

Revealing List-Level Control in the Stroop Task by Uncovering Its Benefits and a Cost

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Interference is reduced in mostly incongruent relative to mostly congruent lists. Classic accounts of this list-wide proportion congruence effect assume that list-level control processes strategically modulate word reading. Contemporary accounts posit that reliance on the word is modulated poststimulus onset by item-specific information (e.g., proportion congruency of the word). To adjudicate between these accounts, we used novel designs featuring neutral trials. In two experiments, we showed that the list-wide proportion congruence effect is accompanied by a change in neutral trial color-naming performance. Because neutral words have no item-specific bias, this pattern can be attributed to list-level control. Additionally, we showed that list-level attenuation of word reading led to a cost to performance on a secondary prospective memory task but only when that task required processing of the irrelevant, neutral word. These findings indicate that the list-wide proportion congruence effect at least partially reflects list-level control and challenge purely item-specific accounts of this effect.

Keywords: list-wide proportion congruence, item-specific proportion congruence, cognitive control, prospective memory

In the Stroop task participants name the ink color of color words (Stroop, 1935). Stroop interference refers to the decrement to performance on incongruent (e.g., RED in blue ink) relative to congruent trials (e.g., RED in red ink). One factor that is commonly manipulated in the Stroop task is list-wide proportion congruence, or the percentage of congruent relative to incongruent trials within a list (i.e., block) (e.g., Kane & Engle, 2003; Lindsay & Jacoby, 1994; Logan & Zbrodoff, 1979; Logan, Zbrodoff, & Williamson, 1984; Lowe & Mitterer, 1982; Shor, 1975; West & Baylis, 1998). Stroop interference is reduced when this percentage is low [i.e., a mostly incongruent (MI) list] as compared to when this percentage is high [i.e., a mostly congruent (MC) list]. This pattern is termed the list-wide proportion congruence effect, and the classic explanation for it centers on how control settings change depending on the predictability of certain trial types within a particular list. In MI lists, incongruent trials are expected and this expectation purportedly motivates participants to strategically attenuate word reading, which effectively minimizes interference (Lindsay & Jacoby, 1994; see also Botvinick, Braver, Barch, Carter, & Cohen, 2001, for the view that color processing is amplified). In contrast, in MC lists, where congruent trials are

expected, participants permit word processing because the words are facilitative on most trials. This leads to prolonged reaction times on the relatively infrequent incongruent trials and increased interference.

Of primary concern in the present study is an ongoing theoretical debate that questions the locus of the list-wide proportion congruence effect. The classic account, which we refer to as *list-level control*, assumes that control strategies are global and based on list-level expectancies (e.g., causing attention to be biased away from word processing even prior to stimulus onset in the MI list). However, recent studies have provided evidence against these assumptions (e.g., Blais, Robidoux, Risko, & Besner, 2007; Blais & Bunge, 2010; Bugg, Jacoby, & Toth, 2008). These studies instead implicate a stimulus-driven mechanism that responds on a transient or trial-by-trial basis based on expectancies pertaining to individual items (and is thus labeled *item-specific*).

In the color-word Stroop task, the term “item” is used to refer to a particular word, and not the color or a color-word combination (see Jacoby, Lindsay, & Hessels, 2003; Schmidt & Besner, 2008).¹ For instance, participants may learn to expect that the word

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¹ The following evidence supports the assumption that item refers to the word and not the color or word-color combination. First, the process-dissociation procedure yielded higher word reading estimates for mostly congruent as compared with mostly incongruent items, but equivalent color-naming estimates as a function of item-specific proportion congruence (Jacoby et al., 2003). As Jacoby et al. reasoned, if the color-word combination or the color alone functioned as the signal of item-specific proportion congruency (ISPC), then differences in the color-naming estimates should have been found. Jacoby et al. further reasoned that any stimulus–response associations acquired via exposure to the ISPC manipulation must be independent of the color. Indeed, the contingency account posits that the word signals the stimulus–response contingencies associated with a particular item and directs production of the response that is most frequently paired with that word (Schmidt & Besner, 2008).

GREEN will be mostly incongruent, and quickly dampen word processing after presentation of the word GREEN (cf. Bugg, Jacoby, & Chanani, 2010), or learn to predict the response (e.g., “blue”) that is most frequently associated with (contingent on) the word GREEN (Schmidt & Besner, 2008; Schmidt, Crump, Cheesman, & Besner, 2007). According to this contemporary view, then, the list-wide proportion congruence effect emerges from the operation of item-specific mechanisms that are implemented in response to the information signaled by a particular word, and not information signaled by the list.

To fully appreciate the origins of this theoretical debate and the challenge of disentangling the two accounts, it is important to consider the typical design used to examine the list-wide proportion congruence effect. Specifically, the lists are composed in such a fashion that list-wide proportion congruence is equivalent for each word in the stimulus set (e.g., GREEN, BLUE, and RED). In the MC condition, for example, 70% of the trials involving the words GREEN, BLUE, and RED appear in their congruent ink color and 30% appear approximately equally often in each of the incongruent ink colors. Critically, such designs perfectly confound list-wide proportion congruence with item-specific proportion congruence. Item-specific proportion congruence refers to the proportion congruence level associated with particular items (i.e., words; Jacoby et al., 2003). In the above example, GREEN, BLUE, and RED are MC items. As shown by Jacoby et al., in lists that are 50% congruent, Stroop interference is reduced for *items* that are mostly incongruent relative to *items* that are mostly congruent (i.e., the item-specific proportion congruence effect). In the typical design, because of the confound between list-wide and item-specific proportion congruence, one cannot adjudicate between an account of list-wide proportion congruence effects that posits implementation of a list-level control mechanism from one that posits that item-specific mechanisms are responsible.

Bugg et al. (2008) implemented a novel design to tease apart these two accounts. Two sets of items were used. One set of items (e.g., the words GREEN and WHITE) was 75% incongruent (or in a separate condition, 75% congruent) at the item level. The other set of items (e.g., the words RED and BLUE) was 50% congruent and 50% incongruent at the item level (in both conditions). These two sets were intermixed such that the corresponding list was 67% incongruent (or 67% congruent in the alternative condition). As such, it was possible to examine the list-wide proportion congruence effect for a set of items that had no item-specific bias (i.e., those that were 50% congruent and 50% incongruent) but existed within a list that was mostly incongruent (or mostly congruent). No evidence of a list-wide proportion congruence effect was found for the items that were 50% congruent and 50% incongruent. Interference was, however, attenuated for the 75% incongruent items relative to the 75% congruent items. These findings are consistent with the view that list-wide proportion congruence effects reflect the action of an item-specific mechanism that operates on a trial-by-trial basis based on information (e.g., proportion congruency; contingencies) signaled by the item (see Blais & Bunge, 2010, Experiment 1, for converging evidence with a similar design).

The findings of Bugg et al. (2008) and Blais and Bunge (2010) strongly favor an item-specific account of the list-wide proportion congruence effect. In fact, up until this point there has been no unequivocal evidence supporting the interpretation that the list-wide proportion congruence effect reflects list-level

control, independent of item-specific influences. A possible exception is the work of Fernandez-Duque and Knight (2008) who manipulated list-wide proportion congruence in a nonstandard Stroop paradigm in which trials alternated between a color-word Stroop task and a number Stroop task. In this paradigm, reduced interference was observed for a 50% congruent color-word when it was placed in a mostly incongruent list as compared to a mostly congruent list. It is uncertain, however, the extent to which requiring participants to maintain and rapidly switch between two task sets contributed to the use of a list-level control mechanism in this study. In the current study, our objective was to determine whether there is any merit to the theoretical notion that list-wide proportion congruence effects in a standard single-task (color-word Stroop) paradigm do entail list-level control. This is the dominant paradigm in which list-wide proportion congruence effects and the contribution of item-specific mechanisms have been investigated.

Taking the preceding evidence into consideration, it could be claimed that the studies supporting an item-specific account (Blais & Bunge, 2010; Bugg et al., 2008) contain a design feature that potentially biases results in favor of the item-specific view. In particular, as noted by Bugg et al. (2008), placing items in sets and using only two items to compose such sets may have made it easy for participants to detect and use item-specific mechanisms, especially associative (i.e., contingency) learning (i.e., “respond “white” when you see the word GREEN; Jacoby et al., 2003; Schmidt & Besner, 2008), thereby obviating the need for list-level control. As such, it is possible that unequivocal list level control effects might still be evidenced in color-word Stroop if design features were modified from those of prior studies examining proportion congruence effects. Consequently, in the current set of experiments, we implement designs that intentionally avoid such features (i.e., increase the size of the stimulus sets) but still allow us to unambiguously contrast the list-level and item-specific accounts of the list-wide proportion congruence effect.

The critical design feature in our experiments is the inclusion of neutral trials within a Stroop task in which a standard list-wide proportion congruence manipulation is implemented. The neutral trials involve concrete English nouns (e.g., ARM, GOLF, PLANE, RABBIT) that are written in different ink colors but, importantly, are not associated with any item-specific proportion congruence level. That is, unlike the color words (e.g., GREEN, BLUE, etc.) used to create the list-wide bias (e.g., mostly congruent or mostly incongruent) that are 70% congruent or 70% incongruent, respectively, at both an item- and list-level, the neutral words simply reside within a mostly congruent or mostly incongruent list but have no item-specific bias. As such, any differential processing of neutral words across lists can be attributed to control strategies that are developed based on the proportion congruency of a particular list. In other words, if a list-level control mechanism is operative independent of any item-specific influence, then one should observe differential modulation of word reading on neutral trials in the MC and MI lists. We tested this prediction by examining both the benefits (Experiment 1 and Experiment 2) and a cost (Experiment 2) of list-level control.

Experiment 1

In Experiment 1, participants encountered neutral trials within MC and MI lists of the Stroop task. A mostly neutral (MN) list was also included and was composed of 70% neutral, 15% congruent, and 15% incongruent trials. At first blush, this design seems similar to that of Tzelgov, Henik, and Berger (1992) who showed that increasing the percentage of color-words within a list leads to a reduction in Stroop interference. Tzelgov et al. did not, however, implement the list-wide proportion congruence manipulation. Rather, color words were 50% congruent across lists, and participants' expectations for color words (some of which were interfering) were manipulated by altering the percentage of neutral items in the list. Tzelgov et al. observed that Stroop interference decreased as the percentage of neutral items decreased, that is, when expectations for color-words were higher. An open question, however, is whether this pattern reflects the operation of a list-level control mechanism. Indeed, their manipulation did not affect performance on neutral items, and Tzelgov et al. explicitly interpreted their effects in terms of a reactive (i.e., postlexical) mechanism, in which "the meaning of a word is suppressed only if it creates interference" (p. 728).

In the current study, we sought to provide evidence for list-level control of Stroop interference by examining performance in MC, MI, and MN lists. Inclusion of the MN list allows us to gain traction on the important theoretical question of *why* list-level control is implemented (assuming it would be) as well as whether interference is similarly dampened in MI and MN lists. In other words, what information is provided by the list that might trigger list-level control over word reading? Is there any additional benefit of the item-specific mechanism that could be contributing in the MI but not the MN list? The dominant trial type in the MN list is one for which the irrelevant word is not informative for responding (Dishon-Berkovits & Algom, 2000; Melara & Algom, 2003). This is also true in an MI list, although in an MI but not an MN list the dominant trial type is also one for which response interference (conflict) is present. If participants only use list-level control to attenuate word reading when the list signals the presence of a high degree of response conflict, then such control effects would be present in MI but not MN lists. Alternatively, if list-level attenuation of word reading is implemented when the list signals a high frequency of stimuli for which the word dimension is irrelevant to responding, then one would expect list-level control effects in both the MI and MN lists.

The primary predictions were as follows. We expected Stroop interference to be reduced in the MI and MN lists as compared to the MC list. Moreover, we thought it possible that less Stroop interference would be observed in the MI list as compared to the MN list. This prediction reflects the fact that color-words in the MI list have an item-specific bias (i.e., 70% incongruent) in addition to a list-wide bias, whereas those in the MN list do not, and thus performance in the MI list might be additionally benefited by item-specific influences. This is the first experiment to directly evaluate the possibility that Stroop interference in MI lists may reflect an additional component to that which is operative in MN lists.

Stroop interference patterns (i.e., reduced interference in MI as compared to MC lists) alone would not, however, provide unambiguous evidence for the operation of list-level control because

item-specific mechanisms could produce the same patterns. To gain leverage on this issue, we examined neutral trial performance. If list-level control is operative, then color naming should be faster on neutral trials that occur within the MI and MN lists where word reading is presumably dampened compared with the MC list where word reading is permitted. If only an item-specific mechanism is operative, then this difference should not emerge.

Method

Participants. Forty-two Washington University undergraduates participated for course credit or monetary compensation (\$10). They were tested individually in 1-hr sessions.

Materials. The experiment was programmed in E-Prime 1.1. Stimuli were presented in the standard color palette ("red," "blue," "green," "white," "purple", and "yellow") in 36-point Arial font against a light gray ("silver") background. The words RED, BLUE, GREEN, WHITE, PURPLE, and YELLOW were used to create the congruent and incongruent stimuli. The neutral stimuli were three-, four-, five-, or six-letter concrete nouns (matching the length of the incongruent and congruent stimuli). One set of 36 unique neutral words appeared in each block (MC, MI, and MN). Within each set, an equal number (six) of neutral words appeared in each ink color. The sets of neutral words were rotated across blocks and were equated for length ($M = 4.5$) and frequency ($M_{Log_Freq_HAL} = 9.21$) (Balota et al., 2007). Filler neutral words, which appeared only in the MN blocks, consisted of the words CAT, BEAR, TIGER, and MONKEY presented 22 times each and appearing approximately equally often in the six ink colors.

Design and procedure. A 3×3 within subjects design was used with proportion congruence (MC vs. MI vs. MN) and trial type (congruent vs. incongruent vs. neutral) as factors.

The order of the MC, MI, and MN blocks was counterbalanced across participants such that an equal number of participants completed the task in each of six possible orders. Each block was split into two halves as separated by a brief break. The percentage of neutral trials (15%) was held constant across the MC, MI, and MN blocks. The remaining trials were 70% congruent and 15% incongruent in the MC block, 70% incongruent and 15% congruent in the MI block, and 15% congruent, 15% incongruent, and 55% filler neutrals in the MN block.

The experiment was conducted in a small room with the experimenter present. After providing informed consent, participants were instructed to name the ink color the words were printed in as quickly as possible without sacrificing accuracy. After 15 practice trials (five congruent, five incongruent, and five neutral), participants completed the three blocks (MC, MI, MN) with each consisting of 240 trials. Proportion congruence was preserved during each third of each block, though the exact stimulus order within each third was random. For each trial, the stimulus was presented in the center of the screen and remained visible for 2000 ms until a vocal response was detected, at which point the stimulus was erased. The experimenter entered the participant's response via keyboard. Trials on which the voice-key was tripped by extraneous noise or imperceptible speech were considered scratch trials. The next stimulus was presented 500 ms later. Reaction time (RT) (ms) and error rate were recorded.

Results and Discussion

RTs less than 200 ms (<1% of trials) and those from error trials were excluded from the reaction time analyses. As shown in Table 1, errors were infrequent (less than 2% of all trials). Nonetheless, we conducted a set of omnibus analyses of variance (ANOVAs) identical to those reported below for reaction time and confirmed that the effects for error rate did not display patterns that were opposite of the reaction time data. The significant effects that emerged from these analyses are presented in Table 2. The alpha level was set at .05 for all analyses. Partial eta-squared (η_p^2) is reported as the measure of effect size for significant effects. Other than those reported, no effects were significant.

Neutral trial color-naming. A one-way, within-subjects ANOVA was conducted with proportion congruence as the factor. The main effect of proportion congruence was significant, $F(2, 82) = 4.89$, $MSE = 874.10$, $\eta_p^2 = .107$. To understand the locus of this effect, we conducted two orthogonal comparisons. First, we compared performance in the MI ($M = 691$, $SE = 15$) and MN ($M = 708$, $SE = 14$). Neutral trials encountered in the MI and MN lists were responded to faster than neutral trials within the MC list, $F(1, 82) = 9.81$, $MSE = 874.10$, $\eta_p^2 = .107$ (see Figure 1).² Next, we compared neutral trial color naming times in the MI and MN lists. There was no difference in RT, $F < 1$, which suggests that list-level control had a comparable influence on performance in these two lists. These findings support the notion that a list-level control mechanism was indeed operative in the MI and MN lists because neutral words possess no item-specific bias (i.e., the words used on neutral trials are not associated with a particular proportion congruence level). These data also yield some answers to the question of why list-level control is implemented in certain task contexts. It appears that participants attend to the words more fully

in lists where words are expected to be predictive of the correct response (i.e., MC condition) and attenuate word reading when they expect words to yield a high degree of response interference (i.e., MI condition) or when they expect words to be irrelevant and uninformative as to the correct response (i.e., MI and MN conditions).

List-wide proportion congruence effect for congruent and incongruent trials. To confirm that the magnitude of Stroop interference was modulated by proportion congruence, a 3×2 within-subject analysis of variance (ANOVA) was conducted for reaction time with proportion congruence (MC vs. MI vs. MN) and trial type (congruent vs. incongruent) as factors. The main effect of trial type was significant, $F(1, 41) = 416.95$, $MSE = 2437.29$, $\eta_p^2 = .910$, and was qualified by a significant proportion congruence \times trial type interaction, $F(2, 82) = 65.14$, $MSE = 663.68$, $\eta_p^2 = .614$. We decomposed this interaction by conducting two orthogonal comparisons that conceptually mirror those presented above for neutral trial RTs. The first contrasted Stroop interference (i.e., the difference between the incongruent and congruent trial RTs) in the MI and MN conditions (collapsed) with Stroop interference in the MC condition. Stroop interference was significantly attenuated in the MI ($M = 80$ ms) and MN ($M = 131$ ms) conditions as compared to the MC condition ($M = 170$ ms), $F(1, 82) = 87.69$, $MSE = 663.68$, $\eta_p^2 = .517$ (see Figure 1).² This finding suggests that in lists for which the expected (i.e., dominant) trial type was incongruent (i.e., MI) or neutral (i.e., MN), processing of words was dampened relative to the list in which congruent trials were expected (i.e., MC).

A second comparison revealed that interference was significantly reduced in the MI relative to the MN condition, $F(1, 82) = 41.15$, $MSE = 663.68$, $\eta_p^2 = .334$. One possible explanation for this pattern is that both list-level control and item-specific mechanisms contributed to the reduction of Stroop interference in the MI condition, but only list-level control contributed in the MN condition where the color words were 50% congruent. Consistent with this possibility, RTs were faster for incongruent trials in the MI condition than the MN condition, $F(1, 41) = 7.31$, $MSE = 613.86$, $\eta_p^2 = .151$, and congruent trials were correspondingly slower, $F(1, 41) = 27.35$, $MSE = 1017.91$, $\eta_p^2 = .400$. This is the pattern one would expect if a list-level control mechanism were similarly minimizing word processing for all items in the MI and MN lists (as performance on the neutral trials indicated) but an additional suppressive mechanism was activated in response to color-words when such words also possessed an item-specific bias (as in the MI list).

Experiment 2

Experiment 1 provided new evidence against the view that the list-wide proportion congruence effect reflects entirely item-specific mechanisms. Specifically, neutral trial RT differences

Table 1
Mean Error Rate (With Standard Error) as a Function of Trial Type and Proportion Congruence in Experiment 1 and as a Function of Cue Type and Block in Experiment 2

	Trial type		
	Congruent	Incongruent	Neutral
Experiment 1			
Mostly congruent	.003 (.001)	.084 (.010)	.005 (.003)
Mostly incongruent	.007 (.002)	.025 (.003)	.008 (.002)
Mostly neutral	.003 (.002)	.036 (.006)	.009 (.002)
Experiment 2			
Word cue			
Control block			
Mostly congruent	.010 (.002)	.087 (.015)	.025 (.006)
Mostly incongruent	.016 (.004)	.036 (.004)	.016 (.004)
Secondary task block			
Mostly congruent	.014 (.002)	.090 (.016)	.016 (.004)
Mostly incongruent	.006 (.002)	.043 (.005)	.024 (.005)
Spatial-pattern cue			
Control block			
Mostly congruent	.010 (.002)	.089 (.015)	.023 (.006)
Mostly incongruent	.016 (.004)	.034 (.004)	.011 (.004)
Secondary task block			
Mostly congruent	.009 (.002)	.114 (.016)	.015 (.004)
Mostly incongruent	.005 (.002)	.036 (.005)	.015 (.005)

² An additional comparison is the contrast between the MN and MC conditions. We recognize it is not a contrast that is orthogonal to those reported in the text. Nevertheless, because of its theoretical significance, we note that neutral trial color-naming was significantly faster in the MN relative to the MC condition, $F(1, 82) = 7.78$, $MSE = 874.10$, and Stroop interference was significantly reduced in the MN relative to the MC condition, $F(1, 82) = 24.08$, $MSE = 663.68$.

Table 2
Significant Effects From the Omnibus ANOVAs Conducted for Error Rate in Experiments 1 and 2

	Factor (levels)	df	F	MSE	Significance
Experiment 1					
Congruent and incongruent color-naming	PC (3)	2,82	26.11	0.001	**
	TT (2)	1,41	60.28	0.002	**
	PC × TT	2,82	36.01	0.001	**
Experiment 2					
Neutral color-naming	Block × PC	1,46	5.94	<.001	*
Congruent and incongruent color-naming	PC (2)	1,46	43.91	0.002	**
	TT (2)	1,46	84.36	0.003	**
	Block × TT	1,46	6.21	0.001	*
	PC × TT	1,46	45.14	0.002	**

Note. PC = proportion congruence; TT = trial type.
* $p < .05$. ** $p < .001$.

cannot be accounted for by an item-specific mechanism that uses information about the proportion congruency of the word to modulate reliance on the word during color-naming. This is because the neutral words do not have an item-specific bias. One might, however, posit that item-specific mechanisms could also be triggered by the color of an item. The neutral trials share colors with the congruent and incongruent trials, and it is therefore possible that an item-specific mechanism that uses information about the proportion congruency of the color influenced the neutral trial RTs in Experiment 1. A challenge to this type of account, however, is that there is no evidence to date suggesting that item-specific mechanisms are triggered by the color in the color-word Stroop task (but see Bugg et al., 2010, for evidence that the relevant dimension can dictate item-specific control in a picture-word Stroop task). There is only evidence to the contrary (Jacoby et al., 2003), and for this reason all extant item-specific accounts view item-specific mechanisms in the color-word Stroop task as being triggered by the word dimension (e.g., Jacoby et al., 2003; Schmidt & Besner, 2008)¹. Nonetheless, we sought more conclusive evidence to

rule out this alternative account in Experiment 2 and provide converging evidence for the operation of list-level control.

In Experiment 2, we used a novel approach that entailed an opposition procedure. Participants were given the standard Stroop color-naming instructions plus, in some blocks, a secondary (prospective memory) task for which they were asked to remember to press a response key whenever they encountered a particular neutral word (e.g., HORSE). As in Experiment 1, we predicted that the increased percentage of incongruent trials in the MI list would produce a benefit such that Stroop interference would be less pronounced in the MI as compared with the MC condition. (The MN condition was not included in Experiment 2.) This, however, would be the case if item-specific mechanisms or list-level control (or both) were operative. The critical comparisons for disentangling their contributions focus on two measures. One is color-naming performance on the neutral trials (as in Experiment 1). We predicted that use of a list-level control strategy that attenuates processing of words (MI) would result in a benefit in the form of faster responding on the neutral trials relative to the MC condition.

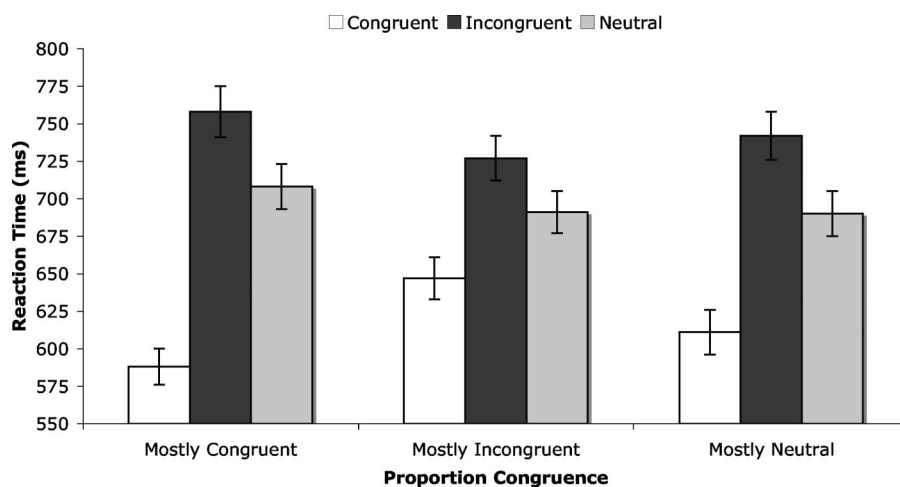


Figure 1. Mean reaction time (with standard error) as a function of trial type and proportion congruence in Experiment 1.

Use of item-specific mechanisms to resolve Stroop interference should, in contrast, have no effect on neutral trial color-naming performance, unless these mechanisms are triggered by the color of the item rather than the word.

The unique prediction of Experiment 2 is that use of list-level control would be revealed by a cost to performance on the secondary, prospective memory task as measured by the percentage of times participants remember to press the response key when they see the word HORSE. We reasoned that engagement of a list-level control strategy that attenuates word processing (as in the MI condition) should run in opposition to the word processing that is required for completion of the secondary task, thus disrupting task coordination and harming performance. On the other hand, permitting oneself to more fully process words (as in the MC condition) is complementary to the word processing that is required for completion of the secondary task and should therefore promote task coordination (cf. Einstein & McDaniel, 2005). According to the view that list-level control is operative, accuracy in responding to the word HORSE (for the secondary task) should be impaired in the MI as compared to the MC list. The rationale is that a mechanism that amplifies (MC list) or attenuates (MI list) word processing and can operate in advance of a stimulus based on list-level expectancies should have an influence on whether participants notice and respond to the word HORSE. By contrast, any item-specific adjustment, even one that could be triggered by the proportion congruency of the color, would be expected to modulate the influence of the word only after the word is processed (i.e., poststimulus onset) and would specifically modulate reliance on the word to produce the color-naming response. Such adjustments should not produce differences in secondary task performance (performing the prospective memory response). In this way, the secondary task may be conceived of as a transfer task for which performance should not be influenced by item-specific adjustments pertaining to color-naming but should be influenced by any global shifts in the degree to which words are attended in advance of the stimulus (due to list-level expectancies).

The possibility might be raised that an obtained disadvantage for secondary, prospective memory task performance (i.e., responding to HORSE) in the MI relative to the MC condition may still not reflect list-level control but might instead be explained by a difficulty account.³ By this account, the MI condition is simply more difficult to coordinate with a secondary task because of the large percentage of incongruent trials. In consideration of this possibility, we included an additional condition in which a spatial pattern was used as the secondary task cue. Participants in this condition completed blocks of the Stroop task that were identical in all respects to those in the word cue condition, except that a spatial pattern bordered each Stroop stimulus, and a particular spatial pattern was used as the prospective memory cue (see Figure 2). Such a cue is unlikely to be routinely processed when attempting to perform the ongoing Stroop task, in part because the spatial pattern is separated from rather than integrated with the ongoing task stimulus. Thus, this type of cue is expected to heighten demands on task-coordination (Einstein & McDaniel, 2005). If the MI condition is simply more difficult to coordinate with a secondary task than the MC condition, then secondary task performance should be impaired in the MI condition relative to the MC condition when *either* the word (i.e., HORSE) cue or spatial-pattern cue is used. In contrast to the difficulty account, the list-level control



Figure 2. A sample stimulus from the spatial-pattern cue condition in Experiment 2. The particular spatial-pattern shown in this border is the target cue for the secondary (prospective memory) task.

account predicts that the disadvantage should be selective to the word cue condition because list-level control in the Stroop task is presumed to influence word processing specifically, and not spatial processing or spatial attention more generally.

Method

Participants. The 48 participants were Washington University undergraduates who received course credit or monetary compensation (\$10) for their participation. They were tested individually in sessions lasting approximately 1 hour.

Design and procedure. A $2 \times 2 \times 2 \times 3$ design was used in which cue type (for the secondary task; word vs. spatial pattern) was a between subjects factor, and block (secondary task vs. control), proportion congruence (MC vs. MI), and trial type (congruent, incongruent, neutral) were varied within subjects. Twenty-four participants performed the word cue condition. An additional 24 participants were subsequently recruited to perform the spatial-pattern cue condition. Participants in both conditions performed two blocks of the Stroop task. One block included the secondary task and one block did not (which we refer to as the “control block”). The proportion of congruent trials changed halfway through each block. Participants completed the task in one of the following two orders: Block 1 MC/MI, Block 2 MC/MI *or* Block 1 MI/MC, Block 2 MI/MC. The secondary task and control blocks were counterbalanced across participants within each cue type condition, such that half of the participants in each of the two possible task orders performed the control block first.

The composition of the MC and MI half-blocks was identical to the composition of the full MC and MI blocks in Experiment 1 with a few exceptions. Each half-block in the present experiment consisted of 262 trials [6 buffers at beginning and end, 246 main trials, and 4 trials on which the target cue appeared (i.e., HORSE in the word cue condition and the control word PLANE in control blocks, or, the target spatial pattern in the spatial-pattern cue condition and a control spatial pattern in control blocks)]. Proportion congruence was preserved during each quartile of each half-block, though the exact stimulus order within quartiles was random. The buffer trials were always neutral words. A brief break was provided after each

³ We thank Joseph Tzelgov and an anonymous reviewer for pointing out the possibility that the MI condition may be more difficult, and for suggesting that we test the difficulty account by including a condition in which a spatial cue is used.

half-block. Participants were not instructed that proportion congruence would change after the break.

After informed consent, participants first received the color-naming instructions for the Stroop task that were identical to those in Experiment 1. Eighteen practice trials followed. As in Experiment 1, the experimenter coded the participants' color naming responses, but in Experiment 2 the next stimulus was presented 1250 ms after the experimenter's response. After practice, half of the participants received the secondary task instructions. They were instructed that we had a secondary interest in their ability to remember to perform an action in the future. Participants in the word cue condition were instructed to press a button box if they ever saw the word "HORSE," while participants in the spatial-pattern cue condition were instructed to press the button box if they ever saw a particular spatial pattern bordering the Stroop stimulus (see Figure 2). Participants were then shown the target pattern. Participants were required to explain the instructions and complete forms (e.g., demographics questionnaire) before beginning the first block. After block completion, participants were instructed that their secondary task was finished and that they would no longer have to press the button box. Participants who performed the control block first received their secondary task instructions after completing the first Stroop block, explained these instructions to the experimenter, and completed forms before performing the second block. A postexperimental questionnaire was administered, and participants were debriefed.

Materials. The materials were identical to Experiment 1 with the following exceptions. The color and word "black" was used instead of the color and word "blue." Four unique lists of six neutral words were rotated across half-blocks such that, unlike in Experiment 1, each neutral word was presented multiple times (i.e., a single neutral word appeared six times, once in each of the six colors, during one half-block). As in Experiment 1, the neutral words that appeared in each half-block were matched on word length and frequency. In the word cue condition, the target word HORSE and the control word PLANE were presented four times, on Trials 61, 122, 183, and 243, and appeared in the colors yellow, purple, white, and green, respectively. In the spatial-pattern cue condition, a rectangular border surrounded each stimulus during the ongoing Stroop task (see Figure 2). Nine different spatial patterns were used to fill the borders on nontarget cue trials. Each nontarget pattern was presented approximately equally often in each block, for each level of proportion congruence, and for each trial type. A tenth spatial pattern was used as the target spatial pattern and an eleventh as the control spatial pattern. These patterns were presented four times, on Trials 61, 122, 183, and 243 in the secondary task block and control block, respectively. As in the word cue condition, a neutral word in yellow, purple, white, or green appeared on these trials. However, the word was not HORSE but was one of the standard neutral words that occurred within each half-block of the Stroop task. A different neutral word occurred on each of the four target cue trials so as to prevent any association between a word and the presence of the target spatial pattern. Following Scullin, McDaniel, and Einstein (2010), in both cue-type conditions, we counted a prospective memory response as correct if participants pressed the button box on the target trial or the following two trials.

Results and Discussion

For consistency with Experiment 1, RTs less than 200 ms and greater than 2000 ms (<1% of trials) were excluded from the RT analyses, as were error trials. Errors on the Stroop task were again low (~3%) (see Table 1) and in no case did the error rate data display patterns that were opposite to the RT data. Table 2 presents a summary of the significant effects from the omnibus ANOVAs for error rate that correspond to the RT analyses below. Other than those reported, no other effects were significant.

Neutral trial color-naming. To illuminate whether a list-level control mechanism contributed to Stroop task performance, we first conducted a 2 (Cue Type) \times 2 (Block) \times 2 (Proportion Congruence) mixed-subjects ANOVA for neutral trial color naming. A main effect of block, $F(1, 46) = 53.00$, $MSE = 1727.89$, $\eta_p^2 = .535$, was qualified by a significant block \times cue type interaction, $F(1, 46) = 12.84$, $MSE = 1727.89$, $\eta_p^2 = .218$. Slowing in the secondary task block relative to the control block was more pronounced for the spatial-pattern cue ($M = 65$) than the word cue ($M = 22$), likely reflecting the greater difficulty of coordinating the former with the ongoing Stroop task. Most importantly, a main effect of proportion congruence was observed, $F(1, 46) = 7.68$, $MSE = 1278.03$, $\eta_p^2 = .143$. Consistent with Experiment 1, participants more quickly named the color of ink on the neutral trials in the MI condition ($M = 728$, $SE = 15$) than in the MC condition ($M = 743$, $SE = 16$) (see Figure 3), and this pattern did not interact with block, $F(1, 46) = 1.97$, $MSE = 1653.36$, or cue type, $F < 1$.

Secondary task performance. Next, we performed the critical comparison of secondary task performance in the MC and MI condition for the word cue HORSE and the spatial-pattern cue by conducting a 2 (Cue Type) \times 2 (Proportion Congruence) ANOVA.⁴ A main effect of cue type, $F(1, 46) = 6.01$, $MSE = .07$, $\eta_p^2 = .116$, was qualified by a significant interaction of cue type and proportion congruence, $F(1, 46) = 4.13$, $MSE = .05$, $\eta_p^2 = .082$ (see Figure 4). We examined the source of this interaction by examining the effects of proportion congruence in the word cue and spatial cue conditions separately.

As predicted by a list-level control account, a cost was observed in the word cue condition such that participants were significantly less likely to remember to respond to HORSE in the MI ($M = .85$, $SE = .02$) as compared with the MC condition ($M = .96$, $SE = .04$), $F(1, 23) = 4.83$, $MSE = .027$, $\eta_p^2 = .174$. This finding, like the neutral trial color naming patterns just described, challenges extant item-specific accounts that view item-specific mechanisms as being triggered by the proportion congruency of each word. Further, it challenges an alternative item-specific account that views item-specific mechanisms as being triggered by the proportion congruency of particular colors because such item-specific adjustments would not be expected to transfer to the secondary, prospective memory task. As such, the secondary task data for the word cue condition support the notion that list-level control was

⁴ A very small percentage (<1%) of false alarms, responses to a spatial pattern other than the target pattern, were produced in the spatial-pattern cue condition. Use of a measure that corrects for these false alarms (i.e., proportion of responses to target spatial pattern–proportion of responses to nontarget patterns) did not change the obtained result.

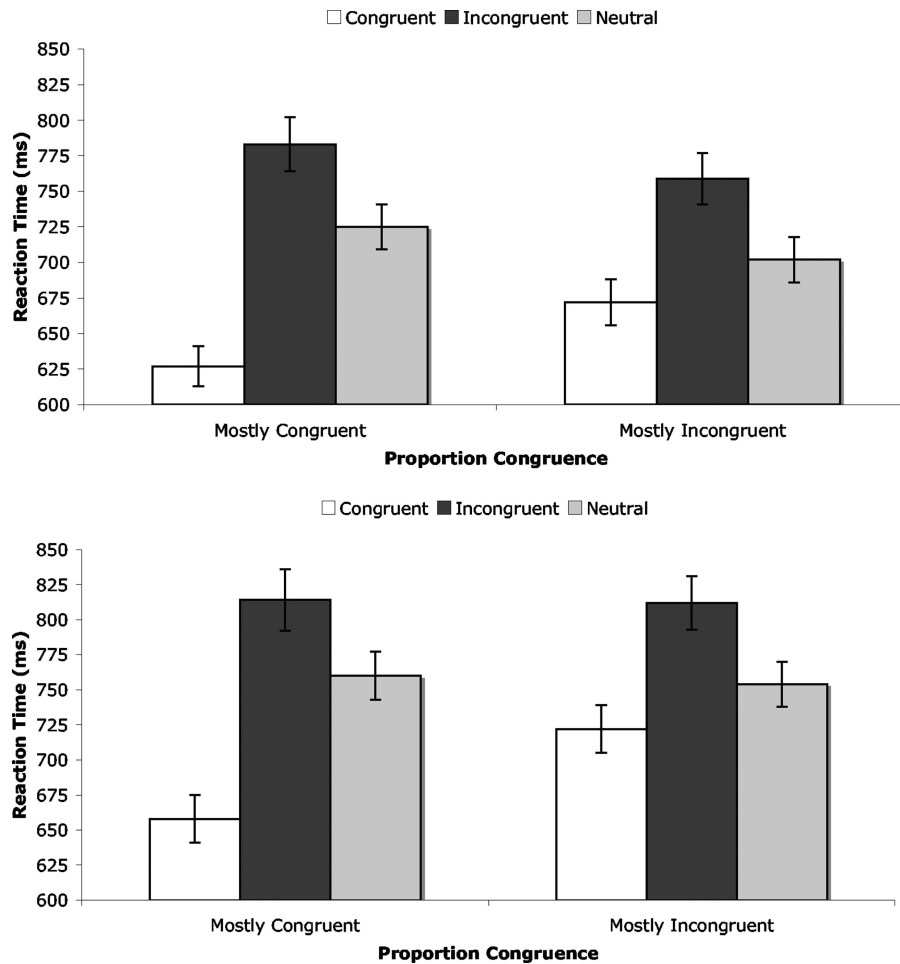


Figure 3. Mean reaction time (with standard error) as a function of trial type and proportion congruence in the control (upper panel) and secondary task (lower panel) blocks in Experiment 2. Data from the secondary task block are collapsed across the two cue-type conditions.

operative and is at least partially responsible for the list-wide proportion congruence effect.

Still, another alternative explanation that must be considered is that the MI condition was simply more difficult and, accordingly, the secondary task was more detrimentally impacted in the MI than the MC condition. The spatial cue condition allowed us to examine this possibility. Inconsistent with a difficulty explanation, secondary task performance in this condition did not significantly differ between the MC ($M = .73$, $SE = .05$) and MI ($M = .81$, $SE = .05$) conditions, $F(1, 23) = 1.11$, $MSE = .075$. In fact, performance was nominally better in the MI condition as compared to the MC condition, which is completely opposite to the prediction of a difficulty account.

Importantly, the absence of a cost for the MI condition in the spatial-pattern cue condition cannot be attributed to differential ongoing Stroop task performance relative to the word-cue condition. No significant differences in Stroop task performance were observed as a function of cue-type or interactions with this factor (see the neutral trial color-naming analyses described above and list-wide proportion congruence analyses described below) with the only exception being that performance slowed more signifi-

cantly in the secondary task block relative to the control block for the spatial-pattern cue condition. This is consistent with our assumption that the spatial-pattern cue condition is more challenging to coordinate with the ongoing Stroop task than is the word cue condition. Indeed, this finding suggests that we provided a strong test of the difficulty account because any effect of the difficulty of the MI condition relative to the MC condition should have been especially likely to emerge under these circumstances, but it did not. Therefore, we can be confident that the critical finding of a cost to secondary task performance in the MI condition when responding to a word cue relates to the attenuation of word reading via list-level control and is not simply a byproduct of the MI condition being more difficult.

List-wide proportion congruence effect for congruent and incongruent trials. To examine changes in Stroop interference as a function of cue type, block, and proportion congruence, a $2 \times 2 \times 2 \times 2$ mixed-ANOVA was conducted that included cue type (word vs. spatial pattern) as a between-subjects factor, and block (secondary vs. control), proportion congruence (MC vs. MI) and trial type (congruent vs. incongruent) as within-subjects factors. Main effects of proportion congruence, $F(1, 46) = 13.27$, $MSE =$

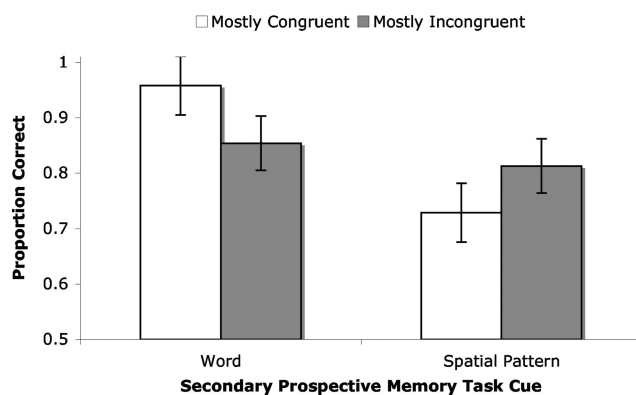


Figure 4. Mean secondary task (prospective memory) performance as a function of cue type and proportion congruence in Experiment 2.

3178.58, $\eta_p^2 = .224$, and trial type, $F(1, 46) = 349.95$, $MSE = 4094.11$, $\eta_p^2 = .884$, were qualified by a significant proportion congruence \times trial type interaction, $F(1, 46) = 81.38$, $MSE = 1330.52$, $\eta_p^2 = .639$. Stroop interference was reduced in the MI condition ($M = 89$) as compared with the MC condition ($M = 155$), replicating the list-wide proportion congruence effect in Experiment 1. A main effect of block, $F(1, 46) = 36.66$, $MSE = 4424.69$, $\eta_p^2 = .443$, was also qualified by two interactions. First, a block \times cue type interaction was observed, $F(1, 46) = 13.53$, $MSE = 4424.69$, $\eta_p^2 = .227$, indicating that slowing was more pronounced in the secondary task block relative to the control block for the spatial-pattern cue condition ($M = 66$) as compared to the word cue condition ($M = 16$). Second, a significant block \times proportion congruence interaction was observed, $F(1, 46) = 4.34$, $MSE = 2250.27$, $\eta_p^2 = .086$. The interaction reflects a disproportionately greater slowing in the secondary task block than in the control block for the MI condition (767 vs. 716, respectively) as compared with the MC condition (736 vs. 705, respectively), although slowing was significant in both conditions [$F(1, 47) = 34.57$, $MSE = 1819.21$, $\eta_p^2 = .424$ and $F(1, 47) = 11.03$, $MSE = 2092.23$, $\eta_p^2 = .190$, respectively]. Importantly, the absence of a significant block \times proportion congruence \times trial type interaction, $F < 1$, indicates that despite the greater slowing in the MI condition in the secondary task block, participants similarly modulated cognitive control as a function of proportion congruence in the control and secondary task blocks (i.e., the list-wide proportion congruence effect was equivalent in the control and secondary task blocks; see Figure 3).

Collectively, the findings of Experiment 2 demonstrate a possible trade-off associated with the global, proactive (i.e., prestimulus) aspects of list-level control (cf. Braver, Gray, & Burgess, 2007). That is, although use of a list-level control strategy produces benefits in the form of faster color-naming on neutral trials and a reduction in Stroop interference in the MI condition, a cost was also revealed via a unique experimental strategy in which a secondary task requiring word processing was placed in opposition to the Stroop task. Participants were less likely to perform the secondary, prospective memory response when a target word cue was presented in the MI condition than the MC condition. We believe that this cost specifically reflects the fact that the list-level strategy used in the MI condition (i.e., attenuation of word reading)

runs in opposition to the word processing that is required to perform the secondary task. The fact that the same cost was not observed when a spatial-pattern was used as the target cue for the secondary task converges on this conclusion, and rules out a difficulty account of the cost observed in the word-cue condition.

General Discussion

The current novel findings of differential neutral trial color-naming and secondary task performance as a function of list-wide proportion congruence are of theoretical importance as they support and possibly restore early notions that suggested list-level control mechanisms do play a role in list-wide proportion congruence effects. It is not possible to quantify the relative degree to which list-level control and item-specific mechanisms contributed to the list-wide proportion congruence effects observed in the current experiments. However, the performance patterns on the neutral trials indicate that the effect was at least partially driven by list-level control. The neutral trial performance patterns are especially revealing because the words that appeared on neutral trials (including the target words used in the secondary task in Experiment 2) have no item-specific proportion congruence bias. Therefore, any attenuation of word reading as a function of proportion congruence on the neutral trials cannot be attributed to an item-specific mechanism that uses information about the proportion congruency of the word to modulate reliance on the word. Such attenuation was observed in the form of both benefits and a cost.

In Experiment 1, attenuation of word reading produced a benefit such that participants were faster to name the ink color of neutral words in the MI and MN as compared to the MC condition, and in Experiment 2 the RT advantage for the MI condition was replicated. In Experiment 2, we also documented a cost associated with the list-level attenuation of word reading. Participants were less likely to complete an intended action in response to a target neutral word (but not in response to a target spatial pattern) when this secondary prospective memory task was presented in the context of a MI condition of Stroop as compared to a MC condition. To our knowledge, this is the first study to demonstrate use of list-level control in Stroop via an opposition procedure involving a secondary task. That the same procedure produced no difference in secondary task performance for a spatial pattern cue, another novel observation, speaks to the target of list-level control. The list-wide proportion congruence manipulation appears to stimulate modulation of word reading (processing) rather than modulation of spatial attention more generally.

The secondary task data are important in ruling out another possible, but heretofore unspecified, item-specific account that posits use of an item-specific mechanism that is triggered by the color of an item rather than the word. By such an account, the benefit we observed for neutral trial color naming in Experiments 1 and 2 reflects that the neutral words shared colors with the color-words, and therefore had an item-specific bias (e.g., colors were 70% congruent in the MC list). Such an account would predict that evidence of list-level control would disappear if the neutral trials did not share colors with the color-words that establish the list-wide bias. Contrary to this prediction, Bugg (2009), in unpublished work, showed that list-wide proportion congruence effects are observed independent of item-specific influences even when the colors and words used to examine list-level control are

distinct from the colors and words used to establish the bias in each list. The cost to secondary, prospective memory task performance in the MI as compared with MC condition for the word cue in Experiment 2 further challenges this color-based item-specific account. That is, it is unclear how an item-specific mechanism that, poststimulus onset, modulates reliance on the word for color-naming could influence secondary, prospective memory task responding. By contrast, a list-level mechanism that attenuates word processing before stimulus onset in the MI list but allows fuller processing of words in the MC list could explain why participants were less likely to notice and respond to the word cue HORSE in the MI condition. These data thus provide additional evidence for the operation of list-level control independent of an item-specific influence.

Implications for Modeling and Theory

The current findings have important implications for contemporary computational models and theoretical accounts. The neutral trial performance patterns, in particular, pose a challenge to the item-specific conflict-monitoring model (Blais et al., 2007), which does not include a list-level control mechanism. This model posits that item-level response conflict (e.g., the conflict associated with the word RED) is associated with the biasing of attention toward the color associated with that item (see Verguts & Notebaert, 2008, for a view that attention is also biased away from the word in response to item-level conflict). In other words, the suggestion is that participants utilize information about the words (i.e., proportion congruency) to bias attention accordingly. The neutral words in the present experiment have no item-specific proportion congruence bias. That is, unlike the color words used to examine the list-wide proportion congruence effect, the neutral words are always 100% neutral. Although the item-specific conflict-monitoring model could account for the list-wide proportion congruence effect (particularly the MI vs. MC comparison), it is unclear how such a model would account for a) the finding that neutral trial color naming was faster in the MI and MN conditions as compared to the MC condition, and b) secondary task performance (associated with neutral trials) was worse in the MI as compared to MC condition for the word cue HORSE.

It might be suggested that the neutral trial performance differences reflect sequential (i.e., carry-over) effects whereby performance on neutral trials in the MI condition benefits from any *item-specific* conflict-related adjustments that might take place on a preceding color-word trial.⁵ This is quite unlikely, however, because as the item-specific model posits, the encountering of the word RED in green ink in an MI list, for example, should boost attention to the color green for that particular item (i.e., word), not to color processing more generally (i.e., at the level of boosting attention to the entire color-naming pathway for all words). As noted by Blais et al. (2007), “The word’s impact on the following trial would only be reduced when the stimulus repeats” (p. 1084). In short, there is no pathway-level mechanism in this item-specific model that would produce conflict-adaptation related benefits for neutral trials that are unique from the color words.

In contrast, the conflict-monitoring model of Botvinick and colleagues (2001) readily accommodates not only the list-wide proportion congruence effect but also the neutral trial performance patterns that are indicative of list-level control. According to this

model, list-level control strategies are established by a conflict-monitoring signal in anterior cingulate cortex that, based on the relative frequency with which response conflict (i.e., incongruent trials) is encountered (i.e., list-wide proportion congruency), triggers a loosening (i.e., MC) or tightening (i.e., MI) of control over an entire task pathway. Evidence from the process-dissociation procedure suggests that it is likely the word-reading pathway that is modulated by list-level control because word reading but not color naming estimates fluctuate as a function of list-wide proportion congruence (Lindsay & Jacoby, 1994). Such estimates accord with early accounts of the list-wide proportion congruence effect that suggested participants might use different word reading strategies in the MC and MI condition (Logan et al., 1984; Lowe & Mitterer, 1982). The inclusion of a pathway level mechanism means that any adjustments in control that are based on information about the *list* could also affect performance on neutral trials.

An interesting theoretical question concerns the nature of such adjustments. One possibility is that they include trial-to-trial modulation of interference via list-level conflict adaptation (i.e., Gratton effects; Botvinick et al., 2001). That is, performance may be faster when neutral trials are preceded by a greater degree of conflict (i.e., more frequent presentation of incongruent trials), as in the MI list relative to the MC list. Verguts and Notebaert’s (2008) Hebbian learning model of cognitive control, which simulates item-specific proportion congruence effects, includes elements of the model of Botvinick et al. and thus, too, could account for the neutral trial performance patterns via item nonspecific Gratton effects. Some findings, however, challenge a view of list-level control that is based entirely on conflict-adaptation. First, inconsistent with this view, and the conflict-monitoring (Botvinick et al.) and conflict-modulated Hebbian learning (Verguts & Notebaert) models, is the present finding that list-level control was equally evidenced in the MI condition wherein response conflict was frequent and in the MN condition which had as few incongruent trials as the MC condition. Based on conflict-adaptation alone, one would have predicted equivalent performance on the neutral trials within the MN and MC conditions, but instead we observed faster color-naming in the MN condition. Second, in prior studies examining list-wide proportion congruence, no evidence of list-level control was observed when item-specific influences were controlled (Blais & Bunge, 2010; Bugg et al., 2008). Yet those studies and the current study used similar levels of list-wide proportion congruence (67% vs. 70%, respectively). In other words, it could be argued that similar levels of conflict preceded the occurrence of the Stroop words that did not have an item-specific bias (i.e., were 50% congruent) in these past studies and the occurrence of the neutral words that did not have an item-specific bias in the current study. It is uncertain as to why a

⁵ A second sequential account is one that refers to sequential contingency effects, the finding that contingency effects are larger after high contingency trials (Schmidt et al., 2007). High contingency trials, in particular those that are congruent, exist only in the MC condition in the current experiments. As such, performance on these trials could be speeded because of the enhanced contribution of contingency mechanisms. High contingency responses do not, however, exist for the incongruent trials or, most critically, the neutral trials. Therefore, sequential contingency effects could not produce performance differences on these two trial types or on the secondary task as a function of list (e.g., MC vs. MI).

conflict-adaptation mechanism that heightens (or weakens) attention based on the frequency of incongruent trials would be specific to the current study.

Another possibility, which is more true to the term “list-wide” control, is that the control adjustments are sustained in nature. Some theories such as the dual-mechanisms of control account (Braver et al., 2007) characterize list-level control as a proactive mechanism that acts in a sustained fashion across a list (see also De Pisapia & Braver, 2006). Indeed, the novel secondary task prospective memory differences we observed for the word cue condition are consistent with a mechanism that is producing global shifts away from word reading, in advance of stimulus onset, in the MI list but is permitting fuller processing of words in the MC list. Fernandez-Duque and Knight (2008) also attributed their finding of a list-wide proportion congruence effect in the task-switching Stroop paradigm to a sustained control mechanism. We cannot be certain as to the extent to which list-level control was operating in a sustained fashion in the current experiments where trials did not alternate between two Stroop tasks. However, it seems reasonable to assume that some sustained modulation of word reading was present because the neutral color-naming trials occurred relatively infrequently (and the secondary task targets extremely infrequently) and randomly within lists. Moreover, particularly in the MN list, only 15% of trials were incongruent which means that there were few opportunities for performance on the neutral trials to be speeded by any list-based conflict-related adjustments that may have occurred on preceding incongruent trials. Regardless of whether list-level control is achieved primarily through a sequential trial-by-trial adjustment or a sustained modulation of word reading (or some combination of both), the key point for present purposes is that the mechanism is operating based on list-level information and not item-specific information.

Although list-level control is often referred to as strategic, such a mechanism need not be voluntary or deliberate. As the tectonic theory of Stroop effects posits, participants’ attention may be differentially attracted to the word dimension in the MC, MI, and MN conditions because words are not equally informative (e.g., correlated with responses) across these conditions (Dishon-Berkovits & Algom, 2000; Melara & Algom, 2003). Attention may be less drawn to the word dimension in the MI and MN conditions because words carry less information about the target dimension (i.e., the color) in these conditions than they do in the MC condition. That is, in the MI and MN conditions, 85% of the time the word dimension does not provide information regarding the correct response. In contrast, in the MC condition, 70% of the time the words are perfectly predictive of the correct response. It may therefore be that list-level control is best modeled as a biasing of attention toward or away from processing of the irrelevant dimension depending on the irrelevant dimension’s utility to current goals, and not depending on the degree of response conflict as conflict-monitoring accounts posit.

Contextual Influences on List-Level Control

Having established that list-level control does play a role in list-wide proportion congruence effects, a subsequent theoretical challenge is delineating when participants engage list-level control in color-word Stroop as opposed to relying on item-specific mechanisms that are also available. This challenge presents itself when

one considers the current findings in light of the two prior studies that manipulated list-wide proportion congruence and did not observe list-level control when item-specific influences were controlled (Blais & Bunge, 2010; Bugg et al., 2008). We believe that there are two primary factors that contributed to the emergence of list-level control in the present study. The first is that the color words were not split into sets with different degrees of proportion congruency (e.g., one set 50% congruent, a second set 75% congruent). Instead, all color words within a list had the same degree of proportion congruency (e.g., 70% congruent). The most optimal strategy in such a context may, therefore, be a global or sustained modulation of word reading. When items within a list vary in proportion congruency, participants may be drawn to the specific information conveyed by different items and less inclined to simply dampen word reading in a uniform fashion across the list. The second is that the color-word set used to establish list-wide proportion congruence was composed of six items. In a six-item set, unlike the case of two-item sets used previously, it is impossible to predict responses on incongruent trials via associative (i.e., contingency) learning, a potent item-specific mechanism. This is because there are, for example, five possible response options on incongruent trials. As Bugg et al. (2008) suggested, participants might be especially disinclined to implement list-level control when a strong bias exists for them to rely on associative learning to predict high contingency responses, and when such an approach is effective on the majority of trials, as was arguably the case in the previous studies but not the current study. Increasing the size of the stimulus set may also increase the perceived difficulty associated with the task due to greater variability in color-word pairings. Such a manipulation may, like other manipulations such as having participants switch back and forth between two different Stroop tasks (Fernandez-Duque & Knight, 2008), motivate participants to adopt a list-level control strategy to minimize expected interference.

A Cost of List-Level Control

Use of cognitive control is often associated with improved or more efficient task performance. However, our novel finding using a secondary task opposition paradigm highlights that list-level control can also be associated with a cost to performance (cf. Braver et al., 2007). If the target (e.g., a word) of a list-level control mechanism that aims to attenuate or inhibit processing (such as that which characterizes the MI list) is relevant to performance of a secondary task, then the more intact or robust the list-level control, the greater the likelihood that secondary task performance may suffer. The implication of such a trade-off for prospective memory performance, which was examined in the current study, is that individuals may fail to complete prospective memory intentions if the cue that signals the intention is filtered to accomplish a concurrent goal (cf. Goschke & Dreisbach, 2008). Importantly, prospective memory was not disadvantaged by list-level control when the prospective memory cue was unrelated to the processing attenuated by that control (i.e., the spatial-pattern cue), which speaks to the precise target of list-level control in our paradigm. This trade-off raises the promise and potential of examining secondary task performance during the Stroop in populations for which the ability to implement list-level control may be compromised, such as older adults. In older adults, performance on

measures of list-level control from the Stroop task might be worse but such individuals might also outperform younger adults on the secondary task, a finding that would be in line with recent work showing the potential benefits of older adults' processing of distraction (e.g., Kim, Hasher, & Zacks, 2007).

Conclusions

The current findings are theoretically revealing in showing that list-wide proportion congruence effects in the Stroop task are not *entirely* item-specific effects in disguise. There is merit to the idea that list-wide proportion congruence effects involve, at least in part, list-level control (cf. Hutchison, in press). List-level control arises in response to information signaled across the list regarding the frequency of response conflict or the irrelevant word's utility to current goals. We have shown that the attenuation of word reading via list-level control is beneficial for reducing Stroop interference and speeding color naming on neutral trials (Experiments 1 and 2) but can also lead to a cost, as shown in Experiment 2. These findings are inconsistent with a purely item-specific account of the list-wide proportion congruence effect.

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