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ORIGINAL ARTICLE

Proactive control of irrelevant task rules during cued task switching

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Abstract In task-switching paradigms, participants are often slower on incongruent than congruent trials, a pattern known as the task-rule congruency effect. This effect suggests that irrelevant task rules or associated responses may be retrieved automatically in spite of task cues. The purpose of the present study was to examine whether the task-rule congruency effect may be modulated via manipulations intended to induce variation in proactive control. Manipulating the proportion of congruent to incongruent trials strongly influenced the magnitude of the task-rule congruency effect. The effect was significantly reduced in a mostly incongruent list relative to a mostly congruent list, a pattern that was observed for not only biased but also 50 % congruent items. This finding implicates a role for global attentional control processes in the task-rule congruency effect. In contrast, enhancing the preparation of relevant (cued) task rules by the provision of a monetary incentive substantially reduced mixing costs but did not affect the task-rule congruency effect. These patterns support the view that there may be multiple routes by which proactive control can influence task-switching performance; however, only select routes appear to influence the automatic retrieval of irrelevant task rules.

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Introduction

Rapidly switching between two (or more) tasks presents a central cognitive challenge (Jersild, 1927). Decades of research have demonstrated that meeting this challenge requires a significant degree of cognitive control, as evidenced by robust performance costs (including switch and mixing costs), benefits of preparation time for reducing switch costs, and residual switch costs that are observed even with extended preparation (Allport, Styles, & Hsieh, 1994; Meiran, 1996; Rogers & Monsell, 1995; for review, see Kiesel et al., 2010; Vandierendonck, Liefooghe, & Verbruggen, 2010). A less frequently researched task-switching phenomenon that also presents control demands is a second type of "cost" due to interference that arises when the target stimulus elicits activation of both the task-relevant and task-irrelevant rules. Consider, for example, a musician who switches between playing a saxophone and playing a clarinet during a performance. Upon picking up the clarinet, the rules (here, note-key mappings) for playing the saxophone are (at least temporarily) irrelevant. Yet, encountering particular stimuli (notes) may automatically activate the tendency to press the keys that correspond to those notes were the saxophone in hand, yielding interference.

In the laboratory analog of this situation, participants rapidly switch between two simple tasks that are governed by distinct rules specifying task-relevant stimulus–response mappings. For example, in a parity judgment task, participants press the left key if the digit dimension of a bivalent stimulus (e.g., A3) is odd and the right key if it is even. In a vowel/consonant judgment task, participants press the left key if the letter dimension is a vowel and the right key if it is a consonant. Because there is response overlap between tasks, a given stimulus will be congruent or incongruent.

For example, the stimulus "A3" represents a congruent trial because the correct response is to press the left key regardless of which task rule is activated. By contrast, "B3" represents an incongruent trial—when performing the parity judgment task, activation of the irrelevant task rule (i.e., for the vowel/consonant judgment task) leads to conflict. The task-rule congruency effect (TRCE) refers to the slowed response times on incongruent as compared to congruent trials (for first demonstration of TRCE in task switching, see Sudevan & Taylor, 1987). This suggests that irrelevant task rules and associated responses may be automatically or reflexively retrieved in spite of task instructions (e.g., Meiran & Kessler, 2008; Tzelgov, 1997).

Interestingly, the TRCE might be viewed as reflecting the operation of an optimal control system. That is, the presence of the TRCE indicates that individuals have maintained a readiness to perform either task on a moment-by-moment basis (e.g., Meiran, Hsieh, & Dimov, 2010). As Meiran et al. (2010) noted this is advantageous in task-switching paradigms because the task that is momentarily irrelevant is one that will need to be performed again soon. Thus, it is potentially non-optimal for individuals to strongly deactivate (inhibit) the irrelevant task rules when performing a given task (see Koch, Gade, Schuch, & Philipp, 2010, for review of inhibition in task switching). Nonetheless, the question of whether the TRCE can be controlled (e.g., by inhibiting task rules to a greater degree when this is contextually appropriate) is of theoretical interest as it may offer insight into the underlying processes that produce the effect.

To date, several patterns have emerged suggesting variation in the magnitude of the TRCE, albeit not necessarily due to variations in control. For instance, the TRCE is often found in mixed-task conditions but reduced in single-task conditions (Fagot, 1994; Meiran, 2000, 2005). Also, the TRCE is often larger on switch trials than on repeat trials (e.g., Wendt & Kiesel, 2008). One idea is that having just performed Task A on trial $n - 1$ accessibility of the irrelevant task rules (those associated with Task A) on trial n will be higher than if the relevant (and irrelevant) task repeated. Another idea is that irrelevant task rules are inhibited at the end of trial $n - 1$, reducing their accessibility on trial n, which results in slowed responding on switch trials (for evidence of competitor rule suppression, see Meiran et al., 2010, in which all trials were switch trials). In addition, the TRCE is evident for familiar but not novel response categories (Meiran & Kessler, 2008). These patterns converge in suggesting that the TRCE appears to be larger in those contexts in which the intra-experimental or trial-by-trial activation of competing task rules is robust and strong associations exist between a given stimulus dimension and the corresponding (but irrelevant) categorization response (see also Askren, 2010). In this sense, there are notable similarities between the classic Stroop effect (Stroop, 1935; MacLeod, 1991), the finding that response times are slowed when naming the ink colors on incongruent (e.g., RED in blue ink) as compared to congruent (e.g., BLUE in blue ink) trials and the TRCE effect (see Meiran & Kessler, 2008, for discussion of similarities and differences). Both imply automatic activation of information that is associated with an irrelevant stimulus dimension in spite of a task instruction or cue (Tzelgov, 1997). This analogy raises the question of whether the activation of irrelevant task rules in a task-switching paradigm may be modulated via manipulations that are known to influence the magnitude of the Stroop effect.

One such pattern is the list-wide proportion congruence effect (for reviews, see Bugg, 2012; Bugg & Crump, 2012), the finding that the Stroop effect is reduced in lists in which trials are mostly incongruent as compared to mostly congruent. Originally reported by Logan and Zbrodoff (1979), this effect is often described as reflecting the attentional biases participants adopt in different lists. In a mostly incongruent list, attention is repelled from the irrelevant dimension (word) because it tends to conflict with the to-be-named color. By contrast, in a mostly congruent list, attention is attracted to the irrelevant dimension because it tends to be correlated with (predictive of) the to-be-named color. Indeed, it has been shown that failures of selective attention (i.e., activation of irrelevant dimension) are robust when the irrelevant dimension tends to activate the same response as the relevant dimension but the Stroop effect shrinks or even disappears when the irrelevant dimension is not predictive of responses (Dishon-Berkovits & Algom, 2000; Melara & Algom, 2003).

There is evidence that the list-wide proportion congruence effect at least in part reflects a global shift in attentional control (i.e., reduced processing of the irrelevant dimension across items in a mostly incongruent list; Bugg, 2014; Bugg & Chanani, 2011; Bugg, McDaniel, Scullin, & Braver, 2011; Hutchison, 2011; but see Bugg, Jacoby, & Toth, 2008; Blais & Bunge, 2010; see also Schmidt, 2013; 2014 for an alternative view based on temporal learning). Such a mechanism aligns with the concept of proactive control as described in the dual-mechanisms of control account (Braver, Gray, & Burgess, 2007). According to this account, proactive control involves the preparatory biasing of attention toward goal-relevant information in advance of stimulus onset. In support of this characterization, De Pisapia and Braver (2006) found performance of the Stroop task in a mostly incongruent list to be associated with sustained activation of prefrontal cortex with this activation serving to minimize the interfering (conflicting) effects of incongruent stimuli (as indicated by transient anterior cingulate activation).

Given the noted similarities to the Stroop effect, it seems plausible that the list-wide proportion congruence manipulation should affect the TRCE. Providing initial support for this assumption, Braverman and Meiran (2015) found a smaller response congruency effect in response time (RT) in a mostly incongruent block compared to a mostly congruent block when using univalent stimuli that enabled the researchers to hold task conflict constant across conditions. However, it has also been suggested that the Stroop effect and TRCE are fundamentally different. For instance, as Meiran and Kessler (2008) noted, the TRCE is at least in part due to intra-experimental acquisition of (new) rules, whereas participants know the S-R rules of Stroop prior to the experiment. Perhaps more important for present purposes, in the Stroop task but not in a cued task-switching paradigm, which dimension is relevant and which is irrelevant is consistent across trials. This feature may result in the Stroop effect being especially amenable to modulations of proactive control (i.e., global biasing of attention away from irrelevant dimension) or application of a strong and consistent inhibition of the irrelevant dimension (see Meiran et al., 2010). Finding that the list-wide proportion congruence manipulation affects the TRCE via a global mechanism would be theoretically important because it would imply that the TRCE is nonetheless subject to variable proactive control. However, a test of this prediction has yet to be reported in the literature.

Experiment 1

The purpose of Experiment 1 was to determine if a list-wide proportion congruence effect would be found for the TRCE using a cued task-switching paradigm with bivalent stimuli. If different attentional control settings are adopted based on the frequency of congruent and incongruent trials, as in Stroop, then a list-wide proportion congruent effect should be observed. That is, a smaller TRCE should be found in a mostly incongruent relative to a mostly congruent list. Such a pattern may suggest control of the automatic activation of irrelevant task rules.

Method

Participants

55 undergraduates participated for course credit or monetary compensation ($10/h). Participants were native English speakers with normal or corrected-to-normal vision. Three participants did not show up for the second session and were excluded.

Stimuli and design

A 2 (list-wide proportion congruent: mostly congruent vs. mostly incongruent) × 2 (switch type: repeat vs. switch) × 2 (congruency: congruent vs. incongruent) within-subjects design was employed. The bivalent stimuli were composed of a letter-number pairing. There were 16 stimuli, half in which the letter was on the right side of the stimulus and half in which the letter was on the left side (1A, A1, 2A, A2, 1B, B1, 2B, B2, 3E, E3, 4E, E4, 3D, D3, 4D, and D4). The letters and numbers were chosen such that there were two consonants and two vowels as well as two even and two odd digits. A left key (i.e., letter "a" on standard QWERTY keyboard) and right key (i.e., letter "l") were used as response keys with, for example, the left key corresponding to a "vowel" or "odd" categorization response and the right key to a "consonant" or "even" categorization response in the letter and number tasks, respectively. In this example, stimuli such as "1A" and "D4" represent congruent trials and "2A" and "E4" represent incongruent trials. The four possible sets of category-response mappings were counterbalanced across participants.

Procedure

The experiment took place in two sessions separated by 48 h and the task-switching task was administered following a picture-word Stroop task in each session. Informed consent was obtained in the first session. Each session began with orientation to the task rules. Participants were instructed that a letter and number would appear on each trial and prior to their appearance, a task cue ("ATTEND LETTER" or "ATTEND NUMBER") would indicate which task to prepare. They were told to press the response key that corresponded to the correct categorization according to the cue (e.g., if attend letter is shown, press the key that corresponds to a vowel if the letter is a vowel and a consonant if the letter is a consonant). Responses were made with the index finger of each hand (e.g., left index finger used to press left key).

In each session, eight practice trials were administered and feedback was provided. On incorrect trials, participants were reminded of the task rules. Following practice, they completed one block of 192 trials in which the two tasks were randomly intermixed. Half of the trials involved the letter task cue and the other half the number task cue. In one session, the block was mostly congruent and in the other session it was mostly incongruent and block order was counterbalanced across participants. In the mostly congruent block, 75 % of trials were congruent. For each task, each congruent stimulus (eight total) was presented nine times and each incongruent stimulus (eight total) three times. In the mostly incongruent block, 25 % of trials were congruent. For each task, each congruent stimulus (eight total) was presented three times and each incongruent stimulus (eight total) nine times.

As shown in Fig. 1, on each test trial, a fixation cross appeared for 300 ms followed by the task cue for 500 ms.

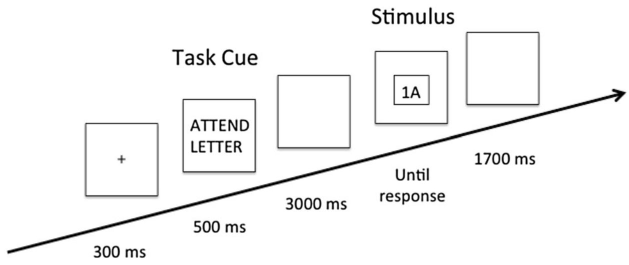

Fig. 1 Trial-level procedure used in Experiment 1. The interval between the cue and stimulus was 2000 ms in Experiment 2 and 1500 ms in Experiment 3. In Experiment 3, feedback intervened for 1250 ms between the response and blank slide. Task cue and stimuli are not to scale

The cue-to-stimulus interval (CSI) was 3000 ms. The target stimulus appeared centrally surrounded by a frame (cf. Frings & Rothermund, 2011) and remained on-screen until a response was made at which point the screen went blank for 1700 ms. RT and accuracy were recorded.

Results

Trials on which RTs were ≤200 or ≥3000 ms were excluded (<2 % of trials were RT outliers) as were trials following errors. Error trials were also excluded from the RT analysis. The alpha level was .05 for this and the subsequent experiments. We report both partial eta squared (η^2_p) and generalized eta squared (η^2_G) as measures of effect size.[1] Other than those reported, no other main effects or interactions were significant.[2]

To examine whether the magnitude of the TRCE effect varied across lists of relatively more or less incongruent trials, a 2 (list-wide proportion congruence: mostly congruent vs. mostly incongruent) × 2 (congruency: congruent vs. incongruent) × 2 (switch type: repeat vs. switch) within-subjects anova was performed on the RT data (see Table 1 for means). Switch Type was included as a factor because there is some evidence that the TRCE is increased on switch relative to repeat trials (e.g., Wendt & Kiesel, 2008); we confirmed that the proportion of switch trials did not differ between the mostly congruent (51 % switch trials) and mostly incongruent block (50 % switch trials). There was a significant TRCE effect as indicated by the slower RTs on incongruent ($M = 899$) as compared to congruent ($M = 866$) trials, $F(1, 51) = 11.11$, $p = .002$, $\eta^2_p = .18$, $\eta^2_G = .004$. There was also a significant switch cost as indicated by slower RTs on switch ($M = 904$) relative to repeat ($M = 861$) trials, $F(1, 51) = 26.36$, $p < .001$, $\eta^2_p = .34$, $\eta^2_G = .006$. Most importantly, there was a list-wide proportion congruence × congruency interaction, $F(1, 51) = 14.35$, $p < .001$, $\eta^2_p = .22$, $\eta^2_G = .003$. The TRCE was smaller in the mostly incongruent block as compared to the mostly congruent block (see Fig. 2). One-sample t-tests indicated that the 64 ms TRCE in the mostly congruent block was significantly different from 0, $t(51) = 4.31$, $p < .001$, but the 3 ms TRCE in the mostly incongruent block was not significantly different from 0, $t < 1$. The TRCE effect was not modulated further by switch type (for three way, $F < 1$).

Although it has been suggested that the TRCE in error rate represents a fundamentally different process (i.e., applying the wrong rule; e.g., Meiran & Kessler, 2008), for completeness, we also report the analysis of error rate. The pattern of effects was almost identical (see Table 2 for means). There was a main effect of congruency, $F(1, 51) = 39.67$, $p < .001$, $\eta^2_p = .44$, $\eta^2_G = .08$, indicative of a TRCE in error rate and a main effect of switch type due to more errors on switch than repeat trials, $F(1, 51) = 14.52$, $p < .001$, $\eta^2_p = .22$, $\eta^2_G = .01$. There was a trend for a congruency × switch type interaction due to the TRCE being larger when a switch occurred, $F(1, 51) = 3.32$, $p = .07$, $\eta^2_p = .06$, $\eta^2_G = .002$. Finally, the list-wide proportion congruence effect was evident due to the TRCE being larger in the mostly congruent block than the mostly incongruent block, $F(1, 51) = 15.57$, $p < .001$, $\eta^2_p = .23$, $\eta^2_G = .006$.

Analysis of sequential congruency effects

To determine whether sequential congruency effects (see Egner, 2007, for review) played a role in the list-wide proportion congruence effect, we conducted a 2 (congruency: congruent vs. incongruent) × 2 (previous trial congruency: congruent vs. incongruent) × 2 (list-wide proportion congruence: mostly congruent vs. mostly incongruent) ANOVA on RT. The effect of previous trial congruency was not significant, $F(1, 51) = 2.60$, $p = .11$, nor was there a congruency × previous trial congruency interaction, $F(1, 51) = 1.66$, $p = .20$, a list-wide proportion congruence × previous trial congruency interaction, $F < 1$, or a 3-way interaction with list-wide proportion congruence, $F < 1$. For error rate, neither the main effect

[1] Generalized eta squared is a more conservative estimate of effect size than partial eta squared. Generalized eta squared is additionally reported because some have suggested that it may facilitate a more common framework for effect size that can be compared across experiments of varying types (Bakeman, 2005; Olejnik & Algina, 2003).

[2] Relevant material for this paper can be accessed via Open Science Framework at https://osf.io/fveqb/. This repository contains the raw data for Experiments 1–3 as well as a summary document detailing all of the analyses conducted by T.S.B. for the manuscript, using the R statistical language, and summarized via R Markdown. This document should provide the code needed to reproduce the relevant results contained within the manuscript for each experiment. Please note that this material has not been peer-reviewed.

Table 1 Mean reaction times (SE) and mean TRCE as a function of list-wide proportion congruence, congruency, and switch type in Experiments 1 and 2

Experiment	Item type	Congruency	Mostly congruent list		Mostly incongruent list	
			Repeat	Switch	Repeat	Switch
1	Biased	Congruent	840 (15)	870 (15)	858 (14)	897 (19)
		Incongruent	895 (18)	943 (18)	852 (13)	907 (14)
		TRCE	56	72	−5	11
2	Biased	Congruent	831 (24)	878 (31)	923 (23)	983 (28)
		Incongruent	954 (30)	1028 (23)	938 (21)	1015 (24)
		TRCE	123	150	15	33
2	Unbiased	Congruent	940 (27)	935 (35)	989 (38)	1089 (41)
		Incongruent	949 (37)	1005 (44)	969 (40)	1037 (42)
		TRCE	9	69	−19	−52

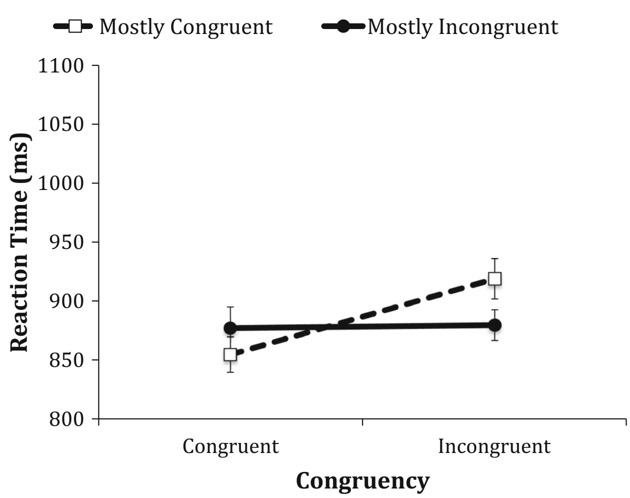

Fig. 2 Mean reaction time as a function of list-wide proportion congruence and congruency in Experiment 1. The TRCE is represented by the magnitude of the difference between congruent and incongruent trials within each list-wide proportion congruence condition. *Error bars* represent within-subjects SE of mean

of previous trial congruency nor any interaction with this factor was significant, Fs ≤ 2.14, $ps \geq .15$.

Discussion

The primary finding of Experiment 1 was a list-wide proportion congruence effect for the TRCE in a cued task-switching paradigm. The TRCE in RT (as well as error rate) was significantly smaller in the mostly incongruent list compared to the mostly congruent list (cf. Braverman and Meiran 2015), and this pattern was not modulated by the congruency of the previous trial. The list-wide proportion congruence effect for the TRCE replicates the pattern that has been observed for Stroop congruency effects (e.g., Lindsay & Jacoby, 1994; Logan & Zbrodoff, 1979; Logan, Zbrodoff, & Williamson, 1984; Lowe & Mitterer, 1982; West & Baylis, 1998; for evidence of this pattern in other conflict tasks, see Gratton et al., 1992; Hommel, 1994; Toth et al., 1995; Wendt & Luna-Rodriguez, 2009). Applying Melara and Algom's (2003) tectonic theory, one potential account of this pattern is that participants' attention was attracted to the irrelevant stimulus dimension (e.g., the number if the cue was "Attend Letter") to a greater degree in the mostly congruent than the mostly incongruent list. That is, in the mostly congruent list, participants (likely implicitly) learned that the irrelevant dimension tended to activate the correct (compatible) response and attention was attracted to it, exacerbating the interfering effect of the irrelevant response rules on the occasional incongruent trial. By contrast, in the mostly incongruent list, participants learned that the irrelevant dimension tended to activate the incorrect (conflicting) response and thereby attention was repelled from it (for a similar view based on conflict monitoring, see Botvinick, Braver, Barch, Carter, & Cohen, 2001). Notably, if this explanation is valid, this form of inhibition appears to differ from competitor rule suppression as observed by Meiran et al. (2010). They showed that competitor rule suppression did not affect the entire pathway represented by the irrelevant dimension, as our explanation implies. However, there are a number of differences between our paradigm (75 % incongruent trials; ~50 % switches; CSI = 3000 ms) and their paradigm (100 % switches; CSI = 500 ms) that may limit direct comparisons.

Experiment 2

The purpose of Experiment 2 was to replicate the list-wide proportion congruence effect and examine whether differences in the TRCE across mostly incongruent and mostly congruent lists reflect shifts at the global or item level (see Bugg, 2012, for a review of this debate in the Stroop literature). The tendency to process the irrelevant dimension to a lesser or greater degree could reflect the operation of a

Table 2 Mean error rates (SE) and mean TRCE as a function of list-wide proportion congruence, congruency, and switch type in Experiments 1 and 2

Experiment	Item type	Congruency	Mostly congruent list		Mostly incongruent list	
			Repeat	Switch	Repeat	Switch
1	Biased	Congruent	.036 (.006)	.037 (.007)	.038 (.007)	.056 (.007)
		Incongruent	.096 (.008)	.118 (.011)	.073 (.006)	.104 (.008)
		TRCE	.059	.081	.034	.048
2	Biased	Congruent	.035 (.008)	.040 (.008)	.051 (.010)	.054 (.008)
		Incongruent	.095 (.016)	.154 (.015)	.084 (.007)	.122 (.010)
		TRCE	.060	.114	.032	.067
2	Unbiased	Congruent	.043 (.018)	.071 (.015)	.038 (.015)	.058 (.014)
		Incongruent	.095 (.022)	.157 (.024)	.096 (.020)	.106 (.021)
		TRCE	.052	.086	.058	.048

globally operating mechanism such as proactive control that uniformly influences stimuli within a given list. The absence of an effect of previous trial congruency on the list-wide proportion congruence effect in Experiment 1 appears consistent with an account based on a global mechanism. However, an alternative possibility is that the shifts may occur reactively, post stimulus onset based on information conveyed by the current (trial *n*) item such as the proportion congruence of a stimulus dimension (i.e., item-specific mechanisms; cf. Blais, Robidoux, Risko, & Besner, 2007; Bugg, Jacoby, & Chanani, 2011; Bugg & Hutchison, 2013; Schmidt & Besner, 2008; Verguts & Notebaert, 2008). For instance, participants may have learned that the number "1" tended to be associated with activation of an irrelevant (conflicting) response rule in the mostly incongruent list. If so, upon presentation of a stimulus that included the number "1", attention may have quickly shifted away from the irrelevant letter. There is some evidence that list-wide proportion congruence effects in the Stroop task reflect such reactive (i.e., item-specific) mechanisms (see Blais & Bunge, 2010; Bugg, 2014, Experiments 2 and 3; Bugg et al., 2008, for evidence of such a mechanism operating within the Stroop task), and the same may be true for the TRCE (cf. Waszak, Hommel, & Allport, 2003). Indeed, preliminary support for this possibility stems from Braverman and Meiran (2015) who found that an item-specific proportion congruence manipulation modulated the magnitude of the response congruency effect (controlling for task conflict) in a task-switching paradigm, although the pattern was found in error rate only and not RT.

To determine whether a global mechanism such as proactive control was operative, we conducted an experiment very similar to Experiment 1 but in addition to having stimuli within the lists that were biased (e.g., each stimulus dimension in the mostly incongruent block was mostly incongruent), we also incorporated a unique set of 50 % congruent stimuli, stimuli for which a given dimension was associated with a congruent response on half of the trials and an incongruent response on the other half of trials (cf. Blais & Bunge, 2010; Bugg, 2014; Bugg & Chanani, 2011; Bugg et al., 2008, for use of this methodology in Stroop studies). Importantly, these 50 % congruent stimuli were randomly intermixed with the biased items in each list and were presented at the same frequency in both lists. Thus, if it is the proportion congruency of the item that is producing the list-wide proportion congruence effect for the TRCE, then we should find the list-wide proportion congruence pattern for the biased items but not the 50 % congruent items. That is, the TRCE should be equivalent for 50 % congruent items in the mostly congruent and mostly incongruent lists. By contrast, if the list-wide proportion congruence effect at least in part reflects a global shift (e.g., in attention), then a TRCE should also be evident for the 50 % congruent items.

Method

Participants

32 undergraduates meeting the same inclusion/exclusion criteria as in Experiment 1 participated for course credit or monetary compensation ($10/h).

Stimuli and design

The design and stimuli were identical to Experiment 1 save for the following exceptions. There was an additional factor (item type: biased or 50 % congruent) that was manipulated within-subjects resulting in a 2 (list-wide proportion congruence: mostly congruent vs. mostly incongruent) × 2 (switch type: repeat vs. switch) × 2 (congruency: congruent vs. incongruent) × 2 (item type: biased or 50 % congruent) within-subjects design. The stimulus set was expanded such that 16 stimuli served the role of biased items (1A, A1, 2A, A2, 1B, B1, 2B, B2, 5I, I5, 6I, I6, 7G, G7, 8G, and G8) and 8 stimuli served the role of 50 % congruent items (3E, E3, 4E, E4, 3D, D3, 4D, and D4).

Procedure

The procedure was identical to Experiment 1 with the following exceptions. The experiment was conducted in a single session with order of the mostly congruent and mostly incongruent blocks counterbalanced across subjects. The CSI was 2000 ms instead of 3000 ms, and each block of test trials comprised 224 randomly intermixed test trials. Of those trials, 192 were biased items, which were presented in the mostly congruent and mostly incongruent blocks according to the same stimulus frequencies as in Experiment 1. The remaining 32 items were unbiased (50 % congruent). For each task within a given block (mostly congruent or mostly incongruent), each unbiased congruent stimulus (four total) was presented twice and each unbiased incongruent stimulus (four total) was presented twice.

Results

The alpha level was .05. The same trimming procedures were used as in Experiment 1 resulting in the exclusion of <2 % of the data due to RT outliers.

Biased items

First, we analyzed performance on the biased items. A 2 (list-wide proportion congruence: mostly congruent vs. mostly incongruent) × 2 (congruency: congruent vs. incongruent) × 2 (switch type: repeat vs. switch) within-subjects ANOVA was performed on the RT data (see Table 1 for means). We confirmed that the proportion of switch trials did not differ between the mostly congruent (49 % switch trials) and mostly incongruent block (50 % switch trials). A TRCE effect was evident due to slower RTs on incongruent ($M = 984$) as compared to congruent ($M = 904$) trials, $F(1, 31) = 11.61$, $p = .002$, $\eta_p^2 = .27$, $\eta_G^2 = .02$. A switch cost was also observed, $F(1, 31) = 20.88$, $p < .001$, $\eta_p^2 = .40$, $\eta_G^2 = .01$. Most importantly, we replicated the list-wide proportion congruence effect because the TRCE was significantly reduced in the mostly incongruent block compared to the mostly congruent block, $F(1, 31) = 10.40$, $p = .003$, $\eta_p^2 = .25$, $\eta_G^2 = .01$ (see Fig. 3). Similar to Experiment 1, one-sample t-tests indicated that the TRCE of 136 ms in the mostly congruent block was significantly different from 0, $t(31) = 3.85$, $p < .001$, but the TRCE of 24 ms in the mostly incongruent block was not significantly different from 0, $t(31) = 1.10$, $p = .28$.

The analysis of error rate again revealed almost an identical pattern of effects (see Table 2 for means). There was a main effect of congruency, $F(1, 31) = 33.24$, $p < .001$, $\eta_p^2 = .52$, $\eta_G^2 = .16$, and switch type, $F(1, 31) = 32.84$, $p < .001$, $\eta_p^2 = .51$, $\eta_G^2 = .03$. There was also a congruency × switch type interaction due to a larger TRCE on switch than repeat trials, $F(1, 31) = 16.85$, $p < .001$, $\eta_p^2 = .35$, $\eta_G^2 = .02$, and a list-wide proportion congruence × congruency interaction, $F(1, 31) = 5.12$, $p = .03$, $\eta_p^2 = .14$, $\eta_G^2 = .01$, due to a larger TRCE in error rate in the mostly congruent compared to the mostly incongruent block.

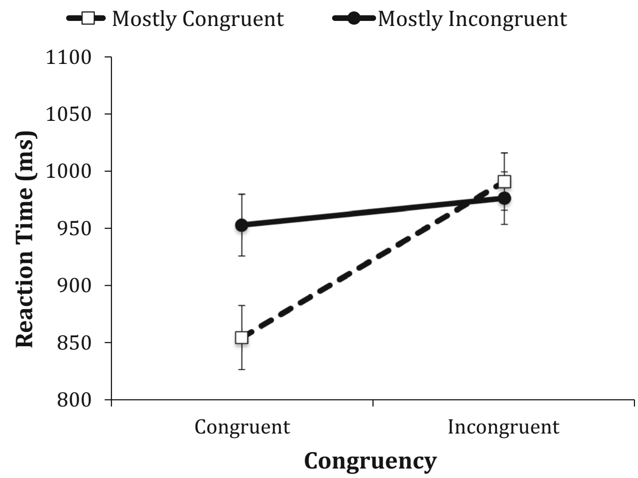

Fig. 3 Mean reaction time as a function of list-wide proportion congruence and congruency for biased items in Experiment 2. The TRCE is represented by the magnitude of the difference between congruent and incongruent trials within each list-wide proportion congruence condition. *Error bars* represent within-subjects SE of mean

50 % congruent items

Next we analyzed RT on the 50 % congruent items to determine if list-wide proportion congruence had an effect on the TRCE independent of reactive, item-specific adjustments (see Table 1 for means). The 2 (list-wide proportion congruence: mostly congruent vs. mostly incongruent) × 2 (congruency: congruent vs. incongruent) × 2 (switch type: repeat vs. switch) within-subjects ANOVA showed a main effect of switch type $F(1, 31) = 4.28$, $p = .05$, $\eta_p^2 = .12$, $\eta_G^2 = .007$, and most importantly a list-wide proportion congruence × congruency interaction, $F(1, 31) = 5.27$, $p = .03$, $\eta_p^2 = .15$, $\eta_G^2 = .003$. As was the case for the biased items, the TRCE was reduced for 50 % congruent items in the mostly incongruent as compared to the mostly congruent block (see Fig. 4). One-sample t tests indicated that neither the 39 ms TRCE in the mostly congruent block, $t(31) = 1.44$, $p = .16$, nor the reversed TRCE of—36 ms in the mostly incongruent block differed from 0, $t(31) = 1.37$, $p = .18$. For error rate, the ANOVA indicated main effects of congruency, $F(1, 31) = 25.14$, $p < .001$, $\eta_p^2 = .45$, $\eta_G^2 = .06$, and switch type, $F(1, 31) = 5.14$, $p = .03$,

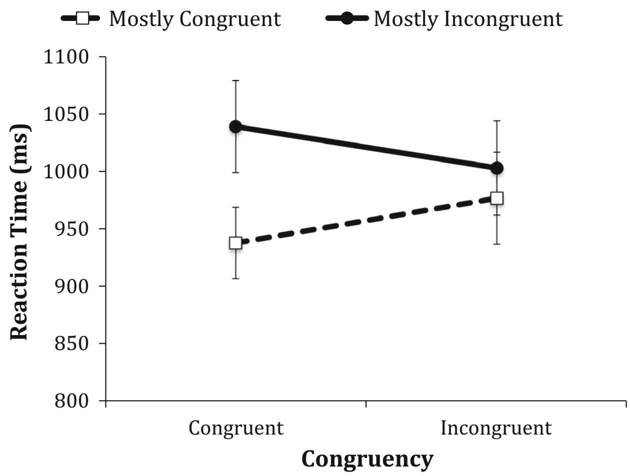

Fig. 4 Mean reaction time as a function of list-wide proportion congruence and congruency for unbiased (50 % congruent) items in Experiment 2. The TRCE is represented by the magnitude of the difference between congruent and incongruent trials within each list-wide proportion congruence condition. *Error bars* represent within-subjects SE of mean

$\eta_p^2 = .14$, $\eta_G^2 = .02$, due to more errors on incongruent than congruent trials and on switch than repeat trials, respectively (see Table 2 for mean error rates).

Analysis of sequential congruency effects

A congruency × previous trial congruency × list-wide proportion congruence ANOVA was conducted for RT on the biased items. It yielded neither a main effect of previous trial congruency nor any interaction with this factor, Fs < 1. For error rate, there were also no significant effects of previous trial congruency (largest $F = 2.26$, $p = .14$ for 3-way interaction). Overall, these findings converge with Experiment 1 in suggesting that the list-wide proportion congruence effect in the TRCE is not influenced by previous trial congruency (i.e., sequential effects). The same ANOVA could not be conducted for the unbiased items in Experiment 2 because there were subjects with no trials in certain cells due to the small number of unbiased items within each list. However, conducting the above analyses on all items (biased and unbiased combined) did not change the pattern of effects for RT or error rate, although the trend in error rate for the list-wide proportion congruence effect to be larger following a congruent than an incongruent trial was stronger ($F(1, 31) = 3.85$, $p = .06$, for 3-way interaction).[3]

[3] We additionally conducted a more sophisticated regression approach that included effects of prior trial congruency up to *five* trials back. The results confirmed the analysis of the effects of previous trial congruency (in this and all other experiments). Notably, this approach allowed us to examine the effects of prior congruency selectively on the unbiased trials in Experiment 2 and none of the effects of local conflict were reliable in the MC or MI list.

Discussion

Replicating Experiment 1, in Experiment 2, the list-wide proportion congruence manipulation modulated the magnitude of the TRCE for biased items (see also Braverman and Meiran 2015). The TRCE was significantly reduced in the mostly incongruent as compared to mostly congruent list. The novel finding in Experiment 2 was that this pattern was also evident for unbiased (50 % congruent) items that were equated across and embedded within the mostly congruent and mostly incongruent lists. The theoretical importance of the list-wide proportion congruence effect for the unbiased items is that it implies the contribution of a global shift in control to the modulation of the TRCE effect. In other words, the mechanism that produced the difference in the TRCE across lists is one that appears to influence all items within a block and not just those items that could reactively trigger differential adjustments in control due to their item-level proportion congruence. This differs somewhat from Braverman and Meiran's (2015) findings of list-wide (in RT in Experiment 1) and item-specific (in error rate in Experiment 2) proportion congruence effects, although their design did not allow for an evaluation of the contribution of a global (proactive) mechanism to the list-wide effect. One important difference is that in Braverman and Meiran's experiments, univalent stimuli were used on trials examining the response congruency effect and there was no preparatory period between the task cue and the impending stimulus, which may be important for the emergence of proactive control.

Also supporting the view that the current patterns reflect a global rather than a local mechanism, as in Experiment 1, there was little effect of previous trial congruency on the TRCE and the list-wide proportion congruence effect for the biased items was not influenced by previous trial congruency. This suggests that sequential modulations (i.e., Gratton effects; conflict adaptation effects) are not the source of the reduced TRCE in mostly incongruent as compared to mostly congruent lists. A relatively small number of unbiased items limited our ability to examine the effects of previous trial congruency selectively for the unbiased items; however, there was still little effect of previous trial congruency on RT when we combined the unbiased and biased items (see also Footnote 3).

As described earlier, one possibility is that the list-wide proportion congruence effect for the TRCE reflects variation across lists in the degree to which attention is globally attracted to the irrelevant dimension. That is, attention may be more (mostly incongruent list) or less (mostly congruent list) restricted to the cued (relevant) dimension. This may minimize response competition directly (i.e., the irrelevant dimension does not have the opportunity to elicit retrieval of the associated response from long-term memory *a la* the non-mediated route; e.g., Kiesel, Wendt, & Peters, 2007; Waszak, Pfister, & Kiesel, 2013; Wendt & Kiesel, 2008; Yamaguchi & Proctor, 2011) or indirectly by virtue of its effect on categorization of the irrelevant dimension (e.g., *a la* the mediated route; e.g., Schneider & Logan, 2005, 2010). If the latter, this calls into question the assumption that both the cued and uncued dimensions are categorized regardless of the task cue; in certain contexts (i.e., mostly incongruent list) control may be limiting task categorization processes to the relevant dimension. We further consider these possibilities in the General Discussion.

Experiment 3

Experiments 1 and 2 demonstrated that the TRCE is modulated by list-wide proportion congruence and the modulation appears to reflect global shifts in attention, consistent with a proactive control mechanism (Braver et al., 2007). The goal of Experiment 3 was to examine whether an alternative approach to stimulating proactive control produces benefits that are similar to those observed in the prior experiments (i.e., a reduction in the TRCE). In particular, we investigated the effects of a monetary incentive manipulation on the magnitude of the TRCE by comparing a mostly congruent baseline block to a mostly congruent incentive block in which participants had the opportunity to earn a performance-based monetary incentive on a random subset of trials. One potential mechanism via which incentives could affect the TRCE is by encouraging participants to more fully prepare (i.e., maintain activation of) the relevant task rules (e.g., if cue is "attend letter", participant actively holds in mind "if vowel, press left; if consonant, press right"). According to Mayr and Kliegl (2000) task preparation corresponds to the loading of the relevant rules into working memory and the removal of the irrelevant rules. On this view, one might expect increased preparation in the incentive block to be associated with reduced activation of irrelevant task rules on incongruent trials, thereby reducing the TRCE.

However, prior literature also suggests a different prediction. Schuch and Koch's (2003) response-selection account suggests that inhibition of the competing task occurs only during response selection, that is after stimulus onset once a response has been chosen. Accordingly, one might predict that increased preparation in the incentive block would have no effect on the TRCE. Additional patterns also cast doubt on the effectiveness of a preparation-oriented manipulation in reducing the TRCE. For instance, the TRCE is not affected by working memory load (Kiesel et al., 2007; Kessler & Meiran, 2010) or by preparation time (e.g., Meiran, 1996; 2000; but see Goschke, 2000; Monsell & Mizon, 2006), variables that are related to proactive control. Indeed, the latter finding has been referred to as one of the "more surprising" in the task-switching literature (Monsell et al., 2003, p. 338). Yet it is also possible that these latter patterns could be attributed to motivational factors, such as a "failure to engage" in response to the task cues (De Jong, 2000; Nieuwenhuis & Monsell, 2002). Thus, we thought that the use of monetary incentives would minimize the likelihood of a failure to engage as the source of the TRCE effect.

To validate that the incentive did in fact enhance motivation and, as a consequence, task preparation, we also measured mixing cost in addition to the TRCE effect. Mixing cost refers to the slowed responding on repeat trials within a mixed-task block relative to (repeat) trials within a single-task block. There are a few patterns in the literature that converge on the prediction that the mixing cost should be reduced in the incentive block. One, sustained activation of anterior prefrontal cortex, which is thought to represent the loading up of relevant task rules in a task-switching paradigm, is negatively related to mixing costs (Jimura & Braver, 2010). Second, in other paradigms, the presence of incentives produces a shift toward sustained activation of prefrontal cortex. That is, the incentives shift control toward a proactive mode not only on incentive trials but also on non-incentive trials in blocks in which incentives are available (Chiew & Braver, 2013; Jimura, Locke, & Braver, 2010). If the mechanism underlying the anticipated reduction in mixing costs in the incentive block (i.e., proactive preparation of the relevant task rules) affects activation of the irrelevant task rules (cf. Mayr & Kliegl, 2000), then the TRCE should also be reduced on non-incentive trials in the incentive block. On the other hand, if the control mechanisms that underlie the TRCE effect are distinct from task preparation effects, then we should see a preservation of the TRCE effect even in the context of an incentive-related reduction in mixing costs.

Method

Participants

32 undergraduates meeting the same inclusion/exclusion criteria as in the prior experiments participated for monetary compensation ($10/h).

Stimuli and design

The same (biased) stimuli were used as in Experiment 1—there were no unbiased stimuli. The design was similar to Experiment 1 save for the following exceptions. Both lists of trials were mostly (75 %) congruent with one list corresponding to the baseline block and the other list corresponding to the incentive block. Each list was preceded by single-task blocks in which participants performed either the letter task or the number task. This resulted in a 2 (incentive context: baseline vs. incentive) × 2 (block: single vs. mixed) × 2 (switch type: repeat vs. switch) × 2 (congruency: congruent vs. incongruent) within-subjects design.

Procedure

The procedure was similar to Experiment 1 with the following exceptions.[4] The experiment was conducted in a single session. Participants first completed two, 64-trial single-task blocks (one for the letter task and one for the number task; order was counterbalanced across participants) followed by the first list of 128 trials, which was a mixed-task baseline block. After completion of that block, participants again performed two single-task blocks (one for each task) before completing the second list of 128 trials, which was a mixed-task incentive block. Both the baseline block and the incentive block were mostly congruent (as were the single-task blocks). For each task within a block, each congruent stimulus (eight total) was presented six times and each incongruent stimulus (eight total) twice.

Prior to the start of the incentive block, participants received the following instructions: "from here to end, you can obtain more payment on top of regular compensation by responding faster than before and maintaining accuracy. A green cue will let you know if you are performing a trial where you can obtain a larger reward." On non-incentive trials, the task cue (attend letter or attend number) appeared in red (similar to all trials in the baseline block) and on incentive trials, the task cue appeared in green. Incentive (50 %) and non-incentive trials (50 %) were randomly intermixed within the incentive block. Participants had to respond more quickly than the median RT from the baseline block while maintaining accuracy in order to be rewarded for a given trial in the incentive block. If participants earned the reward, "Reward!" appeared in blue ink for 1250 ms following the response. If the response was too slow or inaccurate, "Too Slow!" or "Incorrect" appeared for the same duration in red ink. To equate the incentive and baseline blocks on the provision of feedback, in the baseline block, "correct" (in blue ink) or "incorrect" (in red ink) appeared for 1250 ms following the response. Other than this change and the use of a CSI that was 1500 ms, the trial-level procedure was identical to Experiment 1 (see Fig. 1).

[4] In addition to the exceptions noted in the text, for purposes unrelated to the present experiment, we presented half of the trials in the single and mixed-task blocks for both the baseline and incentive blocks in the upper portion of the screen and the other half in the lower portion of the screen. Presentation of stimuli and tasks was perfectly balanced across the two locations (i.e., each location was mostly congruent). We also asked participants to perform a secondary task in which they had to press the "y" key anytime the letter F or the number 5 occurred regardless of what task was cued during the mixed-task blocks. There were 11 trials of this nature. It was stressed that this task was secondary and it was most important for participants to perform the primary task well and try to earn an incentive when possible. Because the magnitude of the TRCE was extremely similar in Experiment 3 and in Experiment 1, which employed neither of these design features, we do not believe they had any influence on the primary results (cf. Kessler & Meiran, 2010, finding that holding in mind irrelevant task rules, such as those of the secondary task, does not affect the TRCE; see also Rubin & Meiran, 2005, finding that holding multiple stimulus-response rules in working memory does not affect mixing cost).

Results

The alpha level was .05. The same trimming procedures were used as in the prior experiments resulting in the exclusion of <1 % of the data due to RT outliers.

Manipulation check

It was possible for each participant to earn up to $5 during the incentive block. No participant earned less than $4. A one-way within-subjects ANOVA compared mean RT on non-incentive trials in the mixed-task baseline block to non-incentive and incentive trials in the mixed-task incentive block. To concentrate on "pure" incentive effects, we examined congruent repeat trials. There was a main effect of trial type, $F(2, 62) = 107.51$, $p < .001$, $\eta_p^2 = .78$, $\eta_G^2 = .43$. Post hoc tests indicated that performance was significantly faster for both incentive and non-incentive trials in the incentive block relative to the baseline block, $ps < .001$ (see Fig. 5). Moreover, in the incentive block, the incentive trials were faster than the non-incentive trials, $p < .001$. These patterns suggest that the incentive was effective in speeding performance both locally and globally within the incentive block. An effect of trial type was also found for error rate, $F(2, 62) = 4.11$, $p = .02$, $\eta_p^2 = .12$, $\eta_G^2 = .07$, and post hocs showed that error rate was 2.5 % lower in the baseline block ($M = 2.6$ % errors) than on either trial type in the incentive block ($M = 5.1$ % errors), $ps \leq .02$, suggesting the incentive manipulation produced a slight speed-accuracy tradeoff.[5]

[5] We performed two supplementary analyses to examine whether a speed-accuracy tradeoff (SAT) was responsible for the incentive-driven shifts in performance. Neither the approach of plotting a cumulative accuracy distribution nor the Heitz (2014) approach of examining macro-SAT and conditional accuracy functions supported the assumption that a SAT was driving the primary patterns pertaining to incentive-related changes in performance (i.e., faster responding and reduction in mixing cost for RT on non-incentive trials in incentive compared to baseline block). These analyses were instead consistent with a non-specific increase in errors in the incentive block.

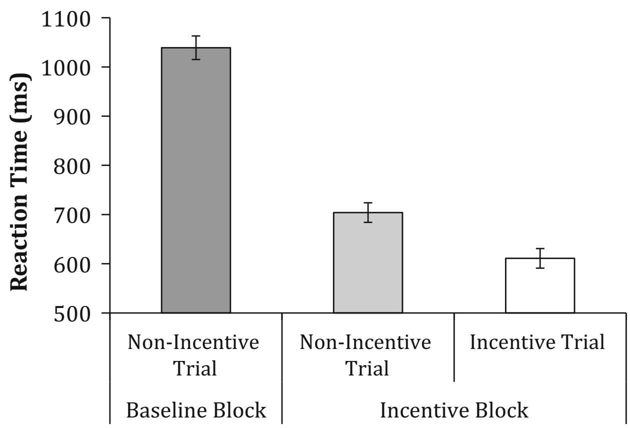

Fig. 5 Mean reaction time as a function of incentive context and trial type in Experiment 3. *Error bars* represent within-subjects SE of mean

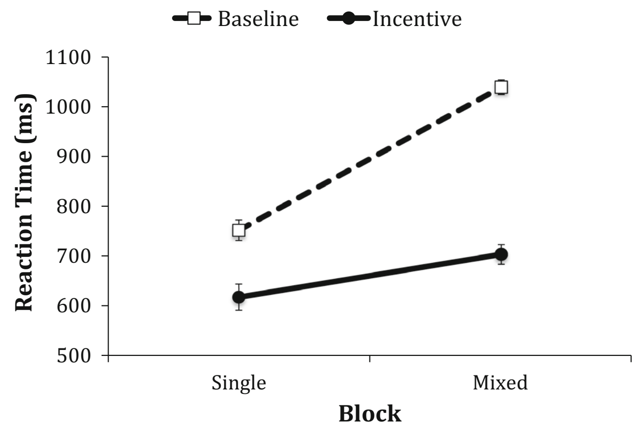

Fig. 6 Mixing costs as a function of incentive context in Experiment 3. The mixing cost is reflected in the difference between the reaction time in the single and mixed blocks. It was derived for congruent, non-incentive, repeat trials in each incentive condition. Mixed refers to congruent/repeat trials in a mixed-task block. *Error bars* represent within-subjects SE of mean.

Mixing cost

The mixing cost was derived by comparing the reaction time in the single and mixed blocks and it was derived for congruent, non-incentive, repeat trials in each incentive context. A 2 (block: single vs. mixed) × 2 (incentive context: baseline vs. incentive) within-subjects ANOVA was performed on the RTs. Main effects of block and incentive context were qualified by a significant block × incentive context interaction, $F(1, 31) = 57.66$, $p < .001$, $\eta^2_p = .65$, $\eta^2_G = .06$. The interaction was due to mixing costs (i.e., slowing in mixed relative to single-task block) being much smaller in the incentive block (86 ms) than the baseline block (287 ms; see Fig. 6). For error rate, the same analysis indicated only a main effect of incentive context, $F(1, 31) = 13.55$, $p < .001$, $\eta^2_p = .30$, $\eta^2_G = .11$, with error rate being higher in the incentive ($M = .053$) than baseline ($M = .025$) blocks (see Footnote 5). The block × incentive context interaction was not significant, $F(1,31) = 1.73$, $p = .20$.

Analysis of sequential incentive effects

To examine whether the effects of prior incentive trials might have contributed to the pattern of reduced mixing costs on non-incentive trials in the incentive condition, we conducted an analysis of previous trial type. A 2 (block: single vs. mixed) × 2 (previous type of trial: incentive vs. non-incentive) ANOVA was performed for non-incentive trial RTs. There was a main effect of previous type of trial, $F(1, 31) = 11.44$, $p = .002$, $\eta^2_p = .27$ $\eta^2_G = .004$, due to participants being 20 ms faster following an incentive than a non-incentive trial. There was also a marginal block × previous type of trial interaction, $F(1, 31) = 3.94$, $p = .06$, $\eta^2_p = .11$, $\eta^2_G = .001$, because the speed-up following an incentive relative to a non-incentive trial was larger in the mixed ($M = 32$ ms) compared to single-task ($M = 8$ ms) blocks. For error rate, again looking selectively at the non-incentive trials, there was a main effect of previous type of trial, $F(1, 31) = 5.39$, $p = .03$, $\eta^2_p = .15$, $\eta^2_G = .01$, due to participants making 1.4 % more errors following an incentive than a non-incentive trial, indicating a slight speed-accuracy tradeoff. The block × previous type of trial interaction was not significant, $F < 1$.

TRCE

To examine the effects of the incentive manipulation on the magnitude of the TRCE, we conducted a 2 (incentive context: baseline vs. incentive) × 2 (congruency: congruent vs. incongruent) × 2 (switch type: switch vs. repeat) within-subjects ANOVA on RT (see Table 3 for means). We confirmed that the proportion of switch trials did not differ between the baseline (52 % switch trials) and incentive block (52 % switch trials). To ensure comparability of the TRCE across the baseline and incentive blocks, this analysis was restricted to the non-incentive trials in the mixed blocks. There were two significant effects. One was a main effect of incentive context, $F(1, 31) = 90.68$, $p < .001$, $\eta^2_p = .75$, $\eta^2_G = .30$, due to faster RTs in the incentive block than the baseline block. The other was a main effect of congruency, $F(1, 31) = 8.79$, $p = .006$, $\eta^2_p = .22$, $\eta^2_G = .01$, due to faster RTs on congruent than incongruent trials (i.e., TRCE). The main effect of switch type approached significance, $F(1, 31) = 3.73$, $p = .06$. Most importantly, there was not an incentive

Table 3 Mean reaction times (SE) and error rates (SE) and mean TRCE as a function of incentive context, congruency, and switch type in Experiment 3

	Congruency	Baseline block		Incentive block			
				Non-incentive trials		Incentive trials	
		Repeat	Switch	Repeat	Switch	Repeat	Switch
RT	Congruent	1039 (19)	1091 (26)	704 (22)	731 (20)	611 (14)	631 (15)
	Incongruent	1129 (30)	1123 (34)	749 (26)	786 (35)	668 (27)	678 (21)
	TRCE	90	32	46	55	57	47
Error rate	Congruent	.026 (.009)	.031 (.008)	.048 (.010)	.064 (.014)	.054 (.009)	.058 (.012)
	Incongruent	.072 (.011)	.112 (.016)	.124 (.023)	.170 (.026)	.152 (.020)	.145 (.020)
	TRCE	.046	.081	.077	.106	.097	.087

context × congruency interaction, $F < 1$. The magnitude of the TRCE did not differ as a function of the incentive condition (61 ms for baseline block vs. 50 ms for incentive block; see Fig. 7). Finally, because it was of theoretical interest, we derived the TRCE for incentive trials within the incentive block. It was 52 ms, which did not differ from the non-incentive trials within the incentive block ($F < 1$ for trial type × congruency interaction). For error rate, there were main effects of incentive context, $F(1, 31) = 10.24$, $p = .003$, $\eta_p^2 = .25$, $\eta_G^2 = .05$, congruency, $F(1, 31) = 36.01$, $p < .001$, $\eta_p^2 = .54$, $\eta_G^2 = .16$, and switch type, $F(1, 31) = 6.87$, $p = .01$, $\eta_p^2 = .18$, $\eta_G^2 = .02$ (see Table 3 for means). As was the case for RT, the incentive context × congruency interaction was not significant, $F(1, 31) = 1.90$, $p = .18$. There was also no reduction in the TRCE for the incentive relative to non-incentive trials in the incentive block ($F < 1$ for trial type × congruency interaction) (see Table 3).

Analysis of sequential congruency effects

We conducted a congruency × previous trial congruency × incentive context ANOVA to examine sequential effects of previous trial congruency in the baseline and incentive blocks. (Because all lists in Experiment 3 were mostly congruent, proportion congruence was not a factor). There were no significant effects involving previous trial congruency other than a congruency × previous trial congruency interaction, $F(1, 31) = 5.88$, $p = .02$, $\eta_p^2 = .16$, $\eta_G^2 = .003$. The TRCE effect was 73 ms following a congruent trial and reduced to 22 ms following an incongruent trial, and this pattern did not differ as a function of incentive context ($F < 1$ for the 3-way interaction and all other effects of previous trial congruency). For comparability to Experiments 1 and 2, we separately examined the baseline block, and the congruency × previous trial congruency interaction was not significant, $F(1, 31) = 1.90$, $p = .18$. For error rate, the congruency × previous trial congruency × incentive context ANOVA revealed no significant effects involving previous trial congruency, $F\text{s} < 1$.

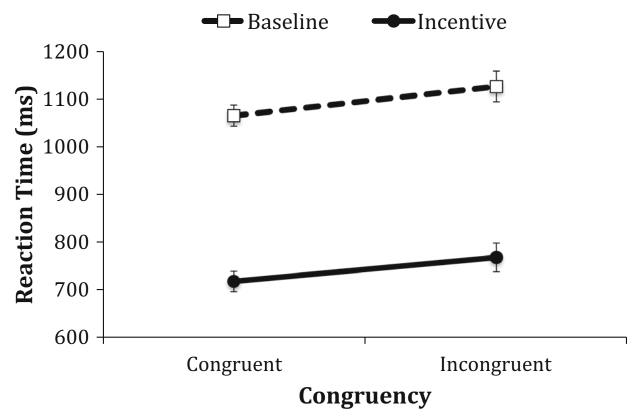

Fig. 7 Mean reaction time as a function of incentive context and congruency for non-incentive trials in Experiment 3. The TRCE is represented by the magnitude of the difference between congruent and incongruent trials within each incentive context. *Error bars* represent within-subjects SE of mean

Discussion

There were two primary findings in Experiment 3. First, there was a significant reduction in mixing costs in an incentive block in which a random half of trials was associated with a performance-based monetary reward relative to a baseline block. Because this pattern was observed for non-incentive trials, it suggests that participants were using the incentive context to prepare the relevant task rules (e.g., by loading them into an active portion of working memory) globally (i.e., not just on incentive trials) during the incentive block (cf. Jimura et al., 2010). This pattern suggests that the incentive block reduced “failure to engage” tendencies that typically occur in response to task cues during task-switching blocks, such as in the baseline condition (cf. De Jong, 2000; Nieuwenhuis & Monsell, 2002). In fact, it appears that the speed-up on non-incentive trials in the incentive block partially reflects a local effect of having just performed an incentive trial (i.e., been motivated to engage in preparation) on the previous trial; however, the speed-up associated with being within the incentive block as compared to the baseline block was much greater, suggesting that the reduction in mixing costs in the incentive block was primarily due to a sustained mechanism (cf. Jimura & Braver, 2010).

The suggestion that being within the incentive context led to global preparation of the relevant task rules that extended beyond the cued incentive trials to non-incentive trials is also consistent with the fact that the incentive manipulation had an equivalent effect on switch and repeat trials. Yet this latter pattern may at first appear inconsistent with a proactive control mechanism. In a recent review, Ruge, Jamadar, Zimmerman, and Karayanadis, (2013) proposed that not all preparatory modes in task-switching paradigms are associated with differential effects on switch vs. repeat trials. One type of preparatory mode involves resolution of competition created by the action or attentional set from the previous trial via direct adjustments to the configuration of those sets, and is associated with enhanced activation on switch relative to repeat trials. However, the second type of preparatory mode, which involves only advance goal activation, is associated with a similar recruitment of preparatory processes for switch and repeat trials, in line with the findings of Experiment 3.

Quite interestingly, in spite of participants' enhanced preparation in the incentive block, there was no effect of the incentive manipulation on the TRCE either on non-incentive trials or incentive trials. Instead, for both lists (mostly congruent with and without incentive), the magnitude of the TRCE was similar to that which was observed in the mostly congruent list of Experiment 1 using similar stimuli and a similar procedure. This seems somewhat inconsistent with views that characterize preparation in a task-switching paradigm as involving activation of the relevant task rules and removal of the irrelevant task rules from working memory (Mayr & Kliegl, 2000). One possibility is that preparation may serve to activate the relevant rules above some baseline level in working memory but may not serve to deactivate the irrelevant rules. This is entirely consistent with the response-selection account of task inhibition (Schuch & Koch, 2003) which posits that deactivation of irrelevant task rules is triggered only at the point of response selection, and not prior to it (during the preparatory period). Another possibility is that both sets of rules may be stored in long-term memory (e.g., Meiran & Kessler, 2008) and the irrelevant rules (or responses) may be activated automatically regardless of the degree of activation of the relevant task rules. For instance, attention to the irrelevant dimension at the time of stimulus onset may directly activate the associated response (i.e., via the non-mediated route).

General discussion

The present set of experiments examined two theoretically motivated approaches to modulating the TRCE, the interference that arises when an irrelevant task rule and corresponding response is activated in a task-switching paradigm. One approach, presenting relatively more incongruent trials within a given list, was highly effective in reducing the magnitude of the TRCE (cf. Braverman and Meiran 2015). By contrast, the second approach, which entailed enhancing preparation of relevant task rules by providing monetary incentives for performance, was ineffective in modulating the TRCE. The TRCE did not differ across two mostly congruent lists that differed only in whether the incentive was available (or was not available) on a proportion of trials.

These patterns are provocative as they raise the theoretically important question of why two seemingly proactive manipulations had differing effects on the TRCE. One possible explanation is that we are mischaracterizing one or both manipulations as inducing proactive control. It seems that the incentive manipulation used in Experiment 3, in which participants were cued about the potential for reward in advance of stimulus onset, can comfortably be viewed as a manipulation that induces proactive control. This assumption is supported by prior neuroimaging and behavioral data (Chiew & Braver, 2013; Jimura & Braver, 2010; Jimura et al., 2010). It is more debatable as to whether the list-wide proportion congruence manipulation used in Experiments 1 and 2 also induces proactive control. Indeed, this is presently debated within the Stroop literature (for reviews see Bugg, 2012; Bugg & Crump, 2012). However, an important contribution of Experiment 2 was demonstrating that the list-wide proportion congruence pattern reflected a global shift in attention, that is, one that affected all items in the list not just those that could have triggered reactive, item-specific adjustments (see also Bugg, 2014, Experiments 1 & 4; Bugg & Chanani, 2011; Bugg, McDaniel, Scullin & Braver, 2011; Hutchison, 2011 for evidence in support of this view from the Stroop literature). This type of global mechanism aligns well with conceptualizations and neural signatures of proactive control in the Stroop task (Braver et al., 2007; De Pisapia & Braver, 2006). The idea that proactive control contributed to the reduced TRCE in the mostly incongruent list also potentially explains why Meiran and Kessler (2008) found a significant and sizeable TRCE of 84 ms when they employed a mostly incongruent list (there was no mostly congruent list for comparison), whereas the TRCE in the mostly incongruent lists (on the comparable biased items) in the present experiments ranged from 3 to 24 ms. In their study, the CSI ranged from 0 to 900 ms and CSI interacted with congruency. Possibly, there was not sufficient time to (proactively) restrict attention to the cued dimension on the shorter CSI trials in their experiment; in the present experiments, the shortest CSI in a mostly incongruent list was more than double the length of their longest CSI.

In addition to ruling out item-specific adjustments as the explanation for the list-wide proportion congruence effect in the TRCE, we additionally showed that the effect was not driven by sequential modulations in control caused by local (previous trial) congruency. The relatively long ISIs (see Fig. 1) used in Experiments 1 and 2 may have led to dissipation of any modulations in control triggered by conflict on the previous trial.[6]

Another potential explanation for the list-wide proportion congruence pattern relates to a different mechanism–temporal learning. The temporal learning account suggests that the effect is not due to proportion congruence per se but instead reflects that participants are learning different rhythms of responding across lists (Schmidt, 2013, 2014). If participants respond to most previous trials quickly (i.e., mostly congruent list), this prepares them to respond early on subsequent trials, which benefits congruent trials and results in no advantage on incongruent trials. If participants respond to most previous trials slowly (i.e., mostly incongruent list), this prepares them to respond later, which benefits incongruent but not congruent trials. The combination leads to the list-wide proportion congruence pattern. This account has not been applied to task switching and it is unclear if participants do learn the rhythm of responding in such paradigms given the relatively (compared to Stroop) long inter-trial intervals. Yet even if participants are sensitive to the rhythm of responding during task switching, a finding that challenges the temporal learning account is the fact that incentives did not modulate the TRCE in Experiment 3. A "pure" proactive control account of the TRCE related to proportion congruence would not predict any difference related to the incentive manipulation because in both baseline and incentive blocks proportion congruence was equally high (75 %). However, the pace of responding differed dramatically across these blocks (see Fig. 5), which presumably should have led to modulation of the TRCE if temporal learning processes contributed to task performance.

Another potential explanation for the differing effects of the two seemingly proactive manipulations relates to Meiran et al.'s (2010) proposal that overcoming the TRCE requires a "subtle" or "fine-tuned" form of inhibition. The idea is that one cannot strongly or fully inhibit the irrelevant task rules upon performance of a given task because one needs to maintain readiness to activate these rules in the near future. In this respect, a potentially important difference between the two manipulations used herein is that the list-wide proportion congruence manipulation might be viewed as inducing an unintentional or implicit form of control, whereas incentives arguably involve intentional or explicit adjustments. There is evidence from the Stroop literature that participants are not explicitly aware of the proportion of congruent and incongruent trials in a given list; moreover, accuracy in reporting proportion congruency is unrelated to the magnitude of the list-wide proportion congruence effect (Blais, Harris, Guerrero, & Bunge, 2012). Blais et al. (2012) have therefore argued that this effect reflects implicit adaptations in control (e.g., based on the statistical learning of the frequencies of particular trial types). The same may be true for the effects observed in Experiments 1 and 2. That is, the reduced TRCE in the mostly incongruent list may not be due to any intentional strategy participants are utilizing but may instead reflect implicit attentional biases that develop based on experience with a list (e.g., attention is more or less attracted to the irrelevant dimension). Nevertheless, such an implicit account does not rule out a proactive control interpretation, in which the implicit shift in attentional bias is tonically maintained across trials.

The incentive manipulation of Experiment 3 may have evoked a different set of processes, however, in which participants intentionally and differentially prepared for the upcoming task in the block in which incentives were frequently and randomly available (relative to the baseline block). As described earlier, this preparation likely involved activating the relevant rules for the upcoming task. However, the irrelevant task rules may have been equally accessible when incentives were available as when they were not (cf. Meiran & Kessler, 2008; Schuch & Koch, 2003), and hence no modulation of the TRCE was observed. In general, the idea that implicit adjustments in control may allow for a more subtle or fine-tuned inhibitory influence on irrelevant task rules than may be produced via intentional adjustments converges with recent Stroop findings. Here it was found that control was better or as well calibrated to the list-wide context (likelihood of interference) when operating (implicitly) based on experience with the task, as compared to when participants were explicitly cued regarding proportion congruency and instructed to prepare based on such expectations (see Bugg, Diede, Cohen-Shikora, & Szelmecy, 2015).

It is also important to consider how the current findings fit with extant explanations of the TRCE. One is the mediated route explanation (e.g., Schneider, 2014;

[6] The relatively long ISIs used in the present experiments may also explain why switch costs were relatively small. The relatively long cue-to-stimulus interval provided ample time for preparation of the current task set, while at the same time the relatively long response-to-cue interval allowed for dissipation of the previous set. Both may have served to reduce switch costs.

Schneider & Logan, 2005, 2010) that explains TRCE effects as reflecting dual-task processing—the categorization of stimuli according to both tasks even when a given task (e.g., attend number) is selectively cued. It is possible that the list-wide proportion congruence manipulation influenced the TRCE by differentially limiting task categorization processes to the relevant dimension. Categorization of the irrelevant dimension may have been more or less likely depending on the surrounding context (i.e., less likely when categorization tended to yield conflicting responses as in a mostly incongruent list). The non-mediated route explanation of the TRCE effect attributes the effect to the retrieval of responses from long-term memory based on a build-up of instances of target-response experiences that have accumulated during the task. Applying this explanation to the findings of Experiments 1 and 2, in the mostly incongruent condition, there may have been a shift towards proactively constraining attention to the relevant dimension (thereby limiting activation of responses associated with the irrelevant dimension) or towards a heavier weighting of the response activated by the relevant dimension (assuming both responses are retrieved) (cf. Logan, 1980, for a similar account in Stroop). Further research is needed to adjudicate between these views and others. For instance, Meiran, Kessler, and Adi-Japha (2008) proposed a theory differentiating response set and stimulus set biasing. Possibly the TRCE is especially sensitive to the strength of response set biasing (whereas mixing costs may be sensitive to stimulus set biasing).

In summary, the present study enhanced our understanding of factors that influence the activation of irrelevant task rules during cued task switching. Manipulating the proportion of congruent to incongruent trials strongly influenced the magnitude of the TRCE for biased and unbiased (50 % congruent) items providing a clear demonstration that the TRCE is linked to variation in global attentional control processes (cf. Waszak et al., 2013, for evidence that automatic S-R translation of arbitrary S-R mappings is influenced by instructions via top-down control). In contrast, manipulating task preparation by the provision of a monetary incentive did not affect the TRCE. These patterns support the view that there may be multiple routes by which proactive control can influence task-switching performance; however, there appears to be a selective effect of particular routes on the automatic retrieval of irrelevant task rules as evidenced by the divergent effects of the proportion congruence and incentive manipulations on the TRCE. Future research might combine the two manipulations in a single experiment to further evaluate whether proportion congruence and incentives have independent effects on cued task-switching performance.

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