believe to be the case. However, although the detection threshold for sensory asynchrony is often cited as being about 20 ms, based on the classic work of Hirsh and Sherrick (1961), this applies only to highly trained psychophysical observers and not necessarily to sensorimotor asynchrony at all. In Repp (2000), I tested six observers' ability to detect asynchronies between a finger tap and a metronome beat. Five out of six performed at or near chance level within a normal range of asynchronies (up to ± 80 ms), but all exhibited normal phase correction. Although detection performance can be improved with specific training (Koch, 1999), untrained observers are generally unaware of small asynchronies but nevertheless correct for them. One reason for the poor detection of SEs may be that the metronome beats are perceived as if they were consequences of the taps, and this "binding" reduces the subjective magnitude of the SE (cf. Haggard, Clark, & Kalogeras, 2002).

Despite this recent evidence in the literature, T&K argue, as did Thaut et al. (1998), that "corrections for phase error may be much more conservative than corrections based on the period error" (T&K, pp. 332–333). I understand this sentence to mean that phase correction is more conservative than period correction (which is incorrect, in my view), not that phase adaptation is slower than period adaptation (which is obviously correct as a description of T&K's data). T&K also refer to a "perceptual threshold for perceived coincidence," which clearly concerns the alleged conservatism of phase correction, but then they go on to identify this threshold with "about 5% of the stimulus interval" and a "preferred synchronization error" (p. 333). Unfortunately, these three quotations refer to entirely different things: (a) the detection threshold for a SE (lack of coincidence), (b) the detection threshold for changes in interval duration, and (c) the mean SE.

Paradoxically, T&K do not mention at all the detection threshold most relevant to their experiment, namely that for a step change. For musically trained observers, that threshold is typically around 2% of the IOI duration (Drake & Botte, 1993; Friberg & Sundberg, 1995; Repp, 2001b; Repp & Keller, 2004). This is the magnitude of the step change T&K used in their stimuli, which therefore was not really subliminal, although they claim that none of their musically trained participants detected any tempo changes. That claim is not implausible because the participants had not been told to listen for any changes and were interviewed (I am inferring this, as T&K do

not say it explicitly) only after the experiment. However, to say that “these results concur with previously reported psychophysical data, as well as previous data regarding thresholds of perception” (p. 326) is slipshod because psychophysical data never concern listeners who are not told about the changes to be detected. T&K do not refer at all to the psychophysical studies that specifically concerned tempo discrimination, such as those cited above.

Contrary to T&K’s views, it is period correction that is conservative, not phase correction. In Experiment 1 of Repp (2001b), I showed that the transition between the different patterns of adaptation to small and large step changes coincided approximately with the participants’ detection threshold for the step changes. Thus, the IRI overshoot, which evidently reflects the additive effects of simultaneous phase correction and period correction, seemed to be contingent on awareness of a change in tempo. Further evidence that period correction is dependent on, or at least facilitated by, awareness of a step change, was obtained in Experiment 2 of my study, in which participants were required to continue tapping at the final tempo of the metronome sequence after it had terminated. Assuming that the average IRI of continuation tapping is a direct reflection of period correction (i.e., the setting of P , not of the IRI) achieved by the end of the sequence, I predicted that period correction would be less effective when a given step change in the sequence had not been detected than when it had been detected. Analysis of the continuation tapping IRIs contingent on the detection responses confirmed this prediction. In general, the continuation tapping IRIs fell short of the changed sequence IOIs, whereas the IRIs during synchronization adapted rapidly (in the case of small tempo changes) or overshoot the changed IOI (in the case of larger tempo changes). This finding is readily interpretable as a result of the fact that phase correction contributes strongly to IRI adaptation during synchronization, but not at all during continuation tapping whose IRIs reflect only “frozen” period correction (as long as no drift occurs).

These findings were replicated in a recent study (Repp & Keller, 2004) which showed, in addition, that period correction requires attention whereas phase correction does not. In a dual-task condition in which participants had to perform mental arithmetic while carrying out the synchronization–continuation–detection task just described, period correction was selectively impaired relative to a condition without a secondary task. We separated the contributions of phase correction and period correction during synchronization by fitting the DPM to the IRI data. These results further supported the hypothesis that period correction is a consciously controlled cognitive process, whereas phase correction is automatic. However, there was also some evidence for a small automatic component of period correction. This, incidentally, is consistent with the gradual SE adaptation that occurs following small, undetected step changes in synchronization. Although this gradual change (in averaged data) could reflect the increasing probability of late detection of a PE, it may well indicate that the period correction process is also mildly sensitive to perceptual information other than the PE, such as the SE or IRI. In the DPM, this is simply represented by a small period correction parameter (see Section 6).

Recent neurophysiological studies have suggested different neural substrates for phase correction and period correction. Stephan et al. (2002) identified different

activation patterns in prefrontal cortex during synchronization with a modulated metronome, depending on whether the modulation was or was not detectable. Although they cautiously interpreted their findings in terms of conscious versus subconscious processes, their results are open to an interpretation in terms of period correction and phase correction, respectively. Praamstra, Turgeon, Hesse, Wing, and Perryer (2003) used the phase-shift paradigm of Repp (2000) and obtained electrophysiological evidence for activation in medial frontal cortex during episodes of period correction, which apparently occurred in addition to phase correction following large phase shifts.

5. Phase correction in antiphase tapping

Why did T&K investigate antiphase tapping? Their stated goal was to “further differentiate between contributions of phase-based (SE) and period-based (IRI) corrections to rhythmic sensorimotor synchrony, because effects of phase corrections must be absent or at minimum strongly reduced and less effective due to the 180° antiphase shift of the motor response relative to the rhythmic stimulus” (p. 323). Four comments can be made on that statement.

First, it is not clear whether “period-based (IRI) correction” refers to IRI adaptation (because the IRI is not an error measure on which correction could be based) or to period correction (i.e., correction based on period *error*). Presumably the latter, because “phase-based (SE) correction” seems to refer to phase correction. Furthermore, if phase correction can occur, why should its effects be absent? What T&K mean, I believe, is that phase correction itself is absent because of the presumed lack of phase error (SE) information in antiphase tapping.

Second, given that extremely slow SE adaptation following small changes in metronome frequency had already been demonstrated during in-phase tapping and (incorrectly) interpreted as an insensitivity to small SEs (Thaut et al., 1998), what more could T&K expect to find in antiphase tapping? Did they expect to find a *total* absence of SE adaptation? Specific predictions are absent from T&K’s paper.

Third, if there were no phase correction in antiphase tapping, participants should not be able to maintain the antiphase relationship at all. It is well known that mere period matching without phase correction results in-phase drift, because of cumulative SE variance (Vorberg & Wing, 1996). Whenever participants are able to maintain the antiphase relationship in antiphase tapping, with or without a tempo change in the metronome, there *must* be phase correction in antiphase tapping, just as it must occur during in-phase tapping.

Fourth, the suggestion that phase correction would be seriously hampered in antiphase tapping is incorrect on both theoretical and empirical grounds. There is no obvious reason why registration of phase errors and consequent phase correction should be restricted to relative phases near zero. When the intention is to tap at a relative phase of 180°, why should it not be possible to monitor deviations from that particular behavioral goal and base corrections on these deviations? True, one might expect that phase correction would be *somewhat* less effective during

antiphase tapping than during in-phase tapping because it seems that participants must monitor either a very large SE (the interval between taps and metronome beats, intended to be half the IOI) or a virtual SE (the interval between taps and the predicted IOI midpoint, intended to be zero). Or, from a dynamical systems perspective, the sensorimotor coupling may be less strong in antiphase tapping. This was indeed my own expectation when I undertook several experiments that compared phase correction in response to phase perturbations during in-phase and antiphase tapping (Repp, 2001a, Experiment 2; Repp, 2002a, Experiments 2–4), but my results showed that phase correction is in fact equally effective in the two tasks. Unfortunately, T&K make no reference to these previous findings, which predicted their results.

T&K make much of the fact that IRI adaptation already occurred in the IRI during which the step change occurred, referring to this as “an immediate adaptation of the motor tempo even when SE corrections are not possible and only differences in period duration are available to the brain” (p. 333). It is, of course, not true that SE adaptation (or do T&K mean phase correction?) is not possible and that only differences in period duration are available to the brain. On the contrary, automatic and subconscious phase correction may be triggered by the changed temporal relationship between the last tap and the shifted metronome beat, or alternatively, by a difference between the mentally predicted IOI midpoint and the shifted metronome beat, whereas the PE information is ineffective without awareness. Although my results concerning phase correction during in-phase and antiphase tapping (Repp, 2001a, 2002a) were reported in terms of SE adaptation rather than IRI adaptation, they clearly implied adaptation of the IRI containing the perturbation in antiphase tapping because the adaptation of the SE immediately following the perturbation was similar to that observed during in-phase tapping. That this adaptation was due to phase correction, and not period correction, was confirmed in Experiment 1 of Repp (2001a), which used the synchronization–continuation tapping paradigm described earlier and showed that there is negligible period correction following a single changed IOI (i.e., a phase perturbation).³

To explain the similarity of results for in-phase and antiphase tapping, it is helpful to consider a possible alternative to the standard assumption that phase correction is based on subconscious perception of the SE. Phase correction may instead result from the competition of two *phase resetting* processes: one that occurs with reference to the most recent metronome beat, and the other with reference to the most recent tap (Hary & Moore, 1985, 1987; Repp, 2001a). Formally, this is equivalent to basing phase correction on the SE, but conceptually it is different because perception of the SE as such is no longer required. In both in-phase and antiphase tapping, each tap is

³ However, because period correction is to a large extent cognitively controlled, it can be employed strategically, for example when participants expect a tempo change to occur. Research in progress is showing that under those conditions even a simple phase perturbation can elicit period correction in addition to the ever-present phase correction. A small amount of automatic period correction may also occur normally in response to phase perturbations (Praamstra et al., 2003; Repp & Keller, 2004).

essentially a timed reaction to the preceding metronome beat. In in-phase tapping, it is timed so as to coincide with (or slightly anticipate) the next beat; in antiphase tapping, it is timed to coincide with the IOI midpoint. Because a shorter interval is being timed in antiphase than in in-phase tapping, the variability should be lower in antiphase tapping, and this is indeed what T&K found (see also Semjen, Schulze, & Vorberg, 1992). This difference disappears and changes sign when the interval becomes shorter than about 250 ms (Semjen et al., 1992). T&K explain the lower variability in antiphase tapping in terms of memory decay for IOI duration, but this is unnecessary in view of the well-documented relationship between interval duration and variability (e.g., Peters, 1989), and it does not explain the nonlinearity around 250 ms. The precise intervals to be timed can be understood as being generated by an internal *forward model* (see, e.g., Mehta & Schaal, 2002) of the tapping task that is constructed rapidly on the basis of sensory feedback. Apart from the fact that different intervals are being timed, the task situation is the same in in-phase and antiphase tapping. In particular, the period of the motor activity is the same, and a tendency to maintain that period (a form of motor inertia, conceptualized as phase resetting with reference to the preceding tap) is assumed to retard what otherwise would be immediate and complete phase adaptation to any perturbation in the metronome sequence. This way of viewing phase correction as the result of two coupled phase resetting processes explains not only the similarity of results for in-phase and antiphase tapping, but also why phase correction does not depend on any perceptual threshold for the SE.

A thought experiment (cf. Experiment 7 in Repp, 2001a) further helps to clarify these ideas. Instead of requiring participants to synchronize a series of taps, in phase or in antiphase, with a metronome containing a small step change, participants could be asked to make a single tap that either coincides with a beat or bisects an IOI. The selected beat or IOI would vary across blocks of trials (e.g., by asking participants to count or by providing an advance cue), so that at the end of the experiment there would be data for each position in the metronome sequence, just as if participants had tapped continuously. These composite data from single-tap trials, I predict, would show immediate and *perfect* “IRI” adaptation (apart from random variability) following the tempo change, even though no IRIs are ever produced in the experiment and only phase resetting processes are operating. However, if internal period correction proceeds as slowly in this situation as during continuous tapping (cf. Large & Jones, 1999; McAuley & Jones, 2003), then only very gradual adaptation of the SE will be observed.

This thought experiment goes to show that the IRI adaptation observed by Thaut et al. (1998) and T&K is due to phase correction (phase resetting), not to period correction or frequency entrainment. With a single tap there is no motor frequency to be entrained.⁴

⁴ Admittedly, there may be frequency entrainment at the perceptual level (Large & Jones, 1999; McAuley & Jones, 2003), but this issue is too complex to be addressed here.

6. The model of T&K

By fitting a mathematical model containing both SE and PE terms to their data, T&K found that the PE makes a much larger contribution than the SE to the observed IRI adaptation. This would seem to imply that period correction rather than phase correction was the dominant process. On closer inspection, however, T&K's model seems to have some serious problems.

The model is a linear autoregression equation consisting of seven additive components that predict the current IRI. For the present discussion, it is sufficient to focus on the first four terms, with the notation revised to conform to the abbreviations used here.⁵ These terms represent the weighted sum of the immediately preceding IOI, IRI, PE, and SE:

$$IRI_i = w_1 IOI_{i-1} + w_2 IRI_{i-1} + w_3 PE_{i-1} + w_4 SE_{i-1}. \quad (1)$$

The first two weights are constrained to add up to 1, so that there are three free parameters in this part of the model. This immediately raises the question: Why three parameters when there are only two hypothetical processes, phase correction and period correction? One degree of freedom must be redundant.

The redundancy is not difficult to find. Given that $w_1 = 1 - w_2$, and given that T&K define the PE as $IRI - IOI$, the first two terms can be rewritten as

$$\begin{aligned} (1 - w_2)IOI_{i-1} + w_2 IRI_{i-1} &= IOI_{i-1} + w_2 (IRI_{i-1} - IOI_{i-1}) \\ &= IOI_{i-1} + w_2 PE_{i-1} \end{aligned} \quad (2a)$$

or alternatively as

$$\begin{aligned} w_1 IOI_{i-1} + (1 - w_1)IRI_{i-1} &= IRI_{i-1} - w_1 (IRI_{i-1} - IOI_{i-1}) \\ &= IRI_{i-1} - w_1 PE_{i-1}. \end{aligned} \quad (2b)$$

Thus, the first two terms of the model (Eq. 1) actually *contain period correction*, as does the third term. The first four terms in the model thus reduce to

$$IRI_i = IOI_{i-1} + (w_2 + w_3)PE_{i-1} + w_4 SE_{i-1} \quad (3a)$$

or equivalently

$$IRI_i = IRI_{i-1} - (w_1 - w_3)PE_{i-1} + w_4 SE_{i-1}, \quad (3b)$$

which has only two free parameters because $w_2 + w_3$ in Eq. (3a) and $w_1 - w_3$ in Eq. (3b) in fact constitute a single coefficient.

By fitting this model (with added noise) to their data after setting $w_1 = w_2 = 0.5$, T&K estimated w_3 to be 0.5 and w_4 to be 0.1. This result led to their conclusion that “the perception of period error might play a more dominant role in response period adaptation than the perception of SE” (p. 330). However, it is difficult to make sense

⁵ The additional terms are a noise term, which is superfluous because T&K make no attempt to model the statistical properties of the raw data, and two terms intended to account for the change in average SE between in-phase and antiphase tapping, an aspect of their data that is ignored here. See also Footnote 1.

of these parameter values and the conclusions based on them. Given that $w_1 = w_2 = 0.5$, it is evident from Eqs. (3a) and (3b) that a value of $w_3 = 0.5$ gives the period error either a weight of 1 or a weight of 0, depending on how the model is formulated. More importantly, the stated parameter values do not seem to be correct. I carried out a simple simulation in Excel to determine what parameter values would give results resembling the actual empirical findings. Appropriate values would be $w_3 = -0.15$ (which gives the PE a weight of 0.35 in Eq. (3a), and of 0.65 in Eq. (3b)) and $w_4 = -0.15$. These weights are very different from the ones T&K report, which do not lead to adaptation at all. One could argue that the values I obtained still suggest a relative dominance of period correction over phase correction (according to Eqs. (3a) or (3b)), but I suspect the model is simply not the right one to use.

For comparison, the DPM (Mates, 1994a, with changed notation for comparability) predicts an expected value of IRI as follows:

$$\text{IRI}_i = P_i - \alpha \text{SE}_{i-1} \text{ (phase correction),} \quad (4a)$$

$$P_i = P_{i-1} - \beta \text{PE}_{i-1} \text{ (period correction),} \quad (4b)$$

$$\text{PE}_{i-1} = P_{i-1} - \text{IOI}_{i-1} \text{ (period error).} \quad (4c)$$

Combining expressions (4a) and (4b) leads to

$$\text{IRI}_i = P_{i-1} - \beta \text{PE}_{i-1} - \alpha \text{SE}_{i-1}. \quad (5)$$

Although this looks superficially similar to Eq. (3b), there are three crucial differences from the T&K model. First, the first term after the equal sign is the internal timekeeper period P , not an observable interval like IRI or IOI. Second, period correction is not applied to the IRI but to P . Third, the PE is defined not as the difference between IRI and IOI, but as the difference between P and IOI. Data roughly resembling those obtained by T&K can be obtained by setting one parameter to 0.65 and the other to 0.15, which (except for differences in sign) happen to be the same values I obtained from the T&K model (Eq. (3b)). The values of the parameters α and β are interchangeable in the DPM as far as synchronization performance is concerned, but other considerations, discussed at length above, lead me to assign the higher value to α (phase correction) and the lower one to β (period correction) for the case of adaptation to small step changes. The DPM leads to a reasonable interpretation of the T&K data, whereas their own model does not. The main reason for the inadequacy of their model is that it is based entirely on observable variables: It regresses the data on themselves and therefore has only descriptive but no explanatory value.

The T&K model also predicts that, if tapping were to continue after termination of the sequence, the continuation IRIs would be equal to the last synchronization IRI, or perhaps to the average of that IRI and the last metronome IOI. Neither prediction is correct, particularly if the metronome sequence terminates soon after a small step change: The continuation IRIs are biased in the direction of the pre-change IOI (Repp, 2001b; Repp & Keller, 2004). As explained earlier, the DPM

explains this by assuming that the continuation IRI reflects the internal timekeeper period P achieved through period correction by the end of the sequence. The T&K model cannot explain unpaced tapping at all because there is no P that could generate the first IRI. It seems that T&K did not intend their model to apply to unpaced tapping, but this limits its range of applicability, whereas the DPM is equally applicable to free and synchronized tapping.⁶

7. Conclusion

In this commentary, I have argued that the results of T&K are not surprising in the light of previous research and can be explained easily within the DPM framework. T&K's own explanation for their findings is unconvincing and is based on apparently incorrect model parameter estimates derived from a questionable model. The major shortcoming of the model and of T&K's approach in general is their exclusion of the concept of a flexible internal timekeeper, which is essential to explaining human timing behavior, both in general and in the specific task situation considered by T&K.

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⁶ A complete version of the DPM would have to include multiple noise sources, which would make it an extension of the well-known Wing–Kristofferson model (see Vorberg & Schulze, 2002; Vorberg & Wing, 1996).

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