Time-out for conflict monitoring theory:

Preventing rhythmic biases eliminates the list-level proportion congruent effect

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Abstract

The proportion congruent (PC) effect is the observation that congruency effects are smaller when most trials are incongruent rather than congruent. The list-level PC (LLPC) effect is the finding that a PC effect can transfer from biased inducer items to unbiased diagnostic items. Such effects are generally interpreted as resulting from conflict monitoring and attentional adaptation. An alternative view proposes that PC effects result from simple learning biases unrelated to conflict. The temporal learning account proposes that LLPC effects stem from a different task rhythm in the mostly congruent and mostly incongruent conditions. Two prime-probe experiments provide a critical test of this notion. In both, half of the participants were forced to withhold responding for a short period of time on inducer trials. This equates the task rhythm in the mostly congruent and mostly incongruent lists, while still maintaining differing levels of conflict. Consistent with the temporal learning account, but inconsistent with the conflict monitoring account, the LLPC effect was eliminated when rhythms were equated.

Keywords: temporal learning; proportion congruent effect; conflict adaptation; conflict monitoring; rhythmic responding

Introduction

The rhythmic timing of actions is a critical part of our interaction with the world (Rosenbaum & Collyer, 1998). Although often overlooked, rhythmic timing plays a key role in shaping behaviour in response time experiments (e.g., Grosjean, Rosenbaum, & Elsinger, 2001), frequently representing an unintended confound (e.g., Lupker, Brown, & Colombo, 1997). The present report aims to demonstrate how a phenomenon typically interpreted in terms of higher-order attentional control processes can be better understood in terms of simple rhythmic biases. The manuscript concludes by discussing the broader implications for a wide range of research questions.

Consider the Stroop effect (Stroop, 1935). Participants are presented coloured colour words and are asked to ignore the word and name the print colour. Response times are slower (and errors more common) when the word is *incongruent* with the colour (e.g., “orange” printed in blue), rather than *congruent* (e.g., “blue” in blue). This impact of the distracting word on performance indicates that selective attention has not fully filtered out the word. Similar *congruency effects* are observed in a broad range of other paradigms (Eriksen & Eriksen, 1974; Neumann & Klotz, 1994; Simon & Rudell, 1967).

Now consider the *proportion congruent (PC) effect* (Logan & Zbrodoff, 1979; Lowe & Mitterer, 1982), which is the observation that congruency effects are diminished if the task contains *mostly incongruent* trials (e.g., 75% incongruent, 25% congruent), rather than *mostly congruent* trials (e.g., 75% congruent, 25% incongruent). According to the *conflict adaptation account*, the PC effect indexes control over selective attention (Cohen, Dunbar, & McClelland, 1990; Lindsay & Jacoby, 1994). For instance, the highly-influential *conflict monitoring account* (Botvinick, Braver, Barch, Carter, & Cohen, 2001), a variant of the conflict adaptation account, argues that participants detect a higher level of conflict in the mostly incongruent condition, and this triggers an adjustment of attention away from the distracting stimulus and/or toward the target stimulus. That is, conflict signals the need for increased attentional control. The result of this conflict monitoring process is a reduced congruency effect in the mostly incongruent condition.

An alternative view proposes that PC effects are not caused by conflict adaptation, but instead by learning of conflict-unrelated regularities in the task (Schmidt, 2013b; Schmidt, Notebaert, & Van den Bussche, 2015). For instance, Schmidt and Besner (2008; see also, Schmidt, Crump, Cheesman, & Besner, 2007) proposed that the PC effect primarily results from simple contingency learning biases. Specifically, distracting words accurately predict the correct response on congruent trials in the mostly congruent condition, and (in many task variants) accurately predict a specific incongruent response in the mostly incongruent condition (e.g., if "red" is presented most often in yellow). Considerable debate has focussed on whether contingency learning provides a sufficient account of the PC effect, or whether conflict adaptation must also be assumed (e.g., Abrahamse, Duthoo, Notebaert, & Risko, 2013; Atalay & Misirlisoy, 2012, 2014; Bugg, 2014, 2015; Bugg & Hutchison, 2013; Bugg, Jacoby, & Chanani, 2011; Grandjean et al., 2013; Hazeltine & Mordkoff, 2014; Levin & Tzelgov, 2016; Schmidt, 2013a, 2014a).

However, the present investigation focuses on the *list-level PC (LLPC) effect*, which is defined here as a PC effect for non-manipulated, contingency-unbiased items. The standard approach for studying LLPC effects is to manipulate some *inducer items* for PC across participants, and then intermix non-manipulated *diagnostic items* to test whether the PC effect transfers from biased inducer items to unbiased diagnostic items. That is, the PC of the list is set by the inducer items, which are either completely/mostly congruent or completely/mostly incongruent, whereas the diagnostic items (which are not manipulated for PC or contingencies) are simply intermixed with either the mostly congruent or mostly incongruent inducers. Said differently, at the level of the items, diagnostic items have the same congruent:incongruent ratio in the mostly congruent and mostly incongruent lists, but fall within different PC list contexts. In many reports, the PC effect did not transfer to diagnostic items (e.g., Blais & Bunge, 2010; Bugg, Jacoby, & Toth, 2008). However, in some conditions a LLPC effect *is* observed (e.g., Bugg, 2014; Bugg & Chanani, 2011; Bugg, McDaniel, Scullin, & Braver, 2011; Hutchison, 2011; Wühr, Duthoo, & Notebaert, 2015).

Of course, contingency learning cannot account for LLPC effects, because diagnostic items are not contingency biased. As a result, the LLPC effect can be regarded as one of the strongest pieces of evidence in support of the notion that conflict adaptation may, indeed, exist in some scenarios. However, although conflict adaptation is certainly one potential explanation for LLPC effects, at least one other account does exist. This alternative is the *temporal learning account* (Schmidt, 2013b, 2013c, 2014b; see also, Kinoshita, Mozer, & Forster, 2011; Schmidt, Lemercier, & De Houwer, 2014). According to this account, participants learn a different task rhythm in the mostly congruent and mostly incongruent conditions. Specifically, participants anticipate a relatively fast response in the mostly congruent condition (i.e., because most trials are fast congruent trials). This *temporal expectancy* for a quick response works to the advantage of congruent trials, because participants have sufficient evidence for a response when they anticipate being able to respond (i.e., early), and this results in a shortcut in responding. On incongruent trials, evidence for a response accrues too slowly to benefit from the expectancy to respond early, however. Thus, the congruency effect increases. In the mostly incongruent condition, participants anticipate a relatively slow response (i.e., because most trials are slow incongruent trials). Incongruent trials benefit from this late expectancy, whereas congruent trials do not. Thus, the congruency effect is decreased. The temporal learning account therefore proposes that the LLPC effect results from simple rhythmic biases in the two list types, and not from conflict detection and attentional adjustment.

Three types of evidence have been forwarded as lending plausibility to the temporal learning account of the LLPC effect. First, it has been shown that previous trial RTs (a measure of rhythms) is strongly predictive of the congruency effect on the current trial (e.g., Kinoshita et al., 2011; Schmidt, 2013c). Second, it has been shown that PC-like effects can be observed in a non-conflict task by creating fast and slow trials with a manipulation other than congruency (e.g., the brightness of a target letter; Schmidt, 2013c, 2014b; Schmidt, Lemercier, et al., 2014). Third, a variant of Parallel Episodic Processing (PEP) model was shown to produce a LLPC effect with a temporal learning mechanism alone (Schmidt, 2013c).

Although these results clearly demonstrate the plausibility of the temporal learning account, they do not provide sufficient evidence to falsify the conflict adaptation perspective in favour of temporal learning. In particular, the computational modelling results only demonstrate the proof of principle that temporal learning could *hypothetically* produce a LLPC effect. Similarly, the autocorrelation between previous trial RT and current trial congruency may actually be due to a process other than temporal learning (e.g., conflict adaptation). Finally, PC-like effects with non-conflict stimuli do not necessarily prove that a similar interaction with conflict stimuli is due, in whole or in part, to temporal learning. For that matter, it might even be the case that both temporal learning *and* conflict adaptation contribute to the LLPC effect. As such, there is currently insufficient evidence to argue that conflict adaptation does not play a role in the LLPC effect.

Experiment 1

In order to demonstrate the sufficiency of the temporal learning account, it is necessary to directly dissociate between conflict-driven and rhythmic processes. This is a challenge, of course, because list-level speed of responding and list-level conflict are almost inherently confounded with each other. That is, mostly congruent lists will tend to promote a faster rhythm than mostly incongruent lists. However, Experiment 1 attempts a dissociation between the two by forcing equivalent rhythms in the mostly congruent and mostly incongruent conditions.

The procedure for Experiment 1 is displayed in Figure 1. On each trial, a briefly-presented distracting location word (prime) precedes a briefly-presented target location word (probe). Like the Stroop task, the distracting prime and target probe can be either congruent (e.g., “left” as a prime to “left”) or incongruent (e.g., “up” as a prime to “down”). This prime-probe task was used (e.g., instead of a Stroop task) for two reasons. First, this paradigm has been used recently (Schmidt & Weissman, 2014, 2015, 2016; Weissman, Egner, Hawks, & Link, 2015; Weissman, Jiang, & Egner, 2014) to investigate the congruency sequence effect (see Gratton, Coles, & Donchin, 1992), another purported measure of conflict adaptation. This has proven highly effective at finding robust effects in the absence of several important confounds (for a discussion of the confounds, see Hommel, Proctor, & Vu, 2004; Mayr, Awh, & Laurey, 2003; Mordkoff, 2012; Schmidt & De Houwer, 2011; Schmidt, De Schryver, & Weissman, 2014). The same controls for confounds eliminated the effect in more traditional paradigms, such as the Stroop and flanker tasks. Thus, if conflict adaptation exists, this particular task should be especially sensitive at finding evidence for it. The second reason for using this task will be outlined in the General Discussion section. It is also noteworthy that this design is nearly identical to the Simon experiments of Wühr and colleagues (2015).

(Figure 1)

The stimulus frequencies for Experiment 1 are presented in Table 1. As with previous investigations of LLPC effects, the current design uses inducer and diagnostic items (in addition to biased items, which will be discussed later). In particular, two of the words (e.g., “up” and “down”) were used as inducers, which were either 100% congruent (mostly congruent) or 100% incongruent (mostly incongruent). The other two words (e.g., “left” and “right”) were used as diagnostic items. These items had the same contingency-unbiased ratio of congruent and incongruent trials (i.e., 50:50) in both proportion congruent conditions. Thus, as in previous reports, inducers set the PC level of the list and the non-manipulated diagnostic items are used to assess list-level learning. An informed reader might note that this design makes use of a simple contingency matrix in which there are some strongly predictive inducers and some unpredictive diagnostic items. Such simple manipulations have often proved unsuccessful in the past (e.g., Blais & Bunge, 2010; Bugg et al., 2008). However, as noted earlier, the prime-probe task is particularly potent (e.g., Schmidt & Weissman, 2014), unlike the Stroop task. This potency also seems to be true of the Simon task (Weissman et al., 2014; but see, Mordkoff, 2012) and a similar design to the current one was used by Wühr and colleagues (2015) with the Simon task, which revealed a robust LLPC effect. Thus, it was deemed likely that the prime-probe task would produce an observable LLPC effect (which proved true).

(Table 1)

Novel to the present investigation, participants were forced to withhold responding on inducer items for a minimum amount of time (for similar manipulations in the word reading literature, see Balota & Chumbley, 1985; Forster & Chambers, 1973). Specifically, a rectangle cue surrounded the target stimulus on inducer trials, which indicated that participants should wait to respond until the rectangle disappeared. In the *short wait* condition, which serves as a baseline control condition, this delay was short (200 ms). As such, participants only have to withhold a response briefly. In the *long wait* condition, however, the delay was 800 ms. This longer waiting period should thus require participants to wait, albeit not long, on both congruent and incongruent inducer items. As such, there will be no difference in RT between congruent and incongruent inducers. That is, participants will have sufficient time to identify the target and determine the response, meaning that they only need to wait briefly before executing their response. The typical difference in response times between congruent and incongruent trials should, therefore, be lost in the delay, even though conflict will likely still be occurring (see the General Discussion for further discussion). Since the remaining items are identical in the mostly congruent and mostly incongruent lists, there will be no overall RT differences between mostly congruent and mostly incongruent lists. That is, the only items that differ in the mostly congruent and mostly incongruent lists are the inducer items, but these items are matched for RT due to the wait manipulation.

If temporal learning is the correct interpretation of LLPC effects, then equating response speed to inducer items in the mostly congruent and mostly incongruent conditions should eliminate the effect for diagnostic items. That is, while a LLPC effect should be observed in the short wait condition (where there is a difference in RT between inducers in the mostly congruent and mostly incongruent lists), it should *not* be observed in the long wait condition (where inducer RT is matched). In contrast, the conflict adaptation account should predict a LLPC effect in *both* conditions. The meaning of the word and the colour still match for congruent inducers, and mismatch for incongruent inducers. Having to withhold a response for a few hundred milliseconds does not change this fact. That is, there is still an informational conflict between the meaning of the distracter and the target on incongruent, but not congruent, trials. It is noteworthy, however, that the conflict adaptation account might be interpreted in several different ways. Here, the conflict adaptation account is described in the way that would seem to follow from the dynamics of the conflict monitoring computational model (Botvinick et al., 2001). However, in the General Discussion, alternative interpretations of the conflict adaptation account will be considered.

As a final note, in addition to inducer and diagnostic items, some *biased items* were also included. Similar to diagnostic items, no waiting was required for these items. However, these items are not free of contingency biases, due to the fact that the prime and probe stimuli overlapped with the inducer items. That is, if inducer items were made up of the words “up” and "down," biased items also used the words "up" and "down." Averaging across the biased and inducer items, primes were predictive of the congruent response in the mostly congruent condition (e.g., an "up" prime strongly predicts an "up" probe) and of the incongruent response in the mostly incongruent condition (e.g., "up" predicts "down"). Thus, a PC effect for these items could result from either item-specific processes (e.g., contingency learning) or list-level processes (e.g., conflict adaptation). These items are therefore not informative for the main question of the manuscript (viz., whether temporal learning or conflict adaptation explains LLPC effects). These items were simply included in order to decrease the number of wait trials in the procedure and are therefore incidental to the design. These items are somewhat interesting, however, as they (unlike diagnostic items) do have a contingency bias.

Method

Participants. Fifty-one undergraduates of Ghent University participated in exchange for €5.

Apparatus. The experiment was performed on a PC laptop. Stimulus and response timing were controlled with E-Prime 2 software (Experimental Software Tools, Pittsburgh, PA). Responses were registered on the laptop AZERTY keyboard using the F-key with the left middle finger for "Left" responses, the G-key with the left index finger for "Right" responses, the J-key with the right middle finger for "Up" responses, and the N-key with the right index finger for "Down" responses. Note that these key positions are spatially compatible with the direction words.

Design. All stimuli were presented in white on a black background. The distracter (prime) and target (probe) stimuli consisted of the Dutch direction words "Links" (Left), "Rechts" (Right), "Boven" (Up), and "Beneden" (Down) presented in bold Courier New font. Distracters were presented in 20 pt font and targets were presented in 10 pt font. Horizontal primes (i.e., "Left" or "Right") were only presented with horizontal probes, and vertical primes (i.e., “Up” or “Down”) were only presented with vertical probes. One of these two sets (e.g., horizontal) functioned as the diagnostic items, which were presented on even trials. Diagnostic items were equally congruent and incongruent. The other set of stimuli (e.g., vertical) functioned as inducer and biased items, which were presented on odd trials. Biased items, presented on 10% of the trials, were equally congruent and incongruent. Notably, these items are confounded by contingencies with inducer items (unlike diagnostic items). Inducer items, presented on 40% of the trials, were the only items that differed across blocks/conditions. They were either 100% congruent (mostly congruent) or 100% incongruent (mostly incongruent). These items therefore set the PC of the list. Inducer items required that participants withheld a response for either 200 ms (short wait condition) or 800 ms (long wait condition).

Which stimuli (horizontal or vertical) served as diagnostic items and which served as biased/inducer items was counterbalanced across participants. PC was manipulated within participants in two separate blocks, counterbalanced for order (and orthogonal to the stimulus assignment counterbalancing). Each block consisted of 200 trials, presented at random with replacement (with the odd/even trial constraint mentioned above), for a total of 400 trials. Orthogonal to the other manipulations, wait duration for inducer items was manipulated between participants to prevent contamination (i.e., transfer of learning) between blocks (deemed crucial after pretesting with a related non-conflict procedure).

Procedure. Each trial consisted of five sequential events: a distracting word for 133 ms, a blank screen for 33 ms, the target for 133 ms, a blank screen (response window) until a response was made or 1367 ms elapsed, and a feedback screen (see Figure 1). Additionally, for inducer items, a 5 pixel white outline of a 40 x 80 pixel rectangle was presented around the target, which stayed on the screen for another 67 ms (200 ms total) or 667 ms (800 ms total; i.e., depending on the wait condition) before the response window was presented.

Following correct responses, the feedback screen was blank and presented for 500 ms. For all other trials, the feedback screen was presented for 1500 ms and one of the following three messages was presented in red, bold, 18 pt Courier New font: “Fout!” (Error!) for incorrect responses, “Te Traag!” (Too Slow!) if a participant failed to respond before the end of the response window, or “Te Snel!” (Too Fast!) if participants responded before the target/wait signal disappeared. In the case where participants responded too quickly, the trial preceded immediately to the error message.

Data analysis. Both correct response times and percentage error rates were assessed. All trials preceding an error were eliminated from analyses. All participants had sufficiently high accuracy rates (>70%), so no participants were excluded. All response times are reported from target stimulus onset. In the analyses to follow, the counterbalancing order of the mostly congruent and mostly incongruent blocks is ignored. It was first confirmed that counterbalancing did not influence the main contrast of interest (i.e., the three-way interaction between PC, congruency, and wait condition) and then this factor was discarded as noise. The same is true of Experiment 2 to follow. However, for the interested reader, this analysis did replicate the asymmetric list shifting effect (Abrahamse et al., 2013), though this effect is difficult to interpret due to a practice confound (Schmidt, 2016).

Results

The results section is divided, for each dependent measure, into subsections for each item type. In particular, first inducer items are assessed as a manipulation check (i.e., to ensure that the wait manipulation significantly affected the congruency effect for inducers). Next, the critical diagnostic items are assessed to test for LLPC effects. Finally, the (more incidental) biased items are considered, though they are not informative for the main question of this research (i.e., given the contingency confound).

Response times. ***Inducer items.*** In the short wait condition, responses were significantly faster to congruent trials (554 ms; *SE* = 22) than to incongruent trials (654 ms; *SE* = 23), $t(23) = 5.323$, $SE_{diff} = 19$, $p < .001$, $\eta_p^2 = .55$, demonstrating a standard congruency effect of 100 ms. In the long wait condition, this difference between congruent (1077 ms; *SE* = 12) and incongruent trials (1084 ms; *SE* = 13) was not significant, $t(26) = .883$, $SE_{diff} = 8$, $p = .385$, $\eta_p^2 = .03$. Though the latter 7 ms numerical difference was not completely zero, these results indicate that the wait manipulation was successful in (roughly) equating the speed of congruent and incongruent inducer items.

Diagnostic items. Most importantly for the present purposes are the diagnostic items. The data for diagnostic items are presented in Figure 2. A 2 PC (mostly congruent vs. mostly incongruent) by 2 congruency (congruent vs. incongruent) by 2 wait condition (short vs. long) ANOVA was conducted on diagnostic items. This produced a significant main effect of congruency, $F(1,49) = 217.992$, $MSE = 1947$, $p < .001$, $\eta_p^2 = .82$, because responses were faster to congruent trials than to incongruent trials. There was also an interaction between PC and congruency, $F(1,49) = 13.191$, $MSE = 785$, $p < .001$, $\eta_p^2 = .21$, indicating a LLPC effect. Critically, this was further modified by a significant interaction between PC, congruency, and wait condition, $F(1,49) = 6.712$, $MSE = 785$, $p = .013$, $\eta_p^2 = .12$, because the PC effect was larger in the short wait condition. No other effects were significant (*F*s ≤ 1.065, *p*s ≥ .307). To decompose the three-way interaction, the two wait conditions were analysed separately. Most critically, the PC effect was significant in the short wait condition (effect: 49 ms), $F(1,23) = 14.324$, $MSE = 1002$, $p < .001$, $\eta_p^2 = .38$, but not in the long wait condition (effect: 8 ms), $F(1,26) = .762$, $MSE = 593$, $p = .391$, $\eta_p^2 = .03$. For this last test, power was high (.8) to detect an effect as small as 27 ms and medium (.5) to detect an effect as small as 19 ms. Thus, the LLPC effect was eliminated in the long wait condition.

(Figure 2)

Biased items. More incidentally, the design also included biased items. The proportion congruent effect for these items was significant, F(1,49) = 13.091, MSE = 2328, p < .001, η_p^2 = .21, and the interaction between PC, congruency, and wait condition was marginal, F(1,49) = 3.850, MSE = 2328, p = .055, η_p^2 = .07, because the PC effect was larger in the short wait condition. The PC effect was significant in the short wait condition (effect: 76 ms), F(1,23) = 15.093, MSE = 2268, p < .001, η_p^2 = .40, but not in the long wait condition (effect: 22 ms), F(1,26) = 1.425, MSE = 2381, p = .243, η_p^2 = .05. It is noteworthy, however, that biased items were of relatively few in number (e.g., five times less than diagnostic items), and there was some hint of an effect in the long wait condition.

Error percentages. ***Inducer items.*** In the short wait condition, there were marginally less errors to congruent trials (5.1%; SE = 1.1) than to incongruent trials (7.4%; SE = 1.2), t(23) = 1.994, SE_{diff} = 1.1, p = .058, η_p^2 = .15, demonstrating a standard congruency effect of 2.3%. In the long wait condition, there were significantly less errors to congruent trials (1.0%; SE = 0.3) than to incongruent trials (1.8%; SE = 0.4), t(26) = 2.387, SE_{diff} = 0.4, p = .025, η_p^2 = .18, albeit numerically much smaller than in the short wait condition. Thus, this 0.8% congruency effect indicates that the wait manipulation was mostly (but not entirely) successful at eliminating rhythmic biases in errors, similar to the response time data.

Diagnostic items. The data for diagnostic items are presented in Figure 3. Again, a 2 PC (mostly congruent vs. mostly incongruent) by 2 congruency (congruent vs. incongruent) by 2 wait condition (short vs. long) ANOVA was conducted. This produced a significant main effect of congruency, F(1,49) = 35.372, MSE = 41.0, p < .001, η_p^2 = .42, because there were less errors to congruent trials than to incongruent trials. There was also a significant main effect of wait condition, F(1,49) = 4.504, MSE = 70.3, p = .039, η_p^2 = .08, because there were more errors in the short wait condition than the in the long wait condition. PC and wait condition marginally interacted, F(1,49) = 3.246, MSE = 21.9, p = .078, η_p^2 = .06, because there were more errors in the mostly congruent short wait and mostly incongruent long wait conditions. The interaction between PC and congruency was not significant, $F(1,49) = 2.611$, $MSE = 20.3$, $p = .113$, $\eta_p^2 = .05$. There was also no interaction between PC, congruency, and wait condition, $F(1,49) = 1.828$, $MSE = 20.3$, $p = .183$, $\eta_p^2 = .04$. No other effects were significant (Fs $\leq .860$, ps $\geq .358$). Despite the lack of a LLPC effect or three-way interaction, for completeness the two wait conditions were again analysed separately. Most critically, the PC effect was marginal in the short wait condition (3.7%), $F(1,23) = 3.786$, $MSE = 22.3$, $p = .064$, $\eta_p^2 = .14$, but was not present in the long wait condition (0.3%), $F(1,26) = .040$, $MSE = 18.5$, $p = .842$, $\eta_p^2 < .01$. For this last test, power was high (.8) to detect an effect as small as 4.8% and medium (.5) to detect an effect as small as 3.4%. Thus, the error data were similar to the RT data, but less robust.

(Figure 3)

Biased items. Again, biased items are briefly considered. The proportion congruent effect was marginal for these items, $F(1,49) = 2.813$, $MSE = 55.4$, $p = .09985$, $\eta_p^2 = .05$, as was the interaction between PC, congruency, and wait condition, $F(1,49) = 2.873$, $MSE = 55.4$, $p = .096$, $\eta_p^2 = .06$, indicating a larger PC effect in the short wait condition. The PC effect was significant in the short wait condition (effect: 7.0%), $F(1,23) = 5.840$, $MSE = 50.9$, $p = .024$, $\eta_p^2 = .20$, but not in the long wait condition (effect: < 0.1%), $F(1,26) < .001$, $MSE = 59.3$, $p = .990$, $\eta_p^2 < .01$.

Discussion

The results of Experiment 1 achieved two things. First, the experiment successfully demonstrates for the first time that a contingency-unbiased LLPC effect can be observed in the prime-probe task, which also conceptually replicates a near-identical Simon design introduced by Wühr and colleagues (2015). Second, the experiment demonstrated for the first time that the robust LLPC effect observed in the short wait condition can be eliminated by controlling for rhythmic biases. In the long wait condition, participants were forced to respond at equal speed to the inducer items that set the PC of the list. This eliminated the LLPC effect for the critical diagnostic items. This provides the strongest evidence to date that temporal learning is a plausible account of the LLPC effect. The conflict monitoring account, in contrast, should have predicted a LLPC in the long wait condition, potentially even of equivalent magnitude to that observed in the short wait condition (see General Discussion for further discussion, however).

Experiment 2

Experiment 2 addresses a potential limitation with the design of Experiment 1. In Experiment 1, the response-stimulus interval (RSI) was kept constant on all trials (with a correct response). That is, the time between making a response and the presentation of the next stimulus was fixed. One potential issue with this is that inter-stimulus-interval (ISI) did vary from trial to trial. That is, the time between the onset of one stimulus on one trial and the next stimulus on the following trial was determined based on the time it took a participant to respond. Though it is typical procedure to fix the RSI (rather than the ISI), this did mean that the ISI was particularly long following a long wait inducer item in Experiment 1. It could therefore be argued that the conflict detection signal from a long wait trial decays too much before the following trial is presented. If so, then this may be the *real* reason that the LLPC effect was eliminated in the long-wait condition.

In order to rule out this possibility, in Experiment 2 the exact same experiment was conducted with one exception: the 500 ms RSI following long wait inducer items was removed. As such the ISI following long wait inducers will be comparable or even shorter than the remaining trials. If the decrease in the LLPC effect with long wait inducers was due to decay of the conflict signals, then the decrease in the LLPC effect should clearly no longer be observed with the new manipulation. In contrast, if the reason for the elimination of the LLPC effect was due to rhythmic disruption, the elimination should again be observed. The author would like to thank an anonymous reviewer for this suggested modification.

Method

Participants. Fifty-one undergraduates of Ghent University participated in exchange for €5.

Apparatus, design, and data analysis. The apparatus, design, and data analysis for Experiment 2 were identical in all respects to Experiment 1.

Procedure. The procedure of Experiment 2 was identical in all respects to Experiment 1, with one exception. The 500 ms feedback screen for correct responses was eliminated following long wait inducer items.

Results

As in Experiment 1, the results are again divided into separate analyses on each item type for each dependent measure. First, inducer items are analysed to ensure that the wait manipulation was successful in modulating the congruency effect (i.e., manipulation check). Next, the critical diagnostic items are assessed to test for a (contingency-unbiased) LLPC effect. Finally, the biased items are considered in a supplementary analysis.

Response times. ***Inducer items.*** In the short wait condition, responses were significantly faster to congruent trials (523 ms; *SE* = 13) than to incongruent trials (612 ms; *SE* = 16), $t(25) = 6.127$, $SE_{diff} = 15$, $p < .001$, $\eta_p^2 = .60$, demonstrating a standard congruency effect of 89 ms. In the long wait condition, this difference between congruent (1071 ms; *SE* = 14) and incongruent trials (1083ms; *SE* = 13) was not significant, $t(24) = .856$, $SE_{diff} = 14$, $p = .400$, $\eta_p^2 = .03$. As before, this small 12 ms numerical difference indicates that the wait manipulation was successful in (roughly) equating the speed of congruent and incongruent inducer items.

Diagnostic items. Most importantly for the present purposes are the diagnostic items. The response time data for diagnostic items are presented in Figure 4. Again, a 2 PC (mostly congruent vs. mostly incongruent) by 2 congruency (congruent vs. incongruent) by 2 wait condition (short vs. long) ANOVA was conducted on diagnostic items. This produced a significant main effect of congruency, $F(1,49) = 177.399$, $MSE = 2461$, $p < .001$, $\eta_p^2 = .78$, because responses were faster to congruent trials than to incongruent trials. Curiously, the interaction between PC and congruency was not significant, $F(1,49) = .611$, $MSE = 765$, $p = .438$, $\eta_p^2 = .01$. More importantly, however, there was a significant three-way interaction between PC, congruency, and wait condition, $F(1,49) = 9.271$, $MSE = 765$, $p = .004$, $\eta_p^2 = .16$, because the PC effect was larger in the short wait condition. The main effect of wait condition was again significant, $F(1,49) = 13.832$, $MSE = 17921$, $p < .001$, $\eta_p^2 = .22$, as was the interaction between wait condition and PC, $F(1,49) = 4.791$, $MSE = 3012$, $p = .033$, $\eta_p^2 = .09$. No other effects were significant (Fs $\leq .448$, ps $\geq .507$). To decompose the three-way interaction, the two wait conditions were analysed separately. The PC effect was marginally significant in the short wait condition (effect: 18 ms), $F(1,25) = 3.253$, $MSE = 615$, $p = .083$, $\eta_p^2 = .12$, but was significantly *reversed* in the long wait condition (effect: −30 ms), $F(1,24) = 5.958$, $MSE = 922$, $p = .022$, $\eta_p^2 = .20$. This reversal is, of course, the opposite of what the conflict adaptation account predicts. It is equally difficult, however, to imagine what other account might explain such a reversal (e.g., the temporal learning account should predict a null). Though this significant finding should not be discounted out of hand, it remains possible that this observation is a Type 1 error. For instance, no similar studies have observed such a reversal and closer inspection of the data seems to indicate that the reversal is driven by a few notably outlying participants (with the trend for the remaining participants centered around zero). What is clear, however, is that the LLPC effect was certainly not maintained in the long wait condition.

(Figure 4)

Biased items. As a supplementary analysis, biased items are again assessed. The proportion congruent effect was significant for these items, $F(1,49) = 22.782$, $MSE = 2027$, $p < .001$, $\eta_p^2 = .32$, but the three-way interaction between PC, congruency, and wait condition was not, $F(1,49) = .566$, $MSE = 2027$, $p = .456$, $\eta_p^2 = .01$, albeit trending in the expected direction. The PC effect was significant in both the short wait condition (effect: 70 ms), $F(1,25) = 15.664$, $MSE = 2015$, $p < .001$, $\eta_p^2 = .39$, and the long wait condition (effect: 51 ms), $F(1,24) = 7.879$, $MSE = 2040$, $p = .010$, $\eta_p^2 = .25$.

Error percentages. ***Inducer items.*** In the short wait condition, there were significantly less errors to congruent trials (4.8%; $SE = 1.4$) than to incongruent trials (9.6%; $SE = 1.4$), $t(25) = 3.024$, $SE_{diff} = 1.6$, $p = .006$, $\eta_p^2 = .27$, demonstrating a standard congruency effect of 3.8%. In the long wait condition, the difference between congruent trials (3.4%; $SE = 0.9$) and incongruent trials (5.2%; $SE = 1.4$) was not significant, $t(24) = 1.456$, $SE_{diff} = 1.2$, $p = .158$, $\eta_p^2 = .08$. Thus, this 1.8% congruency effect indicates that the wait manipulation was (mostly) successful at eliminating rhythmic biases in errors, similar to the response time data.

Diagnostic items. The data for diagnostic items are presented in Figure 5. Again, a 2 PC (mostly congruent vs. mostly incongruent) by 2 congruency (congruent vs. incongruent) by 2 wait condition (short vs. long) ANOVA was conducted on diagnostic items. This produced a significant main effect of congruency, $F(1,49) = 42.890$, $MSE = 99.2$, $p < .001$, $\eta_p^2 = .47$, because there were less errors to congruent trials than to incongruent trials. The interaction between PC and congruency was not significant, $F(1,49) = .581$, $MSE = 30.8$, $p = .449$, $\eta_p^2 = .01$. There was also no interaction between PC, congruency, and wait condition, $F(1,49) = .086$, $MSE = 30.8$, $p = .771$, $\eta_p^2 < .01$. No other effects were significant (Fs ≤ 1.324, ps $\geq .256$). Despite the lack of a LLPC effect or three-way interaction, for completeness the two wait conditions were again analysed separately. The PC effect was not significant in both the short (−0.7%), $F(1,25) = .095$, $MSE = 36.7$, $p = .761$, $\eta_p^2 < .01$, and long wait conditions (−1.6%), $F(1,24) = .682$, $MSE = 24.7$, $p = .417$, $\eta_p^2 = .02$.

(Figure 5)

Biased items. Again, biased items are briefly considered. The proportion congruent effect for these items was not significant, $F(1,49) = 1.133$, $MSE = 74.2$, $p = .292$, $\eta_p^2 = .02$, but there was a significant three-way interaction between PC, congruency, and wait condition, $F(1,49) = 4.308$, $MSE = 74.2$, $p = .043$, $\eta_p^2 = .08$, indicating a larger PC effect in the short wait condition. The PC effect was marginally significant in the short wait condition (effect: 7.6%), $F(1,25) = 3.757$, $MSE = 99.3$, $p = .065$, $\eta_p^2 = .13$, but not significant in the long wait condition (effect: −2.4%), $F(1,24) = .774$, $MSE = 48.1$, $p = .388$, $\eta_p^2 = .03$.

Discussion

As in Experiment 1, the results of Experiment 2 again are consistent with the notion that the LLPC effect might be due (primarily or even entirely) to temporal learning. LLPC effects were again observed in the short wait condition, albeit only marginally. In the long wait condition, the effect was actually in the wrong direction (i.e., a reversed LLPC effect). This is certainly inconsistent with the conflict adaptation account, but also not predicted by any other account the author can imagine. Type 1 error seems probable, though further investigation would be warranted if a similar reversed effect emerges in subsequent investigations. In either case, the critical interaction with wait condition was again replicated, consistent with the temporal learning view. Given the shortening of the RSI following long wait inducers in the current experiment, this seems to rule out the proposal that the true reason for the elimination of the LLPC effect in Experiment 1 was decay of the conflict signal. The experiment also provided evidence of PC effects for biased items, even in the long wait condition. As already mentioned, biased items share contingencies with the manipulated inducer items, so these items are not a measure of the LLPC effect and are not informative for the main question of this research. The observed effect for biased items can represent contingency learning or any other item-specific process (e.g., item-specific conflict adaptation). This pattern was also evident in Experiment 1, but was only significant in the current experiment.

General Discussion

The present results help to distinguish between two competing accounts of the LLPC effect. While the conflict adaptation account proposes that greater levels of conflict in the mostly incongruent condition lead to an attenuated congruency effect, the temporal learning account proposes that differences in rhythms explain this same effect. The experiments in the present report aimed to dissociate between these two mechanisms by equating rhythms in the mostly congruent and mostly incongruent conditions, while simultaneously maintaining a difference in conflict proportions. This manipulation led to the elimination of the LLPC effect in the long wait condition for diagnostic items, which was also significantly smaller than the effect observed in the short wait control condition. The present results therefore contribute the strongest evidence to date for the temporal learning account and provide a potential problem for the conflict adaptation perspective.

Implications For Conflict Adaptation Theory

Although the present results seem difficult to reconcile with the conflict adaptation perspective, there may still be ways to construe the account in a way to fit the present results. Here, some of these possibilities are considered. First, it might be proposed that the mere presence of wait trials in the procedure impairs either conflict detection or attentional control in some way. This might be argued to especially be the case in the long wait condition, where withholding a response is necessary. According to this argument, then, the real reason why the LLPC effect was attenuated in the long wait condition is because conflict adaptation was impaired. It is not immediately clear why long wait trials would “shut off” the attentional adaptation process and added provisions to the conflict adaptation account would certainly be needed in order to accommodate the present findings (i.e., unlike the temporal learning account, which innately predicts an effect of rhythms on the LLPC effect). Still, some version of the conflict adaptation account might be able to specify a reason why an impaired rhythm interferes with conflict adaptation.

Second, it might be suggested that by forcing participants to withhold a response (albeit only temporarily) on inducer trials, conflict is prevented. In other words, it might be argued that congruent and incongruent inducer items do not actually differ in conflict in the long wait condition. Of course, the (near) lack of a difference in response time and errors between congruent and incongruent inducers in the long wait condition is poor evidence for a lack of conflict (i.e., because one would expect a null difference simply by virtue of the added waiting time). Still, by forcing participants to respond at equal speed to congruent and incongruent inducers there is no longer a metric that demonstrates conflict occurred. Perhaps it might be proposed that participants can engage in some form of relaxed stimulus processing during long wait trials that prevents conflict from occurring. This seems somewhat unlikely in the design used in the present investigation. Indeed, the second reason that the prime-probe task was selected (the first was discussed in the Introduction) was because of the brief stimulus durations. The distracting stimulus was presented only briefly and *before* the rectangle wait cue. Thus, it is impossible that participants processed long wait inducer distracters any differently than in the other trials (i.e., because whether waiting was required on a trial was not known until *after* the distracter was removed from the screen). Given the congruency effects observed for diagnostic and biased items in the long wait condition, it is clearly the case that the distracting primes were processed. Further, while the target was presented concurrently with the cue, the target also appeared on the screen very briefly. It is therefore necessary for participants to process the target immediately, and it is hard to imagine how the just-experienced distracter would not interfere with this processing. For instance, in the conflict monitoring model of Botvinick and colleagues (2001) close presentation of the prime and probe will lead to increased Hopfield energy between the response nodes corresponding to the prime and the probe, which should signal increased control. Nevertheless, a variant of the conflict adaptation account that proposes that conflict only occurs under the right time pressure constraints and can be immediately “switched off” on detection of a wait cue could be considered consistent with the present results.

Third, it might be argued that by forcing participants to wait for 800 ms on long wait trials, conflict adaptation decays. That is, a control process is initiated, but weakens with time. As a result, the true conflict adaptation effect might be lost in these delays. This might then explain why the LLPC effect is eliminated in the long wait condition. However, this proposal seems less plausible than the notions discussed above for two reasons. First, most previous investigations of the LLPC effect actually had *longer* delays between one trial and the next than in the current report (e.g., Bugg, 2014; Hutchison, 2011; Wühr et al., 2015), even relative to long wait inducer trials. Thus, the idea that conflict decay led to the elimination of the LLPC effect in the present paradigm seems inconsistent with past reports. Second, Experiment 2 seems to directly falsify this notion, as the time between stimulus onset and the next trial was actually somewhat *shorter* on long wait trials than on other trials in that experiment. Still, one way to reconcile the current results with the conflict adaptation account might be to propose that the conflict decays with time and the control signal is determined by the amount of conflict at the time of a response (i.e., rather than summed or peak conflict). Thus, it might be proposed that conflict *does* occur on long wait trials, but this conflict is resolved by the time a response is made. Though not consistent with the modelling dynamics in the originally reported conflict monitoring model (i.e., there is no decay of conflict), an adaptation might be construed to fit the present results.

As the above paragraphs indicate, there may be several ways to construe the conflict adaptation account in a way consistent with the present results. However, added assumptions about how conflict is monitored and how attention is controlled are needed. Thus, even if the temporal learning proposal of the present manuscript is wrong and conflict adaptation *does* occur in the task, the present results might help to specify more clearly the dynamics of the attentional control mechanism (e.g., that it is time-pressure dependent or subject to rapid decay). Future experimental and/or computational modelling work might prove highly informative in this regard. At present, however, the current results provide support for the notion that the LLPC effect might result from rhythmic response biases.

Implications For Future Work

More broadly, the present results demonstrate an important caveat that should be considered in any experiment where rhythmic biases might play a role in behaviour. The temporal learning account discussed in the present work suggests that effects of any sort should be larger when there are more of the easier trial types than the harder trial types (e.g., see Schmidt, 2013c, 2014b; Schmidt, Lemercier, et al., 2014). Thus, any manipulation involving an adjustment of item type proportions or filler type difficulty may unintentionally introduce a rhythmic bias. Most typically, such manipulations (e.g., PC manipulations) are designed to study how the informational content of items influences processing, and not rhythmic processes per se (for an in depth discussion of this point, see Grosjean et al., 2001). The present methodology provides one approach for eliminating these rhythmic biases when they are not of interest and represent a confound.

Temporal learning has also been proposed to play a role more locally. For instance, the congruency sequence effect is the finding that the congruency effect is smaller following an incongruent trial than following a congruent trial (Gratton et al., 1992). One potential interpretation of this effect is conflict adaptation: following a conflicting incongruent trial, attention to the word is diminished (e.g., Botvinick et al., 2001). Another potential interpretation is temporal learning: congruency effects are larger following a fast congruent trial than following a slow incongruent trial (see Schmidt & Weissman, 2016). The current paradigm might thus be adjusted to pit these two accounts against one another. According to the temporal learning account, following a congruent or incongruent long wait trial no difference in the congruency effect should be observed. Thus, a similar manipulation to that used in the current manuscript might be predicted to eliminate the congruency sequence effect following long wait trials. The conflict adaptation account, however, will predict a preservation of the congruency sequence effect, because conflict still varies on congruent and incongruent long wait trials. Note that the present experiment is ill designed to test this hypothesis, as there was only one type of long wait trial (i.e., congruent or incongruent) per list. However, this confounding of list type and previous trial congruency could easily be removed in future investigations.

Caveats

In addition to the possible interpretational ambiguities regarding the conflict adaptation account discussed above, one potential caveat with the present work is that a (relatively) non-traditional prime-probe task was used to assess LLPC effects (though this paradigm has been used extensively in the congruency sequence effect literature; Kunde & Wühr, 2006; Schmidt & Weissman, 2014, 2015, 2016; Weissman et al., 2014; 2015). This might be regarded as a relatively minor caveat, given that both experiments evidenced LLPC effects in the short wait condition (albeit only marginally in Experiment 2). Indeed, as discussed in the Introduction, the ability of the prime-probe task to detect so-called attentional control phenomena in confound-minimized designs was part of the reason for selecting this task. Still, future work might be done to test whether the same results hold in the most commonly used paradigm (i.e., the Stroop task), in addition to other congruency paradigms (e.g., Simon, flanker, and picture-word Stroop). It is at least possible that LLPC effects in some tasks might be entirely due to rhythmic biases, whereas in other tasks conflict adaptation plays an additional role.

An additional caveat is that the argument outlined in the current work presumes that the wait manipulation equates rhythms without introducing other nuisance processes that prevent the typical LLPC interaction from being observed. This point also relates to the discussion of alternative interpretations of the conflict adaptation account discussed above. For instance, it might be argued that the secondary task demand of having to watch for the rectangle cue (i.e., to know if one needs to withhold a response) might increase the difficulty of the task. It might be proposed that a concomitant increase in cognitive load impairs the default conflict adaptation strategy, or simply encourages some other attentional strategy. Similarly, the switching between having to wait (on inducer trials) and not having to wait (on all other trials) might also impair the typical attentional strategy in some way. Subjectively, the long wait procedure feels negligibly different than a task without waiting. For instance, the wait cues are certainly not as mentally taxing as cued task switching (Meiran, 1996), where cue identities (rather than presence) must be determined in order to decide both how to process the target and which response to select (rather than simply when to execute a pre-determined response). Still, the addition of wait cues to the present design does introduce the possibility that an unintended confound is added to the design (e.g., cognitive load) while attempting to rule out another unintended confound (i.e., temporal learning). Future work might aim to test for such confounding directly. For instance, if the LLPC effect is eliminated in the long wait condition due to increased cognitive load or impairments from task switching, then presumably it does not matter *which* items participants must wait to respond to. That is, if long wait trials are introduced into mostly congruent and mostly incongruent blocks, but the remaining *no wait* trials still preserve the PC of the list, then the LLPC effect should *still* be eliminated. In contrast, if temporal learning is the key mechanism, then the LLPC effect should re-emerge, because it is important that it is the inducer items that require waiting.

As a final caveat, the present manuscript focused primarily on distinguishing between the conflict adaptation and temporal learning accounts, but there remain other potentially-viable accounts of the LLPC effect. One possibility is the dual mechanisms of control account (Braver, 2012; Braver, Gray, & Burgess, 2007; De Pisapia & Braver, 2006). According to this account, proactive control (i.e., preparatory attentional control) requires reliable contextual information in order to prepare for perceptual inputs, some information of which might be temporal in nature. Thus, it might be argued that the variation in wait durations in the long wait condition (i.e., inducer trials that require waiting and the remaining trials that do not) might impair proactive control. There are two potential limitations with this view. First, an impairment in proactive control in the long wait condition should presumably lead to an overall inflation of conflict effects (in both the mostly congruent and mostly incongruent conditions, given that the variation in waiting is exactly identical in the two cases). This was not observed. Second, the "irregularities" are only in the target-response intervals. The time between the prime onset and offset, the blank screen duration, and between probe onset and offset remained fixed and perfectly reliable on all trials. Similarly, the time between a response and the next prime was consistent in Experiment 1 and the inter-trial interval was consistent in Experiment 2. Thus, the contextual cues are strongly indicative of the stimulus onsets. As such, it would have to be assumed that variability in the participant's own response speed impairs the ability to process the other reliable contextual cues in the procedure to prepare proactive control. However, if variability in stimulus-response timing were sufficient to impair proactive control, then presumably simply mixing congruent and incongruent trials together in a task (as is essentially always the case) would be sufficient to eliminate proactive control. Still, as with the conflict adaptation account, there may be some argument to be made that proactive control, or some other hypothesized mechanism driving the LLPC effect, is impaired by the present task variant. In general, the temporal learning account of the present data seems the most straightforward and parsimonious explanation, but every dataset can be (re-)explained in multiple ways. It is hoped that the above sections demonstrate that even if the temporal learning account is false, the current data are useful in constraining alternative accounts to those that can clearly predict an impairment of the LLPC effect with the wait manipulation newly introduced in the present manuscript.

Conclusions

The current results add to a growing body of evidence that temporal learning biases may play an important role in proportion congruent effects. Most critically, this paper presents for the first time evidence that controlling for rhythmic biases with the novelly-introduced "wait" manipulation significantly reduces LLPC effects in the prime-probe task. Indeed, the current results are the only results to date that enable a dissociation between temporal learning and conflict adaptation. Though more work is certainly needed before any strong conclusions can be drawn (one way or the other), it is hoped that this new approach to controlling rhythmic biases will further open a dialogue regarding the potential links between the rhythmic timing, learning, and cognitive control domains.

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Table 1. Experiment 1 and 2 stimulus frequencies for diagnostic, biased, and inducer primes.

					Inducer Primes			
	Diagnostic Primes		Biased Primes		Mostly Congruent		Mostly Incongruent	
Probes	**Left**	**Right**	**Up**	**Down**	**Up**	**Down**	**Up**	**Down**
Left	25	25						
Right	25	25						
Up			10	10	30			30
Down			10	10		30	30	

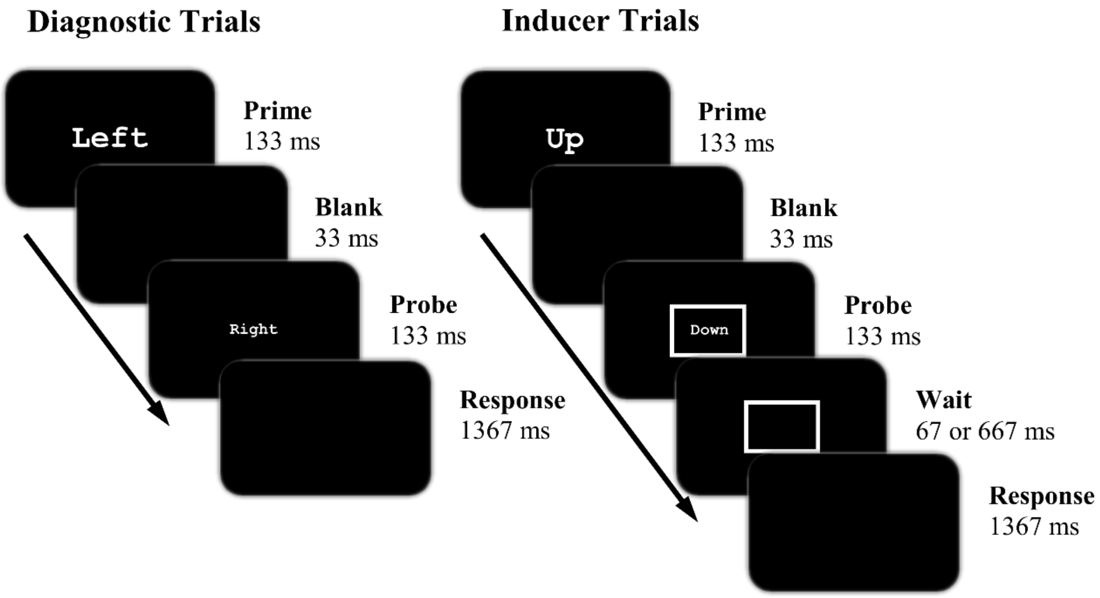

Figure 1. Experiment 1 trial procedure for diagnostic (and biased) items and inducer items.

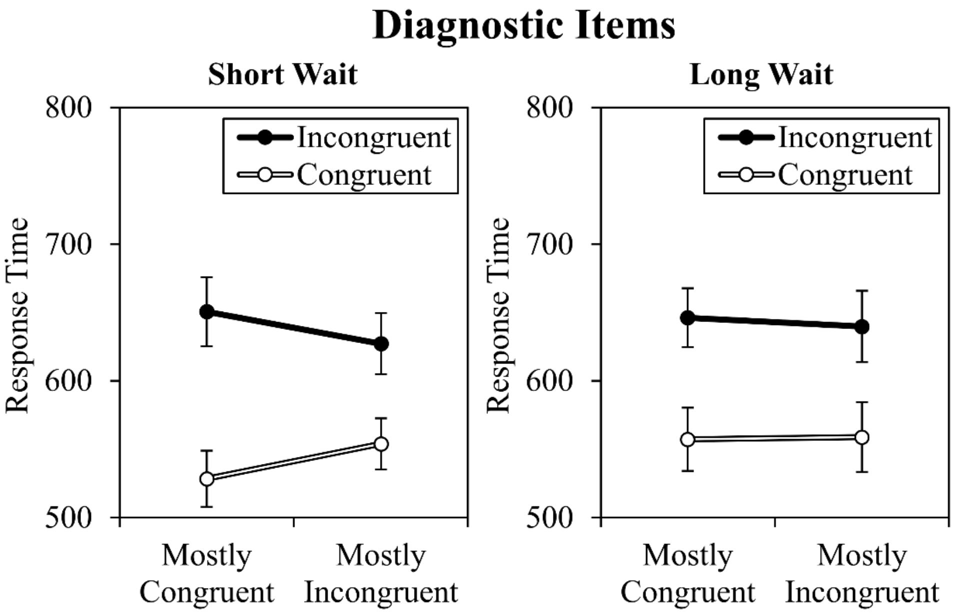

Figure 2. Experiment 1 response times for short and long wait diagnostic items.

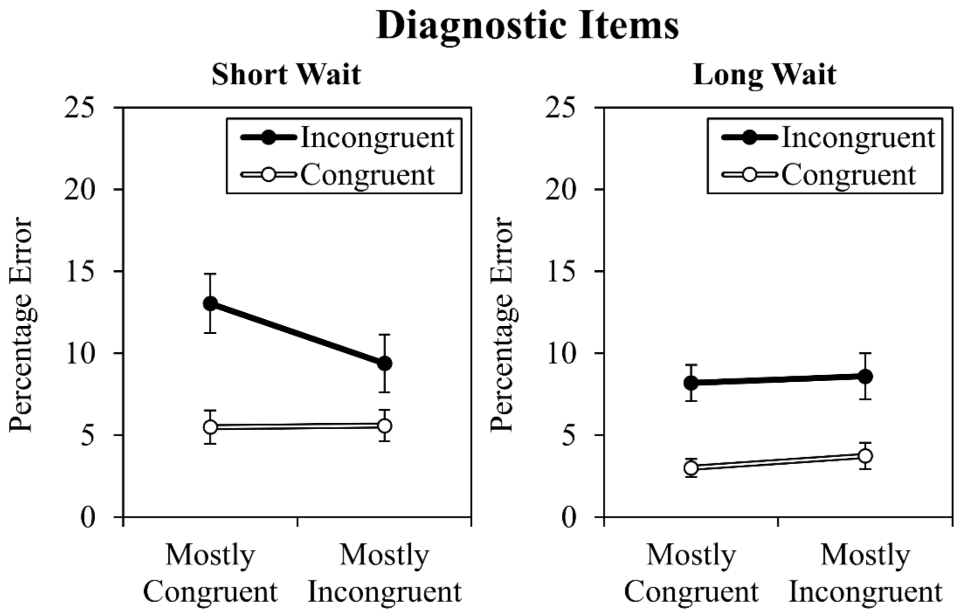

Figure 3. Experiment 1 percentage errors for short and long wait diagnostic items.

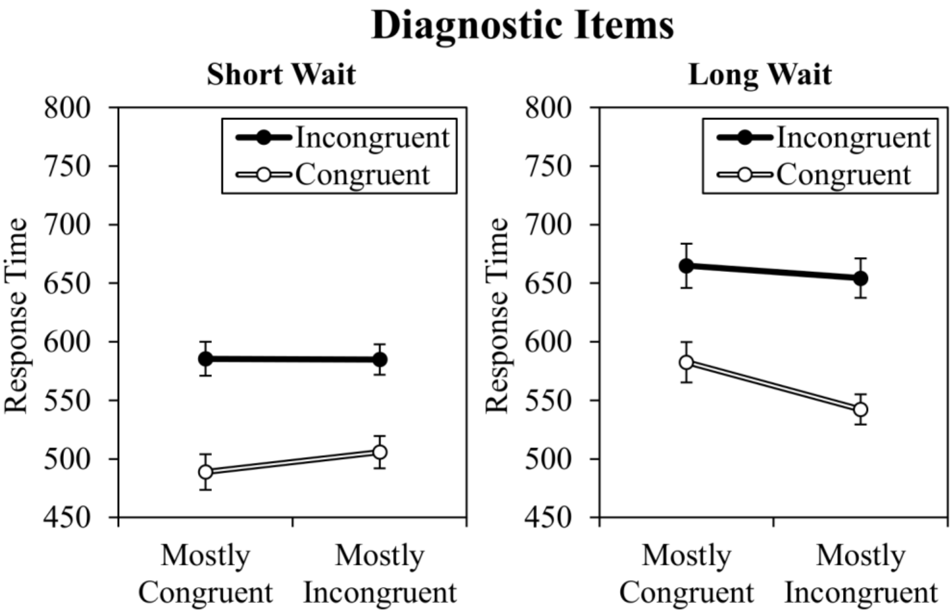

Figure 4. Experiment 2 response times for short and long wait diagnostic items.

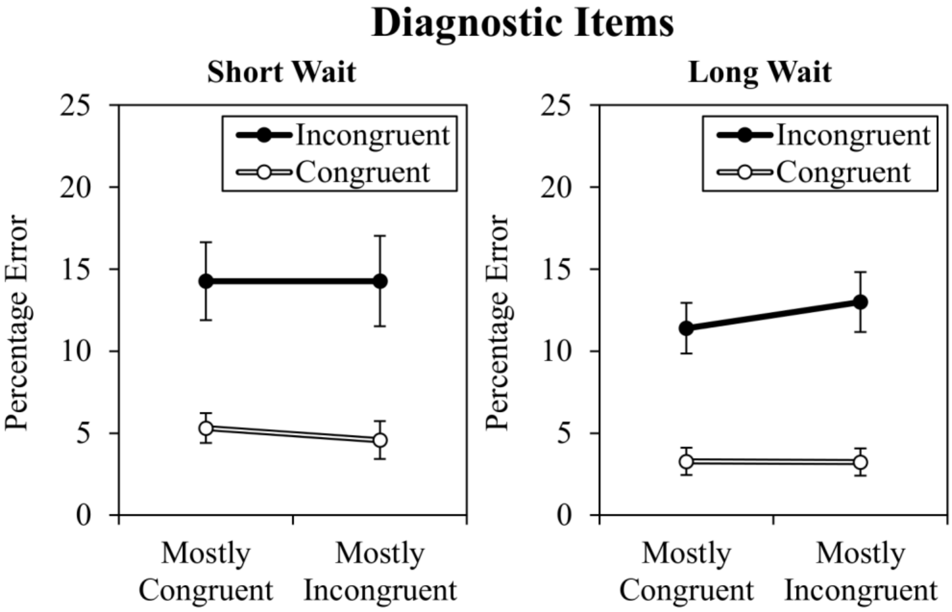

Figure 5. Experiment 2 percentage errors for short and long wait diagnostic items.