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Proactive Control in the Stroop Task: A Conflict-Frequency Manipulation Free of Item-Specific, Contingency-Learning, and Color-Word Correlation Confounds

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In the Stroop task, congruency effects (i.e., the color-naming latency difference between incongruent stimuli, e.g., the word BLUE written in the color red, and congruent stimuli, e.g., RED in red) are smaller in a list in which incongruent trials are frequent than in a list in which incongruent trials are infrequent. The traditional explanation for this pattern is that a conflict-monitoring mechanism adjusts attention to task-relevant versus task-irrelevant information in a proactive fashion based on list-wide conflict frequency. More recently, however, multiple alternative explanations have been advanced that could explain the pattern without invoking this form of proactive control: Individuals might only adapt to conflict frequency specific to individual items (as opposed to list-wide conflict frequency), they could learn word–color contingencies (e.g., how often a particular word and color are paired), or they could adapt attention based on whether the words are informative of the color (even if many word–color pairings are incongruent) in the list as a whole. To examine this issue, we designed a new paradigm that should eliminate any impact of these alternative mechanisms. In that paradigm, the proportion of neutral (e.g., XXX in red) and incongruent stimuli was manipulated across lists. Paralleling the results in the original paradigm, there was a smaller latency difference between incongruent and neutral stimuli in a list in which incongruent trials were frequent than in a list in which incongruent trials were infrequent, suggesting that proactive control in response to list-wide conflict frequency is a process humans can and do use.

Keywords: conflict adaptation, conflict monitoring, proactive control, proportion-congruent effect, Stroop

A question that has received increasing research interest in the last decade is whether the expectation of conflict between task-irrelevant and task-relevant information can induce individuals to adjust attention between those sources of information (Schmidt, 2013a). This putative conflict-adaptation mechanism would bias attention toward task-relevant information when conflict is expected, but not when conflict is unexpected (Botvinick, Braver, Barch, Carter, & Cohen, 2001). A typical example of those types of situations is represented by manipulations of conflict frequency in the Stroop (1935) task.

In the Stroop task, participants name the ink color of a letter string, typically a color word, which can be congruent with the color (e.g., the word RED in red color), incongruent with the color (e.g., the word BLUE in red), or neutral (e.g., the consonant string XXX in red). The typical result is a congruency effect, with faster and/or more accurate responses to congruent than to incongruent items. This effect usually results from interference from incongruent items (typically producing slower latencies than neutral items) combined with facilitation from congruent items (typically producing faster latencies than neutral items). The relative magnitudes of interference and facilitation depend on a host of factors, for example, the nature of the neutral items used. When consonant strings are used as neutral items, for example, interference typically outweighs facilitation. In contrast, facilitation can be equivalent or even larger than interference if pronounceable words are used as neutral items (Brown, 2011; MacLeod, 1991) or if another type of baseline is used (e.g., performance in a separate list in which the same word is presented on all trials: Sabri, Melara, & Algom, 2001; see also Eidels, Townsend, & Algom, 2010).

What is most relevant for the present discussion is that the magnitude of the overall congruency effect varies as a function of conflict frequency, a situation typically examined by using proportion-congruent (PC) manipulations. In the standard (list-wide) PC manipulation, performance in a list in which conflict is frequent (when incongruent items are more frequent than congruent items, i.e., a mostly incongruent [MI] list) is compared with performance in a list in which conflict is infrequent (when congruent items are more frequent than incongruent items, i.e., a mostly congruent [MC] list). The typical result is that the congru-

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ency effect is smaller in the MI list than in the MC list (e.g., Logan & Zbrodoff, 1979).

This finding, known as the PC effect, has traditionally been interpreted as evidence for the use of a process of adaptation to list-wide conflict frequency. According to this explanation, a control mechanism exists that monitors conflict and adapts attention accordingly (Botvinick et al., 2001). Specifically, a situation in which task-irrelevant information (i.e., the word) is frequently conflicting (i.e., an MI list) will cause the emission of a signal indicating a need for more focused attention to task-relevant information (i.e., the color). Interference from irrelevant information will thus be minimized, producing a small congruency effect. In contrast, when conflict from task-irrelevant information is infrequent (i.e., in an MC list), attention will be relaxed because there is less reason to increase control. Thus, interference from irrelevant information on the rare incongruent items in an MC list will be especially problematic to overcome, producing a large congruency effect.

According to one of the control models often used to interpret the PC effect, the conflict-monitoring model (Botvinick et al., 2001), this effect is the result of transient control, the type of control that is also observed in what is known as the Gratton effect (i.e., reduced congruency effects following incongruent than congruent items: Gratton, Coles, & Donchin, 1992). Because in an MI list, but not in an MC list, conflict often accumulates over the course of the experiment, this transient control would typically lead to a tightening of attention to task-relevant information in an MI list, resulting in a reduced congruency effect (see also Jiménez & Méndez, 2013, 2014; for a challenge to the control interpretation of the Gratton effect, see Mayr, Awh, & Laurey, 2003).

More recent research, however, has suggested that the PC effect is dissociable from trial-to-trial control adjustments (and, hence from the Gratton effect, e.g., Torres-Quesada, Funes, & Lupiáñez, 2013; Torres-Quesada, Lupiáñez, Milliken, & Funes, 2014) and that control, in general, appears to operate at both short and longer time scales (e.g., Braver, 2012; Braver, Gray, & Burgess, 2007; De Pisapia & Braver, 2006; Kane & Engle, 2003; Jiang, Heller, & Egner, 2014). For example, in the Dual-Mechanism of Control framework proposed by Braver (2012; Braver et al., 2007), an MI list would favor a proactive mode of control that minimizes interference from the word by maintaining the color-naming goal to the extent possible. An MC list, on the other hand, would favor a more transient, reactive mode of control whereby the color-naming goal is often neglected and is only retrieved upon presentation of an infrequently occurring incongruent stimulus (De Pisapia & Braver, 2006; see also Kane & Engle, 2003). Thus, although the distinction between proactive and reactive control is likely blurrier than this explanation suggests (see, e.g., Aben, Verguts, & Van den Bussche, 2017), the PC effect (more precisely, the reduced congruency effect in an MI list) is assumed to result from a proactive form of control, in the sense that this control is applied *before* any specific item appears (e.g., Bugg, 2014; Gonthier, Braver, & Bugg, 2016; see also Verguts & Notebaert's, 2008, notion of "nonspecific" conflict adaptation).[1]

In recent years, however, the idea that the PC effect results from the implementation of a proactive process in high-conflict situations has received considerable criticism (Blais & Bunge, 2010; Blais, Robidoux, Risko, & Besner, 2007; Schmidt, 2013a, 2019). This criticism stems from the realization that a proactive control process is not necessary for generating a PC effect, as this effect could also result from alternative processes that PC paradigms typically allow. These processes, described below, include learning to associate items to responses and/or control states, and learning to adjust attention based on how informative (rather than how conflicting) the items in the task are.[2] In the present research, we aimed to demonstrate that when all of these alternative processes are accounted for, a PC-like effect can still be observed, providing evidence for the existence of proactive adaptation to conflict frequency.

Reactive Accounts of the List-Wide Proportion-Congruent Effect

The first challenge to a proactive control account of the list-wide PC effect came from Jacoby, Lindsay, and Hessels's (2003) report of an "item-specific" PC effect. Jacoby et al. (2003) designed a new version of the PC paradigm in which half of the words were mainly presented in their congruent color (MC items) and the other half were mainly presented in an incongruent color (MI items), with all words intermixed in a single list. Similar to the list-wide PC effect, an item-specific PC effect emerged, with MC items eliciting a larger congruency effect than MI items.

Because in Jacoby et al.'s (2003) paradigm congruent and incongruent items were equally probable in the list as a whole, the PC effect they obtained could not have been produced by a proactive process based on list-wide conflict frequency (i.e., a process that is applied *before* any specific item appears). Instead, it must have been produced by a reactive process that is initiated *after* an item is presented in response to the nature of that specific item. This type of reactive process could take two basic forms. First, it might be a conflict-adaptation process in which recognition of specific stimuli regulates the recruitment of appropriate control processes (e.g., Bugg & Hutchison, 2013; Gonthier et al., 2016). Specifically, the recognition of an MI word, for example, BLUE, would favor focused attention to the color, producing reduced interference. On the other hand, the recognition of an MC word, for example, GREEN, would favor relaxation of attention, a process that would encourage word processing, thus producing a large interference effect when the MC word does conflict with the color. Alternatively, the process producing the item-specific PC effect might be one whereby a contingency is learned between a word and the response typically made to that word (Schmidt & Besner, 2008). For example, if the MI word BLUE appears most often in red, individuals will use that word to predict a red response, with

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[1] In the following, for simplicity, we refer to adaptation to list-wide conflict frequency as a "proactive process" even though, in a Dual-Mechanism of Control framework (Braver, 2012; Braver et al., 2007; see also Kane & Engle, 2003), this adaptation would entail the engagement of both proactive control (in a frequently conflicting list) and a form of reactive control (in an infrequently conflicting list).

[2] It is important to emphasize that all these competing accounts of the PC effect are, in essence, learning accounts (see, e.g., Egner, 2014). For example, the process of adaptation to list-wide conflict frequency could be described as the process whereby participants *learn* to focus attention to the task-relevant dimension in a frequently conflicting list and *learn* to relax attention in an infrequently conflicting list. What does distinguish these accounts is what is being learned, e.g., associations between words and responses (a contingency-learning process) versus associations between contexts and control settings (a conflict-adaptation process).

the result being that red responses will be produced relatively rapidly even though the word itself is BLUE, leading to a reduced congruency effect. Conversely, individuals will use the MC word GREEN to predict the (congruent) green response. Hence, latencies will speed up for these congruent stimuli, producing an increased congruency effect.

Whether a conflict-adaptation or a contingency-learning process is responsible for the item-specific PC effect (for discussions, see, e.g., Bugg & Hutchison, 2013; Schmidt, 2013a, 2019), the question this effect raises is whether either of these reactive strategies might also explain the list-wide PC effect. The reason this question is relevant is that, in the list-wide PC manipulation, all items appearing in an MC list are MC items (i.e., all words appear most often in a congruent color) and all items appearing in an MI list are MI items (i.e., all words appear most often in one or more incongruent colors). Thus, it is possible that the list-wide PC effect is not produced by a proactive process dependent on list-wide conflict frequency, but by whatever reactive process produces the difference between MC and MI items in the item-specific PC paradigm (for a demonstration of this possibility within the framework of the conflict-monitoring model, see, e.g., Blais et al., 2007).

To address this question, Bugg (2014; see also Blais & Bunge, 2010; Bugg, Jacoby, & Toth, 2008) developed a new list-wide PC manipulation which allows for a dissociation of a proactive control process from reactive processes. Bugg divided her items into two sets, referred to as the “context” set and the “transfer” set, and manipulated congruency proportion for the context set only. The transfer items were 50:50 congruent/incongruent and were intermixed in a list with either mostly congruent context items (creating an overall MC list) or mostly incongruent context items (creating an overall MI list). Note that, from the participants’ perspective, in this type of manipulation there is no obvious separation between the two sets of stimuli used in the task, nor are the participants informed about their existence. However, using context and transfer stimulus sets provides the researcher with a meaningful tool for examining the processes normally involved in list-wide PC manipulations. Specifically, the rationale is that while a PC effect obtained with the context items might result from any of multiple processes, the only possible explanation for a PC effect on the transfer items would be adaptation to list-wide conflict frequency.

Indeed, in addition to the expected PC effect for context items in all situations (which, as just noted, is compatible with multiple explanations), a PC effect for transfer items did emerge in one of the situations that Bugg (2014) examined. Specifically, a PC effect with transfer items emerged when the MI list was a list in which contingency learning was impossible for the MI context items due to the fact that the context words appeared in four equally probable colors (one congruent and three incongruent) and, hence, no word-response contingencies existed for those words.[3] That is, only in this circumstance did transfer items show a smaller congruency effect in the MI list than in the associated MC list. In contrast, when contingency learning was possible for context items in the MI list (i.e., when each of the context words appeared more frequently in one specific incongruent color), no PC effect was obtained on the transfer items (i.e., the congruency effects on the transfer items were the same size in the MC and MI lists).

To explain these results, Bugg (2014) suggested that adaptation to list-wide conflict frequency is possible, but its usage “will primarily be evident when one cannot rely on use of [word-response contingencies] to guide responding on most trials in an effort to achieve task goals (i.e., minimization of Stroop interference)” (p. 568). That is, Bugg’s suggestion is that when one cannot rely on the use of word-response contingencies on most trials in the list, for example, in the MI list that did not involve any word-response contingencies for the context items, the process being used is a process involving a focus of attention on the color, that is, a conflict-adaptation process. The result is a congruency effect for the items in that list (including the transfer items) that was smaller than that in the MC list (in which a contingency-learning process potentially is being used), producing a PC effect. On the other hand, when reliable contingencies exist in the MI list, no conflict-adaptation process is engaged in either list. Rather, contingency learning is the only process being engaged in both MC and MI lists. As a result, the transfer items are unaffected, causing them to produce the same size congruency effect in the two lists.

The Role of Stimulus Informativeness and Color-Word Correlations

Bugg’s (2014) results would seem to make a clear case for the existence of a process of adaptation to list-wide conflict frequency, at least in certain circumstances. Recently, however, Schmidt (2019) has argued that an alternative interpretation for Bugg’s results is possible, one that does not involve a role for adaptation to list-wide conflict frequency. According to this explanation, the PC effect on transfer items reported by Bugg when the context MI items did not have a more probable incongruent color resulted from a process in which attention to task-relevant and task-irrelevant information *is* adapted, however, this adaptation is based on what he termed “stimulus informativeness” rather than conflict frequency.

According to Schmidt (2019), the term “stimulus informativeness” refers to the degree to which words in a list allow learning of word-response contingencies. In an MC list, words would be relatively informative because contingencies can be learned for at least some of the words (i.e., the context words). Thus, attention to words (including transfer words) would be enhanced in that list. Because attention to transfer words is enhanced, they will produce more interference, leading to a large congruency effect. In contrast, because words are relatively uninformative in an MI list if no contingencies exist for the context items, attention to those words (including transfer words) will be reduced. Transfer words will thus produce less interference in this situation, leading to a reduced congruency effect. Note that this account would also explain why no PC effect is obtained for transfer items when contingencies can be learned for both MC and MI context items. The reason is that context words would be informative in both situations. Thus, attention would be directed to (all) words in both MC and MI lists.

Schmidt’s (2019) stimulus-informativeness account echoes an idea proposed previously by Algom and collaborators in the context of their input-driven account of the Stroop effect (Dishon-Berkovits & Algom, 2000; Melara & Algom, 2003; Sabri et al., 2001; see also Algom & Chajut, 2019). Their claim is that attention to the task-irrelevant dimension is increased when the values on the task-relevant dimension (e.g., the colors) and the values on

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[3] Note that contingency learning is always possible for MC context items, as the congruent color is, unavoidably, also the more probable color.

the task-irrelevant dimension (e.g., the words) are correlated. That is, when the words in the Stroop task provide information about the colors they appear in (even if those colors are incongruent ones), the word dimension will receive more attention than in a situation in which the words and the colors are randomly paired (a zero-correlation situation). As a result of receiving increased attention in a correlated situation, the words will produce considerable interference, interference that may be reduced or, potentially, absent altogether in a zero-correlation situation.

Although the strength of the correlation between words and colors may also be viewed as a measure of stimulus informativeness, this measure is actually somewhat different from Schmidt's (2019) notion of stimulus informativeness. On the one hand, "informative" situations (in Schmidt's sense of the word) where word-response contingencies exist for most or all words in a list inevitably entail a large color-word correlation. On the other hand, "uninformative" situations (also in Schmidt's sense of the word) where word-response contingencies exist for few or no words in a list may still entail a sizable color-word correlation. The reason is that the presence of contingencies is not required for a large color-word correlation to arise: A situation can be created where no word in the list is associated with one color response in particular, yet words and colors are correlated because words appear in some of the colors used but not in others. For example, if the word RED appears in the colors red and blue equally often in a list, but never in green and yellow (the other two colors used in the experiment), no contingency would exist for RED but the situation would still be one where the relationship between words and colors is quite strong.

The strength of the relationship between words and colors can be expressed as a chi-squared based correlation (C), which takes on positive values when the conditional probability of congruent stimuli is relatively large, and negative values when the conditional probability of incongruent stimuli is relatively large (a value of zero would correspond to no correlation; Melara & Algom, 2003). C is typically different from zero in most Stroop experiments in the literature, including experiments using a 50:50 congruent/incongruent ratio. For example, C is higher than zero in the popular 4 × 4 design in which four words and four colors are combined to form 16 stimuli (four congruent and 12 incongruent). The reason is that, in order for congruent stimuli and incongruent stimuli to be equally frequent in this design, each of the four congruent stimuli must be repeated three times as often as each of the 12 incongruent stimuli. As a result, a (positive) color-word correlation is introduced ($C = .5$). This correlation would encourage attention to the word dimension and thus increase (or, potentially, create) the congruency effect obtained in this situation.

The potential impact of having a high C value (in absolute terms) would be quite pronounced for list-wide PC manipulations. In particular, the nature of an MC list is such that words inevitably tend to be correlated with their congruent colors. For example, in a 4 × 4 design, an MC list with 75% congruent items would involve presentation of each of the four congruent stimuli nine times more often than each of the 12 incongruent stimuli. As a result, a large (positive) color-word correlation would be introduced (e.g., $C = .76$). This large correlation, rather than the fact that conflict is infrequent in the list, would greatly encourage attention to the word dimension, and thus, potentially lead to a large congruency effect. In contrast, a color-word correlation is not inevitable in an MI list. For example, in a 4 × 4 design, in an MI list with 25% congruent items, each of the four congruent stimuli would be presented exactly as often as each of the 12 incongruent stimuli. The color-word correlation would be zero in this case ($C = 0$). The absence of a correlation, rather than the fact that conflict is frequent in the list, would discourage attention to the word dimension and thus, produce a small congruency effect. In sum, when a high-correlation MC list and a zero-correlation MI list are being compared, the PC effect that is typically observed could result from attention being modulated by sensitivity to correlations rather than to conflict frequency.

This account would be capable of explaining the data pattern in the majority of list-wide PC manipulations in the literature (Algom & Chajut, 2019; Melara & Algom, 2003). However, in more recently reported experiments which involve both context items for which the congruency proportion is manipulated and transfer items for which the congruency proportion is not manipulated (Blais & Bunge, 2010; Bugg, 2014; Bugg et al., 2008), the situation is somewhat different. Because distinct sets of words and colors are used for transfer and context items (with words in the context set never appearing in colors in the transfer set, and vice versa), these transfer paradigms inevitably introduce large color-word correlations in the list. Therefore, unlike in traditional PC manipulations such as the one discussed in the previous paragraph, large correlations are introduced in *both* MC and MI lists (not only in the MC list) in this sort of experiment.

From the perspective of the input-driven account of the Stroop effect proposed by Algom and Chajut (2019), these large color-word correlations in transfer paradigms imply that the overall congruency effect obtained in these situations could be largely (or entirely) produced by the fact that the large color-word correlation encourages attention to the word dimension. Note, however, that because PC manipulations are typically constructed in order to provide a means of examining the control account (Botvinick et al., 2001), the issue is not how interference is produced but rather whether conflict frequency affords control over the interference. In other words, the result of interest is not the overall congruency effect but the relative magnitude of this effect in MC versus MI lists, particularly for the transfer items, items that are identical in the two lists. Thus, the question of interest is whether a PC effect would be observed for the transfer items in a situation in which the color-word correlation, albeit high, has similar strength in the MC and MI lists (i.e., if the absolute value of C, $|C|$, is the same for the two lists). This result could only be explained by conflict-induced control but not by sensitivity to correlations.[4]

However, a PC effect on the transfer items has never been obtained when the strength of the color-word correlation was perfectly balanced across the two lists (Blais & Bunge, 2010; Bugg, 2014; Bugg et al., 2008). The situations examined by Bugg (2014) are especially informative in this regard. As Bugg (2014)

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[4] In Algom and collaborators' input-driven account, the color-word correlation is not the only factor assumed to have a role in the Stroop task. For example, the relative salience of task-relevant and task-irrelevant information is also an important determinant of the magnitude and direction of Stroop effects (Melara & Algom, 2003). Overall, these factors provide a compelling explanation of the circumstances under which congruency effects in Stroop-like tasks would be observed. However, they do not necessarily explain why the magnitude of congruency effects typically differs for MC versus MI conditions, particularly when those conditions are matched on the model-relevant factors (e.g., in situations in which the relative salience of words and colors is typically the same for stimuli in MC vs. MI conditions).

noted, in the situation in which she obtained a PC effect on the transfer items (the situation in which contingencies could not be learned for the context words in the MI list), words and colors were always more strongly associated in the MC list than in the MI list (|C| was higher for the MC list than for the MI list, as is typical in traditional PC manipulations). In contrast, in the situation in which Bugg failed to obtain a PC effect on the transfer items (the situation in which contingencies could be learned for the context words in the MI list), the strength of the relationship between words and colors in the two lists was the same (|C| for the MC list was the same as that for the MI list). As such, similar to Schmidt's (2019) stimulus-informativeness argument, the hypothesis could be entertained that the real reason that Bugg obtained a PC effect in the former situation has to do with the fact that the two lists in that situation differed in the strength of the color-word correlation (whereas they did not in the situation in which no PC effect on the transfer items was obtained). Specifically, the stronger color-word correlation in the MC list would have drawn attention to the word dimension to a larger extent than the (weaker) correlation in the MI list. As a result, a larger congruency effect would have been obtained in the MC list than in the MI list even if no process of adaptation to list-wide conflict frequency was in place.

In discussing her results, Bugg (2014) considered such an explanation implausible because (a) in all of the situations she examined, the color-word correlation was high ($|C| > .76$), as noted, and (b) because in the situations in which the strength of the color-word correlations for the MC list and the MI list did differ, it did not differ greatly (the difference between |C| for the MC list and |C| for the MI list was less than .1). Thus, the claim that differences in the strength of color-word correlations between MC lists and MI lists determined the nature of the PC effect obtained in those situations would have to be based on the assumption that participants are sensitive to very small differences in |C| in the presence of overall high |C| values, an idea that, according to Bugg, appeared unlikely. Even so, the fact remains that Bugg obtained a PC effect on the transfer items when list-wide conflict frequency and strength of color-word correlations were confounded, but did not obtain one when the strength of those correlations was matched across the MC and MI lists. Further, some evidence exists suggesting that individuals are sensitive even to modest differences in correlations (Kareev, 2000). Therefore, in spite of Bugg's (2014) claim that a PC effect on transfer items was produced by adaptation to list-wide conflict frequency, alternative accounts related to the nature of the stimuli used in the experiment (e.g., either Schmidt's (2019) stimulus-informativeness account or Algom and Chajut's (2019) account based on the strength of the color-word correlation) could potentially explain that effect.

The Present Research

To address the issues, described above, that hinder the interpretation of Bugg's (2014) results, in the present research we further modified the paradigm developed by Bugg so that a modulation of Stroop interference for transfer items as a result of list-wide conflict frequency could not be explained by either (a) a reactive control process of adaptation to item-specific conflict frequency or nonconflict processes of contingency learning, or (b) adaptation to stimulus informativeness, or (c) adaptation to the strength of color-word correlations. Hence, the only remaining explanation would be proactive control.

We achieved this goal by using a proportion-neutral (PN), rather than a proportion-congruent, manipulation, that is, a paradigm in which the proportion of incongruent and neutral items (e.g., XXX; congruent items were not used in the present experiment) in the context set is manipulated to create mostly neutral (MN) and mostly incongruent (MI) lists. We then evaluated whether interference effects (the color-naming difference between incongruent and neutral items) on transfer items would be affected by the PN manipulation.

Note that, from the perspective of Botvinick, Braver, Barch, Carter, and Cohen's (2001) model, this change should be uninfluential, as what is critical for proactive control engagement is the frequency of conflict elicited by incongruent items, items that are more frequent in the MI list than in the MN list. Note also that Tzelgov, Henik, and Berger (1992) have already demonstrated that, similar to the PC effect obtained in the typical PC paradigm, increasing the proportion of neutral items in a list leads to an increase in interference, but not an increase in facilitation (i.e., the latency difference between neutral and congruent items), a pattern Botvinick et al. (2001) were able to simulate in their model.[5]

The reason neutral items are especially useful in the present circumstances is that negating any impact of individuals' sensitivity to stimulus informativeness (Schmidt, 2019) is impossible when the proportion of *congruent* and incongruent items is manipulated (i.e., in the standard PC paradigm), because, as noted, the MC list (but not necessarily the MI list) inevitably contains informative words, that is, words for which contingencies can be learned. Therefore, contingencies can always be learned for context MC words in a design like Bugg's (2014). In contrast, neutral

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[5] Specifically, Tzelgov et al. (1992) maintained a 1:1 ratio between congruent and incongruent items while parametrically manipulating the proportion of color words (i.e., the words creating the congruent and incongruent items) and neutral words (either noncolor words or consonant strings) in the list. In the four lists they used, color words represented 25%, 50%, 75%, and 100% of the stimuli, respectively, with the remaining stimuli being neutral words (in the 100% list, there were no neutral items). The congruency effect (incongruent−congruent) showed a monotonic decrease from the 25% to the 100% color-word list. Further, the lists including the neutral items (25%, 50%, and 75%) revealed that this decrease was entirely driven by interference (incongruent−neutral) being reduced as the proportion of color words increased, whereas facilitation (neutral−congruent) remained constant. These results, which have received scarce attention in the recent debate about the existence of conflict-adaptation processes, are not easily explained by nonconflict processes such as contingency learning and stimulus informativeness. However, it must be noted that Tzelgov et al. (1992) had no control in their experiment on the impact of reactive control processes. For example, all of the colors they used appeared relatively frequently in incongruent items in the 100% color-word list (i.e., 50% of the time) whereas all of the colors appeared relatively infrequently in incongruent items in the 25% list (i.e., 12.5% of the time). As explained below, such a situation could have induced participants to engage in a reactive control process of adaptation to color-specific conflict frequency (i.e., focus attention for frequently-conflicting colors vs. relax attention for infrequently-conflicting colors) in addition to a proactive control process of adaptation to list-wide conflict frequency (i.e., focus attention in frequently-conflicting lists vs. relax attention in infrequently-conflicting lists). Thus, it is not clear what type of control (reactive vs. proactive) produced the pattern of results Tzelgov et al. (1992) obtained. As explained below, the present research represents an improvement on this aspect of Tzelgov et al.'s (1992) procedure as it allowed us to dissociate reactive and proactive forms of control.

items allowed a manipulation of list-wide conflict frequency in a situation in which contingencies cannot be learned in either the MN list (i.e., the list in which conflict is infrequent, as is also the case in the MC list in the standard paradigm) or the MI list. In this situation, the words appearing in those lists are equally uninformative (in the sense conveyed by Schmidt's (2019) definition of informativeness), thus eliminating the stimulus-informativeness confound present in Bugg's (2014) experiments.

In addition, in our experiment, we were able to equate the absolute strength of the color-word correlation in the MN list and the MI list. Note that, because congruent items were not used and, for the reasons described below, the stimuli were divided into two nonoverlapping sets (leaving most possible color-word combinations unused), this correlation was inevitably strong (as it was in Bugg's, 2014 experiments). Specifically, participants could learn that each word appeared in a specific set of colors (even though the colors that the word appeared in were equally probable) but not in other colors. Because words could be used to anticipate the colors they would appear in, the input-driven account (Algom & Chajut, 2019; Dishon-Berkovits & Algom, 2000; Melara & Algom, 2003; Sabri et al., 2001) suggests that this situation might have induced participants in both lists to attend to the task-irrelevant dimension considerably more than they would have done in a zero-correlation situation, that is, a situation in which colors and words are paired randomly. As a result of receiving attention, incongruent words would elicit much more interference in this type of situation than in the zero-correlation situation. Therefore, from the perspective of the input-driven account, the basic Stroop interference effect in the present experiment could have been inflated or created altogether because of the strong word–color correlation that we introduced.[6] What was crucial for present purposes, however, was that this correlation had the same strength in the MN list and the MI list (i.e., that C had the same absolute value in the two lists). The reason for doing so was that what we were interested in was the modulation of interference based on list-wide conflict frequency, not the interference effect itself. Because words and colors had an equivalent strength of association in the MN list and the MI list, attention to the word dimension induced by this correlation could not explain any difference in the magnitude of interference across the two lists.

Note that while removing contingencies from the design can solve the problems of stimulus informativeness and of color-word correlation strength, it does not prevent use of a different type of reactive process, that is, adaptation to color-specific conflict frequency. Bugg and Hutchison (2013) showed that in addition to learning associations between words and conflict frequency, individuals can also learn associations between *colors* and conflict frequency, as demonstrated by the fact that MC colors (e.g., the color red appearing often with the word RED) elicit larger congruency effects than MI colors (e.g., the color green appearing often with incongruent words). To prevent this potential color-specific effect in the transfer items, the colors used for the transfer items did not overlap with those used for the context items. That is, although context items appeared only in MN colors in the MN list and only in MI colors in the MI list, transfer items appeared in a different set of colors and those colors appeared on neutral versus incongruent transfer trials equally often in the two lists. Thus, while a PN effect (i.e., larger interference in the MN list than in the MI list) on the context items would be compatible with either proactive adaptation to list-wide conflict frequency or reactive adaptation to color-specific conflict frequency, a PN effect on the transfer items would only be compatible with the former process (adaptation to list-wide conflict frequency).

For both context and transfer items, we anticipated that a potential PN effect would be mainly driven by the incongruent items. That is, we expected slower latencies to incongruent items in the MN list than in the MI list, but approximately equivalent latencies to neutral items in the two lists. The reason for this expectation is that, from a control perspective, focused attention to task-relevant information in the MI list (compared with relaxed attention in the MN list) should benefit responding to items that produce interference (i.e., the incongruent items) but not to items that produce little or no interference (e.g., the neutral items).

Consistent with this idea, previous research suggests that latencies to incongruent items are easily affected by the congruency proportion of the list (e.g., Lowe & Mitterer, 1982; Tzelgov et al., 1992). The situation for neutral items appears more complex (e.g., Bugg, McDaniel, Scullin, & Braver, 2011; Kinoshita, Mills, & Norris, 2018; Lowe & Mitterer, 1982; Tzelgov et al., 1992). In some cases, latencies to neutral items are faster in lists where these items represent the majority versus the minority of the trials (Kinoshita et al., 2018, Experiments 2–4). In other cases, MN lists actually produce equivalent or even slower latencies to neutral items compared with lists where these items are less frequent (Kinoshita et al., 2018, Experiment 1; Lowe & Mitterer, 1982; Tzelgov et al., 1992). More relevant to the present research, Bugg, McDaniel, Scullin, and Braver (2011) found that latencies to a fixed set of color-unrelated neutral words (e.g., the word RABBIT) were equivalent when those words appeared in an MN list versus an MI list, the type of contrast examined in the present research (however, latencies to those words were longer in a third, MC list, a finding we discuss in the General Discussion). Based on these results, we expected our PN manipulation to have potentially an effect on the incongruent items but little or no effect on the neutral items.

Method

Participants

Eighty students at the University of Western Ontario (age 17–29 years) participated for course credit or $10. We did not conduct a

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[6] A reviewer of an earlier version of this article contended that "created," rather than "inflated," would be the most accurate description of what our large color-word correlation did to the interference effect in our experiment. However, the idea that a color-word correlation could cause Stroop effects by itself implies that those effects would disappear completely when the color-word correlation is zero. This pattern is not the one typically reported, however, at least in the color-word Stroop task (for a different type of task in which the effect does go away in the presence of a zero correlation, see Dishon-Berkovits & Algom, 2000). For example, Sabri et al. (2001) found sizeable congruency effects in many situations in which a zero color-word correlation was used, especially when an oral response to the color was required. What this result suggests is that the color-word correlation is unlikely to be the only determinant of Stroop effects in the classic color-word Stroop task (although other input-driven factors, e.g., the relative salience of words and colors, may contribute to determining those effects; see Footnote 4). Thus, we believe that it is more accurate to say that the large color-word correlation we introduced might have "inflated," not "created," the overall interference effect in our experiment.

power analysis to determine this sample size. Instead, we determined the sample size based on a pilot experiment conducted in our lab examining a PN effect. Because the PN effect in that experiment could have been due to either or both of two processes (adaptation to list-wide conflict frequency and/or adaptation to color-specific conflict frequency), we decided to double the sample size tested in that experiment ($N = 40$) for the present experiment, an experiment in which the process of adaptation to list-wide conflict frequency was isolated (for transfer items). All participants were native English speakers and had normal or corrected-to-normal vision.

Materials

Six color names (RED, YELLOW, BLACK, BLUE, GREEN, WHITE) and six neutral "words" of matching lengths (XXX, ZZZZZZ, KKKKK, QQQQ, JJJJJ, HHHHH) were used as distractors, and six colors (red [R: 255; G: 0; B: 0], yellow [R: 255; G: 255; B: 0], black [R: 0; G: 0; B: 0], blue [R: 0; G: 112; B: 192], green [R: 0; G: 176; B: 80], and white [R: 255; G: 255; B: 255], corresponding to "red," "yellow," "black," "blue," "green," and "white" in the standard DMDX palette) were used as targets. For the neutral words, we used consonant strings instead of color-unrelated words because it is known that any readable stimulus can create some degree of interference in the Stroop task (e.g., Dalrymple-Alford, 1972; Klein, 1964). As a result, color-unrelated words may not create the strongest contrast with incongruent words in terms of interference. For example, in Bugg et al.'s (2011) study in which color-unrelated words were used as neutral items, Stroop interference (incongruent−neutral) was, if anything, smaller than Stroop facilitation (neutral−congruent). In contrast, much larger Stroop interference than facilitation is routinely observed when neutral items are consonant strings (MacLeod, 1991). Thus, we reasoned that manipulating the frequency of the conflict elicited by incongruent items may be more effective in a situation in which the incongruent items are compared to neutral items, items that produce little, if any, conflict (i.e., consonant strings), than when the incongruent items are compared to neutral items that produce some conflict (i.e., color-unrelated words).[7]

The frequency of word–color combinations is represented in Tables 1 and 2 for the MN and the MI list, respectively. The "words" were divided into two sets; one set (RED, YELLOW, BLACK, XXX, ZZZZZZ, KKKKK) appeared only in red, yellow, and black whereas the other set (BLUE, GREEN, WHITE, QQQQ, JJJJJ, HHHHH) appeared only in blue, green, and white. Each word in each set appeared equally often in two of the three colors (for the color words, neither of these colors was the congruent one). One set (e.g., the words appearing in red, yellow, and black) served as the context set and the other set (e.g., the words appearing in blue, green, and white) served as the transfer set. In the MN list, the colors in the context set appeared 84 times with a neutral word and 12 times with an incongruent word (the color-specific proportion of neutral items [PNI] was thus 87.5%). The reverse mapping was used in the MI list (such that color-specific PNI = 12.5%).

The colors in the transfer set appeared 48 times with a neutral word and 48 times with an incongruent word (color-specific PNI = 50%) in both lists. In both lists, the context set and the transfer set were randomly intermixed. Overall, there were 132 neutral items and 60 incongruent items in the MN list (list-wide PNI = 68.75%), with those numbers reversing in the MI list (list-wide PNI = 31.25%), for a total of 192 items in each list. The assignment of the two sets to context and transfer items was counterbalanced across participants, as was the order with which the MN and MI lists were presented. Finally, for each list, we calculated the contingency coefficient measuring the strength of the color-word correlation, C, using Melara and Algom's (2003) formula (with the exception that C was allowed to take on positive values when the conditional probability of neutral, rather than congruent, stimuli was relatively large, i.e., in the MN list). C was .82 for the MN list and −.82 for the MI list (the absolute strength of color-word correlations was thus the same in the two lists).

Procedure

Each trial began with a fixation symbol ("+") displayed for 250 ms in the center of the screen followed by a colored word displayed for 2,000 ms or until the participant's response, which was recorded with a microphone connected to the testing computer. Participants were instructed to name the color of the word as quickly and as accurately as possible while ignoring the word. Stimuli were presented in uppercase Courier New font, point 14, against a medium gray background (R: 169; G: 169; B: 169). No feedback was provided. There was a self-paced pause between the two lists (on average, participants took a pause of about 33 s). No information about the nature of the subsequent list was provided during this pause. The order of trials within each list was randomized. Transfer and context items were intermixed within each list and presented to the participants with no obvious differentiation between the two types of items. Initially, participants performed a practice session including six neutral and six incongruent trials. The experiment was run using DMDX (Forster & Forster, 2003) software. This research was approved by the Research Ethics Board of the University of Western Ontario (protocol # 108956).

Results

The waveforms of responses were manually inspected with CheckVocal (Protopapas, 2007) to determine the accuracy of the response and the correct placement of timing marks. Prior to the analyses, invalid trials due to technical failures and responses faster than 300 ms or slower than the time limit (accounting for 2.0% of the data points) were discarded. A 2 (item type: neutral vs. incongruent) × 2 (list type: mostly neutral vs. mostly incongruent) ANOVA was conducted on both latencies and errors for context items and transfer items separately, paralleling the analyses in previous research using this paradigm (Bugg, 2014; Bugg et al.,

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[7] We do not think that using color-unrelated words instead of consonant strings as neutral words would have dramatically changed the type of processes participants would have used in dealing with the task. However, we do think that detecting an effect of adaptation to list-wide conflict frequency is potentially harder in a situation in which color-unrelated words are used as neutral words. The reason is that, even in an MN list, participants may sometimes feel a need to focus attention to the color dimension to avoid inadvertently reading the frequent color-unrelated words, making the process used in that type of list not particularly different from the process used in an MI list. As a result, detecting a PN effect in that situation may require a much more sensitive protocol (e.g., larger sample size, stronger PN manipulation) than the one used here.

Table 1
Template for the Frequency of Color-Word Combinations in the MN List

		Word											
Set	Color	RED	YELLOW	BLACK	BLUE	GREEN	WHITE	XXX	ZZZZZZ	HHHHH	QQQQ	JJJJJ	KKKKK
Context	Red		2	2					14	14			
	Yellow	2		2				14		14			
	Black	2	2					14	14				
Transfer	Blue					8	8					8	8
	Green				8		8				8		8
	White				8	8					8	8	

Note. MN = mostly neutral.

2008). The mean reaction time (RT) and error rates are presented in Table 3. The raw data and SPSS script used for the analyses are publicly available at https://osf.io/yk57z/.

Context Items

A main effect of item type was found in both latencies, $F(1, 79) = 211.79$, $MSE = 4{,}771$, $p < .001$, $\eta_p^2 = .728$, and error rates, $F(1, 79) = 15.60$, $MSE = .002$, $p < .001$, $\eta_p^2 = .165$, indicating faster and more accurate performance on neutral than on incongruent items. An interaction between item type and list type also emerged in the latencies, $F(1, 79) = 28.12$, $MSE = 2{,}209$, $p < .001$, $\eta_p^2 = .263$, and in the error rates, $F(1, 79) = 11.58$, $MSE = .002$, $p = .001$, $\eta_p^2 = .128$, reflecting larger interference in the MN list (latencies: 140 ms; error rates: 3.5%) than in the MI list (latencies: 85 ms; error rates: −.3%). This interaction was driven by both the fact that incongruent items were faster, $t(79) = 3.75$, $p < .001$, and more accurate, $t(79) = 2.76$, $p = .007$, in the MI list than in the MN list and the fact that neutral items were faster, $t(79) = -2.49$, $p = .015$, and more accurate, $t(79) = -2.28$, $p = .025$, in the MN list than in the MI list.

Transfer Items

For transfer items, we also found a main effect of item type in both latencies $F(1, 79) = 277.68$, $MSE = 2{,}444$, $p < .001$, $\eta_p^2 = .779$, and error rates, $F(1, 79) = 37.19$, $MSE = .001$, $p < .001$, $\eta_p^2 = .320$, with faster and more accurate performance on neutral than on incongruent items. In the latencies, this main effect was qualified by an interaction with list type, $F(1, 79) = 14.08$, $MSE = 759$, $p < .001$, $\eta_p^2 = .151$, indicating a larger interference effect in the MN list (104 ms) than in the MI list (80 ms; no interaction was found in the error rates, $F < 1$). This interaction in the latencies was again driven by both the fact that incongruent items were faster, $t(79) = 2.02$, $p = .047$, in the MI list than in the MN list and the fact that neutral items were faster, $t(79) = -2.14$, $p = .035$, in the MN list than in the MI list.

Discussion

The Proportion-Neutral Effect: Proactive Adaptation to List-Wide Conflict Frequency

A popular control-based explanation for the PC effect in the Stroop task (i.e., the finding that the congruency effect increases as the proportion of congruent items in the list increases) assumes that attention to task-relevant information is proactively (i.e., before the appearance of any specific item) increased in a situation in which conflict is frequent (e.g., an MI list) compared with a situation in which conflict is infrequent (e.g., an MC list; De Pisapia & Braver, 2006; Kane & Engle, 2003). However, multiple alternative explanations have been advanced recently that could explain that effect without invoking this form of proactive control (Schmidt, 2013a, 2019). By replacing congruent items with neutral items in the PC paradigm, we created a situation in which performance in a list in which conflict was infrequent could be compared with that in a list in which conflict was frequent while controlling for information other than list-wide conflict frequency, information that individuals might use to modulate word interference.

A PN effect, similar to the PC effect, emerged, with more interference in the MN list (the list in which conflict was infrequent) than in the MI list (the list in which conflict was frequent), for both context and transfer items. For context items, interference

Table 2
Template for the Frequency of Color-Word Combinations in the MI List

		Word											
Set	Color	RED	YELLOW	BLACK	BLUE	GREEN	WHITE	XXX	ZZZZZZ	HHHHH	QQQQ	JJJJJ	KKKKK
Context	Red		14	14					2	2			
	Yellow	14		14				2		2			
	Black	14	14					2	2				
Transfer	Blue					8	8					8	8
	Green				8		8				8		8
	White				8	8					8	8	

Note. MI = mostly incongruent.

Table 3
Mean RTs and Percentage Error Rates (and Corresponding Standard Errors) for Context and Transfer Items

Item type	RTs MN list	RTs MI list	Error rates MN list	Error rates MI list
Context items				
Neutral	724 (13)	742 (12)	1.4 (.2)	2.5 (.5)
Incongruent	864 (17)	827 (15)	4.9 (.9)	2.8 (.3)
Interference effect	140	85	3.5	.3
Transfer items				
Neutral	732 (13)	744 (13)	1.2 (.2)	1.2 (.3)
Incongruent	836 (15)	824 (14)	3.7 (.5)	3.2 (.4)
Interference effect	104	80	2.5	2.0

Note. RT = reaction time; MN = mostly neutral; MI list = mostly incongruent.

could have been modulated based on either list-wide conflict frequency information or color-specific conflict frequency information. Such is not the case for transfer items, items for which the only viable mechanism for producing this effect would appear to be a proactive mechanism of adaptation to list-wide conflict frequency, as conceived of by, for example, De Pisapia and Braver (2006; see also Kane & Engle, 2003; Gonthier et al., 2016). That is, this effect was obtained in a situation in which, similar to that examined by Bugg (2014), no item-specific conflict frequency information and no word-response contingencies existed that could have produced it (see Spinelli, Perry, & Lupker, 2019, for a PC effect obtained in a similar situation in the picture–word interference task). In addition, unlike the crucial situations examined by Bugg (2014), the present situation was one in which no difference existed in the extent to which words were informative in the two lists and the strength with which the words were correlated with the colors, differences that, in principle, could also lead to participants adjusting attention in a manner compatible with a PC effect (Bugg, 2014; Melara & Algom, 2003; Schmidt, 2019). In sum, the present results would seem to make a good case for the idea that attention can be proactively modulated based on the frequency of conflict in a list. In the following, however, we consider a few alternative explanations that can be offered for the present results.

An Alternative Explanation: Temporal Learning

Although we obtained a PN effect in the absence of item-specific, contingency-learning, and stimulus-informativeness confounds, additional confounds could have existed in our experiment that might explain that effect without invoking a process of adaptation to conflict frequency. One of these potential confounds is that temporal expectancies for the emission of a response are inevitably slower in a list in which most trials elicit a slow response (e.g., an MI list) than in a list in which most trials elicit a fast response (e.g., an MN list). According to Schmidt's (2013b) temporal learning account, a faster temporal expectancy would cause the difference between easy-to-process stimuli (e.g., neutral items) and hard-to-process stimuli (e.g., incongruent items) to increase because easy stimuli, but not hard stimuli, will speed up because they can be processed fast enough to meet that (fast) temporal expectancy. In the case of an MN list (i.e., a situation in which the temporal expectancy is relatively fast), the result would be a large interference effect. Conversely, within Schmidt's (2013b) framework, a slower temporal expectancy would cause the difference between easy and hard stimuli to decrease because hard stimuli may also speed up because they can be processed fast enough to meet the (slow) temporal expectancy in that situation (although hard stimuli appear to be relatively insensitive to temporal expectancies, at least in some situations: Kinoshita & Mozer, 2006). As a result, an MI list in which the temporal expectancy is relatively slow would produce, if anything, a reduced interference effect. In sum, a PN effect (as well as a PC effect in the standard PC paradigm) could be produced by a temporal-learning process rather than a process of adaptation to conflict frequency.

At present, however, there is little convincing evidence in support of a temporal-learning explanation of PC effects. To demonstrate that temporal expectancies could explain PC effects obtained in confound-minimized situations, Schmidt (2013b) reanalyzed the data from one of those situations (Hutchison, 2011) using linear mixed-effects modeling, a type of analysis that, unlike traditional mean-based ANOVAs, allows the evaluation of trial-level predictors. Indeed, in his reanalysis, Schmidt (2013b) included a trial-level predictor functioning as an index of temporal expectancy, the latency on the most recent trial (i.e., RT on trial $n - 1$), in addition to the typical predictors in a PC paradigm (i.e., list type and congruency). Schmidt (2013b) reasoned that, because easy stimuli are more likely to benefit from fast temporal expectancies (i.e., following a fast RT) whereas hard stimuli are more likely to benefit, if anything, from slower temporal expectancies (i.e., following a slow RT), evidence for a temporal-learning process being engaged in the Stroop task should take the form of an interaction between temporal expectancy (RT on trial $n - 1$) and the congruency of the stimulus on trial n. Specifically, the congruency effect on a given trial should be larger following faster responses than following slower responses, an effect that he did obtain. Because faster responses are, by necessity, more common in an MC list than in an MI list, the implication is that this temporal-learning interaction would tend to inflate the congruency effect in the MC list and reduce it in the MI list, resulting in a PC effect.

More recently, however, Cohen-Shikora, Suh, and Bugg (2019) clearly demonstrated that Schmidt's (2013b) results were likely biased because of the nonlinear transformation he applied to the RT data. While transformations of this sort do a decent job of accommodating the assumption made by linear mixed-effects models that the dependent variable be normally distributed (an assumption that raw RTs typically fail to satisfy), they have the downside of systematically altering the pattern and size of interaction terms, making analyses of interactions unreliable overall (Balota, Aschenbrenner, & Yap, 2013). Indeed, Cohen-Shikora et al. (2019) reanalyzed a number of data sets (including Hutchison's, 2011) and were unable to replicate Schmidt's (2013b) temporal-learning interaction when untransformed, rather than transformed, RT data were used in a type of mixed-effects model that tolerates deviations from normality in the dependent variable (a generalized linear mixed-effects model: Lo & Andrews, 2015; see also Spinelli et al., 2019). Several additional attempts to evaluate the impact of temporal learning by Cohen-Shikora et al. (2019) also yielded no convincing evidence that temporal learning contributes to the PC effect to any extent.

In sum, although there was no control in the present experiment for a potential temporal-learning confound (a faster temporal expectancy in the MN list than in the MI list), the extant evidence suggests that this confound does not pose a serious challenge to control-based interpretations of PC/PN effects (although see Schmidt, 2017). In fact, when we reanalyzed the (raw) RT data of the present experiment using RT on trial $n - 1$ as an additional predictor in a generalized linear mixed-effects model, we found no evidence for the temporal-learning interaction which, according to Schmidt (2013b), would support a temporal learning interpretation of the PN effect that we obtained. On the contrary, we found a *reversed* temporal-learning interaction on both context and transfer items, with congruency effects increasing, rather than decreasing, following slower responses on the preceding trial.[8] This reversed pattern, which was occasionally reported in the analyses conducted by Cohen-Shikora et al. (2019; see also Spinelli et al., 2019), is completely inconsistent with Schmidt's (2013b) temporal learning account and makes a strong case that the PN effects we obtained did not emerge from the temporal-learning process Schmidt (2013b) hypothesized.

Another Alternative Explanation: Experience and Practice

We noted above (see The Present Research section) that we expected a potential PN effect on both context and transfer items to be mainly driven by incongruent items rather than neutral items. That is, we anticipated that because attention to task-relevant information should be more focused in the MI list than in the MN list, participants would have an easier time dealing with the interference incongruent items produce, leading to shorter latencies to those items in the MI list. On the other hand, we saw no reason why neutral items should be influenced by whether attention is focused (in the MI list) versus relaxed (in the MN list). Therefore, we expected latencies to those items to be equivalent in the two lists.

The results, however, indicate that the PN effects we obtained were driven by both types of items. As we anticipated, incongruent items were faster and (for context items) more accurate in the MI list than in the MN list. Contrary to our expectations, however, neutral items were also faster and (for context items) more accurate in the MN list than in the MI list.

A reviewer on an earlier version of this article pointed out that, in addition to temporal learning, another potential confound existed in our experiment that could explain this pattern of results. This confound refers to the unequal amount of experience and practice afforded by neutral versus incongruent items in the two lists. For example, in the version of the experiment represented in Tables 1 and 2, the word RED (an incongruent context word) was presented much more often in the MI list than in the MN list. Similarly, the "word" XXX (a neutral context word) was presented much more often in the MN list than in the MI list. As a result, it is possible that participants would (a) learn to ignore those words to a better extent in the lists where they were experienced more frequently, and/or (b) benefit more from practice with those words in the lists where they were practiced more frequently. That is, both experience and practice would speed up responses to frequent context words in our experiment.

One apparent problem with this explanation is the fact, noted above (see The Present Research section), that at least in the context of Stroop experiments of normal length, experiencing/practicing a stimulus does not always result in a benefit for that stimulus, particularly when the stimulus is neutral (e.g., Bugg et al., 2011; Kinoshita et al., 2018, Experiment 1; Lowe & Mitterer, 1982; Tzelgov et al., 1992; but see MacLeod, 1998, for robust practice effects for neutral items in experiments spanning several days). Nevertheless, it is reasonable to assume that any stimulus, including neutral stimuli, may produce faster color naming latencies in situations where that stimulus is frequent compared with situations in which it is infrequent. The implication is that, in our experiment, latencies to context words might have been influenced by the amount of experience/practice those words afforded in MN versus MI lists. Because this amount was unequal for two item types in the two lists (context neutral words were more frequent in the MN list whereas context incongruent words were more frequent in the MI list), this influence could have created a PN effect for those words without adaptation to conflict frequency being involved. In other words, the PN effect obtained for context items could reflect processes of adaptation to list-wide and color-specific conflict frequency (the processes we initially considered), and/or experience/practice processes. As such, it is not at all clear what the PN effect for the context items signifies.

The story is different for transfer items, however. Because those items were identical in the two lists, not only was color-specific conflict information the same for those items in the two lists; experience and practice were also the same. For example, in the version of the experiment represented in Tables 1 and 2, both the word BLUE (an incongruent transfer word) and the "word" QQQQ were presented 16 times each in the MN list and the MI list. Thus, participants in the two lists (a) were equally familiar with those words and thus presumably would have learned to ignore them to the same extent, and (b) practiced those words equally often. Therefore, processes related to experience and practice with a stimulus would appear unable to explain the PN effect we obtained for the transfer items.

That is not to say that a more complex experience/practice account could not be derived that could explain the results of our transfer items. According to this account, a frequently practiced stimulus (e.g., the context neutral "word" XXX in the MN list), compared with an infrequently practiced context stimulus (e.g., XXX in the MI list), would benefit performance for a second stimulus of the same *type* (e.g., the transfer neutral "word" QQQQ) which is actually equally practiced in the two situations. In other words, effects of experience and practice would hold not only for stimuli that are frequently experienced/practiced, but also for similar stimuli in the same list that are not experienced/practiced especially frequently. Thus, in our experiment, neutral and incongruent items in the transfer set would produce a PN effect because of the unequal amount of experience/practice participants acquired with neutral and incongruent items in the context set in the two lists. This explanation would be consistent with the fact that the PN effect for transfer items was driven by both incongruent items (which were 12 ms faster in the MI list than in the MN list) and neutral items (which were also 12 ms faster in the MN list than in the MI list). In contrast, this latter result (faster transfer neutral items in the MN list than in the MI list) is not one that would be

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[8] The procedure and statistical software used for this analysis were the same as that used in Spinelli et al. (2019). The R script used for the analysis is publicly available at https://osf.io/yk57z/.

expected from a control standpoint, because, as noted, whether attention to task-relevant information is focused (in the MI list) or relaxed (in the MN list) should have little impact on responses to items that produce no interference, a fact that makes this general experience/practice account more credible. Nonetheless, prior research would seem to present two rather challenging problems for such an account.

The first problem relates to the neutral items. Because these items elicit little or no interference, it is unclear why practice with a set of neutral items would benefit another set of neutral items to any measurable degree. Indeed, Bugg et al. (2011) did not report such a pattern of results in a manipulation similar to ours. As noted, in that study, Bugg et al. (2011) used a fixed set of color-unrelated words (the transfer set) which were intermixed in three different lists (MC, MI, and MN). That transfer set of neutral items corresponded to 15% of the items in each of the three lists. In the MC and MI lists, that set was intermixed with congruent and incongruent items, but not with other neutral items. In the MN list, that set was intermixed congruent and incongruent items but also with a larger set of (filler) neutral items in which different neutral words were used. Because this filler set represented 55% of the trials in the MN list, overall, neutral items represented 70% of the trials in that list. Thus, participants practiced neutral items much more often in the MN list compared with the other two lists even though the neutral *transfer* items were equally practiced in all lists. In this situation, an account that assumes a key role of experience/practice with the most frequent type of items in the list would predict shorter latencies for transfer items appearing in the MN list (the list where that type of item, i.e., color-unrelated words, was the most frequent) than for transfer items appearing in the other two lists (lists where that type of item was infrequent). As noted, however, there was no difference between the neutral items in the MN and MI lists.

On the other hand, neutral items did produce longer latencies in Bugg et al.'s (2011) MC list than in the other two lists. These results are consistent with Bugg et al.'s (2011) interpretation that inhibiting the automatic tendency for word reading is harder in a situation that favors word reading (i.e., in MC lists in which reading the word would often result in the correct response) than in situations that do not favor word reading (i.e., in MI and MN lists, lists in which reading the word would often result in an incorrect response). However, these results are not consistent with the idea that general experience and practice with a stimulus type would benefit processing of that type of stimulus.[9]

The second problem with an account that predicts that effects of experience and practice would transfer to stimuli of the same type is that such a prediction does not appear to apply more generally. That is, this prediction is contradicted by the fact that, in the literature on blocking effects, hard-to-name stimuli (e.g., nonwords) typically elicit slower, rather than faster, responses when presented in lists with other hard-to-name stimuli (and hence, stimuli of that type are experienced and practiced more) than when presented in lists in which other, easier to name stimuli (e.g., words) are more frequently presented (e.g., Lupker, Brown, & Colombo, 1997; Lupker, Kinoshita, Coltheart, & Taylor, 2003; Spinelli et al., 2019; Taylor & Lupker, 2001, Experiment 2). It is unclear how an account appealing to general processes of experience and practice would explain what would appear to be a reverse practice effect.

In fact, a different account has been proposed to explain both that result and the parallel result that easy-to-name stimuli elicit faster responses when presented in lists in which other easy-to-name stimuli are frequent than when presented in lists in which other easy-to-name stimuli are infrequent (a result that would be more consistent with the idea of practice). The time-criterion account (Lupker et al., 1997) proposes that participants in speeded tasks establish a time criterion for response emission (i.e., the point in time at which they expect, and will attempt, to respond) depending on the characteristics of the stimuli in the list. This criterion would be set at a faster point in lists where easy trials are frequent and slower point in lists where hard trials are more frequent. Thus, responses to the same stimulus would be slower in a hard context (e.g., a mostly hard list) than in an easier context (e.g., a mostly easy list). For example, Spinelli, Perry, and Lupker (2019, Experiment 2) showed that, in a simple picture naming task (with no word distractors), latencies were slower for both high-resolution pictures (easy stimuli) and low-resolution pictures (hard stimuli) in a list where most of the pictures were in low resolution than in a list where most of the pictures were in high resolution. Such a result is quite consistent with a time-criterion explanation while being inconsistent with a general experience/practice account.

Note that, in theory, it is likely that a time-criterion setting process is used, affecting overall latencies, in all speeded response tasks. In interference tasks, a later time criterion would be established in a mostly hard rather than in a mostly easy list, a result compatible with our finding that neutral transfer items were slower in the MN list (a list where most of the trials are relatively easy) than in the MI list (a list where most of the trials are relatively hard). What it would not explain, however, is why our transfer incongruent items were faster in the MI list than in the MN list. The reason is that establishing a late time criterion in the MI list would imply that responses should be slower, not faster, for incongruent items in that list. Thus, an explanation for why incongruent items in interference tasks such as the Stroop task are faster in MI versus MN lists would need to be based on there being an additional process at work (and potentially working against a time-criterion process, i.e., a process that would induce slower responses to incongruent items in MI lists). Proactive adaptation to the frequency of conflict in the list would be the most obvious process. The conflicting nature of incongruent stimuli in Stroop tasks would engage a control process that, by focusing attention to task-relevant information when conflict is frequent, would reduce latencies

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[9] One may ask whether the fact that Bugg et al. (2011) found no difference in the contrast between the MN list and the MI list is also inconsistent with our results in which a difference was observed. Although Bugg et al.'s (2011) results do suggest that MI and MN lists may be similar in that they do not encourage word reading (unlike MC lists), they would not allow the conclusion that those lists were dealt with using exactly the same process(es). Specifically, it is possible that MI and MN lists differed in the processes engaged to deal with Stroop interference (interference that is typically caused by incongruent color words: MacLeod, 1991), an aspect that Bugg et al. (2011) could not evaluate because their transfer items did not include incongruent items. Thus, although both MN and MI lists would not encourage word reading to the same extent that an MC list does, an MN list may lead individuals to relax their attention because words in that list rarely cause substantial interference with color naming. The same would not be true for an MI list because, in that list, dealing with frequent incongruent items would induce more focused attention to the color dimension. Thus, if such a mechanism of adaptation to list-wide conflict frequency were used, the latency difference between incongruent and neutral items should be reduced in an MI list compared to an MN list. In other words, a PN effect, similar to the PC effect in the standard PC paradigm, would have been expected if Bugg et al.'s (2011) transfer set had contained incongruent items.

to those stimuli even in situations that invite establishing a late time criterion (i.e., an MI list).

In sum, what we are proposing is that, on the one hand, simple effects of experience and practice with individual stimuli could explain the results of our context items. While more complex effects of experience and practice transferring to similar stimuli as those practiced would appear to explain the results of our transfer items as well, a proposal of that sort lacks support from the relevant literature. On the other hand, a combination of the time-criterion account (explaining the results for the transfer neutral items) and of a control account (explaining the results for the transfer incongruent items) appears to do a decent job of explaining the results for our transfer items.

That said, the fact that some researchers (Bugg et al., 2011; Kinoshita et al., 2018, Experiment 1; Lowe & Mitterer, 1982; Tzelgov et al., 1992) did not report faster latencies for neutral items in mostly easy contexts (e.g., MN and MC lists) than in mostly hard contexts (e.g., MI lists) is puzzling. One possibility is that those data simply represent random noise. Another possibility is that our manipulation picked up a pattern that would normally require a more sensitive protocol, for example, a comparison between a "pure" list with 100% neutral items and a "mixed" list in which 50% or more incongruent items are intermixed with neutral items. Such a strong manipulation of context is uncommon in the Stroop literature but is not at all uncommon in the literature on blocking effects (where, in fact, it is standard: e.g., Lupker et al., 2003). Future research adopting manipulations along these lines could shed light on how/whether processes of adjustment of a time criterion are operating in interference tasks.

Conclusion

Overall, the present results challenge the argument that adaptation to list-wide conflict frequency is not a process that humans use (Schmidt, 2013a, 2019). Note, however, that the evidence supporting the use of this process was obtained when learning contingencies between words and responses was not a viable option, the only type of situation in which, according to Bugg (2014), a proactive conflict-adaptation process is used. Therefore, although the present results do indicate that this process exists, they do not argue against the possibility that its usage might be restricted to the type of situation in which an alternative, contingency-learning process is not available.

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