

# Expectations and Experience: Dissociable Bases for Cognitive Control?

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Classic theories emphasized the role of expectations in the intentional control of attention and action. However, recent theorizing has implicated experience-dependent, online adjustments as the primary basis for cognitive control—adjustments that appear to be implicit (Blais, Harris, Guerrero, & Bunge, 2012). The purpose of the current study was to evaluate whether explicit expectations play any role in cognitive control above and beyond experience. In a novel *precued lists paradigm*, participants were administered abbreviated lists of Stroop trials. For half of the lists, precues led participants to validly expect lists of varying proportion congruency (e.g., mostly congruent [MC], mostly incongruent [MI]; Experiments 1 to 4). The Stroop effect was greater in cued MC relative to uncued MC lists. By contrast, the Stroop effect was equivalent in cued MI and uncued MI lists. Only when preparation was encouraged via a speed manipulation (Experiment 3) or incentives (Experiment 4) did we find evidence of heightened control when an MI list was expected, in the form of a short-lived reduction in the Stroop effect on the first (experience-free) trial. These patterns suggest (a) expectations play a role in the relaxation of cognitive control, independent of experience (as also shown in Experiment 5, wherein expectations were varied while holding experience constant across lists), but (b) experience is the dominant basis for the sustained heightening of cognitive control (after the first trial). Theoretical implications of dissociating the contributions of expectations and experience to cognitive control are discussed, including interpretations of the list-wide proportion congruence effect.

*Keywords:* cognitive control, Stroop, conscious expectations, conflict-monitoring, color-word correlations

Imagine that a couple of friends, Jimmy and Susan, just got off a bumper-car ride at an amusement park. They reflect on the ride and recall the rough, but mostly laugh-provoking, collisions. Before getting on the ride, Jimmy was explicitly warned by the conductor about the aggressive drivers in the red, green, and blue cars. Hence, Jimmy attributed his “survival” to the effort he made to strategically avoid being jolted by certain colored cars. This tendency to attribute the control of one’s attention and actions to internal forces such as explicit *expectations* (e.g., the warning from the conductor) is common, although not necessarily accurate (Hommel, 2007). For example, Susan was not given an explicit pre-ride warning from the conductor, yet she also succeeded during the course. Susan’s survival thus appears attributable to online (cf. Logan & Gordon, 2001) and possibly “implicit” (cf. Blais, Harris, Guerrero, & Bunge, 2012) adaptations of control, that is, reactive adjustments that occurred in response to the *experience* of encountering the aggressive drivers over and over as she circled the track. This simple example serves to illustrate how both expectations and experience may play a role in successful performance and the challenge of disentangling their contributions (i.e., Jimmy’s sur-

vival may also reflect his experience and not his expectations). The current study will address this challenge in examining expectations and experience as bases for cognitive control.

In the cognitive control literature, there is a rich tradition of theorists attributing control of attention or action to will, strategies, and expectations—factors often considered conscious, intentional, volitional, or effortful (e.g., Ach, 1910; Norman & Shallice, 1986; Posner & Snyder, 1975; Shiffrin & Schneider, 1977; see also Baumeister, Vohs, & Tice, 2007). One view has been that a supervisory attentional system monitors for processing conflicts and willfully biases action in favor of the goal-relevant response (Norman & Shallice, 1986). Similarly, it has been suggested that selection of relevant information in the face of irrelevant information reflects the voluntary exercise of intentions (Posner, & DiGirolamo, 1998; see also Posner, 2012). Most recently, in their dual-mechanisms of control account, Braver, Gray, and Burgess (2007) described a resource-demanding, proactive control mechanism that biases attention toward goal-relevant information in advance of an imperative stimulus, particularly when reliable cues are present that allow participants to anticipate the occurrence of conflict (i.e., interference).

There are several empirical patterns that are frequently cited as evidence for the role of expectations, defined here as explicit and advance knowledge regarding the likelihood of conflict, in modulating cognitive control of interference. However, the interpretation of these patterns is somewhat ambiguous in that the same patterns may be accounted for by implicit adaptations of control that occur online as experience with a task accrues. As we elaborate below, the ambiguity of these patterns in part reflects the difficulty of disentangling expectations and experience, as experience can give rise to expectations and expectations can theoretic-

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cally alter the effects of experience. To gain traction on the theoretical question of whether expectations uniquely affect cognitive control above and beyond the effects of experience, we examined how explicit expectations provided in advance of a list of trials affected performance on the Stroop color-naming task both when the expectations were consistent with actual task experience (Experiments 1 to 4) as well as when they conflicted with experience (Experiment 5). Before describing the paradigm we developed for this purpose, we review the extant empirical patterns that motivated the present study.

### Proportion Congruence Manipulations and the Role of Expectations

In the Stroop task, which is considered by some as the “gold standard” measure of selective attention, participants name the color of ink in which words are rendered while ignoring the word. During incongruent trials, the word and color conflict (e.g., RED in blue ink) and during congruent trials, the word and color match (e.g., BLUE in blue ink; MacLeod, 1992). The Stroop effect refers to the slowed (and sometimes more errant) performance on incongruent relative to congruent trials. A primary empirical pattern that has been cited as evidence for the role of expectations in affecting cognitive control in the Stroop task is the list-wide proportion congruence effect. It refers to the reduction in the Stroop effect in lists that consist of mostly incongruent (MI) trials compared with lists of mostly congruent (MC) trials (Logan & Zbrodoff, 1979). This is a robust pattern that has been replicated in a variety of conflict paradigms including flanker and Simon tasks (e.g., Gratton, Coles, & Donchin, 1992; Hommel, 1994; Kane & Engle, 2003; Lindsay & Jacoby, 1994; Logan, 1980; Logan, Zbrodoff, & Williamson, 1984; Lowe & Mitterer, 1982; Toth et al., 1995; Wendt & Luna-Rodriguez, 2009; West & Baylis, 1998; for review, see Bugg, 2012; Bugg & Crump, 2012).

One set of accounts posits that the inverse relationship between the Stroop effect and the probability of interference (i.e., percentage of incongruent trials) is attributable to top-down shifts in expectations or strategies that lead to variations in the weighting of the word or color dimension, and which influence selective attention to the color dimension (e.g., Lindsay & Jacoby, 1994; Logan & Zbrodoff, 1979; Lowe & Mitterer, 1982; West & Baylis, 1998; cf. Tzelgov, Henik, & Berger, 1992, for similar explanations of the proportion color-word effect). For example, Lowe and Mitterer (1982) concluded that “attentional strategies may be actively chosen to suit prevailing conditions” (p. 684), such that participants selectively focus on the relevant information (in an MI list) or distribute attention across the relevant and irrelevant dimensions *at will* (in an MC list). Similarly, West and Baylis (1998) posited that participants actively maintain the color-naming goal to *strategically* guide performance in the MI list.

An alternative view is that the list-wide proportion congruence pattern is attributable to adjustments in control (e.g., differential weighting of word dimension) that emerge spontaneously during the task and operate on the basis of information that is acquired via experience with stimulus frequencies such as the pairings of particular words and colors (i.e., frequencies of congruent and incongruent trials; Melara & Algom, 2003; cf. Logan, 1988). We will refer to this class of views as *experience-based accounts*. One prominent account, the tectonic theory, focuses on the informa-

tional value of the nominally irrelevant word dimension (Melara & Algom, 2003). According to this account, learning of word-color correlations influences the degree to which attention is drawn to the irrelevant word, thereby producing the list-wide proportion congruence effect. In the MC list, attention is attracted to the word dimension because words tend to be correlated with the correct response (the congruent color). On congruent trials, which constitute the majority of trials, responses can be predicted via attention to the word dimension in the form of associative learning (e.g., respond “red” when word RED is shown; Jacoby, Lindsay, & Hessels, 2003; Musen & Squire, 1993; Schmidt & Besner, 2008). On the occasional incongruent trial, however, the tendency to attend to the word yields slowed response times, resulting in a failure of selective attention and a large Stroop effect. By contrast, in the MI list words tend to be correlated with an incorrect response such that attention is repelled from the word, and the Stroop effect is thereby minimized. In support of this account, Melara and Algom (2003) demonstrated a strong positive correlation between the Stroop effect and the degree to which the word dimension is predictive of the correct response (see also Dishon-Berkovits & Algom, 2000, for evidence that eliminating this correlation eliminates the Stroop effect).

Experience-based accounts also subsume conflict-monitoring accounts. For instance, the globally oriented conflict-monitoring account proposes that patterns such as the list-wide proportion congruence effect stem from conflict-triggered adjustments in control (Botvinick, Braver, Barch, Carter, & Cohen, 2001). Contexts in which conflict occurs frequently (e.g., MI lists) are associated with a global heightening of top-down control relative to contexts in which conflict is rare (e.g., MC list). Item-specific conflict-monitoring accounts similarly attribute the list-wide proportion congruence effect to conflict-triggered adjustments in control (Blais, Robidoux, Risko, & Besner, 2007; Verguts & Notebaert, 2008). However, in item-specific models, control is heightened selectively for particular items (e.g., words) rather than globally (i.e., for all items). The greater the history of conflict for a given item (and thereby Hebbian learning, as in the model of Verguts & Notebaert, 2008), the stronger the control signal. In other words, underlying item-specific control is the learning of the relationship between particular items and particular congruency levels (i.e., a word in an MC list is associated with a low likelihood of interference, whereas a word in an MI list is associated with a high likelihood of interference; cf. Bugg & Hutchison, 2013; Bugg, Jacoby, & Chanani, 2011; Jacoby et al., 2003). Following some learning (experience), encountering an item in the MI list is thought to trigger retrieval of the attentional setting (i.e., one that rapidly attenuates processing of the word dimension) that has been successfully used to respond to the stimulus in the past (i.e., on prior trials; Bugg & Crump, 2012; Crump & Milliken, 2009), thereby facilitating performance. Encountering an item in the MC list triggers retrieval of an alternative setting (e.g., distributed attention to color and word) that tends to facilitate performance (see, e.g., Schmidt & Besner, 2008, for the view that participants might alternatively be retrieving highly contingent responses via item-specific associative learning). These differences produce the list-wide proportion congruence pattern. In support of the item-specific account of the list-wide proportion congruence effect, it has been shown that controlling for item-specific influences (i.e., item-specific control and item-specific associative learning) can

eliminate the difference in the Stroop effect across MC and MI lists (see Blais & Bunge, 2010; Bugg, 2014, Experiments 2 and 3, Bugg, Jacoby, & Toth, 2008; but see Bugg, Experiments 1 and 4; Bugg & Chanani, 2011; cf. Hutchison, 2011).

A third experience-based account—the temporal learning account (Schmidt, 2013a, 2013b)—proposes that the list-wide proportion congruence effect depends on learning about the rhythm of responding in different lists. The general notion is that participants learn *when* to respond to stimuli. If most previous trials are responded to quickly, as in an MC list, then retrieval of this knowledge will lead participants to be prepared to respond at a similarly early point in time on the current trial (benefitting congruent trials and resulting in no advantage on incongruent trials). Conversely, if most previous trials are responded to slowly, as in an MI list, then retrieval should lead participants to be prepared to respond during a later time window (benefitting incongruent but not congruent trials). This view differs from a predecessor, experience-based account termed the adaptation to the statistics of the environment model, which suggests that participants try to respond at a time that optimizes the speed–accuracy trade-off (Kinoshita, Forster, & Mozer, 2008; Kinoshita & Mozer, 2006; Kinoshita, Mozer, Forster, & 2011). The idea is that the control system adapts to previous trial history of *difficulty*, not the rhythm of responding on prior trials or conflict per se. In support of these accounts, Schmidt (2013a) found that Hutchison’s (2011) list-wide proportion congruence effect was attenuated (though still significant) after accounting for the reaction time (RT) of the immediately preceding trial, which may represent a proxy for temporal learning or history of difficulty.

### Teasing Apart Expectation- and Experience-Driven Adjustments in Control

With respect to evaluating the contributions of expectations and experience (collectively defined, as it was not the goal of the current study to contrast different experience-based accounts) to list-wide proportion congruence effects, several extant findings are relevant. Blais et al. (2012) examined the degree to which participants were aware of the list-wide proportion congruence manipulation. The rationale was that if the list-wide proportion congruence effect reflected strategic (e.g., expectation-driven) control adjustments, then participants should exhibit some awareness of the basis for such adjustments (i.e., the relative proportion of congruent to incongruent trials across lists). Findings showed that participants’ accuracy in indicating whether a list contained more congruent or incongruent trials tended to be poor and uncorrelated with the magnitude of the list-wide proportion congruence effect. Blais et al. concluded that implicit adaptations, such as learning the regularities in each list (e.g., frequencies of particular trial types) and subconsciously adapting to them, were likely responsible for the effect, and not an explicit strategy.

One possibility is that participants simply cannot intentionally (consciously) adjust control based on explicit expectancies regarding interference. However, findings from trial-by-trial precueing paradigms in which participants are informed of the congruency of the upcoming trial indicate such adjustments are possible. When a cue was shown indicating a 100% likelihood of encountering a conflicting (incongruent) item on the next trial, participants were faster to name the color than in a condition with an uninformative

precue (Bugg & Smallwood, 2014; Goldfarb & Henik, 2013, Experiment 2; but see Goldfarb & Henik, 2013, Experiment 1). This precue benefit was found in a four-choice Stroop task, which meant that the benefit did not reflect a strategy of attending to the word to predict the color (see Logan & Zbrodoff, 1982). Rather, intentional construction of a more abstract control setting in advance of stimulus onset appeared to be responsible for the benefit.

Still, differences between the trial-by-trial precueing paradigm and the list-wide proportion congruence paradigm leave open the question of whether such expectancy-driven adjustments affect control more generally. For instance, in list-wide proportion congruence experiments, the cue (i.e., a list context) is a probabilistic predictor of interference and there is mixed evidence for precue benefits when trial-by-trial precues are probabilistic (e.g., 75–80% valid; Bugg & Smallwood, 2014; Lamers & Roelofs, 2011; Olsen & Hutchison, 2013). This raises the possibility that participants might not attempt to adjust control in the list-wide proportion congruence paradigm, given the potential costs of doing so on the occasional invalid trial (e.g., if one intentionally distributed attention across the word and color in an MC list and an incongruent trial occurred, the participant may be slower and more error-prone than if they had not adjusted control). However, in the list-wide paradigm, a “cue” is valid for many trials rather than a single trial. Participants might be more apt to make adjustments in response to probabilistic precues when the effort in doing so is likely to reap benefits in the long run. Of course, this is assuming that participants do become aware of the proportion congruency of a list. If they do not (cf. Blais et al., 2012), then they arguably do not have a basis for adjusting control intentionally. The typical list-wide proportion congruence design is thus not ideal for evaluating these possibilities, or for disentangling expectation-driven from experience-driven control adjustments.

### Current Study

There are several plausible experience-based accounts of the list-wide proportion congruence pattern that has been previously taken as evidence of expectation-driven control (e.g., Lowe & Mitterer, 1982). These accounts share the view that participants are not intentionally (willfully) adopting a control strategy (e.g., selectively heightening attention to color) based on the expected probability of interference (proportion congruence) in advance of lists; rather, these accounts posit that it is the experience with stimuli and the learning of regularities (e.g., word-color correlations, frequency of conflict, pace of responding) that underlie the magnitude of the Stroop effect (i.e., modulations of cognitive control) in different lists (e.g., Blais et al., 2012; Botvinick et al., 2001; Melara & Algom, 2003; Schmidt, 2013a, 2013b). The purpose of the current study was to more clearly disentangle the role of expectations from the role of experience as bases for control adjustments.

Cognizant of Hommel’s (2013) argument that correlations between the accuracy of self-report and the use of information for cognitive control purposes (e.g., such as those described by Blais et al., 2012) may be uninformative for evaluating whether conscious processes play a causal role in cognitive control, we adopted the experimental approach of manipulating expectations in advance of performance. To the extent that participants can inten-

tionally configure cognitive control when given explicit information about the probability of interference prior to the start of a list, we reasoned that such a manipulation would affect the magnitude of the Stroop effect and thereby support a role for expectations. If expectations play a negligible role and control is instead dominated by experience-driven adjustments, then performance (the Stroop effect) should be equivalent when advance information about the probability of interference is available and when it is not.

To test these predictions, we developed the *precued lists paradigm*. Rather than presenting participants with a single, long (e.g., ~50 to 200 trials) list of MC items or MI items, as is typically done in list-wide proportion congruence paradigms, we presented multiple abbreviated lists of 10 (Experiments 1 through 4) or 20 items (Experiment 5). Prior to the start of each list, a precue was presented, and it was either informative or uninformative (referred to hereafter as *cued* and *uncued*, respectively). In the cued condition, participants were provided information about the composition of the upcoming list. The cue “80% matching” meant that 80% of stimuli would be congruent items (“the word and color would match”), whereas the cue “80% conflicting” meant that 80% of stimuli would be incongruent items (“the word and color would not match”). In the uncued condition, participants were not given any information about the upcoming list. Thus, expectations were defined as explicit knowledge about the probability of interference provided in advance of any experience with the list.

One advantage of the precued lists paradigm is that comparison of the cued and uncued conditions for a given proportion congruency level (e.g., MI) permits one to examine the effects of explicit expectations above and beyond experience. If participants intentionally modulate control on the basis of explicit expectations, then effects of control on performance should be amplified in the cued conditions relative to the uncued conditions. For instance, if attention is biased away from the word (e.g., Lindsay & Jacoby, 1994; Melara & Algom, 2003) or toward the ink color (Lowe & Mitterer, 1982) prior to the start of a list when one expects most trials to be conflicting, then a smaller Stroop effect should be observed in the cued MI list than the uncued MI list (referred to hereafter as a *cue-induced MI shift*). Conversely, if attention is biased toward the word (Lindsay & Jacoby, 1994; Melara & Algom, 2003) or distributed across the color and word (Lowe & Mitterer, 1982) when one expects most trials to be matching, then a *larger* Stroop effect should be observed in the cued MC than the uncued MC list (referred to hereafter as a *cue-induced MC shift*). By contrast, if participants cannot utilize the cues to intentionally configure cognitive control, then despite explicitly knowing in advance of the start of a list that a high (or low) proportion of trials would evoke interference, there should be no difference in the Stroop effect as a function of cueing. Such a result would be consistent with experience being the primary basis for cognitive control adjustments.

A second advantage of the precued lists paradigm is that it enables the examination of performance on the first trial within a list. Performance on the first trial is of theoretical interest because it is arguably the purest indicator of the influence of expectations created by the precue, because no experience has yet accrued to support the types of learning that underlie the experience-based accounts described above. With the typical list-wide proportion congruence paradigm, there is generally one “first” trial per subject in each proportion congruence condition, given use of single long

lists in the MC and MI conditions. This is not ideal because a single trial (observation) cannot provide a reliable estimate of performance. Moreover, the Stroop effect cannot be calculated on the basis of a single trial (i.e., each participant is shown *either* a congruent or incongruent stimulus on the first trial in a given condition). In the precued lists paradigm, there are as many first trials as there are lists per condition, and this allows for a more stable estimate of how performance on both congruent and incongruent trials is uniquely affected by explicit expectations.

Third, as we will demonstrate, the paradigm readily permits one to examine the role of expectations and experience under conditions in which the precues are valid (e.g., an 80% matching precue is followed by a list in which 80% of trials are congruent; Experiments 1 through 4) as well as conditions in which they are not (e.g., 80% matching and 80% conflicting precues are followed by experience-invariant lists in which 50% of trials are congruent; Experiment 5). If differences in the Stroop effect were observed across lists that are experience invariant but differ in expectations, this would provide strong evidence for an expectation-driven modulation of cognitive control.

On the basis of prior findings and accounts that suggested a role for expectations, strategies, or will (e.g., Logan & Zbrodoff, 1979; Lowe & Mitterer, 1982; Norman & Shallice, 1986; Posner, & DiGirolamo, 1998; West & Baylis, 1998), as well as the resource-demanding proactive control mechanism posited by the dual mechanisms of control account (Braver et al., 2007), it was predicted that evidence for a role of expectations independent of experience would be observed as indicated by three patterns: (a) larger effects of list-wide proportion congruence in the cued compared with the uncued lists because of the presence of a cue-induced MI shift and/or a cue-induced MC shift; (b) a cue-induced MI shift and/or cue-induced MC shift on the first trial; and (c) a cue-induced list-wide proportion congruence effect under cueing conditions in which all lists are 50% congruent but are preceded by precues that produce varying expectations (e.g., 80% matching vs. 80% conflicting).

By contrast, on the basis of prior findings and accounts that pointed to experience as the key basis for adjustments in cognitive control (e.g., Blais et al., 2007, 2012; Botvinick et al., 2001; Melara & Algom, 2003; cf. Kinoshita et al., 2008, 2011; Kinoshita & Mozer, 2006; Schmidt, 2013a), it was anticipated that the list-wide proportion congruence pattern would be experience-driven, as indicated by (a) the absence of a cue-induced MI shift or a cue-induced MC shift, such that effects of list-wide proportion congruence are equivalent in the cued compared with uncued lists; (b) the absence of a cue-induced MI and a cue-induced MC shift on the first trial; and (c) the absence of a proportion congruence effect when the cued MC and MI lists are equated in proportion congruence (i.e., are actually 50% congruent). Needless to say, some combination of the two predicted sets of effects could be found, indicating that expectations and experience both contribute to modulations of control in potentially dissociable ways.

## Experiment 1

The purpose of Experiment 1 was to provide an initial test of the first prediction detailed above using the novel precued lists paradigm, namely, the predicted effects of list-wide proportion con-

gruence in cued and uncued conditions when cues were valid. For MC lists, effects of expectations would be evidenced by a *larger* Stroop effect in the cued MC relative to the uncued MC condition (i.e., a cue-induced MC shift), whereas for MI lists, effects of expectations would be evidenced by *less* Stroop interference in the cued MI relative to the uncued MI condition (i.e., a cue-induced MI shift). This reflects that expectations should lead participants to bias attention toward the word (Lindsay & Jacoby, 1994; Melara & Algom, 2003), or to distribute attention across word and color dimensions (Lowe & Mitterer, 1982) in response to an 80% matching cue, but devote less attention to the word or more to the color in response to an 80% conflicting cue. A test of these predictions as they relate to performance on the first trial of each list is reserved for Experiment 2.

## Method

**Participants.** Twenty-two undergraduates participated for course credit or monetary compensation (\$10).<sup>1</sup> Participants were native English speakers with normal or corrected vision and normal color vision.

**Design and materials.** A 2 (cueing: cued vs. uncued)  $\times$  2 (list-wide proportion congruence: MC vs. MI)  $\times$  2 (trial type: congruent vs. incongruent) within-subjects design was used. There were 32 lists within the experiment; half were cued and half were uncued. Of the cued lists, half were MC and half were MI. Participants were alerted to the proportion congruence of the cued lists in advance of the start of the list via a precue indicating “80% Matching” (for MC lists) or “80% Conflicting” (for MI lists). For the uncued lists, the precue indicated “?????” Half of the uncued lists were MC and half were MI.

Within each list, there were 10 trials and two trial types. Congruent trials were comprised of the word that matched the to-be-named ink color. Incongruent trials were comprised of a word that conflicted with the ink color. All combinations of four possible words (RED, BLUE, GREEN, and YELLOW) and their corresponding colors were used to create the stimuli. For MC lists (cued and uncued), eight congruent and two incongruent trials were randomly presented. For MI lists (cued and uncued), eight incongruent and two congruent trials were randomly presented. So as to minimize repetitions of stimuli within a list, congruent and incongruent trials were drawn from separate lists of all possible congruent and incongruent trials randomly without replacement.

**Procedure.** Participants first received instructions with the color-naming task. They were told to name aloud the color as quickly as possible without sacrificing accuracy, and practiced doing so on eight trials. Then they were informed that prior to the presentation of a list of trials they would see one of three precues: “80% MATCHING,” which meant the word and color would be the same for 8 of the 10 trials in the list; “80% CONFLICTING,” which meant the word and color would differ for 8 of the 10 trials in the list; or “?????” which meant the participant would not be told what percent of trials in the list would be matching or conflicting. They were also told the precues can help improve performance and it was very important that they tried their best to use the information provided by the precue. They were given the example of using the 80%

CONFLICTING precue to try to ignore the word during an upcoming list.

For a given list, the precue remained on-screen until the participant pressed a key on a response box indicating that they were prepared and ready to begin the list. The first trial was shown immediately thereafter. For each trial, the stimulus remained on-screen until the voice key detected a response. The experimenter then coded the response, and the next trial appeared 1,000 ms later. Trials on which the voice key was set off by an irrelevant sound or speech (e.g., a cough or “um”) or imperceptible speech were coded as scratch trials and excluded from subsequent analysis. At the end of a list, the precue for the next list appeared. The four list types were randomly intermixed. RT and error rate were recorded for all trials within each list.

## Results

Trials on which responses were faster than 200 ms or slower than 3,000 ms were excluded, resulting in the trimming of <1% of the data. Additionally, error trials were excluded from the RT analysis. For this and all subsequent experiments, the alpha level was set at .05, partial eta-squared ( $\eta_p^2$ ) is reported as the measure of effect size, and other than those reported, no other effects were significant.

To examine list-level Stroop effects, a 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (trial type) within-subjects ANOVA was conducted on the RT data. A main effect of trial type,  $F(1, 21) = 208.49$ ,  $MSE = 4151$ ,  $p < .001$ ,  $\eta_p^2 = .908$ , indicated faster RTs on congruent ( $M = 638$ ,  $SE = 16$ ) than incongruent ( $M = 778$ ,  $SE = 20$ ) trials (i.e., the Stroop effect). This effect was qualified by a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 21) = 159.82$ ,  $MSE = 1019$ ,  $p < .001$ ,  $\eta_p^2 = .884$ , indicative of the list-wide proportion congruence pattern. A larger Stroop effect was found for MC ( $M = 201$  ms) compared with MI ( $M = 79$  ms) lists. Most importantly, a three-way interaction was found indicating that the list-wide proportion congruence effect was modulated by cueing,  $F(1, 21) = 4.70$ ,  $MSE = 1091$ ,  $p = .042$ ,  $\eta_p^2 = .183$ . The list-wide proportion congruence effect was significant in both the cued and uncued conditions, but the effect was larger in the cued condition ( $\eta_p^2 = .852$ ) than the uncued condition ( $\eta_p^2 = .692$ ; see Figure 1). Notably, the presence of the list-wide proportion congruence pattern in the uncued condition is consistent with the list-wide proportion congruence effect found in the typical paradigm in which much longer lists of MC and MI items are used.

To examine whether there was a cue-induced shift in either proportion congruence condition, we decomposed the three-way interaction by conducting separate 2 (cueing)  $\times$  2 (trial type) ANOVAs for the MC and MI conditions. For the MC condition, a significant Cueing  $\times$  Trial Type interaction was found indicating that the Stroop effect was greater in the cued ( $M = 220$  ms) than the uncued ( $M = 181$  ms) condition,  $F(1, 21) = 5.06$ ,  $MSE = 1694$ ,  $p = .035$ ,  $\eta_p^2 = .194$ . By contrast, for the MI condition, the interaction was not significant,  $F < 1$ . The magnitude of the

<sup>1</sup> We aimed to test 24 participants in each of the first three experiments. The final samples deviated from 24 because of participant sign-ups/show-ups by end of data collection period (e.g., end of the semester).

Stroop effect was equivalent in the cued ( $M = 78$  ms) and uncued ( $M = 81$  ms) conditions (see Figure 1).<sup>2</sup>

Mean error rates are shown in Table 1. A 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (trial type) within-subjects ANOVA was conducted for error rate. There was a main effect of proportion congruence,  $F(1, 21) = 28.20$ ,  $MSE = .002$ ,  $p < .001$ ,  $\eta_p^2 = .573$ , and trial type,  $F(1, 21) = 34.19$ ,  $MSE = .004$ ,  $p < .001$ ,  $\eta_p^2 = .619$ , that were qualified by a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 21) = 19.40$ ,  $MSE = .003$ ,  $p < .001$ ,  $\eta_p^2 = .480$ . Mirroring the RT data, there was a smaller Stroop effect in the MI condition ( $M = .022$ ) compared with the MC condition ( $M = .092$ ). Unlike the RT data, the three-way interaction was not significant,  $F < 1$ . Interference in error rate was very similar for the cued ( $M = .099$ ) and uncued MC ( $M = .084$ ) conditions, as well as the cued ( $M = .025$ ) and uncued ( $M = .018$ ) MI conditions.

## Discussion

Experiment 1 demonstrated that precueing the proportion congruence (i.e., likelihood of encountering interference) of an abbreviated list significantly affected the list-wide proportion congruence effect. The magnitude of the effect was larger when participants were cued than when they were not cued. Critically, this pattern resulted from a selective effect of list level precueing in the MC condition. There was a cue-induced MC shift (i.e., the Stroop effect in RT was significantly larger in the cued compared with uncued MC lists), suggesting that the expectation created by the cue had an effect on participants' performance. This could reflect enhanced facilitation and/or interference. Notably, the cue-induced MC shift was not accompanied by a marked exacerbation of error rates on incongruent trials in the cued compared with uncued MC condition, as would be expected if participants chose to read words rather than name colors upon presentation of a mostly matching cue. The shift is consistent with theories that posit that participants can choose to distribute attention across the color and word dimensions (Lowe & Mitterer, 1982) or increase the weight of the word dimension (Lindsay & Jacoby, 1994; Logan, 1980) when a list is MC.

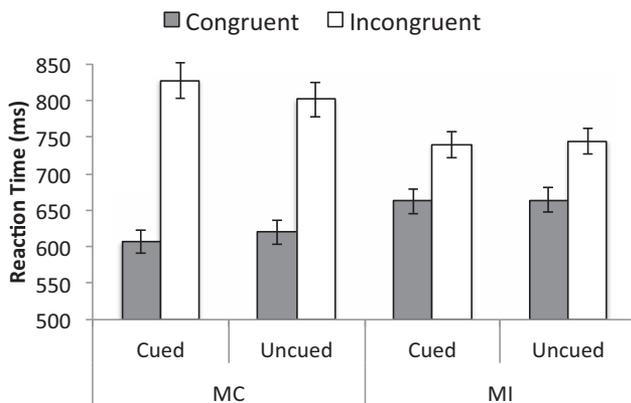


Figure 1. Mean reaction time as a function of list-wide proportion congruence and cueing for congruent and incongruent trials in Experiment 1. Means derived from performance on all 10 trials within a given list (i.e., list level). Error bars reflect standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

Table 1  
Mean Error Rate as a Function of List-Wide Proportion Congruency and Cueing in Experiment 1

Trial type	Condition			
	MC		MI	
	Cued	Uncued	Cued	Uncued
Congruent	.005 (.002)	.003 (.002)	.003 (.003)	.003 (.003)
Incongruent	.104 (.020)	.087 (.019)	.028 (.007)	.021 (.004)

Note. Means derived from performance on all 10 trials within a given list (i.e., list-level). Values in parentheses indicate standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

For the MI lists, the lack of a cue-induced MI shift (i.e., reduction in the Stroop effect in cued MI compared with uncued MI lists) suggests that use of the precues to bias attention away from the word dimension produced no benefit to performance (i.e., reduction in the Stroop effect) above and beyond the effects of experience with MI trials, or participants did not attempt to utilize the precues, explanations that will be pursued in subsequent experiments. That the effects of cueing varied tremendously depending on whether the precues signaled that most trials would be matching (conflict would be infrequent) or most trials would be conflicting (conflict would be frequent) is surprising in light of some prior accounts and findings (e.g., Braver et al., 2007; Lowe & Mitterer, 1982). Before considering the possible theoretical significance of this pattern, we first attempted to replicate the primary findings of Experiment 1.

## Experiment 2

Experiment 2 served as an attempt to systematically replicate the patterns observed in Experiment 1, and to preface the results, the cue-induced shift patterns were replicated. To seek converging evidence for the role of expectations, we examined whether shifts in control were evident on the first trial prior to experience with stimuli within the list. To enable a higher powered analysis of the Stroop effect in the first position, we collapsed the data from Experiments 1 and 2. An effect of expectations on the first trial would be supported by a cue-induced shift in the MC condition (i.e., larger Stroop effect on first trial in cued MC compared with uncued MC list) and/or a cue-induced shift in the MI condition (i.e., smaller Stroop effect on first trial in cued MI compared with uncued MI list).

A second goal of Experiment 2 was to contrast performance in the cued MC and MI conditions to performance in a cued 50% congruent condition. On the one hand, the cued 50% condition might be considered a baseline in which participants adopt a neutral bias (e.g., neither a bias toward nor away from the word dimension), and, as such, the Stroop effect might fall between the MC and MI conditions. On the other hand, prior findings from trial-by-trial precueing paradigms indicate that the biases adopted in response to explicit precues do not differ in cued 50% congruent

<sup>2</sup> An analysis of the proportional decrease in list-level interference in the cued MI relative to the uncued MI condition was conducted. Consistent with the primary analysis, the decrease was not significant.

and cued MI conditions because performance (i.e., Stroop effect) has been shown to be equivalent across such conditions (e.g., Gratton et al., 1992). If a similar pattern were obtained in the present experiment, it would suggest that the default (baseline mode) is to prepare for conflict when one expects conflict to occur on approximately 50% of trials within a given list.

## Method

**Participants.** Twenty undergraduates participated for course credit or monetary compensation (\$10). All were native English speakers with normal or corrected vision and normal color vision.

**Design and materials.** The design and materials were identical to Experiment 1, except that we added 50% congruent cued and uncued lists, resulting in a 2 (cueing: cued vs. uncued)  $\times$  3 (list-wide proportion congruence: MC vs. MI vs. 50% congruent)  $\times$  2 (trial type: congruent vs. incongruent) within-subjects design. A total of 48 lists were presented in this experiment.<sup>3</sup> Half were cued and half were uncued, and a third of the lists within the cued and uncued conditions were from each proportion congruence condition (MC, MI, 50% congruent).

**Procedure.** The procedure was identical to Experiment 1, except that following practice trials participants were also informed of the 50% congruent cue. Specifically, they were told the “50% MATCHING/CONFLICTING” cue meant the word and color would be the same for five of the 10 trials in the list and would differ for the other five trials.

## Results

We used the same trimming procedures as in the previous experiment, which eliminated <1% of trials for RTs faster than 200 ms or slower than 3,000 ms.

**Analysis of list-level Stroop effects.** A 2 (cueing)  $\times$  3 (proportion congruence)  $\times$  2 (trial type) within-subjects ANOVA was conducted on the RT data. There was a main effect of trial type,  $F(1, 19) = 122.48$ ,  $MSE = 5145$ ,  $p < .001$ ,  $\eta_p^2 = .866$ , because of RTs being slower on incongruent ( $M = 716$ ,  $SE = 15$ ) than congruent ( $M = 614$ ,  $SE = 15$ ) trials. This effect was qualified by a significant Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 19) = 43.66$ ,  $MSE = 1287$ ,  $p < .001$ ,  $\eta_p^2 = .697$ , and a Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 19) = 10.24$ ,  $MSE = 614$ ,  $p < .001$ ,  $\eta_p^2 = .350$ , as in Experiment 1.

As a first step in decomposing this three-way interaction, we first examined whether cueing affected the magnitude of the Stroop effect differentially in the MC and MI conditions by conducting a 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (trial type) ANOVA. Replicating Experiment 1, there was a main effect of trial type,  $F(1, 19) = 132.51$ ,  $MSE = 3426$ ,  $p < .001$ ,  $\eta_p^2 = .875$ , that was qualified by a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 19) = 57.81$ ,  $MSE = 1910$ ,  $p < .001$ ,  $\eta_p^2 = .753$ . There was a smaller Stroop effect in the MI condition ( $M = 54$  ms) than the MC condition ( $M = 159$  ms), indicative of the list-wide proportion congruence pattern. There was also a Cueing  $\times$  Trial Type interaction,  $F(1, 19) = 6.63$ ,  $MSE = 1407$ ,  $p = .019$ ,  $\eta_p^2 = .259$ , and these interactions were qualified by a Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 19) = 12.72$ ,  $MSE = 748$ ,  $p = .002$ ,  $\eta_p^2 = .401$ . To decompose the interaction, 2 (cueing)  $\times$  2 (trial type) ANOVAs were conducted separately for

the MC and MI conditions. As in Experiment 1, there was a cue-induced MC shift whereby cueing significantly increased the Stroop effect in the MC condition ( $M = 190$  ms for cued vs. 129 ms for uncued),  $F(1, 19) = 11.76$ ,  $MSE = 1603$ ,  $p = .003$ ,  $\eta_p^2 = .382$ , but there was no cue-induced MI shift ( $M = 54$  ms for cued vs. 54 ms for uncued),  $F < 1$  for the Cueing  $\times$  Trial Type interaction (see Figure 2).

As a second step toward decomposing the three-way interaction from the omnibus ANOVA, we examined whether cueing affected the magnitude of the Stroop effect differentially in the 50% congruent and MI conditions. A 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (trial type) ANOVA was conducted for these two conditions. A main effect of trial type,  $F(1, 19) = 56.08$ ,  $MSE = 3927$ ,  $p < .001$ ,  $\eta_p^2 = .747$ , was qualified by a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 19) = 23.51$ ,  $MSE = 694$ ,  $p < .001$ ,  $\eta_p^2 = .553$ , indicating a reduction in the Stroop effect in the MI ( $M = 54$  ms) compared with the 50% congruent ( $M = 94$  ms) condition. The Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction was not significant,  $F < 1$ . The Stroop effect was reduced from 94 ms in the 50% congruent condition to 54 ms in the MI condition in both the uncued and cued conditions. This reflects that there was no cue-induced shift in the 50% condition, just as there was no cue-induced shift in the MI condition (as reported above; see Figure 2).

Mean error rates are presented in Table 2. For error rate, the 2 (cueing)  $\times$  3 (proportion congruence)  $\times$  2 (trial type) within-subjects ANOVA indicated significant main effects of trial type,  $F(1, 19) = 20.66$ ,  $MSE = .004$ ,  $p < .001$ ,  $\eta_p^2 = .521$ , and proportion congruence,  $F(1, 19) = 11.17$ ,  $MSE = .003$ ,  $p < .001$ ,  $\eta_p^2 = .370$ . These effects were qualified by a Cueing  $\times$  Trial Type interaction,  $F(1, 19) = 3.46$ ,  $MSE = .002$ ,  $p = .042$ ,  $\eta_p^2 = .154$ , because of a larger Stroop effect in the cued than uncued conditions, and a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 19) = 11.56$ ,  $MSE = .002$ ,  $p < .001$ ,  $\eta_p^2 = .378$ . Consistent with the RT data, the Stroop effect in error rate was largest in the MC condition ( $M = .083$ ), intermediate in the 50% congruent condition ( $M = .025$ ), and smallest in the MI condition ( $M = .002$ ). The Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction was not significant. Notably, as in Experiment 1, similar Stroop effects in error rate were found in the cued and uncued MC conditions ( $M$ s = .086 and .079, respectively), as well as the cued and uncued MI and 50% congruent conditions (see Table 2).

**Analysis of first position: Experiments 1 and 2 collapsed.** RTs from the first position were analyzed to determine whether the precues affected performance on the initial trial, prior to any experience (i.e., learning) accruing from exposure to the list. To increase power, we combined data from Experiments 1 and 2 such that the total sample size for the analysis was 42. (This was possible for the MC and MI conditions, but not the 50% congruent condition that was presented only in Experiment 2.) Because of the random presentation of congruent and incongruent stimuli, the trial type that occurred less frequently in a given proportion congruency list (e.g., incongruent items in a mostly congruent list) had instances of missing data. Because missing data were missing com-

<sup>3</sup> Two participants were administered 36 lists instead of 48 lists. Because there was an equal number of lists in all conditions, we did not exclude their data.

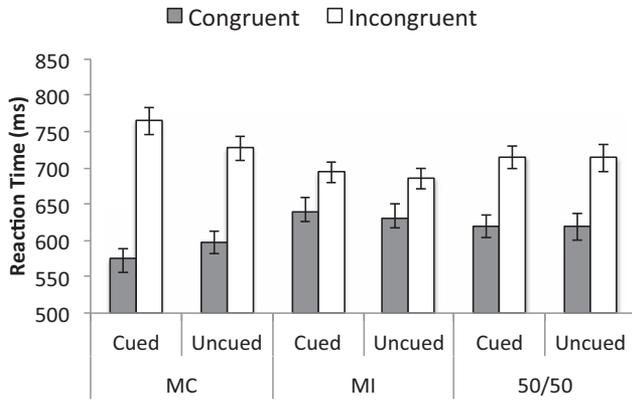


Figure 2. Mean reaction time as a function of list-wide proportion congruence and cueing for congruent and incongruent trials in Experiment 2. Means derived from performance on all 10 trials within a given list (i.e., list level). Error bars reflect standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

pletely at random (i.e., because of random presentation of trials within lists across participants), missing cases were replaced using mean imputation (percent of cases missing for Experiment 1 [Experiment 2]: cued MI congruent, 13.6% [10.0%]; uncued MI congruent, 18.2% [35.0%]; cued MC incongruent, 22.7% [30.0%]; uncued MC incongruent, 18.2% [30.0%]). The mean RT of the first position with regard to proportion congruence, trial type, and experiment was imputed. For example, participants who had a missing value in Experiment 1 for cued MC incongruent items in Position 1 had the Experiment 1 sample mean for cued MC incongruent items in Position 1 imputed for that missing value.

To examine the effects of cueing on performance on the first trial in the MC and MI condition, a 2 × 2 × 2 × 2 mixed-design ANOVA was conducted with experiment as the between-subjects factor, and cueing, proportion congruence, and trial type as within-subjects factors. The main effect of experiment was not significant,  $F(1, 40) = 1.66, MSE = 63401, p = .205, \eta_p^2 = .040$ , nor were any interactions involving this factor,  $ps > .45$  and  $\eta_p^2 < .015$ . The main effect of trial type was significant,  $F(1, 40) = 44.12, MSE = 16069, p < .001, \eta_p^2 = .524$ , a result of quicker RTs on congruent ( $M = 716, SE = 16$ ) compared with incongruent ( $M = 808, SE = 15$ ) trials. A Proportion Congruence × Trial Type interaction was found,  $F(1, 40) = 12.27, MSE = 9996, p < .001, \eta_p^2 = .235$ , along with a Cueing × Trial Type interaction,  $F(1, 40) = 5.97, MSE = 9640, p = .019, \eta_p^2 = .130$ . These interactions were qualified by a

significant Cueing × Proportion Congruence × Trial Type interaction,  $F(1, 40) = 9.39, MSE = 10181, p = .004, \eta_p^2 = .190$ . As can be seen in Figure 3, the proportion congruence pattern (i.e., smaller Stroop effect in MI compared with MC lists) was evident in the cued,  $F(1, 41) = 17.85, p < .001, \eta_p^2 = .303$ , but not the uncued,  $F < 1$ , condition. Of primary interest was whether cue-induced shifts in the MC and/or MI condition were responsible for this pattern. To examine this question, 2 (cueing) × 2 (trial type) within-subjects ANOVAs were performed for each proportion congruence condition. For MC lists, the main effect of cueing,  $F(1, 41) = 5.14, MSE = 9457, p = .029, \eta_p^2 = .111$ , and the main effect of trial type,  $F(1, 41) = 45.94, MSE = 15709, p < .001, \eta_p^2 = .528$ , were significant. Most importantly, there was a cue-induced shift on the first trial as indicated by the significant Cueing × Trial Type interaction,  $F(1, 41) = 13.20, MSE = 11319, p = .001, \eta_p^2 = .244$ . There was a larger Stroop effect in the cued MC condition ( $M = 191$ ) compared with the uncued MC condition ( $M = 71$ ; see Figure 3). For MI lists, by contrast, there was no cue-induced shift ( $F < 1$  for the two-way interaction) on the first trial. Only the main effect of trial type was significant,  $F(1, 41) = 12.12, MSE = 10052, p = .001, \eta_p^2 = .228$ .

The same 2 × 2 within-subjects ANOVAs were conducted for error rate to confirm that the error data did not contradict the RT patterns, and they did not. For MC lists, the main effect of trial type was significant,  $F(1, 41) = 6.09, MSE = 0.008, p = .018, \eta_p^2 = .129$ , because of a greater number of errors on incongruent trials ( $M = .036, SE = .014$ ) compared with congruent trials ( $M = .002, SE = .002$ ). Neither the interaction nor the main effect of cueing were significant, both  $F_s < 1$ . For MI lists, neither main effect was significant, both  $F_s < 1$ , nor was the interaction,  $F(1, 41) = 1.44, MSE = 0.010, p = .237, \eta_p^2 = .034$ .

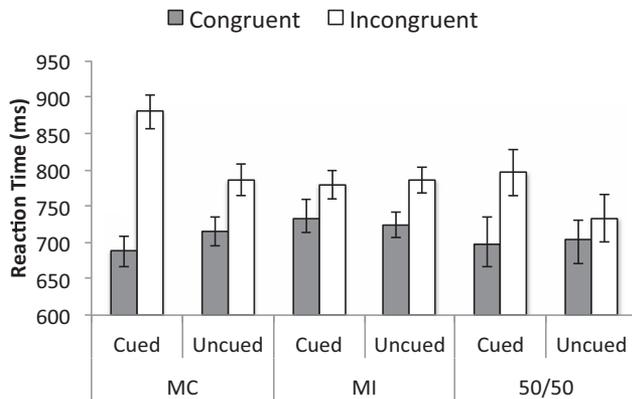
For completeness, we also examined whether a cue-induced shift was apparent on the first trial for the 50% congruent lists by conducting a 2 (cueing) × 2 (trial type) within-subjects ANOVA following mean imputation (percent of cases missing for uncued 50% congruent list congruent, 5.0%; uncued 50% congruent list incongruent, 5.0%; no cases were missing on cued lists). A similar pattern was found as in the MI lists—there was no cue-induced shift (for the two-way interaction,  $F[1, 19] = 2.22, MSE = 10377, p = .153, \eta_p^2 = .105$ ). Only the main effect of trial type was significant,  $F(1, 19) = 12.80, MSE = 6399, p = .002, \eta_p^2 = .402$ . The same was true for error rate, although the main effect of trial type approached significance,  $F(1, 19) = 3.64, MSE = 0.016, p = .072, \eta_p^2 = .161$ . For theoretical reasons, we additionally compared the first-position performances between the cued 50% congruent and cued MI lists. The purpose was to examine if there was any

Table 2  
Mean Error Rate as a Function of List-Wide Proportion Congruency and Cueing in Experiment 2

Trial type	Condition					
	MC		MI		50/50	
	Cued	Uncued	Cued	Uncued	Cued	Uncued
Congruent	.010 (.003)	.006 (.003)	.007 (.005)	.009 (.005)	.006 (.003)	.036 (.010)
Incongruent	.096 (.018)	.085 (.030)	.038 (.007)	.028 (.006)	.014 (.003)	.033 (.007)

Note. Means derived from performance on all 10 trials within a given list (i.e., list-level). Values in parentheses indicate standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

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**Figure 3.** Mean reaction time for first-position items in Experiments 1 and 2 as a function of list-wide proportion congruency and cueing. MC and MI values are from data collapsed across Experiments 1 and 2 ( $n = 42$ ). Fifty-percent congruent data are from Experiment 2 only ( $n = 20$ ). Error bars reflect standard error of the mean. MC = mostly congruent; MI = mostly incongruent; 50/50 = 50% congruent.

evidence for an intentional shift in control on the first trial following receipt of the MI cue compared with the 50% congruent cue that may not have been detected in the comparison of Stroop effects at the list level (see Analysis of List-Level Stroop Effects subsection). For RT, the Stroop effect was equivalent for the MI and 50% congruent lists,  $F(1, 19) = 2.81$ ,  $MSE = 6413$ ,  $p = .110$ ,  $\eta_p^2 = .129$  (see Figure 3). The same was true for error rate ( $F < 1$  for Proportion Congruence  $\times$  Trial Type interaction).

## Discussion

Experiment 2 replicated the patterns observed in Experiment 1. First, the list-wide proportion congruence effect was affected by the cueing manipulation such that a larger list-level Stroop effect was found under cued compared with uncued conditions. Second, this pattern reflected entirely a cue-induced MC shift (i.e., Stroop effect in RT was significantly increased in the cued relative to the uncued MC condition). The Stroop effect was invariant across the cued and uncued MI conditions (i.e., no cue-induced MI shift). Third, the heightened Stroop effect in RT in the MC condition was not accompanied by heightened error rates on incongruent trials, again supporting that an attentional bias (i.e., distribution of attention across word and color; Lowe & Mitterer, 1982), and not a word-reading strategy, was implemented.

Extending the findings of Experiment 1 were the results of the first-position analyses. In line with the idea that expectations for interference can influence performance prior to any experience with the list (e.g., with conflict), there was a cue-induced shift in the MC condition on the first trial. This suggests that participants utilized the 80% matching precues to adjust control in an expectation-consistent fashion (e.g., by biasing attention toward the word dimension or distributing attention across dimensions). By contrast, there was no cue-induced shift in the MI or 50% congruent conditions. These patterns suggest that participants did not utilize these cues to adjust control in advance of the start of the list any more than they used the “?????” cue. It was additionally shown that the first-position Stroop effect was equivalent for cued

MI and cued 50% lists. This suggests that preparation prior to the onset of the first stimulus was equivalent for these two list types. That is, participants did not heighten control disproportionately more in response to cues signaling a 30% greater likelihood of interference.

The absence of a first-position difference between the cued MI and 50% conditions is critical with respect to the interpretation of the list-wide (list-level) proportion congruence effect observed in the present experiment. It was found that the Stroop effect was significantly attenuated in the MI condition compared with the 50% condition. The first-position results support that the decrease in the Stroop effect in the MI condition relative to the 50% congruent condition was entirely attributable to the differential experience (e.g., frequency of encountering incongruent trials) in these lists and not caused by differential expectations. In other words, the more optimal configuration of control in the MI list, as evidenced by the overall reduction in the Stroop effect within this list type, was experience-driven. This finding coincides with trial-by-trial precueing studies showing that intentional modulations of control are similar for MI and 50% precues (e.g., Gratton et al., 1992). Moreover, the fact that the first-position analysis revealed equivalent patterns of means for cued MI and cued 50% conditions, which did not differ from the corresponding uncued conditions (see Figure 3), suggests that the default is to prepare for conflict in the Stroop task when there is at least a 50% likelihood of experiencing conflict. In uncued conditions, the average likelihood of conflict was precisely 50% because all list types were equally likely following a “?????” cue.

## Experiment 3

In the prior experiments, there was no cue-induced MI shift. The Stroop effect was equivalent in MI lists in which participants were provided with no information about the proportion congruence of the list as when they were explicitly informed the list would be 80% incongruent. Although this finding is consistent with the concept of experience-driven control via, for example, implicit adaptations (e.g., increased attention to the color or decreased attention to the word when conflicting trials are experienced frequently, e.g., Blais et al., 2007; Botvinick et al., 2001; Melara & Algom, 2003; temporal learning, e.g., Schmidt 2013a, 2013b), it is surprising in light of some extant findings and accounts that anticipate a role for volition in the instantiation of cognitive control (e.g., Norman & Shallice, 1986; Posner & DiGirolamo, 1998), including in the list-wide proportion congruence paradigm (Lowe & Mitterer, 1982). Importantly, the finding cannot be explained by Kahneman’s (1973) suggestion that one rarely knows prior to beginning a task that it will require control, and therefore it is the actual attempt to perform a difficult task that often leads to the recruitment of cognitive resources. Our precued lists paradigm removed this limitation by making the need for control explicit prior to the first trial, and participants still showed no evidence of heightening control (above baseline) prior to the start of the list when it was expected that 80% of trials would involve conflict. This seems to suggest that participants still adopted a “wait and see” approach, despite the provision of the 80% conflicting precues.

The purpose of Experiment 3 was to examine whether participants would more fully utilize the mostly conflicting cues in a

speeded condition in which the Stroop stimuli were presented very briefly. The theoretical motivation was twofold. First, given the existence of dimensional imbalance in the color-word Stroop task, which is the faster access to the word than the color dimension (e.g., Melara & Algom, 2003), we expected the speed manipulation to place a premium on preparation. If unprepared for a briefly presented Stroop stimulus, the quickly accessed word should have a detrimental influence on response selection, thereby slowing performance and possibly increasing errors. Thus, we expected the speed manipulation to encourage participants to prepare in advance of stimulus onset by adopting a bias against word processing that could prevent or minimize this detrimental influence. Given this potential benefit, it was expected that participants might more fully use the MI precues in the speeded condition, thereby reducing the Stroop effect. Second, we thought it possible that a “wait and see” approach would be less advantageous in the speeded condition because it might be more difficult to gather information about the nature of the list (e.g., frequency of particular word-color pairings) when stimuli were briefly presented. The key question of interest was whether a cue-induced MI shift would be observed in the speeded condition either at the list level or on the first trial.

## Method

**Participants.** Twenty-two undergraduates participated for course credit or monetary compensation (\$10). All were native English speakers with normal or corrected vision and color vision.

**Design and materials.** The design and materials were identical to Experiment 1, except that we contrasted performance in speeded and unspeeded (standard speed used in Experiment 1) lists, resulting in a 2 (speed: speeded vs. unspeeded)  $\times$  2 (cueing: cued vs. uncued)  $\times$  2 (list-wide proportion congruence: MC vs. MI)  $\times$  2 (trial type: congruent vs. incongruent) within-subjects design. A total of 64 lists were presented in this experiment. Half were speeded and half were unspeeded. Within each speed condition, half were cued and half were uncued, and half of the lists within the cued and uncued conditions were from each proportion congruence condition (MC and MI).

**Procedure.** The procedure was identical to Experiment 1, with the following exceptions. Following the practice trials, participants were told that those trials represented the “slower speed” version of the task. Then they received eight additional practice trials for the speeded version of the task and were told those trials represented the “faster speed” version. In the faster speed version, the stimulus was presented for only 100 ms rather than until response, as in the unspeeded version used here and in the preceding experiments. After practice, participants were informed of, and encouraged to use, the proportion congruent precues. Then they were informed of the speed precues (“SLOWER SPEED” or “FASTER SPEED”). They were also encouraged to use these precues to prepare prior to each list. During the task, a single precue screen described the proportion congruent and speed precues (e.g., 80% CONFLICTING, SLOWER SPEED). Once participants read the information and were prepared to begin a given list, they pressed a response key. The first stimulus within the list was presented immediately thereafter.

## Results

We used the same trimming procedures as in the previous experiments, which eliminated <1% of trials for RTs faster than 200 ms or slower than 3,000 ms.

**Analysis of list-level Stroop effects.** A 2 (speed)  $\times$  2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (trial type) within-subjects ANOVA was conducted on the RT data. There was a main effect of trial type indicating slower responding on incongruent relative to congruent trials,  $F(1, 21) = 311.89$ ,  $MSE = 3956$ ,  $p < .001$ ,  $\eta_p^2 = .937$ . The speed manipulation was effective as indicated by faster responding in the speeded ( $M = 558$ ,  $SE = 24$ ) compared with the unspeeded ( $M = 708$ ,  $SE = 25$ ) condition,  $F(1, 21) = 433.37$ ,  $MSE = 4611$ ,  $p < .001$ ,  $\eta_p^2 = .954$ . Moreover, the Speed  $\times$  Trial Type interaction indicated that participants exhibited a smaller Stroop effect in the speeded ( $M = 110$  ms) than the unspeeded ( $M = 126$  ms) condition,  $F(1, 21) = 5.80$ ,  $MSE = 978$ ,  $p = .025$ ,  $\eta_p^2 = .216$ . There were two additional two-way interactions, including a Cueing  $\times$  Trial Type interaction,  $F(1, 21) = 10.04$ ,  $MSE = 722$ ,  $p = .005$ ,  $\eta_p^2 = .323$ , and a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 21) = 182.13$ ,  $MSE = 1157$ ,  $p < .001$ ,  $\eta_p^2 = .897$ , which were qualified by a Cueing  $\times$  Proportion Congruence  $\times$  Trial type interaction,  $F(1, 21) = 21.96$ ,  $MSE = 535$ ,  $p < .001$ ,  $\eta_p^2 = .511$ . Replicating Experiments 1 and 2, the list-wide proportion congruence effect varied as a function of cueing such that a larger effect was found in the cued condition ( $\eta_p^2 = .877$ ) than the uncued condition ( $\eta_p^2 = .601$ ). And, as in the prior experiments, decomposing the interaction by conducting 2 (Cueing)  $\times$  2 (Trial Type) ANOVAs revealed that there was a cue-induced MC shift (i.e., larger Stroop effect in the cued than uncued MC condition),  $F(1, 21) = 12.27$ ,  $MSE = 822$ ,  $p = .002$ ,  $\eta_p^2 = .369$ , but cueing had no effect on the Stroop effect in the MI condition,  $F < 1$ . As Figure 4 illustrates, these patterns were evident for speeded as well as unspeeded lists ( $F < 1$  for four-way interaction).

For error rate, the omnibus ANOVA indicated main effects of proportion congruence,  $F(1, 21) = 23.08$ ,  $MSE = .003$ ,  $p < .001$ ,  $\eta_p^2 = .370$ , and trial type,  $F(1, 21) = 33.47$ ,  $MSE = .003$ ,  $p < .001$ ,  $\eta_p^2 = .370$ , that were qualified by an interaction between these two factors, indicating the list-wide proportion congruence effect,  $F(1, 21) = 11.17$ ,  $MSE = .003$ ,  $p < .001$ ,  $\eta_p^2 = .370$  (see Table 3 for mean error rates).

**Analysis of first position.** Mean imputation was used to handle missing cases, as in the prior experiment (percent cases missing: cued MI congruent, 27.3%; uncued MI congruent, 9.1%; cued MC incongruent, 45.5%; uncued MC incongruent, 31.8%; speeded cued MI congruent, 13.6%; speeded uncued MI congruent, 18.2%; speeded cued MC incongruent, 18.2%; speeded uncued MC incongruent, 27.3%). RTs for the first position were submitted to a 2  $\times$  2  $\times$  2  $\times$  2 ANOVA with cueing, speed, proportion congruence, and trial type as within-subjects factors. Main effects of speed,  $F(1, 21) = 215$ ,  $MSE = 14047$ ,  $p < .001$ ,  $\eta_p^2 = .911$ , and trial type,  $F(1, 21) = 59.83$ ,  $MSE = 16229$ ,  $p < .001$ ,  $\eta_p^2 = .740$ , were qualified by a Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 21) = 11.67$ ,  $MSE = 16321$ ,  $p = .003$ ,  $\eta_p^2 = .357$ . As can be seen in Figure 5, the proportion congruence pattern (i.e., smaller Stroop effect in MI compared with MC lists) was evident in the cued,  $F(1, 21) = 13.38$ ,  $p = .001$ ,  $\eta_p^2 = .389$ , but not the uncued,  $F(1, 21) = 1.99$ ,  $p = .173$ ,  $\eta_p^2 = .086$ , condition. There

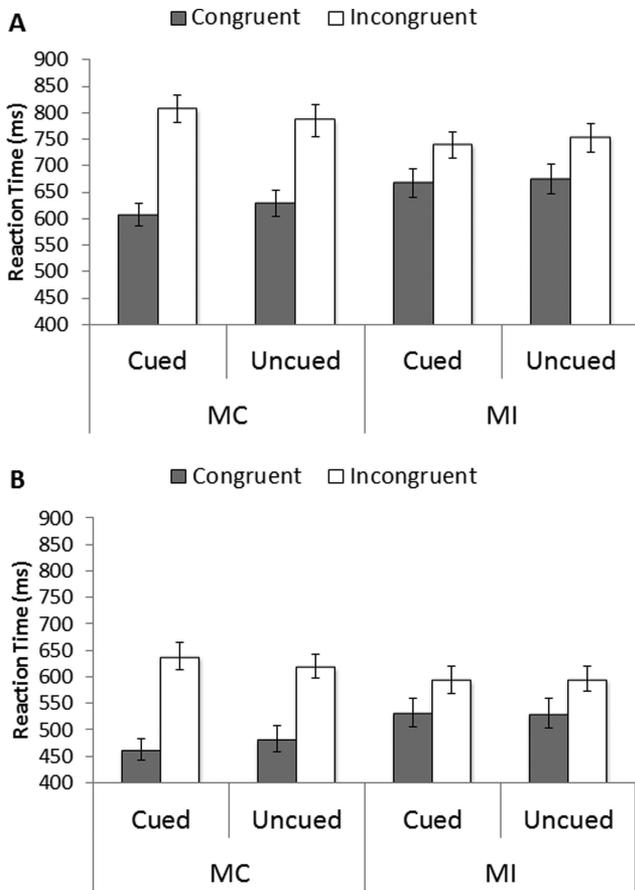


Figure 4. Mean reaction time in Experiment 3 as a function of list-wide proportion congruency and cueing for unspeeded (A) and speeded (B) conditions. Means derived from performance on all 10 trials within a given list (i.e., list-level). Error bars reflect standard error of the mean. Note that the range of the scale is 500 ms to illustrate the effect of the speed manipulation, and not 350 ms, which is used in all other figures. MC = mostly congruent; MI = mostly incongruent.

was also a Speed  $\times$  Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 21) = 4.43, MSE = 6613, p = .047, \eta_p^2 = .174$ . The proportion congruence pattern was marginally significant in the unspeeded condition,  $F(1, 21) = 2.99, MSE = 7496, p = .098, \eta_p^2 = .125$  (Stroop effect in MC lists was 131 ms compared with

67 ms on MI lists), and nonsignificant in the speeded condition,  $F < 1$ . The four-way interaction was not significant,  $F < 1$ .

Because the primary theoretical question of interest was whether a cue-induced MI shift would be observed in the speeded condition, we conducted 2 (cueing)  $\times$  2 (trial type) ANOVAs to examine the cue-induced shift in each speed by proportion congruence condition. For speeded MI lists, the main effect of trial type was significant,  $F(1, 21) = 32.19, MSE = 9117, p < .001, \eta_p^2 = .605$ . Most importantly, the Cueing  $\times$  Trial Type interaction indicated a strong trend for a cue-induced MI shift on the first trial,  $F(1, 21) = 4.23, MSE = 8187, p = .052, \eta_p^2 = .168$ . As shown in Figure 5, there was a 79-ms reduction in the Stroop effect in the cued MI compared with uncued MI lists. For unspeeded MI lists, as in the preceding experiments, the Cueing  $\times$  Trial Type interaction was not significant,  $F(1, 21) = 2.14, MSE = 11697, p = .158, \eta_p^2 = .093$ . Only the main effect of trial type was marginally significant,  $F(1, 21) = 3.76, MSE = 26471, p = .066, \eta_p^2 = .152$ . For speeded MC lists, a main effect of trial type,  $F(1, 21) = 34.55, MSE = 7204, p < .001, \eta_p^2 = .622$ , was qualified by a significant Cueing  $\times$  Trial Type interaction,  $F(1, 21) = 7.57, MSE = 10606, p = .012, \eta_p^2 = .265$ , because of a larger Stroop effect in cued ( $M = 167$ ) compared with uncued ( $M = 46$ ) lists. Similarly, for unspeeded MC lists, the main effect of trial type,  $F(1, 21) = 42.47, MSE = 8903, p < .001, \eta_p^2 = .669$ , was qualified by a significant Cueing  $\times$  Trial Type interaction,  $F(1, 21) = 6.24, MSE = 9634, p = .021, \eta_p^2 = .229$ , because of a larger Stroop effect in cued ( $M = 184$ ) compared with uncued ( $M = 79$ ) lists (i.e., a cue-induced MC shift; see Figure 5).

A series of 2 (cueing)  $\times$  2 (trial type) within-subjects ANOVAs confirmed that the first-position RT patterns were not contradicted by the error rate data. For speeded MI lists, the main effect of trial type was significant,  $F(1, 21) = 10.71, MSE = .003, p = .004, \eta_p^2 = .338$ , because of more errors on incongruent ( $M = .037, SE = .011$ ) compared with congruent ( $M = .000, SE = .000$ ) trials. Neither the main effect of cueing nor the interaction were significant,  $F_s < 1$ . For unspeeded MI lists, neither the main effects of trial type,  $F(1, 21) = 1.56, MSE = .016, p = .225, \eta_p^2 = .069$ , or cueing,  $F(1, 21) = 2.47, MSE = 0.012, p = .131, \eta_p^2 = .105$ , were significant; nor was the interaction,  $F < 1$ . For speeded MC lists, the main effect of trial type was not significant,  $F(1, 21) = 3.09, MSE = .011, p = .093, \eta_p^2 = .128$ , nor was the main effect of cueing or the interaction, both  $F_s < 1$ . For unspeeded MC lists, the main effect of trial type was significant,  $F(1, 21) = 10.80, MSE =$

Table 3

Mean Error Rate as a Function of List-Wide Proportion Congruency and Cueing for the Unspeeded and Speeded Conditions in Experiment 3

Speed	Trial type	Condition			
		MC		MI	
		Cued	Uncued	Cued	Uncued
Unspeeded	Congruent	.004 (.002)	.007 (.002)	.014 (.008)	.000 (.000)
	Incongruent	.091 (.018)	.091 (.024)	.040 (.008)	.034 (.007)
Speeded	Congruent	.004 (.002)	.002 (.001)	.009 (.005)	.003 (.003)
	Incongruent	.088 (.019)	.080 (.018)	.028 (.004)	.020 (.005)

Note. Means derived from performance on all 10 trials within a given list (i.e., list-level). Values in parentheses indicate standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

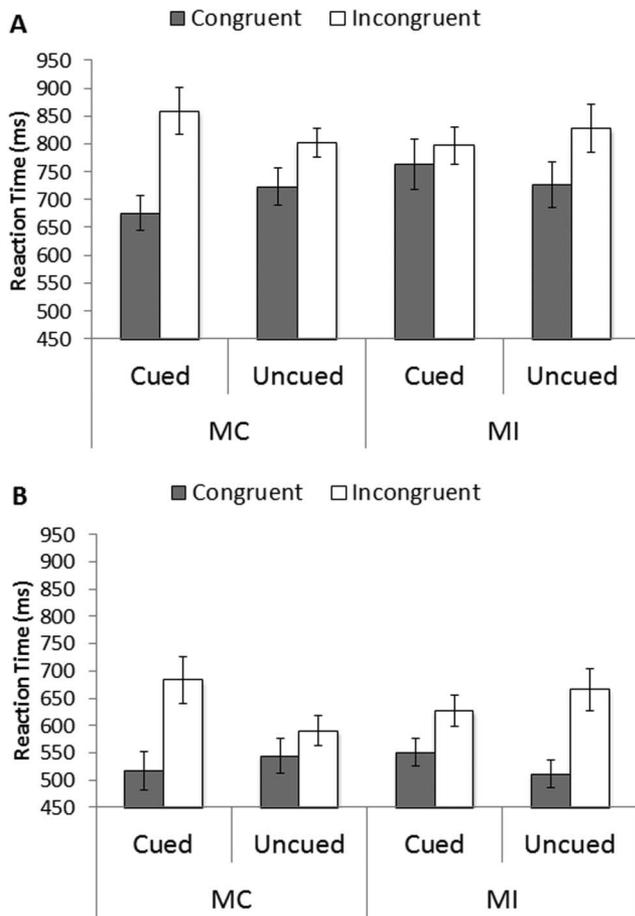


Figure 5. Mean reaction time for first-position items in Experiment 3 as a function of list-wide proportion congruency and cueing for unspeeded (A) and speeded (B) conditions. Error bars reflect standard error of the mean. Note that the range of the scale is 500 ms to illustrate the effect of the speed manipulation, and not 350 ms, which is used in all other figures. MC = mostly congruent; MI = mostly incongruent.

.065,  $p = .004$ ,  $\eta_p^2 = .340$ . There was a greater number of errors on incongruent trials ( $M = .202$ ,  $SE = .052$ ) compared with congruent trials ( $M = .024$ ,  $SE = .010$ ). Neither the interaction nor the main effect of cueing were significant, both  $F_s < 1$ .

## Discussion

The novel question addressed in Experiment 3 was whether a cue-induced MI shift would be observed in a speeded condition in which stimuli were briefly presented. The speed manipulation was effective in speeding RTs, and, as expected, there was a smaller Stroop effect when participants had to respond more quickly. Most interestingly, although the speed manipulation did not produce a cue-induced MI shift in the list-level Stroop effect, there was suggestive evidence for a cue-induced MI shift in the first-position Stroop effect patterns. The magnitude of the Stroop effect was attenuated by 79 ms on the first trial in the cued MI compared with uncued MI condition. This evidence is preliminary, however, as the reduction in the Stroop effect was not quite statistically sig-

nificant ( $p = .052$ ). Still, this is the first hint of a cue-induced MI shift in the present study, and it raises the possibility that participants *can* intentionally heighten control in response to 80% conflicting cues, but may not be able to *intentionally* sustain control across multiple trials (as suggested by the list-level interference data), a possibility we examine further in Experiment 4.

In addition to these novel patterns, we found a cue-induced MC shift but not a cue-induced MI shift at the list level in the unspeeded condition, and the MC shift was not accompanied by an exacerbation of error rates on incongruent trials, further replicating Experiments 1 and 2. Additionally, in the unspeeded condition, we replicated the pattern of a cue-induced MC shift, but no cue-induced MI shift, on the first trial prior to experience with the list. The cue-induced MC shift was unaffected by the speed manipulation. This suggests that participants were as apt to engage expectation-driven adjustments (e.g., distributing attention across color and word dimension) in response to an 80% matching precue when the stimulus presentation was speeded as when it was not.

## Experiment 4

Imposing an external pressure to optimally utilize precues led to preliminary evidence of a cue-induced MI shift in the first-position analysis of Experiment 3. In Experiment 4, we followed up on this result. To increase power, we more than doubled the sample size. In addition, we elected to internalize the source of pressure by incentivizing improved task performance. Much recent work has shown that incentivizing task performance leads to a greater engagement of cognitive control (Stürmer, Nigbur, Schacht, & Sommer, 2011). For instance, Padmala and Pessoa (2011) had participants perform a response conflict task (i.e., identifying an image while ignoring a word) in an fMRI scanner, while alternating between incentivized and nonincentivized conditions. Behavioral results suggested improved filtering of irrelevant information, as both interference and facilitation effects were dampened on incentivized trials. Also, in addition to greater activation in areas of the brain associated with reward processing, areas associated with controlled processing behaved in ways one would expect if incentives enhanced prestimulus preparation (i.e., proactive control). Specifically, the medial prefrontal cortex reacted less to response conflict, whereas there was decreased activation in word-processing regions (Bolger, Perfetti, & Schneider, 2005) and increased activation in attentional control regions (Corbetta & Shulman, 2002). Similarly, using a modified version of the Stroop task, Veling and Aarts (2010) found that participants made faster responses and committed fewer errors on high-incentive compared with low-incentive incongruent trials, suggesting a greater adherence to task goals (cf. Krebs, Boehler, & Woldorff, 2010).

Importantly, there is also evidence that incentives not only lead to transient increases in on-task effort on a trial-by-trial basis but also increase effort over the course of a block of trials. Chiew and Braver (2013), using pupillometry during an AX-continuous performance task, found a tonic increase in effort over the course of incentivized blocks along with a transient increase in effort on incentivized trials compared with nonincentivized trials. These pupil dilation patterns were accompanied by behavioral performance patterns that were indicative of proactive control. Consid-

ering the extant evidence for improved proactive control and sustained engagement of effort during incentivized task performance, it seemed plausible that there would be evidence for the utilization of MI cues in the precued lists paradigm both in the list-level and first-position performance data. Thus, in Experiment 4, it was hypothesized that a cue-induced MI shift would emerge in the high-incentive condition. As in Experiment 3, we also examined the effects of pressure on the cue-induced MC shift. We thought it possible that this shift could *increase* in magnitude in a high-incentive relative to a low-incentive condition.

## Method

**Participants.** A sample of 49 undergraduates participated for course credit or monetary compensation (\$10). Participants were native English speakers with normal or corrected vision and normal color vision. A single participant was excluded from analysis for falling asleep during the experimental procedure; the final sample was therefore 48.<sup>4</sup>

**Design and procedure.** The procedure was a modified version of Experiment 1, changed in three notable ways to implement an incentives manipulation: (a) a baseline assessment was added before the experimental lists to determine an individual's criterion for earning an incentive, (b) a point value cue was presented before each list of Stroop items during the experimental lists, and (c) performance feedback was presented after each list of Stroop items during the experimental lists. This resulted in a 2 (cueing)  $\times$  2 (incentive)  $\times$  2 (proportion congruence)  $\times$  2 (trial type) within-subjects design.

As in the preceding experiments, participants were initially provided instructions on how to respond to Stroop items (i.e., name aloud the color as quickly as possible without sacrificing accuracy) and were familiarized with the precues. Then the computer informed participants that they were going to practice using the precues. This practice period served as an estimate of baseline performance in the absence of incentives. Each of the four list types (e.g., cued MC) was presented twice at random, allowing for a maximum of 20 items per practice list, from which a mean RT could be calculated. Scratch trials or trials on which an error was made were excluded from the baseline average. Four separate baseline criteria were created, one for each list type (e.g., cued MC).

Following the baseline assessment, participants were introduced to the point incentives. They were informed that they would, from then on, receive points by responding quicker than they had during practice.<sup>5</sup> Participants were instructed to "maintain a high level of performance" as opposed to, for example, a 100% accuracy level, in order to not unduly influence participants into adopting any particular strategy to complete the task (see Footnote 5). Participants were not told the precise point values of the incentive manipulation before beginning the experimental lists. The experimental lists were administered in a fashion that was very similar to the preceding experiments. A list-wide proportion congruent precue (e.g., MC) was presented first until a response was made on a response box. Next, a point cue was displayed indicating either a low-incentive (5 point) list or a high-incentive (50 point) list, along with the participant's current score (points earned). When the participant pressed a key on the response box, a list of 10 Stroop stimuli was then presented as in the preceding experiments

(standard [unsped] version). Following conclusion of the list, a feedback slide was presented. The slide included a 3 s animation of a lengthening ellipsis, after which the participant's mean RT for the list was presented. If their mean RT was faster than their baseline criterion for the corresponding list type, the font color was changed to green and the participant was informed of how many points they earned, along with their updated score. If their mean RT was slower than their baseline for that given list type, the font color was changed to red and participants were informed that they earned no points for that list, along with their current score. The feedback slide remained on-screen until a response was made via the response box, beginning the next list. A total of 56 lists was presented per participant, with half being 5-point lists and half being 50-point lists. The four different list types (e.g., cued MC) were distributed equally between the 5-point and 50-point lists. At the end of the experimental lists, a series of debriefing questions were completed and the participant was shown their final score out of the total possible score. Participants were then thanked and dismissed.

## Results

We used the same trimming procedures as in the previous experiment, which eliminated <1% of trials for RTs faster than 200 ms or slower than 3,000 ms.

**Manipulation checks.** To determine whether the incentive manipulation was effective, we examined three measures.

**Self-reported motivation and effort.** Three items from the debriefing questionnaire were examined to assess perceptions of the incentive manipulation. Participants were asked, "Did you find the chance to score points motivating?" to which responses were recorded on a 4-point Likert scale (1 = *not at all motivating*, 2 = *a little motivating*, 3 = *moderately motivating*, 4 = *very motivating*). The mean score was 2.6 ( $SE = .14$ ,  $Mdn = 3.0$ ), suggesting participants found the point incentive motivating, but not to a very large degree. Additionally, participants were asked (in order), "How much effort did you put forth on 5-point lists?" and "How much effort did you put forth on 50-point lists?" to which responses were made on a 4-point Likert scale (1 = *no effort*, 2 = *little effort*, 3 = *some effort*, 4 = *great effort*). For 5-point lists, the mean response was 3.1 ( $SE = .12$ ,  $Mdn = 3.0$ ), suggesting participants, on average, felt they had exerted a moderate amount

<sup>4</sup> We doubled our recruitment goal to 48 participants in order to maximize power to detect the first trial cue-induced MI shift, which was nominally of a large magnitude (79-ms shift) but only approached significance ( $p = .052$ ) in Experiment 3.

<sup>5</sup> During pilot testing, participants were required to perform better than the 30th percentile of their baseline performance, following Chiew and Braver (2013), but this proved to be excessively difficult. The criterion was then modified to require performance better than their median, which lowered difficulty. The mean of baseline performance was finally chosen, however, in order to aid interpretability of success rates. By using baseline means, success rates would thus be interpreted as improving mean list performance against mean baseline performance, instead of improving mean list performance against median baseline performance. Also during pilot testing, participants were required to not make a single error during a list in order to receive incentives. However, this criterion was removed in order to prevent participants from being overly preoccupied with accuracy, leading them to potentially no longer utilize the precues proactively and instead adopt a strategy of waiting for the stimulus and taking as much time as needed to respond accurately.

of effort on 5-point lists. For 50-point lists, the mean response was 3.4 ( $SE = .12$ ,  $Mdn = 4.0$ ), suggesting participants, on average, felt they had exerted a moderate amount of effort on 50-point lists as well—though the median indicated a negative skewing of the distribution (visual inspection of the distributions confirmed that both distributions were negatively skewed). Responses on the effort questions were subjected to a Wilcoxon signed-ranks test to determine if participants subjectively felt they exerted more effort on 50-point lists than 5-point lists. Results of the analysis suggested as such,  $z = -3.63$ ,  $p < .001$ , the difference of which was a large effect,  $r = .52$ .

**Success rates.** Over all subjects and conditions, the mean rate of success to earn an incentive without committing an error was 51.4% ( $SE = 2.34$ ). On 20.1% ( $SE = 1.37$ ) of trials, participants did not commit an error but failed to speed responding sufficiently above baseline RTs to earn an incentive. On 28.5% ( $SE = 2.36$ ) of trials, an error was committed. Participants were slightly more successful in high-incentive conditions ( $M = 52.3%$ ,  $SE = 2.51$ ) than low-incentive conditions ( $M = 50.5%$ ,  $SE = 2.38$ ), but not significantly,  $t(47) = -1.29$ ,  $p = .203$ ,  $\eta^2 = .034$  (see Table 4). Incentives were earned most frequently within the cued MC condition ( $M = 69.8%$ ,  $SE = 3.06$ ), whereas the success rate dropped to 56.4% ( $SE = 3.46$ ) in the uncued MC condition,  $t(47) = 3.89$ ,  $p < .001$ ,  $\eta^2 = .243$ . An opposite pattern was observed between cued and uncued MI conditions. In the uncued MI condition, the rate of success was 57.9% ( $SE = 2.96$ ), which dropped to 21.4% ( $SE = 3.22$ ) in the cued MI condition,  $t(47) = 9.88$ ,  $p < .001$ ,  $\eta^2 = .675$  (see Table 4). The drop in success for cued MI conditions seemed to be due largely to an increased failure rate in the cued MI condition ( $M = 48.4%$ ,  $SE = 3.10$ ) compared with the uncued MI condition ( $M = 13.1%$ ,  $SE = 2.04$ ),  $t(47) = 9.85$ ,  $p < .001$ ,  $\eta^2 = .674$ , because error rate was consistent across both conditions (cued MI:  $M = 30.2%$ ,  $SE = 2.75$ ; uncued MI:  $M = 29.0%$ ,  $SE = 2.64$ ),  $t(47) = .548$ ,  $p = .586$ ,  $\eta^2 = .006$ . Table 4 provides rates for the precue and incentive conditions.

**Point cue dwell time.** The amount of time spent dwelling on point cues was analyzed to determine whether participants spent more time preparing for the upcoming Stroop list when a high-incentive as opposed to low-incentive cue was shown. Point cues remained on-screen until the participant responded on a response

Table 4  
Mean Rate (%) of Outcomes Across Study Conditions in Experiment 4

Condition	Rate		
	Success <sup>a</sup>	Failure <sup>b</sup>	Error <sup>c</sup>
Global	51.4 (2.34)	20.1 (1.37)	28.5 (2.36)
High incentive	52.3 (2.51)	18.3 (1.50)	29.4 (2.45)
Low incentive	50.5 (2.38)	21.9 (1.50)	27.7 (2.49)
MC lists	69.8 (3.06)	0.0 (0.0)	30.2 (3.06)
MC lists (uncued)	56.4 (3.46)	18.9 (3.14)	24.7 (2.57)
MI lists	21.4 (3.22)	48.4 (3.10)	30.2 (2.75)
MI lists (uncued)	57.9 (2.96)	13.1 (2.04)	29.0 (2.64)

Note. Values in parentheses indicate standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

<sup>a</sup> Met incentive criteria without an error. <sup>b</sup> Did not meet incentive criteria, no committed error. <sup>c</sup> Committed an error, whether or not incentive criteria were met.

box. Results of a 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (incentive) within-subjects ANOVA indicated a significant main effect of Incentive,  $F(1, 47) = 8.06$ ,  $MSE = 235824$ ,  $p = .007$ ,  $\eta_p^2 = .146$ , and a significant main effect of proportion congruence,  $F(1, 47) = 5.02$ ,  $MSE = 310569$ ,  $p = .030$ ,  $\eta_p^2 = .096$ . Participants dwelled longer on the point cue if it was a high-incentive cue ( $M = 1536$ ,  $SE = 88$ ) than if it was a low-incentive cue ( $M = 1396$ ,  $SE = 60$ ). Participants also dwelled longer on the point cue before seeing a MI list ( $M = 1530$ ,  $SE = 83$ ) than a MC list ( $M = 1402$ ,  $SE = 69$ ). No other effects were significant (all  $ps > .20$  and  $\eta_p^2s < .031$ ).

**Analysis of list-level Stroop effects.** A 2 (Cueing)  $\times$  2 (Incentive)  $\times$  2 (Proportion Congruence)  $\times$  2 (Trial Type) within-subjects ANOVA was conducted for RT. A main effect of incentive was found,  $F(1, 47) = 9.51$ ,  $MSE = 3509$ ,  $p = .003$ ,  $\eta_p^2 = .168$ , because of quicker RTs in the high-incentive condition ( $M = 595$ ,  $SE = 11$ ) compared with the low-incentive condition ( $M = 608$ ,  $SE = 11$ ). A main effect of cueing was indicated,  $F(1, 47) = 4.56$ ,  $MSE = 1166$ ,  $p = .038$ ,  $\eta_p^2 = .088$ , because of faster responding on uncued lists ( $M = 599$ ,  $SE = 11$ ) compared with cued lists ( $M = 604$ ,  $SE = 11$ ). The main effect of trial type was significant as well,  $F(1, 47) = 251.60$ ,  $MSE = 12070$ ,  $p < .001$ ,  $\eta_p^2 = .843$ , as a result of faster responding on congruent ( $M = 539$ ,  $SE = 9$ ) compared with incongruent ( $M = 664$ ,  $SE = 13$ ) trials. A marginally significant Cueing  $\times$  Trial Type interaction,  $F(1, 47) = 3.94$ ,  $MSE = 1270$ ,  $p = .053$ ,  $\eta_p^2 = .077$ , and a significant Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 47) = 115.42$ ,  $MSE = 2048$ ,  $p < .001$ ,  $\eta_p^2 = .711$ , were qualified by a significant three-way interaction of Cueing  $\times$  Proportion Congruence  $\times$  Trial Type,  $F(1, 47) = 9.88$ ,  $MSE = 1311$ ,  $p = .003$ ,  $\eta_p^2 = .174$ . To decompose the three-way interaction, separate Cueing  $\times$  Trial Type ANOVAs were conducted for MC and MI lists. For MC lists, a significant two-way interaction of Cueing  $\times$  Trial Type was present,  $F(1, 47) = 8.97$ ,  $MSE = 1897$ ,  $p = .004$ ,  $\eta_p^2 = .160$ , because of a larger Stroop effect in cued MC lists compared with uncued MC lists (i.e., cue-induced MC shift). For MI lists, only a main effect of trial type,  $F(1, 47) = 176.19$ ,  $MSE = 4480$ ,  $p < .001$ ,  $\eta_p^2 = .789$ , was detected. Neither the main effect of cueing,  $F(1, 47) = 1.42$ ,  $MSE = 1353$ ,  $p = .24$ ,  $\eta_p^2 = .029$ , nor the Cueing  $\times$  Trial Type interaction was significant,  $F(1, 47) = 1.36$ ,  $MSE = 684$ ,  $p = .25$ ,  $\eta_p^2 = .028$ .<sup>6</sup> As is apparent in Figure 6, these patterns were evident in both the low- and high-incentive conditions (four-way interaction,  $F[1, 47] = 2.26$ ,  $MSE = 835$ ,  $p = .139$ ,  $\eta_p^2 = .046$ ).

Mean error rates are presented in Table 5. A 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (incentive)  $\times$  2 (trial type) within-subjects ANOVA was conducted. A significant two-way interaction of Incentive  $\times$  Trial Type was revealed,  $F(1, 47) = 4.55$ ,  $MSE = .003$ ,  $p = .038$ ,  $\eta_p^2 = .088$ , indicating a greater number of errors on low-incentive congruent trials ( $M = .013$ ) than on high-incentive congruent trials ( $M = .009$ ). The Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction was significant,  $F(1, 47) = 4.49$ ,  $MSE = .002$ ,  $p = .039$ ,  $\eta_p^2 = .087$ . To decompose the interaction, we conducted a Cueing  $\times$  Trial Type ANOVA sepa-

<sup>6</sup> An analysis of the proportional decrease in list-level interference in the cued MI relative to the uncued MI condition was conducted. The results were consistent with the primary analysis.

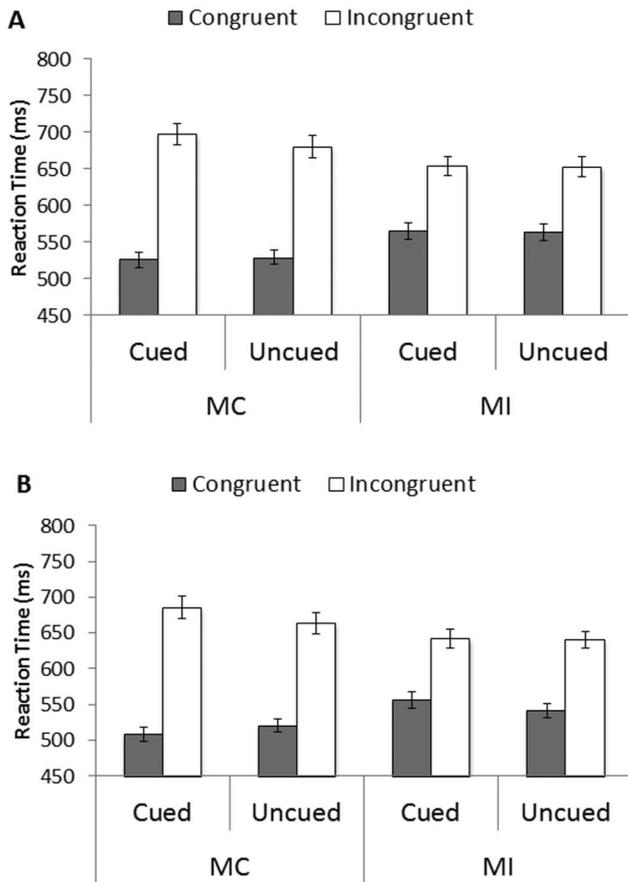


Figure 6. Mean reaction time in Experiment 4 as a function of list-wide proportion congruency and cueing for low-incentive (A) and high-incentive (B) conditions. Means derived from performance on all 10 trials within a given list (i.e., list-level). Error bars reflect standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

rately for MC and MI lists. For MC lists, a significant Cueing  $\times$  Trial Type interaction was revealed,  $F(1, 47) = 8.98, MSE = .030, p = .004, \eta_p^2 = .160$ , indicating a larger Stroop effect in error rate in cued MC lists ( $M = .138$ ) compared with uncued MC lists ( $M = .103$ ). For MI lists, a significant main effect of trial type was found,  $F(1, 47) = 37.03, MSE = .003, p < .001, \eta_p^2 = .441$ ,

indicating fewer errors on congruent trials ( $M = .012, SE = .003$ ) than on incongruent trials ( $M = .047, SE = .005$ ). Neither the main effect of cueing,  $F(1, 47) = 0.46, MSE = .001, p = .502, \eta_p^2 = .010$ , nor the Cueing  $\times$  Trial Type interaction,  $F(1, 47) = 2.55, MSE = .001, p = .117, \eta_p^2 = .052$ , was significant.

**Analysis of first position.** Mean imputation was used to replace missing cases within each proportion congruency and cueing condition (percent of cases missing: cued MC incongruent, 8.3%; uncued MC incongruent, 2.1%; cued MI congruent, 6.3%; uncued MI congruent, 6.3%). A 2 (cueing)  $\times$  2 (proportion congruence)  $\times$  2 (incentive)  $\times$  2 (trial type) within-subjects ANOVA was conducted on first trial RTs. There was no main effect or any interactions of incentive (all  $ps > .16$  and  $\eta_p^2 < .04$ ). A main effect of trial type,  $F(1, 47) = 102.19, MSE = 21220, p < .001, \eta_p^2 = .685$ , and a Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 47) = 12.79, MSE = 8957, p = .001, \eta_p^2 = .214$ , were qualified by a Cueing  $\times$  Proportion Congruence  $\times$  Trial Type interaction,  $F(1, 47) = 4.47, MSE = 11714, p = .04, \eta_p^2 = .087$ . Figure 7 shows that the proportion congruence pattern (i.e., smaller Stroop effect in MI compared with MC lists) was evident on the first trial in the cued condition,  $F(1, 47) = 12.28, p = .001, \eta_p^2 = .207$ , but not the uncued condition,  $F < 1$ , in both the high- and low-incentive conditions. Confirming this observation, the four-way interaction was not significant,  $F(1, 47) = 1.66, MSE = 9679, p = .204, \eta_p^2 = .034$ . However, Figure 7 also shows what appears to be a cue-induced shift in the high-incentive MI condition. To examine this theoretically predicted effect, and the other cue-induced shifts, we conducted separate 2 (cueing)  $\times$  2 (trial type) ANOVAs for each incentive by proportion congruence condition.

For high-incentive MI lists, the main effect of trial type was significant,  $F(1, 47) = 20.23, MSE = 12923, p < .001, \eta_p^2 = .301$ . Most importantly, the interaction was significant,  $F(1, 47) = 4.48, MSE = 7256, p = .040, \eta_p^2 = .087$ , indicating a cue-induced MI shift. There was a smaller Stroop effect in cued MI lists ( $M = 47$ ) compared with uncued MI lists ( $M = 100$ ). For low-incentive MI lists, the main effect of trial type was significant,  $F(1, 47) = 29.00, MSE = 13374, p < .001, \eta_p^2 = .382$ , but there was no cue-induced MI shift ( $F < 1$  for interaction). For high-incentive MC lists, the main effect of trial type was significant,  $F(1, 47) = 105.82, MSE = 8159, p < .001, \eta_p^2 = .692$ , qualified by a marginally significant Cueing  $\times$  Trial Type interaction,  $F(1, 47) = 3.07, MSE = 10023, p = .087, \eta_p^2 = .061$ , with Stroop effects of 159 ms in the cued condition and 108 ms in the uncued condition. For

Table 5  
Mean Error Rate as a Function of List-Wide Proportion Congruency and Cueing for the Low-Incentive and High-Incentive Conditions in Experiment 5

Incentive	Trial type	Condition			
		MC		MI	
		Cued	Uncued	Cued	Uncued
Low	Congruent	.012 (.003)	.008 (.002)	.011 (.004)	.020 (.006)
	Incongruent	.132 (.022)	.107 (.015)	.045 (.006)	.046 (.007)
High	Congruent	.008 (.002)	.009 (.002)	.007 (.003)	.011 (.004)
	Incongruent	.165 (.022)	.117 (.018)	.050 (.006)	.046 (.006)

Note. Means derived from performance on all 10 trials within a given list (i.e., list-level). Values in parentheses indicate standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

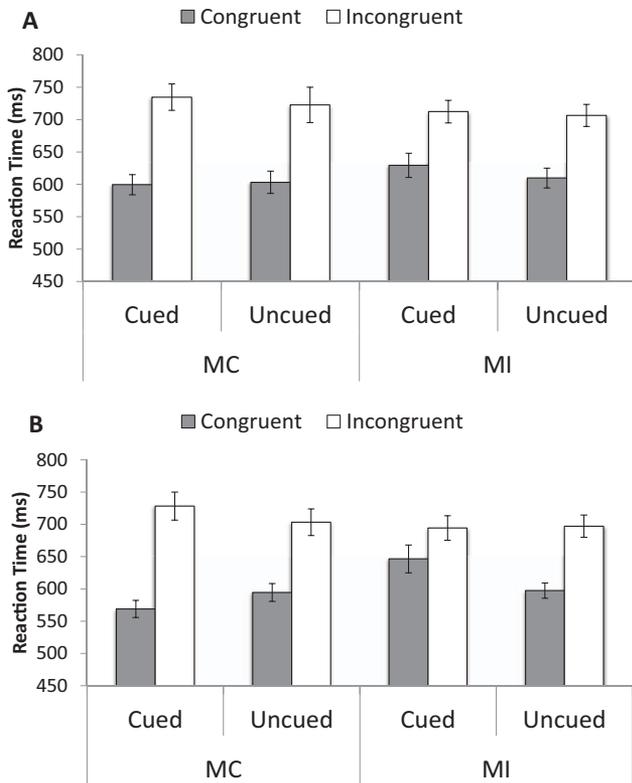


Figure 7. Mean reaction time for first-position items in Experiment 4 as a function of list-wide proportion congruency and cueing for low-incentive (A) and high-incentive (B) conditions. Error bars reflect standard error of the mean. MC = mostly congruent; MI = mostly incongruent.

low-incentive MC lists, the main effect of trial type was significant,  $F(1, 47) = 64.26$ ,  $MSE = 12103$ ,  $p < .001$ ,  $\eta_p^2 = .578$ , but the two-way interaction was nonsignificant,  $F < 1$ .

To confirm that the first-position RT patterns were not contradicted by the error rate data, an analogous series of 2 (cueing)  $\times$  2 (trial type) within-subjects ANOVAs were conducted. For high-incentive MI lists, only the main effect of trial type was significant,  $F(1, 47) = 12.07$ ,  $MSE = .002$ ,  $p = .001$ ,  $\eta_p^2 = .204$ , indicating a greater number of errors on incongruent ( $M = .023$ ,  $SE = .007$ ) compared with congruent trials ( $M = .000$ ,  $SE = .000$ ). The interaction was nonsignificant,  $F(1, 47) = 1.10$ ,  $MSE = .002$ ,  $p = .300$ ,  $\eta_p^2 = .023$ . For low-incentive MI lists, neither the main effect of trial type,  $F(1, 47) = 1.75$ ,  $MSE = .007$ ,  $p = .192$ ,  $\eta_p^2 = .036$ , cueing,  $F(1, 47) = 2.24$ ,  $MSE = .004$ ,  $p = .141$ ,  $\eta_p^2 = .046$ , nor the interaction,  $F < 1$ , were significant. For high-incentive MC lists, only the main effect of trial type was significant,  $F(1, 47) = 7.49$ ,  $MSE = .019$ ,  $p = .009$ ,  $\eta_p^2 = .137$ , indicating more errors being committed on incongruent ( $M = .062$ ,  $SE = .019$ ) compared with congruent ( $M = .007$ ,  $SE = .005$ ;  $F < 1$  for interaction) trials. For low-incentive MC lists, there was a main effect of trial type,  $F(1, 47) = 15.95$ ,  $MSE = .049$ ,  $p < .001$ ,  $\eta_p^2 = .253$ , and a main effect of cueing,  $F(1, 47) = 6.31$ ,  $MSE = .052$ ,  $p = .015$ ,  $\eta_p^2 = .118$ , which were qualified by a significant Cueing  $\times$  Trial Type interaction,  $F(1, 47) = 4.98$ ,  $MSE = .054$ ,  $p = .030$ ,  $\eta_p^2 = .096$ , unlike in the RT data. There was surprisingly a larger Stroop effect in

errors on the first trial in uncued MC ( $M = .201$ ) compared with cued MC ( $M = .053$ ) lists in the low-incentive condition.

## Discussion

By using incentives in Experiment 4, we tested the hypothesis that highly incentivized performance (relative to a low-incentive condition) would lead to a cue-induced MI shift both for list-level and first-trial Stroop effects. We also examined the possibility that the cue-induced MC shift might be more robust in the high-incentive condition. There was mixed evidence in support of these hypotheses. Incentives did have an effect on performance by speeding average RT in the high-incentive condition, as seen in prior studies using Stroop-like tasks (Krebs et al., 2010; Veling & Aarts, 2010). However, providing an internal motivation to utilize precues did not change the list-level Stroop effect patterns seen in Experiments 1 through 3—the typical cue-induced MC shift, but no corresponding cue-induced MI shift, remained. The incentive manipulation was, however, of value in revealing a cue-induced MI shift on the first trial. In the high-incentive condition, there was a significant decrease in the Stroop effect for cued MI compared with uncued MI lists, mirroring the strong trend that was observed in the speeded condition of Experiment 3. By contrast, there was no hint of a cue-induced MI shift in the low-incentive condition. These patterns may reflect that when performance is assigned a high priority (either via pressuring participants to respond more quickly or by making incentives available), the probability of preparation increases (i.e., there is a lower likelihood of a “failure to engage”; De Jong, 2000). The patterns also accord well with the theory of the expected value of control (Shenhav, Botvinick, & Cohen, 2013), which proposes that the amount of control allocated is determined by a consideration of the effort required, the expected reward, and how much control is needed to earn the reward. Providing incentives may have enhanced participants’ perceptions of the expected reward, biasing greater allocation of control, at least on the first trial.

As for the first trial of the MC condition, there was a cue-induced shift in the high-incentive condition in the form of a marginally significant 51-ms increase in the Stroop effect in the cued compared with uncued condition. Unlike the list-level cue-induced MC shift in this experiment, the first-position shift was not accompanied by an exacerbation of error rates on incongruent trials. This suggests that participants did not initially use a strategy of word reading but may have switched to one at a later point in the list. In the low-incentive condition, there was no indication of a cue-induced MC shift on the first trial (and there was, strangely, a larger Stroop effect in error rate in the uncued than cued MC condition). The latter is at odds with Experiments 1 and 2, and with Experiment 3’s finding of a first-position cue-induced MC shift in RT (with no effect in error) in what might also be considered low (or no) incentive conditions (i.e., the standard/unspeeded conditions in those experiments). Possibly, participants viewed the 5-point cues as an opportunity to take a break, or, alternatively, this pattern may be an anomaly.

The evidence for a cue-induced MI shift and a trend for a cue-induced MC shift on the first-trial selectively in the high-incentive condition converge with participants’ reports that they found the high-incentive condition to be more motivating, and with the finding that participants dwelled 140 ms longer on high-

incentive point cues than on low-incentive point cues. Participants also dwelled 128 ms longer before MI lists than MC lists in general. Presuming participants were utilizing dwell time to prepare for the upcoming list, the first-position data suggest that such preparation was not entirely labor in vain. However, the absence of a cue-induced MI shift in the list-level data suggests that a sustained heightening of control may be particularly difficult to achieve, even in the face of motivated and extended preparation.

The rates of success and failure in each condition are informative in regard to the influence of expectations. Although the drop in success rate from cued to uncued MC conditions is intuitive—not knowing what list is forthcoming would impair one’s ability to prepare—the lower success rate for cued MI relative to uncued MI lists is less so. The drop seemed primarily attributable to an increase in failure rate in the cued MI condition. The increased failure rate was not because of increased numbers of errors because similar error rates were observed across these conditions, nor was it attributable to increased difficulty caused by a quicker baseline in the cued condition—the mean baseline criterion in the cued MI condition was 687 ms, whereas in the uncued MI condition, it was 688 ms. The simple expectation of an upcoming MI list may have been sufficient to degrade performance following the first trial, and ironically so given that performance equivalent to an uncued MI list would have been sufficient to attain an incentive. Potentially, the difficulty of the MI lists combined with the self-awareness afforded by the incentive manipulation led to a “choking” effect, whereby performance was most hurt on trials in which peak performance was most required (Baumeister, 1984).

One limitation should be noted in regard to Experiment 4. Though the main effect of incentive found in the primary analysis is indicative of an effective manipulation, and participants reported point incentives to be motivating, albeit not highly so, the median self-reported increase in motivation attributable to low-incentive versus high-incentive cues was only 1 point on a 4-point scale. This difference was statistically significant and the effect size was large; however, it is possible that stronger effects of incentives on cueing (e.g., on list-level Stroop effects) might be found if a more motivating incentive, such as a monetary reward, were utilized in the high-incentive condition (e.g., Chiew & Braver, 2013; Padmala & Pessoa, 2011; but see Shen & Chun, 2011, for evidence of no additional benefit of monetary reward over a point incentives paradigm).

### Experiment 5

The list-level Stroop effect patterns observed across Experiments 1 through 4 were highly consistent. A robust cue-induced MC shift was found, indicating an expectation-driven shift in control (e.g., distributed attention to word and color, Lowe & Mitterer, 1982). However, no cue-induced MI shift was observed at the list level. Participants did not heighten control in a sustained, expectation-driven fashion by biasing attention away from the word (or in favor of the color; Lowe & Mitterer, 1982), despite the fact that (a) the need for control was made explicit by precueing lists as 80% conflicting and the cues were 100% valid (Experiments 1 through 4), (b) a speed manipulation was used to place a premium on prepared responding and minimize reliance on experience (Experiment 3), and (c) incentives were employed to motivate use of the 80% conflicting precues (Experiment 4). These

patterns raise the question of whether it is simply not possible for participants to minimize the list-level Stroop effect below the baseline level that was observed in uncued MI lists. One goal of Experiment 5 was to examine whether the absence of a cue-induced MI shift in the prior experiments was in fact caused by a functional performance ceiling in the MI lists. The average list-level Stroop effect was 75 ms in the uncued MI condition across the first four experiments. Possibly, experience-driven adjustments alone minimized the Stroop effect so substantially that the benefits of (sustaining) expectation-driven control adjustments (i.e., a further reduction in the Stroop effect) could not be observed at the list level in the cued MI condition. In the current experiment, we overcame this possible limitation by employing only lists that were 50% congruent. Experiment 2 indicated that a significant reduction in the Stroop effect could be observed when contrasting 50% congruent to MI lists, suggesting that the level of Stroop interference in 50% congruent lists is not at ceiling.

A second goal of Experiment 5 was oriented toward addressing the theoretically important question of whether a purely *cue-induced list-wide proportion congruence effect* would be found when comparing two lists that were equivalent, save for the type of cue presented in advance of the list (e.g., 80% matching vs. 80% conflicting). In the preceding experiments, precued expectations and actual experience always matched (e.g., an 80% matching cue was followed by an 80% matching list). In Experiment 5, we held experience constant at 50% congruent across lists. This permitted us to examine differences in the Stroop effect based on performance on the entire list of trials (rather than restricting the analysis to the first trial, which is less powerful and could be insensitive to any delayed, expectation-driven adjustments) as an indicator of pure expectation-driven adjustments. We compared performance in lists that were preceded by mostly matching, mostly conflicting, or 50% congruent cues, and also included an uncued condition. A pattern of there being a larger Stroop effect in the cued MC compared with the cued MI lists would suggest that differential expectations alone can lead to adjustments in control that produce the list-wide proportion congruence pattern. Further, a pattern of there being no difference in the Stroop effect between MI and 50% congruent (or uncued) lists would provide converging evidence that the difference in list-level Stroop effects between MI and 50% lists in Experiment 2 was entirely attributable to experience-driven adjustments in control. This pattern would further propel the conclusion that the default is to anticipate and prepare for conflict within the Stroop task, and to do so to the same degree when conflict is 50% probable as when it is more (80%) probable.

Experiment 5 employed a very similar version of the precued lists paradigm used in Experiment 2. The primary difference as noted was that the lists in the current experiment were always 50% congruent. Consequently, some lists were preceded by invalid cues. To minimize participants’ awareness of the invalidity of these precues, we used lengthier (20 trial) lists than in the prior studies. Because this procedure equates experience across lists, any modulation of the Stroop effect as a function of cue type (i.e., a Cueing  $\times$  Trial Type interaction indicating the cue-induced list-wide proportion congruence effect) can be attributed to the unique effect of expectations on cognitive control.

Table 6  
*Mean Reaction Time as a Function of Cue Condition for the First Half (Trials 1 Through 10) and Second Half (Trials 11 Through 20) of Experimental Lists in Experiment 5*

List half	Trial type	Cue condition		
		50% congruent	80% matching	80% conflicting
First	Congruent	606 (13)	604 (12)	617 (16)
	Incongruent	722 (15)	751 (17)	728 (16)
Second	Congruent	615 (14)	618 (16)	612 (17)
	Incongruent	733 (19)	749 (19)	729 (17)

Note. Values in parentheses indicate standard error of the mean.

## Method

**Participants.** Thirty-five undergraduate students completed the experiment for course credit or a \$10 payment.<sup>7</sup> All were native English speakers with normal or corrected-to-normal color vision.

**Design and procedure.** Experiment 5 was identical to the prior experiments, with the exception of the following: Participants completed 28 20-trial experimental lists consisting of 10 congruent and 10 incongruent trials. There were seven lists that were preceded by each of the following cues: 50% matching/conflicting, 80% matching, 80% conflicting, or uncued.<sup>8</sup> This resulted in a 4 (cue: 50% congruent cue vs. 80% matching cue vs. 80% conflicting cue vs. uncued)  $\times$  2 (trial type: congruent vs. incongruent) design.

## Results

We used the same trimming procedures as in the previous experiments, which eliminated <1% of trials for RTs faster than 200 ms or slower than 3,000 ms.

**Preliminary analyses.** To compare the two baseline conditions, we ran a 2 (trial type: congruent vs. incongruent)  $\times$  2 (cueing: uncued vs. cued 50% congruent) repeated measures ANOVA on RT. Results revealed no significant main effect of cueing,  $F < 1$ , a significant main effect of trial type,  $F(1, 34) = 256.57$ ,  $MSE = 496915$ ,  $p < .001$ ,  $\eta_p^2 = .88$ , and no significant interaction between trial type and cueing,  $F < 1$ . For error rate, an identical ANOVA was conducted. Again, the results revealed no significant main effect of cueing,  $F < 1$ , a significant main effect of trial type,  $F(1, 34) = 54.82$ ,  $MSE = .030$ ,  $p < .001$ ,  $\eta_p^2 = .62$ , and no significant interaction between trial type and cueing,  $F < 1$ . Because there were no significant main effects or interactions with cueing, in subsequent analyses aimed at examining the effects of varying explicit expectations (see Analysis of List-Level Stroop Effects section below), we excluded the uncued condition. This simplifies the analyses and restricts them to the three conditions in which an informative precue was shown.

**Analysis of list-level Stroop effects.** Because the prior experiments used only 10-trial blocks, we first examined the influence of having 20-trial blocks by comparing the first half (Trials 1 to 10) with the second half (Trials 11 to 20; see Table 6 for mean RTs). A 3 (cue)  $\times$  2 (trial type)  $\times$  2 (half) repeated measures ANOVA was conducted for RT. The analysis revealed a marginally significant three-way interaction between trial type, cue, and

half,  $F(2, 68) = 2.72$ ,  $MSE = 418$ ,  $p = .07$ ,  $\eta_p^2 = .07$ . We decomposed this interaction by conducting two separate 3 (cue)  $\times$  2 (trial type) repeated measures ANOVAs for the first and second halves. For the first 10 trials, results revealed a marginal main effect of cue,  $F(2, 68) = 2.72$ ,  $MSE = 1161$ ,  $p = .07$ ,  $\eta_p^2 = .07$ , a significant main effect of trial type,  $F(1, 34) = 244.23$ ,  $MSE = 3335$ ,  $p < .001$ ,  $\eta_p^2 = .88$ , and a significant Cue  $\times$  Trial Type interaction,  $F(2, 68) = 9.75$ ,  $MSE = 660$ ,  $p < .001$ ,  $\eta_p^2 = .22$ . The latter indicates a cue-induced list-wide proportion congruence effect. By contrast, for the second 10 trials, there was a main effect of trial type,  $F(1, 34) = 282.69$ ,  $MSE = 2771$ ,  $p < .001$ ,  $\eta_p^2 = .89$ , but no main effect of cue or Cue  $\times$  Trial Type interaction,  $ps > .12$  and  $\eta_p^2s < .060$ .

Conducting the identical ANOVA for error rate revealed the same pattern of effects (for the three-way interaction,  $F[2, 68] = 4.49$ ,  $MSE < .001$ ,  $p = .02$ ,  $\eta_p^2 = .12$ ) caused by a Cue  $\times$  Trial Type interaction in the first half of trials,  $F(2, 68) = 5.27$ ,  $MSE < .001$ ,  $p = .007$ ,  $\eta_p^2 = .13$ , but not the second half of trials,  $F < 1$  (see Table 7 for mean error rates). Thus, it appears the effects of cueing diminished across trials. This pattern is of theoretical interest and will be elaborated further in the discussion; for purposes of comparison with the prior experiments, we next decompose the Cue  $\times$  Trial Type interaction only for the first 10 trials.

To further examine the locus of the cue-induced list-wide proportion congruence effect within the first 10 trials, we ran two repeated measures ANOVAs. First, we compared 80% matching and 80% conflicting precues by running a 2 (cue)  $\times$  2 (trial type) repeated measures ANOVA. This showed a marginal main effect of cue,  $F(1, 34) = 3.82$ ,  $MSE = 805$ ,  $p = .06$ ,  $\eta_p^2 = .10$ , a main effect of trial type,  $F(1, 34) = 297.76$ ,  $MSE = 1876$ ,  $p < .001$ ,  $\eta_p^2 = .90$ , and a significant Cue  $\times$  Trial Type interaction,  $F(1, 34) = 8.28$ ,  $MSE = 669$ ,  $p = .007$ ,  $\eta_p^2 = .20$ . The interaction reflected a larger Stroop effect in the cued 80% matching condition ( $M = 146$  ms) relative to the cued 80% conflicting condition ( $M = 111$  ms; see Figure 8). Note that a comparison of the cued 80% matching condition with the 50% congruent condition ( $M = 116$  ms) revealed the same pattern of a larger Stroop effect for the cued 80% matching condition ( $F[1, 34] = 14.10$ ,  $MSE = 570.19$ ,  $p = .001$ ,  $\eta_p^2 = .29$ , for the Cue  $\times$  Trial Type interaction). Next we compared 50% and 80% conflicting precues by running a 2 (cue)  $\times$  2 (trial type) repeated measures ANOVA. This showed a main effect of trial type,  $F(1, 34) = 232.53$ ,  $MSE = 2007$ ,  $p < .001$ ,  $\eta_p^2 = .87$ , and no significant main effect of cue or Cue  $\times$  Trial Type interaction,  $Fs < 1$ . The lack of interaction indicates that Stroop effects were comparable in the cued 50% congruent and cued 80% conflicting conditions ( $Ms = 116$  and 111 ms, respectively; see Figure 8), and this was the case despite baseline levels of the Stroop effect being off ceiling in the present experiment. Replicating earlier experiments, this suggests that control

<sup>7</sup> We thought that the effects of interest (expectation-driven modulations of control based on invalid cues in lists that were matched on proportion congruency) might be on the smaller side relative to similar effects (e.g., cue-induced MC shift) in the previous experiments. Therefore we aimed to test 36 participants in this experiment.

<sup>8</sup> Instead of being shown five question marks on the precue slide in the uncued condition, the precue simply told participants to press the key when they were ready to begin the next list.

was not heightened above baseline levels (here, levels found in the cued 50% congruent condition) in response to MI cues.

For error rate, we also examined the effects of cue and trial type within the first 10 trials by conducting two repeated measures ANOVAs. First, we compared 80% matching and 80% conflicting precues by running a 2 (cue) × 2 (trial type) repeated measures ANOVA. This showed no main effect of cue,  $F(1, 34) = 2.02$ ,  $MSE < .001$ ,  $p = .16$ ,  $\eta_p^2 = .06$ , a main effect of trial type,  $F(1, 34) = 36.91$ ,  $MSE = .001$ ,  $p < .001$ ,  $\eta_p^2 = .52$ , and a significant interaction between cue and trial type,  $F(1, 34) = 1.52$ ,  $MSE < .001$ ,  $p = .04$ ,  $\eta_p^2 = .23$ . The interaction reflects a larger Stroop effect in error rate in the cued 80% matching condition ( $M = .043$ ) relative to the cued 80% conflicting condition ( $M = .026$ ). Next we compared 50% congruent and 80% conflicting precues by running a 2 (cue) × 2 (trial type) repeated measures ANOVA. This showed a main effect of trial type,  $F(1, 34) = 29.10$ ,  $MSE = .001$ ,  $p < .001$ ,  $\eta_p^2 = .46$ , and no significant main effect of cue or Cue × Trial Type interaction,  $F_s < 1$ . The lack of interaction indicates that Stroop effects in error rate were comparable in the 50% congruent and 80% incongruent cue conditions ( $M_s = .023$  and  $.026$ , respectively). Replicating prior experiments, this shows that the MI cues did not produce an expectation-driven shift in control at the list level.<sup>9</sup>

**Discussion**

In contrast to previous experiments, in the current experiment, experience was held constant (all lists were 50% congruent), allowing us to examine the unique effect of varying expectations (50% congruent, 80% matching, and 80% conflicting precues) on list-level Stroop effects. We observed a cue-induced list-wide proportion congruence effect suggesting that precues modulated list-level Stroop effects, and the pattern of the effect suggested a select role for expectations in affecting cognitive control. Similar to prior experiments, there was no evidence for an expectation-driven heightening of control (i.e., reduced attention to word or greater focus on relevant color; Lowe & Mitterer, 1982) in response to 80% conflicting (MI) precues. The Stroop effect was equivalent for lists that participants expected to be 80% conflicting as those expected to be 50% congruent. In contrast, when a list was expected to be 80% matching, a larger Stroop effect was observed than when a list was expected to be 80% conflicting or 50% congruent, and this was true despite the lists being equated in actual experience (i.e., both were 50% congruent). This suggests

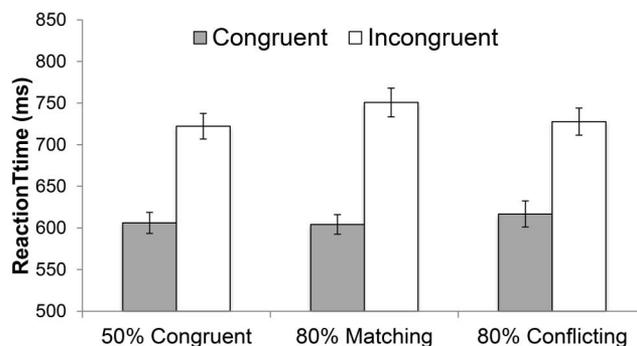


Figure 8. Mean reaction time as a function of cue condition for congruent and incongruent trials in Experiment 5. Means derived from performance on first 10 trials within a given list (i.e., list-level) and all lists were 50% congruent. Error bars reflect standard error of the mean.

participants relaxed control in an expectation-driven fashion (i.e., devoted more attention to the word or distributed attention across the word and color; Lowe & Mitterer, 1982) when shown an 80% matching precue, thereby increasing facilitation and/or interference.

The current findings coincide with those of Experiments 1 through 4. In those experiments, one potential explanation for why we did not find a minimization of the Stroop effect when MI cues were provided was that the baseline against which the cued MI condition was being compared was an uncued MI condition, wherein the Stroop effect was already very low (i.e., a functional ceiling). However, in the current experiment, we used a 50% congruent list as a baseline condition, such that the Stroop effect was sufficiently high (116 ms) to permit a significant reduction in the Stroop effect to be observed when 80% incongruent (MI) cues were provided. Indeed, if participants were capable of heightening control on the basis of the MI precue to the same degree that control was heightened via experience-driven adjustments in control in Experiment 2, then the reduction in the Stroop effect would have been very large in the present experiment. Notably, the finding of no expectation-driven heightening of control in response to 80% incongruent cues (relative to 50% cues) also counters the possibility that participants did not use MI cues in prior experiments because MI lists were so cognitively demanding that they could not engage any additional cognitive control to maintain an expectation. Here, the lists were 50% congruent and evidence for a heightening of control relative to the 50% congruent condition was again absent.

In the current experiment, we utilized 20-trial lists because we wanted to minimize awareness of the invalid nature of a subset of the precues. A novel and theoretically relevant pattern that emerged from the present experiment concerned the diminishing effects of the precues from the first half to the second half of trials within a list. One possibility is that cue use may simply be too difficult to sustain over time, possibly because anticipatory modulation of control based on expectations is too resource demanding

Table 7  
Mean Error Rate as a Function of Cue Condition for the First Half (Trials 1 Through 10) and Second Half (Trials 11 Through 20) of Experimental Lists in Experiment 5

List half	Trial type	Cue condition		
		50% congruent	80% matching	80% conflicting
First	Congruent	.002 (.001)	.001 (.001)	.001 (.001)
	Incongruent	.025 (.005)	.044 (.008)	.027 (.005)
Second	Congruent	.001 (.001)	.002 (.001)	.002 (.001)
	Incongruent	.033 (.007)	.025 (.006)	.031 (.006)

Note. Values in parentheses indicate standard error of the mean.

<sup>9</sup> We analyzed the data with and without participants who reported being aware of the misleading nature of the cues, which we measured with several postexperimental questions. Results did not differ as a function of awareness.

(cf. Braver et al.'s, 2007, characterization of proactive control). In the current study, the expectation-driven shift in control was selective to the cued MC condition when participants devoted more attention to the word, or as Lowe and Mitterer (1982) suggested, distributed attention across word and color. Though it may seem counterintuitive to think of such a control setting as resource demanding, such a setting does require participants to devote above baseline levels of attention to the dimension (word) they are supposed to ignore while simultaneously color naming. A second, nonmutually exclusive possibility is that experience-driven adjustments in control are simply too powerful a basis for control to be overridden by expectation-driven control. That is, although participants were capable of engaging expectation-driven control in the first half of trials (i.e., by devoting more attention to the word) when such trials were 50% congruent, by the second half of the list, the accumulating experience with 50% congruent trials may have culminated in the adoption of a control setting that reflected the actual proportion congruency of the list.

### General Discussion

The central question addressed in the current study was whether expectations and experience contribute to adjustments in cognitive control and, if so, whether their influences could be dissociated. We addressed this question by conducting five experiments using the precued lists paradigm in a Stroop task in which participants were provided with explicit cues that did (cued condition) or did not (uncued condition) lead them to expect a particular proportion of congruent and incongruent trials in the upcoming list. Based on general theoretical accounts that emphasized the contribution of willed, volitional, and/or intentional modulation of cognitive control (Norman & Shallice, 1986; Posner, & DiGirolamo, 1998; West & Baylis, 1998), and accounts of the list-wide proportion congruence effect that emphasized the role of the active selection of strategies in Stroop performance (e.g., Lowe & Mitterer, 1982), we expected to find evidence for expectation-driven modulations of control in the precued lists paradigm.

In Experiments 1 through 4, wherein precues were valid (e.g., if the cue said 80% matching, the list was in fact MC), we examined two patterns of effects (*cue-induced shifts at the list level* and *first-position Stroop effects*) to dissociate the influence of expectations from that of experience. Both patterns of effects provided support for an expectation-driven modulation of control in response to 80% matching precues. First, participants demonstrated a cue-induced MC shift, showing a larger Stroop effect at the list level when MC lists were cued as 80% matching than when they were uncued. This is consistent with Lowe and Mitterer's (1982) claim that participants can choose to distribute attention across the word and color dimension when in a context in which most trials are congruent, a strategy that may have enhanced facilitation and/or interference in the cued MC lists. Second, in Experiment 2, the analysis of first-position Stroop effects, possibly the purest indicator of expectation-driven control when expectations and experience are consistent (as in Experiments 1 through 4), revealed that participants exhibited a larger Stroop effect in the cued MC condition than the uncued MC condition (i.e., there was a cue-induced MC shift). Considering the uncued condition as a baseline condition, the expectation-driven shift in control in response to 80% matching cues might be

viewed as a *relaxation* of control settings that otherwise would more fully attenuate word processing.

In contrast, there was little evidence for an intentional *heightening* of control in an expectation-driven fashion in response to cues that signaled the next list would be 80% conflicting. Participants showed no less interference at the list level when they knew in advance that 80% of trials would be conflicting as when the proportion congruence of the list (MI) was not known (i.e., there was no cue-induced MI shift). This does not support Lowe and Mitterer's (1982) claim that participants can choose a strategy of selectively focusing on the relevant information (color) or attenuating word processing (e.g., Lindsay & Jacoby, 1994) in an MI list, or more effectively maintain the color-naming goal in an active state (e.g., West & Baylis, 1998). The only evidence of an expectation-driven adjustment in control in response to MI cues was the finding of a cue-induced MI shift in the first-position analysis selectively in the speeded (a strong trend in Experiment 3) and high-incentive (a significant effect in Experiment 4) conditions designed to induce pressure and motivate participants to prepare (i.e., use the cue). Although these patterns suggest a minimal role for expectations in the heightening of cognitive control, they are theoretically important, as they challenge pure experience-based accounts (e.g., conflict-monitoring) of cognitive control.

In Experiment 5, we held constant the overall composition (proportion congruence) of the lists at 50% congruent and varied only participants' expectations regarding the upcoming list's proportion congruency. This allowed us to examine a pure indicator of the effects of expectations that was not restricted to the first trial. The findings strongly converged with those of the first four experiments in showing that participants utilized 80% matching cues to relax cognitive control (i.e., more attention devoted to word/distributed across word and color), but they did not utilize the 80% conflicting precues to heighten control above levels observed in the cued 50% congruent condition (which did not differ from an uncued condition). These patterns are consistent with the idea that the baseline mode of control in the Stroop task is one that favors color processing and/or disfavors word processing, and participants did not demonstrate that they could heighten control (e.g., bias attention toward the relevant dimension to a greater degree or away from the irrelevant dimension to a greater degree) in a sustained fashion beyond such baseline levels (Experiments 1 through 4; cf. Gratton et al.'s, 1992, findings with trial-by-trial precueing).

One possible explanation for these patterns relates to our use of what might be termed *dual instructions*. We used the standard selective attention instructions at the start of the task by encouraging participants to name aloud the color as quickly as possible. Participants were then informed about the precues. An 80% matching cue implies that it would be beneficial to attend to the word, although responses are made to the color, somewhat contradicting the original selective attention instructions (i.e., encouraging a division of attention across color and word dimensions; cf. Logan & Zbrodoff, 1979; Lowe & Mitterer, 1982). The consistent evidence for a cue-induced MC shift suggests participants used the cues (as opposed to following the instruction, to the extent possible, of selectively attending to the color), which opened them up to facilitation and/or interference. The 80% conflicting cue, by contrast, might be tantamount to the original selective attention in-

struction to pay attention to the color and not the word, and if so, the invariance of the Stroop effect across cued MI and uncued MI lists may not be surprising. Alternatively, as we discuss next, there may be limits on the extent to which individuals can intentionally heighten control above and beyond some baseline level the original instructions encourage, contrary to some views (e.g., Lowe & Mitterer, 1982; Norman & Shallice, 1986; Posner, & DiGirolamo, 1998). The finding of a cue-induced shift selectively on the first trial under conditions that motivated use of the mostly conflicting precues is compatible with this view, but is difficult to account for on the basis of the dual-instruction explanation.

### Potential Explanations for the Absence of a Cue-Induced MI Shift

In the precued lists paradigm, the lists were relatively brief (10 trials in first four experiments and 20 in the fifth experiment), thereby minimizing demands on the need to sustain control over many trials, as would be required in the typical list-wide proportion congruence paradigm. Additionally, in the first four experiments, the precues provided highly reliable advance information about the expected proportion of trials on which interference would occur. Therefore, it was not the case that participants had to rely on the attempt to perform the task to determine what resources to recruit (Kahneman, 1973)—participants knew in advance it would require control and were encouraged to use the MI precues. Still, we did not observe a cue-induced MI shift at the list level in any experiment. We tested several potential explanations for why there was no evidence of list-level cue usage in the cued MI condition. Experiments 3 and 4 utilized manipulations that were intended to induce pressure for participants to prepare in advance of each list (i.e., more fully adjust control settings in response to the MI precues). In Experiment 3, a speed manipulation was employed, such that participants had limited time to respond to each stimulus, and in Experiment 4, we awarded varying point incentives based on task performance. Despite evidence that the manipulations were effective (e.g., faster responding and smaller Stroop effects when speeded in Experiment 3, and faster responding along with motivated and extended preparation for high-incentive condition in Experiment 4), neither form of pressure produced a cue-induced MI shift at the list level. Experiment 5 examined an alternative account of this pattern, namely, that experience alone minimized the Stroop effect so substantially in the prior experiments (in which cued MI lists were always in fact MI) that the benefits of expectation-driven control adjustments could not be observed in the cued MI condition. In Experiment 5, all lists were 50% congruent, such that the baseline magnitude of the Stroop effect in the cued (or uncued) 50% congruent condition was not at ceiling. Still, no evidence for heightened control in the MI condition was found.

Another potential explanation is that performance in the uncued MI lists was affected by participants' knowledge of the nature of the lists because they were intermixed with cued lists throughout each experiment. For instance, in uncued lists, participants may have adopted a strategy of using the initial trial to self-generate a cue regarding list type (e.g., if incongruent, expect an MI list), resulting in the same explicit expectations as in the cued MI condition. This could explain the absence of list-level cue-induced

MI shifts. However, there are two patterns within the present experiments that are difficult to reconcile with this explanation. One is the asymmetric nature of the cue-induced shifts. If participants were self-generating cues following presentation of the first trial, then one would have also expected there not to be a cue-induced MC shift, because the self-generation of cues in the case of an initial congruent trial should have led to a relaxation of control in uncued MC lists (unless there was some a priori reason to expect such strategies to be used only in the MI condition). Contrary to this prediction, the cue-induced MC shift was a robust and consistent finding across experiments. The second is that a similar pattern of effects was found in the experiments in which the initial trial type was predictive of list type (e.g., Experiment 1, which included only MC and MI lists), as in the experiments in which the initial trial type was less predictive of list type (Experiment 2, because of the inclusion of 50% congruent lists) or not at all predictive, given that list type did not vary (Experiment 5, which comprised only 50% congruent lists). That pattern of effects included the lack of an expectation-driven effect at the list level when an MI list was cued.<sup>10</sup>

The findings stimulate the question of why there is so little evidence for the intentional use of 80% conflicting precues. Said differently, why might there be a strong tendency to rely on, or defer to, experience-driven adjustments in control, perhaps by adopting a wait-and-see strategy, when a high probability of interference is expected? Some experience-driven adjustments in control have been characterized as implicit, unintentional, and conflict-driven (e.g., Blais et al., 2012; Botvinick et al., 2001; Melara & Algom, 2003), and thereby might be assumed to be more automatic and less willed. On this view, one possible answer is that humans might prefer to function in a rather "automatic" mode, relying on the environment as a source of control, because conscious control of behavior is dependent upon a limited pool of cognitive resources that may be depleted (Bargh, 1989; Bargh & Chartrand, 1999; see Hommel, 2007, for a view that will is but one potential source of control).

Another potential explanation stems from consideration of the findings of Experiments 2 and 5. In Experiment 2, a smaller Stroop effect was observed for the cued MI list compared with the cued 50% congruent list (and the same was true for the uncued list comparison). However, analysis of the first-position Stroop effects showed that there was no difference in the Stroop effect between cued MI and cued 50% congruent lists. In other words, the control setting that was adopted at the start of these lists was equivalent. This finding, along with Experiment 5's finding that there was no difference in list-level Stroop effects for cued MI and 50% congruent lists when they were equated in experience (both lists were both 50% congruent), raises the possibility that experience-driven adjustments in cognitive control are more precise (fine-tuned) than expectation-driven adjustments in cognitive control. With experience behind the wheel, the Stroop effect was significantly attenu-

<sup>10</sup> Moreover, in an experiment in which participants were presented with only uncued MC and MI lists, we found that the magnitude of the Stroop effect in the uncued MI condition was very similar to the magnitude observed in the present experiments, regardless of participants' awareness of the nature of the lists, which further challenges the explanation that lack of a cue-induced MI shift relates to the intermixing of cued and uncued lists (Bugg & Dieder, 2014).

ated in the MI list compared with the 50% congruent list, supporting the view that either color processing was more amplified or word processing more attenuated in the context in which incongruent trials were experienced more frequently. With expectations behind the wheel, the “selected” control setting (i.e., extent to which word was processed) was equivalent for cued MI and cued 50% congruent lists, as indicated by the equal Stroop effects in the first-position analysis of Experiment 2 and in the 50% congruent lists of Experiment 5. This suggests that the precision with which participants calibrated expected probabilities of interference with the degree of control over word processing was not nearly as optimal as when they outsourced the regulation of cognitive control to the environment (experience). Indeed, the consistently observed cue-induced MC shift suggests a similar conclusion. Here, expectation-driven adjustments overshot the more optimal level of control that was configured in the uncued MC condition—the Stroop effect was *greater* when expectations contributed to performance than when participants had no expectations, and simply allowed experience to grab hold of the wheel and dictate the appropriate level of attentional biasing toward the word and color (see Jiménez & Méndez, 2013; Schlaghecken & Martini, 2012, for mechanisms that may be responsible for experience-based adaptations to a “lack of conflict”). The precision with which expectation- and experience-driven adjustments in control can be made represents a ripe area for future research.

Yet another possibility is that participants defer to experience-driven adjustments such as conflict monitoring in cued MI and cued 50% congruent conditions, because this more reactive mode of responding solves two dilemmas discussed by Goschke and his colleagues (e.g., Goschke, 2003; Goschke & Dreisbach, 2008). With respect to the control dilemma, relative to an expectation-driven mode of attempting to attenuate word processing or amplify color processing in a sustained fashion across trials (cf. De Pisapia & Braver, 2006), reliance on experience-driven adjustments allows for the task to become automatized more quickly while enabling the resolution of interference as needed (i.e., in a reactive fashion; Braver et al., 2007). Similarly, reliance on experience-driven adjustments can be seen as optimal from the perspective of the shielding-monitoring dilemma. Were participants too strongly favoring shielding, as in the adoption of a proactive mode of attenuating word processing in a sustained fashion when an MI or 50% congruent cue was shown, they would risk a detriment to monitoring. In proportion congruence paradigms, excessive shielding would entail a loss of information that is carried along the irrelevant dimension (e.g., learning which words tend to produce conflict), which is critical for experience-dependent adjustments in control, according to some models (Blais et al., 2007; Botvinick et al., 2001; Melara & Algom, 2003).

### Implications for the List-Wide Proportion Congruence Effect

The utility of the precued lists paradigm is strengthened by the fact that we observed similar patterns of effects in the uncued condition as in the typical list-wide proportion congruence paradigm, in which single, long lists of varying proportion congruence are employed (see Experiments 1 through 4). Quite interestingly, this was the case despite the fact that the current lists were most

often composed of only 10 trials, which affords relatively minimal experience compared with the typical list-wide proportion congruence paradigm that often exposes participants to 100 or so consecutive trials in a given condition.

A central issue that has been examined in the list-wide proportion congruence literature is whether a global (list-wide) control mechanism contributes to the list-wide proportion congruence effect above and beyond the contribution of item-level mechanisms (for reviews, see Bugg, 2012; Bugg & Crump, 2012). Bugg (2014) found that evidence for a contribution of a global control mechanism to the list-wide proportion congruence effect was not ubiquitous. Rather, it was dependent on the degree to which participants could rely on item-specific associative stimulus-response learning processes when performing the Stroop task. When participants could not predict associated responses on the majority of incongruent trials, there was evidence for a globally operating control mechanism.

Bugg’s (2014) study did not, however, address the theoretically relevant question of whether engagement of the global control process was intentional (i.e., volitional; see also Bugg & Chani, 2011; Bugg, McDaniel, Scullin, & Braver, 2011). That is, did participants intentionally attempt to relax or heighten control depending on the proportion congruency of a list, or did the adjustments arise independent of explicit (conscious) intentions? The evidence from the current study strongly questions the possibility of participants intentionally heightening control (in a sustained fashion following the first trial, especially in typical, low pressure conditions) based on explicit expectations. Our data suggest that even when participants know the proportion congruency of the upcoming list will be 80% conflicting, they show equivalent list-level Stroop effects as when they are not informed at all. In the typical list-wide proportion congruence paradigm, participants are not informed of the proportion congruency of a list, introducing the possibility that after some experience with the list they become aware of the proportion congruency, leading to variation in the magnitude of Stroop effects within the MI condition (and thereby variation in the magnitude of the list-wide proportion congruence effect; but see Blais et al., 2012). It seems unlikely, however, that participants would engage an expectation-driven control process to heighten control based on emerging expectations (i.e., during the course of the list) when they showed no evidence that they could do so under the most optimal conditions in the present study (e.g., valid cues, short lists, motivated performance).

Our consistent finding of a cue-induced MC shift, in contrast, does raise the possibility that varying levels of awareness of the proportion congruency of an MC list could lead to variation in the magnitude of the Stroop effect within the MC condition, and thereby variation in the magnitude of the list-wide proportion congruence effect. Some past studies have demonstrated individual differences in Stroop interference that are selective to the MC condition, such as those related to working memory capacity (Kane & Engle, 2003). Kane and Engle (2003) suggested that these differences reflected variation in goal neglect among high- and low-working-memory-capacity individuals, with those lower in capacity showing larger interference effects particularly in error rate. An alternative possibility is that lower working-memory-capacity individuals were more apt to become aware of the proportion congruence of the MC list because of their lessened ability to filter the irrelevant dimension (e.g., Conway, Cowan, & Bun-

ting, 2001; Shipstead & Broadway, 2013). If so, they may have been more likely to intentionally adjust control in the nonoptimal fashion (overrelaxing control by devoting more attention to the word) we observed in the cued MC condition of the present experiments.

The current experiments were not designed to contrast the various experience-based accounts of the list-wide proportion congruence effect—we would merely be speculating if we were to assume that one process (e.g., temporal learning) played a greater role than another process (e.g., conflict-monitoring) in producing the effect. The cue-induced list-wide proportion congruence effect found in Experiment 5 (and, relatedly, the selective cue-induced shifts and first-position proportion effects in earlier experiments) does, however, have implications for the experience-based accounts. Consider the globally oriented conflict-monitoring account, for example, which attributes the list-wide proportion congruence effect to the greater heightening of control caused by the accumulation of conflict over multiple preceding trials in MI compared with MC lists (Botvinick et al., 2001). Or consider the temporal learning account, which explains list-wide proportion congruence effects as resulting from the learning of different rhythms of responding (i.e., temporal expectancies) in MC (faster pace because of more easy trials) versus MI (slower pace because of more difficult trials) lists (Schmidt, 2013a, 2013b). According to these accounts, proportion congruence effects should not be observed when lists are matched on conflict frequencies (i.e., relative frequency of incongruent and congruent trials) or difficulty (i.e., how many easy vs. difficult trials), respectively, as was the case in Experiment 5. Yet we observed a proportion congruence effect as indicated by the larger Stroop effect in cued MC than cued MI or 50% congruent lists. In order to account for this and related findings (e.g., first-position effects) using conflict-monitoring or temporal learning, for example, one would have to posit that precues may instantiate a control setting or response rhythm (temporal expectancy) prior to any experience within the list, an idea that is inconsistent with a pure experience-based account (but see Braver et al., 2007, for a dual-mechanisms account that incorporates expectation-based control adjustments [i.e., proactive control] alongside an experience-based conflict-monitoring framework).

## Conclusions

Using the novel precued lists paradigm, we demonstrated that expectations and experience uniquely contribute to adjustments in cognitive control in a Stroop task, and developed several approaches to dissociate their influence. Evidence for the role of expectations, independent of experience, in the relaxation of cognitive control (e.g., selection and application of a setting that permitted attention to be distributed across the irrelevant and relevant dimensions [Lowe & Mitterer, 1982], thereby leading to a larger Stroop effect) was consistently observed when participants expected a list to entail minimal conflict (i.e., 80% matching cue). In line with experience-based accounts, there was consistent evidence in support of experience-driven adjustments as the primary basis for the reduction in the Stroop effect across lists of MI trials. Only when participants were under external or internal pressure to prepare did we observe evidence consistent with an expectation-driven heightening of control in response to 80% conflicting cues,

and it was in the form of a short-lived cue-induced MI shift on the first trial within a list. Collectively, the findings provide minimal support for the role of intent (i.e., will; volition; effort) in the selection and application of a control setting for minimizing the Stroop effect (e.g., focusing of attention on color; Lowe & Mitterer, 1982; Norman & Shallice, 1986; Posner & DiGirolamo, 1998) in conditions in which control might matter most, that is, when the likelihood of conflict is relatively high.

Future research is needed to examine the ubiquity of these findings in other tasks for which a role of expectation-driven cognitive control has been assumed (e.g., flanker; task switching). For instance, there is evidence that participants adjusted preparation in response to probabilistic precues in a task-switching paradigm, and unlike in the present Stroop task, they were especially likely to use the precues in the more difficult condition (when shifts were expected) as opposed to the easier condition (when repetitions were expected; Dreisbach & Haider, 2006). However, whether expectation-driven control depends on task choice per se, or other factors such as the nature of the precues (i.e., global list-by-list precueing vs. the more local trial-by-trial precueing used by Dreisbach & Haider, 2006) or the information the precues convey (i.e., probability of interference vs. probability of a shift) remains an unanswered and exciting question for future studies.

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