The Unique Effects of Relatively Recent Conflict on Cognitive Control

Jackson Colvett
Washington University in St. Louis

Follow this and additional works at: https://openscholarship.wustl.edu/art_sci_etds

Part of the Cognitive Psychology Commons

Recommended Citation
https://openscholarship.wustl.edu/art_sci_etds/1977
The Unique Effects of Relatively Recent Conflict on Cognitive Control

by

Jackson Stuart Colvett

A thesis presented to
The Graduate School
of Washington University in
partial fulfillment of the
requirements for the degree
of Master of Arts

December 2019
St. Louis, Missouri
# Table of Contents

List of Figures .................................................................................................................... iii
List of Tables ......................................................................................................................... iv
Acknowledgments ................................................................................................................ v
Abstract of the Thesis .......................................................................................................... vii
Introduction .......................................................................................................................... 1

Experiment 1 .......................................................................................................................... 6
  - Experiment 1 Method ........................................................................................................ 8
  - Experiment 1 Results ....................................................................................................... 10
  - Experiment 1 Discussion .............................................................................................. 13

Experiment 2 ........................................................................................................................ 17
  - Experiment 2 Method .................................................................................................... 18
  - Experiment 2 Results ................................................................................................... 19
  - Experiment 2 Discussion ............................................................................................. 22

General Discussion .............................................................................................................. 24
  - Evaluation of Study Goals ......................................................................................... 24
  - Possible Explanations ................................................................................................. 26
  - Limitations and Future Directions ........................................................................... 28

Conclusion ............................................................................................................................ 31

References ............................................................................................................................ 33

Figures and Tables ............................................................................................................... 39
List of Figures

Figure 1: Centered Figure .................................................................39

Figure 2: Centered Figure .................................................................40

Figure 3: Centered Figure .................................................................41
List of Tables

Table 1: Experiment 1 Descriptive Statistics .................................................................42
Table 2: Experiment 2 Descriptive Statistics .................................................................43
Table 3: Experiment 1 $F$ Table ..................................................................................44
Table 4: Experiment 2 $F$ Table ..................................................................................46
Acknowledgments

Thank you to Julie Bugg for the time and effort invested into this project and into my development as a scientific thinker. Special thanks to Lindsay Nobles, Eva Jeliazkova, and Janelli Rodriguez for their effort thinking about this project and to Lindsay and Janelli for their assistance with data collection. Thanks to the Cognitive Control and Aging lab for their helpful feedback on the project. Thank you to Dave Balota and Todd Braver for serving on the thesis committee.

Jackson Stuart Colvett

Washington University in St. Louis

December 2019
Dedicated to Mary and Kyle Colvett. My first and best editors.
ABSTRACT OF THE THESIS

The Unique Effects of Relatively Recent Conflict on Cognitive Control

by

Jackson Stuart Colvett

Master of Arts in Psychological and Brain Sciences

Washington University in St. Louis, 2019

Professor Julie Bugg, Chair

In tasks such as Stroop, our past experiences with conflict influence our ability to attend to goal-relevant information and ignore irrelevant information. There exists evidence that conflict experiences on at least two timescales affect cognitive control. The “immediate” timescale is evidenced by congruency sequence effects while the “long” timescale is evidenced by list-wide proportion congruence effects. What remains underspecified is whether relatively recent experiences with conflict may also uniquely influence cognitive control and how experiences on different timescales are weighted. The present, pre-registered experiments aimed to assess the role of relatively recent conflict by examining the potential effects of an “intermediate” timescale (i.e., several preceding trials). A novel Stroop paradigm was developed to isolate the effects of the intermediate timescale and cognitive control was measured via frequency- and contingency-unbiased diagnostic items. In Experiment 1 (N = 61), I manipulated the level of conflict experienced in the intermediate timescale for lists matched in proportion congruence. Controlling for conflict experiences in the long and immediate timescales, I found that conflict in the intermediate timescale affected cognitive control. Experiment 2 (N = 60) found that the effect of conflict in the intermediate timescale may depend on that conflict defying the long timescale. These novel findings highlight the need to expand theories of cognitive control to incorporate the
intermediate timescale and the interaction of the intermediate timescale with other timescales of cognitive control.
**Introduction**

Cognitive control processes allow the pursuit of goal-directed behavior in favor of alternative compelling or habitual behaviors (Cohen, 2017). Prior experiences resolving conflicts between competing responses affect cognitive control (e.g., whether a focused scope of attention is engaged whereby processing of goal-irrelevant information is decreased and/or goal-relevant information is increased, or a relaxed scope of attention is engaged). Consider driving a car on the highway. A car suddenly cutting in front of you might elicit conflict that heightens your focus on goal-relevant information, demonstrating the effects of conflict on the immediate timescale. On a longer timescale, your focus while driving might be influenced by all accumulated experiences since getting on the highway. For example, if the highway has been mostly busy (or mostly empty), this will likely induce generally focused (or relaxed) attention. But what if you suddenly encounter a lot of traffic in a stretch of highway that was relatively empty? What effect will this experience on an “intermediate” timescale have on cognitive control? Will the control system maintain a relaxed scope of attention (consistent with the long timescale) or will the recent conflict lead to a heightening of control? If it does, will the heightening be above and beyond that caused by the last car that cut in front of you (immediate timescale)?

Prior research on cognitive control, including computational models, has focused primarily on effects of conflict on the immediate and long timescales. One such model is the influential conflict monitoring account that proposed conflict monitoring as a mechanism by which experiences with conflict lead to a recruitment of cognitive control (Botvinick, Braver, Brach, Carter, & Cohen, 2001). According to this model, some control adjustments occur in
response to conflict experiences on the previous trial. Consistent with this idea, individuals are less susceptible to conflict after experiencing an incongruent (i.e., conflicting) trial than after experiencing a congruent (i.e., non-conflicting) trial, presumably because control is heightened when the previous trial is incongruent (Gratton, Coles, & Donchin, 1992; for reviews, Egner 2007; Duthoo, Abrahamse, Braem, Boehler, & Notebaert, 2014). These congruency sequence effects are relatively transient (Egner, Ely, & Grinband, 2010; Duthoo, Abrahamse, Braem, & Notebaert 2014) and exemplify how the “immediate” timescale of conflict accumulation influences cognitive control.

In contrast, a different effect provides an example of how control is affected by conflict experiences that accumulate across dozens (e.g., Bugg, Diede, Cohen-Shikora, Selmeczy, 2015) or hundreds of trials (i.e., a block or list; Logan & Zbrodoff, 1979). The list-wide proportion congruence (PC; what percentage of experienced trials are congruent) effect is the pattern whereby congruency effects are smaller in mostly incongruent (MI) lists than mostly congruent (MC) lists (see Bugg, 2014; Bugg & Chanani, 2011; Gonthier, Braver, & Bugg, 2016; Hutchison, 2011 for evidence of list-wide PC effects when controlling for known confounds; for reviews see Bugg, 2012; Bugg & Crump, 2012). The conflict monitoring account suggests that when higher overall conflict is detected in the list, there is a subsequent increase in cognitive control (Botvinick et al., 2001). In other words, the conflict monitoring model also captures adjustments in control based on conflict accumulation over a long timescale and not just the preceding trial (immediate timescale). Findings show that list-wide PC effects are observed independent of the congruency sequence effect (Torres-Quesada, Funes, & Lupiáñez, 2013; Torres-Quesada,

---

1 The phrase “conflict experiences” is used here to refer to experiences with either conflicting (i.e., incongruent) or non-conflicting (i.e., congruent) trials. The phrase conflict experiences is used rather than conflict, as it is also the case that the absence of conflict is a signal for control adjustments (e.g., Schlaghecken & Martini, 2012).
Milliken, Lupiáñez, & Funes, 2014), suggesting that the long timescale is separable from the immediate timescale.

While effects of the immediate and long timescales have been examined across hundreds of studies, there are several theoretical gaps in the literature. Two important gaps were of interest in the present study. First, do relatively recent experiences with conflict (i.e., the intermediate timescale) influence cognitive control above and beyond the effect of the immediate timescale? That is, do experiences occurring on multiple trials preceding the current trial shape the heightening or relaxation of control beyond the effect of the immediately preceding trial? The conflict monitoring account states that the amount of control on a given trial should be based on “an exponentially weighted average of conflict over multiple preceding trials, rather than only on the immediately preceding trial” (Botvinick et al., 2001; p. 639). This implies that conflict experiences in the intermediate timescale should affect cognitive control.

Only a few prior studies have reported findings that speak to the role of the intermediate timescale. In a flanker task with nine participants, as the number of preceding compatible trials increased from one to six, reaction time on incompatible trials increased (Durston et al., 2003). However, reaction time on incompatible trials did not significantly decrease as a function of the number of preceding incompatible trials. Other studies with larger samples have found a significant effect of several preceding trials, including multiple incongruent trials. In a Simon task, reaction time declined on trial $n$ as a function of the number of consecutive trials of the same trial type preceding trial $n$ for both congruent and incongruent trials (Horga et al., 2011). In a Stroop task, congruency sequence effects were accentuated by multiple preceding congruent trials and attenuated by multiple preceding incongruent trials (Jiménez & Méndez, 2013; Jiménez & Méndez, 2014). These findings suggest that control is adjusted in response to conflict
experiences that occur more trials back than just trial $n - 1$, supporting a role for the intermediate timescale.

The second gap concerns how experiences on different timescales are weighted and what factors affect this weighting. Although the aforementioned studies (e.g., Durston et al., 2003; Horga et al., 2011; Jiménez & Méndez, 2013; Jiménez & Méndez, 2014) provided evidence for the intermediate timescale, these studies uniformly used lists with 50% congruent trials and therefore they could not assess whether effects of the intermediate timescale might depend on the conflict experiences preceding the intermediate timescale. For example, the weighting of conflict experiences on the intermediate timescale might vary based on the long timescale (e.g., whether it is MC or MI). The conflict monitoring account (and related models; see Blais et al., 2007; Verguts & Notebaert, 2008), however, assumes a fixed learning rate (i.e., degree to which new information is weighted when updating attentional settings; Behrens, Woolrich, Walton, & Rushworth, 2007). This implies that the effects of the intermediate timescale should be consistent regardless of context (e.g., preceding trial history).

Here, too, only a few prior studies have examined this issue. Aben and colleagues developed a statistical model that documented the effects of different timescales of conflict accumulation on cognitive control in the flanker task (Aben, Verguts, & Van den Bussche, 2017; see Dey, 2019, for replications using a Stroop task). One key finding from this model was that multiple trials prior to the immediately preceding trial (7 of the preceding 12 trials in flanker, Aben et al., 2017; each of the 8 preceding trials within an 8 trial window in color word Stroop, Dey, 2019) significantly informed the level of cognitive control on trial $n$ controlling for the effect of the other trials. This further supports that the intermediate timescale does play a role in cognitive control adjustments. Most relevant to the second gap in the literature, another key
finding was that there was an interaction such that conflict experiences in the intermediate timescale (recent trials extending beyond \( n - 1 \)) were weighted less strongly in MI lists than in MC lists. Aben and colleagues interpreted this to mean that recent experiences with conflict have less of an influence on cognitive control when the long timescale biases individuals to engage proactive control (i.e., sustain a heightened attentional bias across trials; Braver, Gray, & Burgess, 2007) than when the system is relatively relaxed and dealing with conflict via reactive control. This suggests learning rate may not be fixed, contrary to the conflict monitoring account.

To take stock, prior research provides suggestive evidence that an intermediate timescale of conflict, and not just the immediate and long timescales, affects whether the scope of attention on a moment-by-moment basis is relatively focused or relaxed. In addition, there is initial evidence based on statistical modeling to suggest that the weighting of the intermediate timescale may vary depending on the long timescale. In the current study, I aimed to further understand potential effects of the intermediate timescale on cognitive control. One goal was to test a prediction from the aforementioned statistical models (Aben et al., 2017; Dey, 2019) regarding the interaction between the intermediate timescale and the long timescale. As noted above, the modeling demonstrated that the intermediate timescale had a greater effect in MC lists than MI lists in a flanker task. The present study tests this prediction in the context of a modified Stroop task. Interestingly, and in contrast to the statistical modeling results, prior research has shown that the effects of the immediate timescale, as indexed by the congruency sequence effect, do not interact with the long timescale (Meier & Kane, 2013). This may imply an important difference between the immediate and intermediate timescales, but additional research is needed to inform this possibility.
Another goal of the current study was to extend the scope of the measurement of
cognitive control beyond a single trial as has been the typical approach to evaluating effects of
preceding conflict (e.g., trial \( n - 1 \)) on control (trial \( n \)) in studies investigating the intermediate
timescale (e.g., Durston et al., 2003; Horga et al., 2011; Jiménez & Méndez, 2013; Jiménez &
Méndez, 2014) as well as the immediate timescale. To achieve this goal, I developed a modified
Stroop paradigm in which the effects of conflict experiences across different timescales were
assessed during a diagnostic phase of eight trials that followed the critical manipulations of
conflict. Through this change, I aimed to understand whether an intermediate conflict
manipulation produces transient changes in cognitive control limited to a single trial post conflict
or, potentially, longer-lasting adjustments. Effects on the immediate timescale are typically seen
as transient (fading after a single trial or long delay between trials; Egner et al., 2010; see also
Duthoo et al., 2014), but it is possible that conflict experiences in the intermediate timescale
produce a more sustained effect similar to experiences on the long timescale (e.g., Gonthier et
al., 2016).

**Experiment 1**

Experiment 1 adopted an experimental approach to investigate the potential effects of the
intermediate timescale on cognitive control using a novel variant of the abbreviated lists
paradigm (Bugg et al., 2015). Each list comprised 26 Stroop trials. For expository purposes,
consider that there were two phases in each list (see Figure 1): an induction phase (18 trials) that
was followed by a diagnostic phase (8 trials). Phases were not demarcated from the participants’
perspective. The induction phase represented the long timescale and was MC or MI. The last four
trials of the induction phase represented the intermediate timescale. Critically, in half of the lists
in each PC condition, the intermediate timescale comprised only the infrequent trial type (i.e.,
incongruent trials in an MC list). This manipulation is hereafter referred to as a “present” or “absent” window. For example, in a MC\textsubscript{PRESENT} list, the last four trials would be 100% incongruent, whereas in an MC\textsubscript{ABSENT} list, congruent and incongruent trials were distributed throughout the induction phase in accordance with the PC of the list (in this example, most of those trials were congruent).\textsuperscript{2} Consequently, for the key comparison of MC\textsubscript{PRESENT} and MC\textsubscript{ABSENT} lists (or MI\textsubscript{PRESENT} and MI\textsubscript{ABSENT} lists), the long timescale was equated.

The effects of induction, including the presence versus absence of the window, were assessed during a subsequent diagnostic phase. For all lists, the diagnostic phase was comprised of eight trials. Critically, this meant that, unlike the prior studies investigating the transient effect of the intermediate timescale solely on the immediately following trial (trial \(n\)), the current study assessed whether effects of the intermediate timescale may be sustained beyond that trial to multiple following trials. The diagnostic phase was 50\% congruent and these trials were novel words/colors not used to create the PC bias in the induction phase. The combination of these two features enabled me to rule out explanations of performance on the diagnostic trials related to item-specific mechanisms such as contingency-learning (e.g., Schmidt & Besner, 2008) and bottom-up priming of a focused or relaxed scope of attention (see e.g., Bugg, 2014; see also Braem et al., 2019; Bugg, 2017), and instead attribute differences in Stroop effects between conditions to induced cognitive control. Additionally, I analyzed the diagnostic phase removing the trial immediately following the induction to address the possibility that differences between conditions were driven by differences in the immediate timescale. The key question was whether performance on the diagnostic items would differ between present and absent lists, that is,

\textsuperscript{2} In MC\textsubscript{ABSENT}, trials in the window were 70.9\% congruent (34 congruent trials out of 48 total trials in window across all lists). In MI\textsubscript{ABSENT}, trials in the window were 27.8\% congruent (13 congruent out of 48 total trials in window across all lists).
between lists that had the same PC during induction (i.e., equating the long timescale) but differing experiences in the intermediate timescale.

An account that includes an intermediate timescale of conflict experience that interacts with the other timescales would predict Stroop effects following an MC\textsubscript{PRESENT} induction to be attenuated in comparison to MC\textsubscript{ABSENT}; the account would also predict Stroop effects to be larger following an MI\textsubscript{PRESENT} induction in comparison to MI\textsubscript{ABSENT}. Statistically, this would manifest as a significant three-way interaction between congruence, PC, and window presence. However, if the intermediate timescale has no influence on cognitive control beyond the long and immediate timescales, no differences should be observed between present and absent conditions (and therefore a non-significant three-way interaction). Hypotheses and data for both experiments were pre-registered and are available on OSF (see link in author note).

**Method**

**Participants.** Sixty-one Washington University undergraduates (32 female, Age \( M = 18.49, SD = 0.64 \)) participated for course credit. All participants were native English speakers with normal or corrected vision and color vision. No participants were excluded.

**Design and Stimuli.** I adapted an abbreviated-lists design (Bugg et al., 2015) using 26-trial lists presenting congruent trials comprising a word and color that matched (e.g., RED in red ink) and incongruent trials comprising a word and color that mismatched (e.g., RED in blue ink) (see Figure 1). Lists began with a biased induction phase. The purpose of the induction phase was to present trials that induce relatively focused (i.e., MI list) or relaxed (i.e., MC list) control; the effectiveness of the induction was assessed during the diagnostic phase, which was equivalent between conditions. The key manipulation was the presence or absence of an experience-defying four-trial window (i.e., in the intermediate timescale) at the end of the
induction phase, which preceded assessment of participants’ cognitive control during the diagnostic phase. One set of stimuli (RED, BLUE, PURPLE, and WHITE in red, blue, purple, or white) served as the Induction Set and was presented during the induction phase according to the PC of the list. A second set of stimuli (words GREEN and YELLOW in green and yellow) served as the Diagnostic Set and was presented during the diagnostic phase. A key feature of stimuli in the diagnostic phase is that they were always 50% congruent. One concern in the present design was that participants might become aware that the words/colors green and yellow always appeared at the end of the list and this could inadvertently affect their cognitive control. To alleviate this concern, two preventive measures were taken: First, a congruent and an incongruent trial were randomly selected from trials 1-14 of the induction phase (i.e., two trials from the Induction Set) and interchanged with a congruent and incongruent trial from trials 3-8 of the diagnostic phase (i.e., two trials from the Diagnostic Set). Induction Set trials transplanted into the diagnostic phase were excluded from the analysis of diagnostic phase performance (and vice versa). Second, filler lists were included in which 13 trials from the Induction Set and 13 trials from the Diagnostic Set were randomly intermixed throughout the list. These lists were 50% congruent and excluded from analysis.

Experiment 1 used 56 lists that were presented in random order: 12 lists for each of the following: MC\textsc{present}, MC\textsc{absent}, MI\textsc{present}, and MI\textsc{absent}, plus eight filler lists. The order of the trials within lists was pseudorandom. Each color was equally represented for both the Induction and Diagnostic Sets. For incongruent trials in the Induction Set, there was an equal number of each distractor, such that for an incongruent trial with the color red, the distractor word was equally likely to be PURPLE, BLUE, or WHITE. The order of trials within lists was fixed to establish the manipulation.
**Procedure.** First, a brief demographic survey was administered. After receiving instructions to name the color as quickly as possible without sacrificing accuracy, participants began the first list of the color-word Stroop task. For each trial, a word stimulus was presented centrally on screen in 24-point Arial font. The word remained on screen until the voice key was triggered after which an experimenter coded what response was emitted by the participant. Trials on which the voice key was triggered by irrelevant speech (e.g., “um”) or extraneous noise (e.g., cough), or on which the speech was imperceptible or unintelligible, were coded as scratch trials and excluded. There was a 500 ms blank screen before the next stimulus was presented. Trials within each list were presented continuously (i.e., there was no break between phases within a list). In between each list, participants had an opportunity to rest and verbally told the experimenter when to continue. After completing all lists, participants were debriefed.

**Results**

In the current and subsequent experiment, an alpha of .05 was used for all analyses. In addition, analyses of RT and error rate excluded trials with RTs less than 200 ms or greater than 3000 ms (0.95% of trials were removed; cf. Bugg et al., 2015), and analyses of RT also excluded error trials. The induction and diagnostic trials were analyzed separately (cf. e.g., Bugg, 2014). For each trial type and dependent variable, a 2 x 2 x 2 repeated measures ANOVA with factors of congruence (congruent or incongruent), PC (MC or MI), and intermediate window (present or absent) was performed. All reaction times report milliseconds (ms). See Table 1 for descriptive statistics. Only theoretically relevant inferential statistics are reported; comprehensive analyses are reported in Table 3.
**Reaction Time**

**Induction items.** In order to assess Stroop performance during biased (i.e., MC or MI) trials preceding the diagnostic phase, trials in the induction phase were analyzed. Recall that the induction phase comprised 14 trials pre-window and the four-trial window. There was a main effect of congruence, $F(1, 60) = 467.08, p < .001, \eta^2_p = .886$, such that responses for congruent trials ($M = 599, SE = 11$) were faster than incongruent trials ($M = 702, SE = 12$). The interaction between congruence and PC ($F(1, 60) = 286.28, p < .001, \eta^2_p = .827$) was significant, such that the Stroop effect (Incongruent RT – Congruent RT) was larger in MC than MI inductions (i.e., there was a list-wide PC effect). In addition, there was a significant three-way interaction between congruence, PC, and window, $F(1, 60) = 70.74, p < .001, \eta^2_p = .541$. The Stroop effect was significantly larger in MC_{PRESENT} ($M = 163, SE = 7$) than MC_{ABSENT} ($M = 113, SE = 5$), $F(1, 60) = 107.91, p < .001, \eta^2_p = .643$, whereas the Stroop effect was non-significantly smaller in MI_{PRESENT} ($M = 64, SE = 6$) than in MI_{ABSENT} ($M = 70, SE = 4$), $F(1, 60) = 2.43, p = .124, \eta^2_p = .039$.

**Diagnostic items.** See Figure 2 for diagnostic phase results for reaction time and error rate. In order to assess the effects of the induction on Stroop performance independent of known confounds, the diagnostic phase was analyzed. There was a main effect of congruence, $F(1, 60) = 298.82, p < .001, \eta^2_p = .833$, such that responses to congruent trials ($M = 620, SE = 12$) were faster than incongruent trials ($M = 697, SE = 13$). The interaction between congruence and PC was significant ($F(1, 60) = 20.16, p < .001, \eta^2_p = .251$), such that the Stroop effect was smaller in MC lists. However, this effect was qualified by a significant three-way interaction between congruence, PC, and window ($F(1, 60) = 18.72, p < .001, \eta^2_p = .238$). Consistent with the predicted effects of the intermediate window, MC_{PRESENT} ($M = 62, SE = 5$) had an attenuated
Stroop effect compared to MC\textsubscript{ABSENT} ($M = 78$, $SE = 6$), $F(1, 60) = 8.94$, $p = .004$, $\eta^2_p = .130$, whereas MI\textsubscript{PRESENT} ($M = 93$, $SE = 6$) had a larger Stroop effect than MI\textsubscript{ABSENT} ($M = 76$, $SE = 5$), $F(1, 60) = 8.95$, $p = .0044$, $\eta^2_p = .130$.

Finally, to assess whether results for diagnostic items were driven by a congruency sequence effect based on just the immediately preceding trial of the induction phase, I re-analyzed performance in the diagnostic phase, excluding the first trial immediately following the induction (i.e., trial 19). The results converged with the above patterns: a significant main effect of congruence, $F(1, 60) = 300.90$, $p < .001$, $\eta^2_p = .834$, such that responses to congruent trials ($M = 617$, $SE = 12$) were faster than incongruent trials ($M = 696$, $SE = 13$); a significant interaction between congruence and PC, $F(1, 60) = 14.42$, $p < .001$, $\eta^2_p = .194$, such that the Stroop effect was larger in MI; and most critically, a significant three-way interaction between congruence, PC, and window, $F(1, 60) = 8.15$, $p = .006$, $\eta^2_p = .120$. MC\textsubscript{PRESENT} ($M = 64$, $SE = 5$) had an attenuated Stroop effect compared to MC\textsubscript{ABSENT} ($M = 76$, $SE = 6$), $F(1, 60) = 4.13$, $p = .047$, $\eta^2_p = .064$, and MI\textsubscript{PRESENT} ($M = 94$, $SE = 6$) had a marginally larger Stroop effect than MI\textsubscript{ABSENT} ($M = 81$, $SE = 6$), $F(1, 60) = 3.95$, $p = .051$, $\eta^2_p = .062$, although this difference was marginal.

**Error Rate**

**Induction items.** There was a main effect of congruence, $F(1, 60) = 64.86$, $p < .001$, $\eta^2_p = .519$, such that congruent trials ($M = 0.55\%$, $SE = 0.11\%$) were more accurate than incongruent trials ($M = 4.70\%$, $SE = 0.71\%$). The interaction between congruence and PC was significant ($F(1, 60) = 39.15$, $p < .001$, $\eta^2_p = .295$) such that the Stroop effect was larger in MC inductions. Finally, the three-way interaction between congruence, PC, and window was significant, $F(1, 60) = 12.93$, $p < .001$, $\eta^2_p = .177$. MC\textsubscript{PRESENT} ($M = 7.02\%$, $SE = 1.00\%$) had a larger Stroop effect than MC\textsubscript{ABSENT} ($M = 4.95\%$, $SE = 0.64\%$), $F(1, 60) = 8.42$, $p = .005$, $\eta^2_p = .123$. MI\textsubscript{PRESENT} ($M =$
1.85%, \( SE = 0.36\% \) had a smaller Stroop effect than \( \text{MI}_{\text{ABSENT}} \) (\( M = 2.82\%, \ SE = 0.43\% \)), \( F(1, 60) = 8.39, \ p = .005, \ \eta^2 = .123 \).

**Diagnostic items.** There was a main effect of congruence \( F(1, 60) = 57.67, \ p < .001, \ \eta^2 = .49 \), such that congruent trials (\( M = 0.59\%, \ SE = 0.17\% \)) were more accurate than incongruent trials (\( M = 4.01\%, \ SE = 0.63\% \)). The interactions between congruence and PC (\( F(1, 60) = 0.83, \ p = .367, \ \eta^2 = .014 \)) and between congruence, PC, and window were non-significant, \( F(1, 60) = 0.42, \ p = .521, \ \eta^2 = .007 \).

Although there was no hint of an effect of the intermediate window in the performance on diagnostic trials, for completeness I performed the analysis excluding the first trial. This did not appreciably change any of the above patterns.

**Discussion**

One important goal of Experiment 1 was to understand whether evidence exists for an intermediate timescale of conflict accumulation controlling for the immediate and long timescales. Experiment 1 found that manipulating the presence or absence of an intermediate window in the induction phase affected cognitive control in the diagnostic phase. Incongruent windows in \( \text{MC}_{\text{PRESENT}} \) lists attenuated the Stroop effect during the diagnostic phase in comparison to the \( \text{MC}_{\text{ABSENT}} \) lists; congruent windows in \( \text{MI}_{\text{PRESENT}} \) lists exacerbated the Stroop effect during the diagnostic phase in comparison to \( \text{MI}_{\text{ABSENT}} \) lists. The divergent Stroop effects across conditions matched in PC but varying in the intermediate window experience (e.g., comparing \( \text{MC}_{\text{ABSENT}} \) and \( \text{MC}_{\text{PRESENT}} \)) can be uniquely attributed to the manipulation in the intermediate timescale. The long timescale cannot explain the effect as the amount of conflict (i.e., frequency of congruent and incongruent trials) was equivalent between present and absent conditions. This result is consistent with findings demonstrating that relatively recent experience
is weighted more strongly than relatively distal experience in statistical models of adaptations to conflict (Aben et al., 2017; Dey, 2019). The statistical models also predicted that intermediate conflict experiences would have a larger effect in the MC conditions than in the MI conditions. Inconsistent with those models, the effect sizes were equivalent comparing differences in the diagnostic stage between MC_{PRESENT} and MC_{ABSENT} and between MI_{PRESENT} and MI_{ABSENT}.

Based on these findings, Experiment 1 provides evidence of an effect of intermediate conflict experience on subsequent cognitive control that would not be predicted by the long timescale alone. However, it does not find evidence that the effect of intermediate timescale is modulated by the PC of the long timescale.

Another goal of Experiment 1 was to see whether effects based on intermediate conflict would sustain over the course of the diagnostic phase. An analysis that accounted for the effect of the trial congruency immediately preceding the diagnostic phase was performed. That analysis found that the effect observed in the diagnostic phase was not driven by differences in the immediate timescale. This demonstrates evidence for a sustained adjustment in control following the manipulation in the intermediate timescale.

Although the induction phase (long timescale) was equivalent between MC_{PRESENT} and MC_{ABSENT} and between MI_{PRESENT} and MI_{ABSENT} conditions in that an equal number of congruent and incongruent trials were presented, performances in the induction phase differed. Stroop effects were significantly larger in MC_{PRESENT} than MC_{ABSENT} for RT and error rate, and smaller in MI_{PRESENT} than MI_{ABSENT} for error rate (but equivalent in RT). This is clearly surprising from a frequency perspective. One might be concerned that differences in the induction phase limit conclusions that can be drawn about the critical comparisons from the diagnostic phase (MC_{PRESENT} vs. MC_{ABSENT}, and MI_{PRESENT} vs. MI_{ABSENT}). However, the key question I aimed to
address regarding the diagnostic phase concerned whether the intermediate timescale in the induction had a unique effect on subsequent performance. The differences in Stroop performance in the diagnostic phase (i.e., Stroop effects were smaller in $\text{MC}_{\text{PRESENT}}$ than $\text{MC}_{\text{ABSENT}}$ and larger in $\text{MI}_{\text{PRESENT}}$ than $\text{MI}_{\text{ABSENT}}$) indicate that some element of the induction experience affected cognitive control during the diagnostic phase. Given the differing patterns observed for the induction phase and the diagnostic phase, it can be certain that participants did not simply extend the global control setting that was in effect during the 18-trial induction phase. Instead, these patterns imply that the entire induction experience was not being weighted equally from the 19th trial onward in the diagnostic phase, which is consistent with the conclusion that the intermediate window had a unique and influential effect on diagnostic phase performance. Said differently, the elements of the induction that drove the patterns of performance during induction trials were not identical to the elements of the induction (i.e., the window) that primarily drove diagnostic trial performance.

Although the three-way interaction, and the specific direction of the performance advantages observed in the critical comparisons ($\text{MC}_{\text{ABSENT}}$ vs. $\text{MC}_{\text{PRESENT}}$, and $\text{MI}_{\text{ABSENT}}$ vs. $\text{MI}_{\text{PRESENT}}$) were anticipated assuming an effect of the intermediate timescale, one unanticipated finding was that a list-wide PC effect was not observed when comparing $\text{MC}_{\text{ABSENT}}$ and $\text{MI}_{\text{ABSENT}}$ lists (i.e., Stroop effect is typically larger in the MC condition). This stands in contrast to prior studies that found a list-wide PC effect for diagnostic items (e.g., Bugg, 2014; Bugg & Chanani, 2011; Cohen-Shikora et al., 2018; Gonthier, Braver, & Bugg, 2016; Hutchison, 2011). A methodological consideration is that the diagnostic trials occurred in a separate phase at the end of each list. This design was chosen in order to assess whether the cognitive control adjustments would sustain over the course of several trials following the intermediate conflict
manipulation. In all prior studies, diagnostic trials were randomly intermixed with induction trials throughout the list. It is possible that this difference may have affected how induced cognitive control settings manifested on diagnostic trials. To evaluate this possibility, an exploratory analysis was performed on the Diagnostic Set trials that were integrated in the first 14 trials of the lists and not included in the analysis of the induction phase. These Diagnostic Set trials, like those in the diagnostic phase, were 50% congruent and comprised of unique colors/words from induction trials; however, and most critically for present purposes, these Diagnostic Set trials were intermixed among the induction phase trials and thus comparable to diagnostic trials in prior studies. This analysis revealed significant differences in the Stroop effect for diagnostic trials consistent with the PC of the induction (i.e., typical list-wide PC effects).³

In summary, Experiment 1 provided initial evidence that the intermediate timescale of conflict accumulation influences cognitive control independent of other timescales. However, there may be boundary conditions for this effect. For example, in Experiment 1, the intermediate timescale was strongly “experience defying” in that the window was comprised entirely of the type of trial participants rarely experienced before the window (e.g., a window of incongruent trials in an MC list). As such, in present lists, the intermediate conflict experience differed markedly from the preceding experience (e.g., shift from 93% congruent pre-window to 0% congruent during the window in MC⁰⁰PRESENT induction). Given this rather extreme shift, conflict experiences within the window may have been quite salient and driven adjustments in attention.

³ The analysis assessed the reaction time for Diagnostic Set trials that were integrated into the pre-window section of the induction. There was a significant main effect of congruence (F(1, 60) = 178.65, p < .001, η² = .749) and a significant main effect of condition type (F(3, 180) = 7.88, p < .001, η² = .116). Importantly, there was a significant interaction between congruence and condition (F(3, 180) = 7.99, p < .001, η² = .118) such that the size of Stroop Effect was in accordance with the PC in the pre-window section of each condition (MC⁰⁰PRESENT = 88; MC⁰⁰ABSENT = 74; MI⁰⁰ABSENT = 63; MI⁰⁰PRESENT = 43). Note the limited power, as there was only one congruent and one incongruent trail per list.
**Experiment 2**

Experiment 2 aimed to assess the possibility that the effects of the intermediate timescale depend on the experience within that timescale strongly defying experience in the long timescale. One theoretical possibility is that defiance of experience generates a prediction error (e.g., den Ouden, Kok, & de Lange, 2012) as such errors have been shown to be consequential in producing adjustments to cognitive control (Brown & Braver, 2005; Alexander & Brown, 2011). To address this possibility, Experiment 2 manipulated conflict in the intermediate timescale while reducing the magnitude of the prediction error between the pre-window and the window of the induction. As in Experiment 1, each list was comprised of an induction phase followed by a diagnostic phase. However, in Experiment 2, the induction phase prior to the window was always 50% congruent. Again, the last four trials of the induction phase (i.e., window) were manipulated such that the window was entirely congruent, entirely incongruent, or unbiased (50% congruent). The manipulation in Experiment 2 was employed to examine whether the effect of the intermediate timescale depends on the level of conflict experienced during the long timescale. In Experiment 1, the experience within the window strongly defied previous experience. For example, an entirely incongruent window in Experiment 1 (see Figure 1 panel A) represented a shift from 93% congruent pre-window to 0% congruent during the window in MC\text{PRESENT} induction. In comparison, the experience within the window in Experiment 2 defied previous experience comparatively weakly. An entirely incongruent window in Experiment 2 (see Figure 1 panel B) represented a shift from 50% congruent pre-window to 0% congruent during the window in the condition with an incongruent window. The diagnostic phase was identical to Experiment 1. Therefore, this experiment again had the opportunity to inform the theoretical
question of whether effects of the intermediate timescale are transient or more sustained (across multiple trials).

If the effect of intermediate conflict does not depend on-defying the long timescale, one would predict that the window section of the induction should drive subsequent cognitive control. That is, the congruent window will accentuate the congruency effect (and the incongruent window will attenuate the congruency effect) in the diagnostic phase relative to other conditions. Statistically, this would manifest as an interaction between congruence and window type during the diagnostic phase. Alternatively, if the effects of conflict accumulation in the intermediate timescale rely on strongly defying previous experience, given that there is objectively weaker defiance in Experiment 2, one would not predict a difference between the three conditions. Alternatively, the difference may be smaller. Statistically, there would be a non-significant interaction between congruence and window type.

Method

Participants. Sixty-two Washington University undergraduates (43 female, Age $M = 20.03$, $SD = 1.43$) participated for course credit. All participants were native English speakers with normal or corrected vision and color vision. One participant was excluded for falling asleep during the task, and one participant was excluded for difficulty using the microphone. Therefore, 60 were included in the reported analysis (42 female, Age $M = 20.05$, $SD = 1.43$).

Design and Stimuli. As in Experiment 1, each list was comprised of an induction phase and a diagnostic phase (see Figure 1) and the induction and diagnostic sets were identical to Experiment 1. The pre-window section of the induction phase began with a pseudo-randomly ordered set of seven congruent and seven incongruent trials. Therefore, the pre-window section of the induction was unbiased (i.e., 50% congruent). I manipulated conflict in the four-trial
window at the end of the induction phase (0% congruent, 50% congruent, or 100% congruent). Contrasting Experiment 1, this meant that the long timescale was not equivalent across conditions (induction phases were 61.11%, 50%, and 38.88% in the congruent, unbiased, and incongruent window conditions, respectively); this experimental control was sacrificed in order to manipulate the intermediate timescale while holding experience preceding the manipulation constant. The diagnostic phase was equivalent to Experiment 1.

There were 44 lists in the experiment including 12 lists for each of the following: unbiased with congruent window, unbiased with unbiased window, and unbiased with incongruent window. Additionally, there were eight filler lists. Again, the order of the trials within the lists was pseudorandom and fixed to maintain the manipulation. The order in which the 44 lists were presented was random.

**Procedure.** The procedure was identical to Experiment 1 with the exception that there were 44 lists of 26 trials.

**Results**

0.85% of trials were removed from the RT trim. All analyses used a 2 x 3 repeated measures ANOVA with factors of congruence (congruent or incongruent) and window type (100% congruent, 50% congruent, or 0% congruent). See Table 2 for descriptive statistics. Only theoretically relevant inferential statistics are reported; comprehensive analyses are reported in Table 4. Post-hoc $t$ values apply a Holm Bonferroni correction (Holm, 1979).

**Reaction Time**

**Induction items.** As in Experiment 1, induction analysis included the 14 pre-window trials and the four trials in the window. There was a significant main effect of congruence, $F(1, 59) = 242.68, p < .001, \eta_p^2 = .804$, such that responses to congruent trials ($M = 614, SE = 12$)
were faster than incongruent trials \((M = 719, SE = 15)\). There was a significant main effect of window, \(F(2, 118) = 3.48, p = .034, \eta_p^2 = .056\), such that performance during an induction with a 100% congruent window \((M = 662, SE = 16)\) was marginally faster than during an induction with a 50% congruent window \((M = 668, SE = 16, t = 2.043, p = .09)\) and significantly faster than with a 100% incongruent window \((M = 669, SE = 15, t = 2.49, p = .047)\); performance during inductions with 50% congruent and incongruent windows did not differ, \(t = 0.55, p = .58\). There was a non-significant interaction between window and congruence, \(F(2, 118) = 0.13, p = .876, \eta_p^2 = .002\), such that the Stroop effects did not differ across conditions.

**Diagnostic items.** There was a significant main effect of congruence, \(F(1, 59) = 99.77, p < .001, \eta_p^2 = .628\), such that responses to congruent trials \((M = 629, SE = 13)\) were faster than incongruent trials \((M = 724, SE = 18)\). There was a significant main effect of window, \(F(2, 118) = 3.39, p = .037, \eta_p^2 = .054\), such that responses following a congruent window \((M = 671, SE = 17)\) were non-significantly faster than performance following an unbiased window \((M = 674, SE = 16)\), \(t = 1.95, p = .111\), and were significantly faster than responses following an incongruent window \((M = 683, SE = 17), t = 2.48, p = .048\). Performances following a congruent window or an unbiased window did not differ, \(t = 0.64, p = .526\). There was a non-significant interaction of window and congruence, \(F(2, 118) = 0.42, p = .420, \eta_p^2 = .058\), as Stroop effects did not differ across conditions.

Although there was no hint of an effect of the intermediate window in the performance on diagnostic trials, for completeness I report the analysis excluding the first trial in the diagnostic phase. Again, there was a significant main effect of congruence, \(F(1, 59) = 100.21, p < .001, \eta_p^2 = .629\), such that responses to congruent trials \((M = 624, SE = 13)\) were more accurate than incongruent trials \((M = 721, SE = 18)\). However, the main effect of window was no longer
significant, $F(2, 118) = 1.93, p = .149, \eta^2_p = .032$. The interaction of window and congruence remained non-significant, $F(2, 118) = 1.38, p = .256, \eta^2_p = .023$.

**Error Rate**

**Induction items.** There was a significant main effect of congruence, $F(1, 59) = 74.51, p < .001, \eta^2_p = .558$, such that responses to congruent trials ($M = 0.57\%$, $SE = 0.12\%$) were more accurate than incongruent trials ($M = 4.01\%, SE = 0.45\%$). There was a non-significant main effect of window, $F(2, 118) = 0.46, p = .630, \eta^2_p = .008$. There was a significant interaction of window and congruence, $F(2, 118) = 4.30, p = .016, \eta^2_p = .068$, such that the Stroop effect was smaller in the congruent window condition ($M = 2.17\%, SE = 0.41\%$) than incongruent window ($M = 2.35\%, SE = 0.51\%$) and unbiased window ($M = 2.35\%, SE = 0.48\%$) conditions.

**Diagnostic items.** There was a significant main effect of congruence, $F(1, 59) = 47.96, p < .001, \eta^2_p = .448$, such that responses to congruent trials ($M = 0.51\%, SE = 0.16\%$) were more accurate than incongruent trials ($M = 4.18\%, SE = 0.65\%$). There was a significant main effect of window, $F(2, 118) = 6.41, p = .002, \eta^2_p = .098$, such that performance following an induction with a congruent window ($M = 3.03\%, SE = 0.60\%$) was less accurate than an unbiased window ($M = 2.09\%, SE = 0.51\%$), $t = 2.91, p = .011$, or an incongruent window ($M = 1.92\%, SE = 0.46\%$), $t = 3.02, p = .011$. Performance did not differ following an induction with an unbiased window and an incongruent window, $t = 0.57, p = .572$. There was a significant interaction effect between window and congruence, $F(2, 118) = 5.06, p = .008, \eta^2_p = .079$, such that the Stroop effect was smallest following an induction with an incongruent window ($M = 2.93\%, SE = 0.59\%$), then a congruent window, ($M = 3.2\%, SE = 0.72\%$) and the unbiased window ($M = 3.53\%, SE = 0.63\%$).
In the diagnostic phase, excluding the first trial, there was a significant main effect of congruence, $F(1, 59) = 43.44, p < .001, \eta^2_p = .424$, such that responses to congruent trials ($M = 0.46\%, SE = 0.16\%$) were more accurate than incongruent trials ($M = 4.13\%, SE = 0.68\%$). There was a significant main effect of window, $F(2, 118) = 3.30, p = .040, \eta^2_p = .053$; however, performance following an induction with a congruent window ($M = 2.80\%, SE = 0.60\%$) was non-significantly less accurate than performance following an unbiased window ($M = 2.15\%, SE = 0.55\%$), $t = 2.14, p = .103$, or an incongruent window ($M = 1.95\%, SE = 0.49\%$), $t = 2.16, p = .103$. Performance also did not differ between unbiased and incongruent conditions, $t = 0.49, p = .623$. There was now a non-significant interaction of window and congruence, $F(2, 118) = 2.14, p = .123, \eta^2_p = .035$.

**Discussion**

The key finding of Experiment 2 was that Stroop performance did not differ among conditions during the diagnostic phase. Recall that Experiment 1 and Experiment 2 were similar in their manipulation of the intermediate timescale (i.e., four-trial windows that were entirely congruent or entirely incongruent at the end of the induction phase) but differed in the pre-window section of the induction. The window section of the induction strongly defied the pre-window section in Experiment 1 and weakly defied the pre-window section in Experiment 2. Put simply, the same intermediate conflict manipulation that was effective in modulating control in Experiment 1 (i.e., presentation of four consecutive congruent or incongruent trials) did not affect cognitive control in Experiment 2. I interpret this to mean that if the conflict in the intermediate timescale sufficiently defies previous experience (as set by the induction trials), then conflict information in the intermediate timescale may be salient and weighted more strongly than distal conflict information. Prediction error potentially serves an important role as a
signal to weight new information preferentially. If a large prediction error occurs, then conflict experiences in the intermediate timescale may be signaled as important and thus affect control; however, if no or small prediction error occurs, then conflict in the intermediate timescale may not be signaled as particularly important. Although it remains to be determined what constitutes “sufficiently” defying experience or a sufficiently large prediction error, this result demonstrates a boundary condition of the influence of intermediate conflict experiences on cognitive control.

The results in Experiment 2 may be surprising when compared to studies that manipulated conflict experiences several trials before the diagnostic trial (e.g., Durston et al., 2003; Horga et al., 2011; Jiménez & Méndez, 2013; Jiménez & Méndez, 2014). Those studies used 50% congruent lists and found that cognitive control varied as a function of the amount of conflict experienced on several preceding trials. One might expect that because Experiment 2 used a similar manipulation to those previous studies (i.e., a window of 4 congruent trials or a window of 4 incongruent trials preceded by 50% congruent trials), then a difference between conditions should have been observed during the diagnostic phase. The results in Experiment 2 are not necessarily inconsistent with the aforementioned studies, however, because of two important methodological differences. First, the diagnostic trials in Experiment 2 were from the Diagnostic Set, and they were therefore a different color and word than any trial experienced in the window. To observe an effect on these trials, adjustments in control had to be sufficiently abstract to extend from the induction to new stimulus features in the diagnostic phase. That was not the case in the prior studies where diagnostic trials comprised the same features as the preceding inducer trials. Second, Experiment 2 considered a larger diagnostic scope (n + 8 trials) than prior studies (n + 1 trial). To know whether a similar effect was found in Experiment 2 as the prior studies, one would determine whether there was evidence for an effect of the
immediately preceding trial on the first trial of the diagnostic phase in Experiment 2. Some evidence exists for an effect of intermediate conflict. Specifically, reaction time was slower overall following the incongruent window in the diagnostic phase and error rate was significantly higher following a congruent window in the diagnostic phase but neither effect remained after removing the first trial of the diagnostic phase. This suggests much of these observed effects could be alternatively explained by congruency sequence effects following the final trial of induction.

**General Discussion**

While previous work documenting the effects of conflict experiences on cognitive control adaptation has mostly been limited to effects of the previous trial (immediate timescale) and effects of the entire block (long timescale), I aimed to examine the effects of relatively recent experience (intermediate timescale). To that end, two primary questions were addressed: what evidence exists for an intermediate timescale on its own or in interaction with other timescales and do effects from the intermediate timescale sustain over several trials? Evidence for those questions is hereafter discussed. Following that, I will discuss potential explanations and their theoretical implications, examine limitations for the study, and explore future directions for this research.

**Evaluation of Study Goals**

The first goal was to understand whether there is, as suggested by a number of behavioral studies (e.g., Durston et al., 2003; Horga et al., 2011; Jiménez & Méndez, 2013; Jiménez & Méndez, 2014) and existing statistical models (Aben et al., 2017; Dey 2019), evidence for an intermediate timescale and to examine whether the effect of conflict experiences in the intermediate timescale depends on other timescales. Experiment 1, controlling for conflict
experiences in the long and immediate timescales, found a unique effect of conflict in the intermediate timescale. In MC lists, presenting a run of four incongruent trials at the end of the induction led to an attenuated Stroop effect in the diagnostic phase; in MI lists, presenting four congruent trials at the end of the induction led to a larger Stroop effect in the diagnostic phase. Strikingly, when comparing \text{MC}_{\text{PRESENT}} to \text{MI}_{\text{PRESENT}} lists, conflict experiences in the intermediate timescale led to a reversal of the standard list-wide PC effect such that the Stroop effect was smaller in MC than MI lists. Contrasting Experiment 1, Experiment 2 used an unbiased pre-window section of the induction before the intermediate conflict manipulations. In Experiment 2, conflict in the intermediate timescale did not affect Stroop performance in the diagnostic phase. Taking both experiments into account, it can be concluded that evidence exists for an effect of conflict accumulation in the intermediate timescale that plausibly depends on the preceding conflict experiences in the long timescale. However, in opposition to the findings in statistical models examining timescales of control (Aben et al., 2017; Dey, 2019), there was not a stronger effect of the intermediate timescale (i.e., relatively recent experience) in MC lists than MI lists. The current effect may be driven by defying previous experiences rather than the overall conflict level in the previous experience.

The second primary goal was to understand whether effects from the intermediate timescale sustain across a longer diagnostic phase. In comparison to previous studies (e.g., Durston et al., 2003; Horga et al., 2011; Jiménez & Méndez, 2013; Jiménez & Méndez, 2014) that examined conflict experiences more distally than the immediately preceding trial, Experiments 1 and 2 broadened the understanding of the effects of intermediate conflict by widening the diagnostic scope beyond a single subsequent trial to a phase of eight subsequent trials. In Experiment 1, the intermediate conflict manipulation had an effect on the diagnostic
trials which remained significant after removing the first trial of the diagnostic phase. Although future research is needed to confirm this pattern, it suggests a potentially interesting difference between the intermediate and immediate timescale. The latter is typically thought to produce a transient effect (as indicated by studies examining the congruency sequence effect) but the results from Experiment 1 suggest that the effects of intermediate conflict extend beyond the trial immediately following the manipulation to other trials in the eight-trial diagnostic phase. Experiment 2 did not find an effect of intermediate conflict. Therefore, no evidence can be drawn regarding whether an effect was sustained or transient.

**Potential Explanations**

Though both experiments manipulated the experience of conflict in the intermediate timescale, only in Experiment 1 did the Stroop effect in the diagnostic phase depend on conflict in the intermediate timescale. One potential explanation considers whether it is necessary for conflict experiences in the intermediate timescale to produce a prediction error in order for these experiences to affect control. It is possible that such an error occurred in Experiment 1 but not Experiment 2 given that conflict experiences in the intermediate timescale were likely more salient in Experiment 1 because of the degree to which they defied prior experience in the lists. This salience may have increased the weighting of that information in the intermediate timescale. If the intermediate conflict was less salient in Experiment 2, it was also plausibly weighted less strongly, encouraging the participant to weight all experiences acquired in the list relatively equally. This difference would explain the significant interaction in Experiment 1 and the non-significant interaction in Experiment 2 for reaction time in diagnostic trials.

A second potential explanation interprets the results from the perspective of the volatility modelling framework (Jiang, Heller, & Egner, 2014). In comparison to the fixed learning rate
assumed in the conflict monitoring account, the volatility model incorporates flexible changes in learning rate as a function of volatility, (i.e., the likelihood that conflict is relatively consistent or fluctuates over the course of several trials; Jiang et al., 2014). When volatility is high, the learning rate is also high and accordingly, more recent information (i.e., trials that have been recently experienced) is weighted more strongly when informing whether attention should be heightened or relaxed. When volatility is low, learning rate is low and a larger window of (preceding) trials is used to inform cognitive control adaptations. By accounting for the amount of trials that are weighted to inform attentional settings, the volatility model is capable of forming hypotheses about effects on an intermediate timescale that depend on conflict experiences in the long timescale.

Interpreting these results from the volatility model’s perspective, Experiment 1 is considerably volatile on a list level (take MC\textsc{Present} for example, where participants shift from MC in the induction to entirely incongruent in the intermediate window to unbiased in the diagnostic phase) and on an experimental level (shifting from MC to MI to Unbiased lists randomly). Experiment 2 is less volatile on a list level (unbiased, shifting to a biased or unbiased window, shifting to unbiased) and on an experiment level (most lists are unbiased or close to unbiased). If learning rate increases with increased volatility and a higher learning rate leads to stronger weighting of more recent trials (Jiang, Beck, Heller, & Egner, 2015), then Experiment 1 should have been more likely to yield an effect of the intermediate conflict. Consistent with this model, it did. Interestingly this model also predicted the lack of an asymmetrical effect of intermediate conflict depending on the long timescale (MC vs. MI) in Experiment 1. MC\textsc{Present} and MI\textsc{Present} lists were equivalently volatile to each other; thus, according to the volatility
model, the lists should have encouraged participants to use an equivalent amount of the intermediate information in each list.

**Limitations and Future Directions**

Several limitations merit further discussion. One is the conceptualization of relatively recent experience as a four-trial window of conflict. This number was chosen because prior behavioral studies had included examination of four previous trials (e.g., Durston et al., 2003; Horga et al., 2011; Jiménez & Méndez, 2013; Jiménez & Méndez, 2014) and prior modelling efforts found an effect of conflict at least four trials prior (Aben et al., 2017). To fully understand the effects of conflict on the intermediate timescale, future research should examine different window lengths. In addition, future research should also consider whether a window of a given length (e.g., four trials) may have a different effect on control depending on the length of the pre-window induction, that is, whether the relative size of the window matters or just the absolute size. It is possible that effects of manipulations on the intermediate timescale depend on what proportion of the overall experience the intermediate conflict comprises.

In comparing the results of this study to previous work, some consideration should be given to two key design choices: the abbreviated lists paradigm and the conceptualization of intermediate conflict. While these design choices were chosen to fulfill goals of the study, comparing results across studies should account for these methodological differences. The abbreviated-lists paradigm was used to control for the long and intermediate timescales in each list, while still achieving enough observations for diagnostic trials. Abbreviated lists allow for more lists to be seen by each participant, thus allowing for sufficient observations for each condition. Note that the statistical modelling papers that provided initial evidence for the intermediate timescale (Aben et al., 2017; Dey 2019) were based on longer lists (lists ranged
from 160 to 480 trials). It is possible that when the long timescale is based on a longer history of trials then its influence on subsequent cognitive control changes. Another important difference between this study and previous modelling efforts was the choice of manipulating intermediate conflict experiences as runs of four trials of the same trial type. The statistical models (Aben et al., 2017; Dey 2019) were based on naturally occurring sequences and examined the effect of conflict experiences on the trials preceding the $n$ trial. While it is possible that some runs of four trials occurred in those lists, the effect of a conflict experience on the fourth trial before trial $n$ does not necessitate that all four trials preceding trial $n$ were the same trial type (congruency). A primary goal of this study was to find an effect of the intermediate timescale if such an effect exists; it is likely that manipulating the four trials to be entirely congruent (or entirely incongruent) represents an extreme case of an intermediate conflict experience.

An important consideration in the results of Experiment 1 is the absence of an asymmetry between MC and MI conditions during the diagnostic phase. For reference, an asymmetrical pattern would mean that the effect of the intermediate conflict manipulation was larger in the MC condition than in the MI condition (e.g., Abrahamse, Duthoo, & Notebaert, 2013), as observed in Aben et al., (2017; see also Dey, 2019). An important difference in design regards the presence and placement of diagnostic trials. The effects modelled by Aben et al., (2017) included only biased trials; the list-wide PC effects in Dey (2019) included biased induction trials intermixed with unbiased diagnostic trials. In Experiment 1 of this study, diagnostic trials were presented in a separate phase following induction. The lack of asymmetry in the diagnostic phase for Experiment 1 may therefore not be inconsistent with these models, as they do not model solely diagnostic trials in a list-wide manipulation.
Some theoretical consideration should be given to the surprising result in Experiment 1 that the manipulation of intermediate conflict produced a “reversed” list-wide PC effect comparing MC\text{PRESENT} and MI\text{PRESENT} lists. That is, experiencing an MC list ending with four incongruent trials led to a smaller Stroop effect in the diagnostic phase than experiencing an entirely MI list (i.e., MI\text{ABSENT}). Likewise, experiencing an MI list ending with four congruent trials led to a larger Stroop effect in the diagnostic phase than an entirely MC list (i.e., MC\text{ABSENT}). This finding may speak to the relative strength of conflict in the intermediate timescale. While previous work has shown that list-wide PC effects are dissociable from congruency sequence effects and are not just by an accumulation of immediate timescale adaptations (Torres-Quesada et al., 2013; Torres-Quesada et al., 2014), no research has assessed whether list-wide PC effects are made up of intermediate effects. Simply put, it is possible that list-wide PC effects are driven by several preceding trials rather than whole lists. If adjustments to cognitive control based on intermediate conflict experiences are more sustained than those from the immediate timescale, as the findings of Experiment 1 imply, then intermediate experiences may more plausibly drive list-wide PC effects.

Another limitation of the approach in the current studies is in Experiment 1, PC was held constant across inductions while conflict in the window section of the induction phase was manipulated. Therefore, present and absent lists in Experiment 1 also differed in how much conflict was experienced in the beginning of the induction. It is possible that the initial experiences in a list, rather than the lists differing at the end of the induction phase, drove differences between conditions in the diagnostic phase. Experiment 2 did not differ in the early lists, and significant differences were not observed between conditions in the diagnostic phase. However, this explanation is inconsistent with previous findings that people flexibly adjust
control based on later experiences following conflict experienced at the beginning of a list (Cohen-Shikora et al., 2018).

It is likely the case that many factors contribute to the amount of trials that are represented in the intermediate timescale such as the PC of the overall list (Aben et al., 2017) and the volatility both within a list and experiment-wide (Jiang et al., 2014). At the same time, experimental manipulations that affect prediction error or the salience of the intermediate timescale may modulate its influence on control. Additionally, individual differences, such as working memory capacity, may contribute to how many trials are represented within this timescale. Individuals with a larger working memory capacity may maintain and use conflict experiences that occur more trials back than those with low working memory capacity.

**Conclusion**

The present study demonstrated a unique effect of relatively recent conflict experiences on cognitive control, further suggesting the importance of the intermediate timescale. The present study also provided initial support for the possibility that this effect of recent conflict experiences may depend in part on whether recent experiences are inconsistent with previous experiences. Given that effects from the intermediate timescale were seen in the diagnostic phase after controlling for the effect of the immediate timescale at the beginning of the diagnostic phase, there is evidence that conflict experiences in the intermediate timescale affect control in a sustained fashion. Further research should continue to consider whether certain conflict experiences are more influential than others in terms of informing how people attend to and resolve subsequent conflict. The results reported in this study demonstrate that theories of cognitive control should consider the role of conflict experienced in the intermediate timescale,
as it could lead to a better understanding of how previous conflict experiences are weighted and inform adjustments to cognitive control.
References


34


doi:10.1037/0033-295X.115.2.518
Figures and Tables

Figure 1. List composition for Experiment 1 (panel A) and Experiment 2 (panel B). Shaded squares represent incongruent trials and unshaded squares represent congruent trials. For Experiment 1, note the equivalent number of congruent and incongruent trials for each type of MC list and for each type of MI list. Only the intermediate timescale differs between present and absent lists. For Experiment 2, note the equivalent number of congruent and incongruent trials before the window in each list. In both experiments, note that the diagnostic phase is equivalent across conditions.
Figure 2. Reaction time and error rate results for Experiment 1 diagnostic phase trials. Error bars represent a 95% confidence interval. Panel A shows a significant three-way interaction was seen between congruence, PC, and window in reaction time. The presence of the window attenuated the Stroop effect in MC and exacerbated the Stroop effect in MI. Panel B shows a non-significant three-way interaction between congruence, PC, and window for error rate.
Figure 3. Reaction time and error rate results for Experiment 2 diagnostic trials. Error bars represent a 95% confidence interval. Panel A shows a non-significant interaction between congruence, window type in reaction time. Panel B shows a significant interaction between congruence and window type, such that the Stroop effect was larger following a congruent window in error rate. Note that the significant difference in error rate did not survive removing the first trial of the diagnostic phase.
Table 1

Experiment 1 Reaction Time and Error Rate

<table>
<thead>
<tr>
<th>Phase</th>
<th>PC</th>
<th>Window</th>
<th>Trial Type</th>
<th>Reaction Time</th>
<th>Error %</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Induction</strong></td>
<td>MC</td>
<td>Present</td>
<td>Congruent</td>
<td>570 (74)</td>
<td>0.51 (0.62)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>732 (98)</td>
<td>7.52 (8.00)</td>
</tr>
<tr>
<td>Absent</td>
<td>MC</td>
<td>Present</td>
<td>Congruent</td>
<td>589 (79)</td>
<td>0.64 (0.80)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>703 (89)</td>
<td>5.59 (5.27)</td>
</tr>
<tr>
<td><strong>MI</strong></td>
<td>Present</td>
<td>Congruent</td>
<td>620 (95)</td>
<td>0.56 (0.98)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>684 (90)</td>
<td>2.41 (2.73)</td>
</tr>
<tr>
<td>Absent</td>
<td>MC</td>
<td>Present</td>
<td>Congruent</td>
<td>618 (93)</td>
<td>0.48 (1.06)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>688 (90)</td>
<td>3.30 (3.19)</td>
</tr>
<tr>
<td><strong>Diagnostic</strong></td>
<td>MC</td>
<td>Present</td>
<td>Congruent</td>
<td>636 (97)</td>
<td>0.74 (1.41)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>698 (100)</td>
<td>4.39 (5.58)</td>
</tr>
<tr>
<td>Absent</td>
<td>MC</td>
<td>Present</td>
<td>Congruent</td>
<td>615 (85)</td>
<td>0.59 (1.32)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>693 (95)</td>
<td>4.20 (4.54)</td>
</tr>
<tr>
<td><strong>MI</strong></td>
<td>Present</td>
<td>Congruent</td>
<td>607 (89)</td>
<td>0.69 (1.49)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>700 (102)</td>
<td>3.62 (5.45)</td>
</tr>
<tr>
<td>Absent</td>
<td>MC</td>
<td>Present</td>
<td>Congruent</td>
<td>621 (90)</td>
<td>0.34 (1.11)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Incongruent</td>
<td>697 (98)</td>
<td>3.82 (4.20)</td>
</tr>
</tbody>
</table>

*Note: Mean and Standard Deviation for trials in the induction and diagnostic phases of Experiment 1.*
Table 2

Experiment 2 Reaction Time and Error Rate

<table>
<thead>
<tr>
<th>Phase</th>
<th>Window Type</th>
<th>Trial Type</th>
<th>Reaction Time</th>
<th>Error %</th>
</tr>
</thead>
<tbody>
<tr>
<td>Induction</td>
<td>Congruent Window</td>
<td>Congruent</td>
<td>610 (96)</td>
<td>0.77 (1.08)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Incongruent</td>
<td>714 (121)</td>
<td>3.57 (3.22)</td>
</tr>
<tr>
<td></td>
<td>Incongruent Window</td>
<td>Congruent</td>
<td>614 (94)</td>
<td>0.62 (0.92)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Incongruent</td>
<td>721 (115)</td>
<td>4.08 (3.71)</td>
</tr>
<tr>
<td></td>
<td>Unbiased Window</td>
<td>Congruent</td>
<td>617 (95)</td>
<td>0.33 (0.63)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Incongruent</td>
<td>722 (125)</td>
<td>4.37 (3.63)</td>
</tr>
<tr>
<td>Diagnostic</td>
<td>Congruent Window</td>
<td>Congruent</td>
<td>622 (104)</td>
<td>0.62 (1.36)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Incongruent</td>
<td>720 (139)</td>
<td>5.43 (5.47)</td>
</tr>
<tr>
<td></td>
<td>Incongruent Window</td>
<td>Congruent</td>
<td>637 (101)</td>
<td>0.50 (1.23)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Incongruent</td>
<td>730 (138)</td>
<td>3.33 (4.51)</td>
</tr>
<tr>
<td></td>
<td>Unbiased Window</td>
<td>Congruent</td>
<td>626 (89)</td>
<td>0.40 (1.17)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Incongruent</td>
<td>723 (137)</td>
<td>3.78 (4.88)</td>
</tr>
</tbody>
</table>

*Note:* Mean and Standard Deviation for trials in the induction and diagnostic phases of Experiment 2.
Table 3

Experiment 1 $F$ Table

<table>
<thead>
<tr>
<th>DV</th>
<th>Phase</th>
<th>Effect</th>
<th>Df</th>
<th>$F$</th>
<th>$P$</th>
<th>$\eta^2_p$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reaction Time</td>
<td>Induction</td>
<td>Congruence</td>
<td>1, 60</td>
<td>467.08</td>
<td>&lt; .001</td>
<td>.886</td>
</tr>
<tr>
<td></td>
<td></td>
<td>PC</td>
<td>1, 60</td>
<td>1.80</td>
<td>.184</td>
<td>.029</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Window</td>
<td>1, 60</td>
<td>1.208</td>
<td>.276</td>
<td>.020</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * PC</td>
<td>1, 60</td>
<td>286.28</td>
<td>&lt; .001</td>
<td>.827</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * Window</td>
<td>1, 60</td>
<td>53.33</td>
<td>&lt; .001</td>
<td>.471</td>
</tr>
<tr>
<td></td>
<td></td>
<td>PC * Window</td>
<td>1, 60</td>
<td>2.16</td>
<td>.147</td>
<td>.035</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * PC * Window</td>
<td>1, 60</td>
<td>70.74</td>
<td>&lt; .001</td>
<td>.541</td>
</tr>
<tr>
<td>Diagnostic</td>
<td>Congruence</td>
<td></td>
<td>1, 60</td>
<td>298.82</td>
<td>&lt; .001</td>
<td>.883</td>
</tr>
<tr>
<td></td>
<td>PC</td>
<td></td>
<td>1, 60</td>
<td>2.79</td>
<td>.100</td>
<td>.044</td>
</tr>
<tr>
<td></td>
<td>Window</td>
<td></td>
<td>1, 60</td>
<td>2.85</td>
<td>.096</td>
<td>.045</td>
</tr>
<tr>
<td></td>
<td>Congruence * PC</td>
<td></td>
<td>1, 60</td>
<td>20.16</td>
<td>&lt; .001</td>
<td>.251</td>
</tr>
<tr>
<td></td>
<td>Congruence * Window</td>
<td></td>
<td>1, 60</td>
<td>0.03</td>
<td>.876</td>
<td>.000</td>
</tr>
<tr>
<td></td>
<td>PC * Window</td>
<td></td>
<td>1, 60</td>
<td>12.07</td>
<td>&lt; .001</td>
<td>.167</td>
</tr>
<tr>
<td></td>
<td>Congruence * PC * Window</td>
<td></td>
<td>1, 60</td>
<td>18.72</td>
<td>&lt; .001</td>
<td>.238</td>
</tr>
<tr>
<td>Error Rate</td>
<td>Induction</td>
<td>Congruence</td>
<td>1, 60</td>
<td>64.86</td>
<td>&lt; .001</td>
<td>.519</td>
</tr>
<tr>
<td></td>
<td>PC</td>
<td></td>
<td>1, 60</td>
<td>44.21</td>
<td>&lt; .001</td>
<td>.424</td>
</tr>
<tr>
<td></td>
<td>Window</td>
<td></td>
<td>1, 60</td>
<td>1.69</td>
<td>.199</td>
<td>.027</td>
</tr>
<tr>
<td></td>
<td>Congruence * PC</td>
<td></td>
<td>1, 60</td>
<td>39.15</td>
<td>&lt; .001</td>
<td>.395</td>
</tr>
<tr>
<td></td>
<td>Congruence * Window</td>
<td></td>
<td>1, 60</td>
<td>2.30</td>
<td>.135</td>
<td>.037</td>
</tr>
<tr>
<td></td>
<td>PC * Window</td>
<td></td>
<td>1, 60</td>
<td>11.87</td>
<td>.001</td>
<td>.165</td>
</tr>
<tr>
<td></td>
<td>Congruence * PC * Window</td>
<td></td>
<td>1, 60</td>
<td>12.93</td>
<td>&lt; .001</td>
<td>.177</td>
</tr>
<tr>
<td>DV</td>
<td>Phase</td>
<td>Effect</td>
<td>Df</td>
<td>$F$</td>
<td>$P$</td>
<td>$\eta_p^2$</td>
</tr>
<tr>
<td>----------</td>
<td>-------</td>
<td>----------------------</td>
<td>------</td>
<td>-------</td>
<td>------</td>
<td>------------</td>
</tr>
<tr>
<td>Diagnostic</td>
<td>Congo</td>
<td>1, 60</td>
<td>57.67</td>
<td>&lt; .001</td>
<td>.490</td>
<td></td>
</tr>
<tr>
<td>PC</td>
<td>1, 60</td>
<td>0.23</td>
<td>.633</td>
<td>.004</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Window</td>
<td>1, 60</td>
<td>2.50</td>
<td>.119</td>
<td>.040</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Congo * PC</td>
<td>1, 60</td>
<td>0.83</td>
<td>.367</td>
<td>.014</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Congo * Window</td>
<td>1, 60</td>
<td>0.18</td>
<td>.670</td>
<td>.003</td>
<td></td>
<td></td>
</tr>
<tr>
<td>PC * Window</td>
<td>1, 60</td>
<td>0.04</td>
<td>.842</td>
<td>.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Congo * PC * Window</td>
<td>1, 60</td>
<td>0.42</td>
<td>.521</td>
<td>.007</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Note: F table for Experiment 1 results*
Table 4

Experiment 2 $F$ Table

<table>
<thead>
<tr>
<th>DV</th>
<th>Phase</th>
<th>Effect</th>
<th>df</th>
<th>$F$</th>
<th>$p$</th>
<th>$\eta^2_p$</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Reaction Time</strong></td>
<td>Induction</td>
<td>Congruence</td>
<td>1, 59</td>
<td>242.68</td>
<td>&lt; .001</td>
<td>.804</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Window</td>
<td>2, 118</td>
<td>3.48</td>
<td>.034</td>
<td>.056</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * Window</td>
<td>2, 118</td>
<td>0.13</td>
<td>.876</td>
<td>.002</td>
</tr>
<tr>
<td>Diagnostic</td>
<td></td>
<td>Congruence</td>
<td>1, 59</td>
<td>99.77</td>
<td>&lt; .001</td>
<td>.628</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Window</td>
<td>2, 118</td>
<td>3.39</td>
<td>.037</td>
<td>.054</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * Window</td>
<td>2, 118</td>
<td>0.42</td>
<td>.658</td>
<td>.007</td>
</tr>
<tr>
<td><strong>Error Rate</strong></td>
<td>Induction</td>
<td>Congruence</td>
<td>1, 59</td>
<td>74.51</td>
<td>&lt; .001</td>
<td>.558</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Window</td>
<td>2, 118</td>
<td>0.46</td>
<td>.630</td>
<td>.008</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * Window</td>
<td>2, 118</td>
<td>4.30</td>
<td>.016</td>
<td>.068</td>
</tr>
<tr>
<td>Diagnostic</td>
<td></td>
<td>Congruence</td>
<td>1, 59</td>
<td>47.96</td>
<td>&lt; .001</td>
<td>.448</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Window</td>
<td>2, 118</td>
<td>6.41</td>
<td>.002</td>
<td>.098</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Congruence * Window</td>
<td>2, 118</td>
<td>5.06</td>
<td>.008</td>
<td>.079</td>
</tr>
</tbody>
</table>

*Note: $F$ table for Experiment 2 results*