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Cueing Cognitive Flexibility: Item-Specific Learning of Switch Readiness

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The rich behavioral repertoire of the human species derives from our ability to flexibly reconfigure processing strategies (task sets) in response to changing requirements. This updating of task sets is effortful, as reflected by longer response times when switching a task than repeating it (switch costs). However, some recent data suggest that switch costs can be reduced by cueing switch readiness bottom-up, by associating particular stimuli with frequent switch requirements. This type of “stimulus-control (S-C) learning” would be highly adaptive, as it combines the speed of automatic (bottom-up) processing with the flexibility and generalizability of controlled (top-down) processing. However, it is unclear whether S-C learning of switch readiness is truly possible, and what the underlying mechanisms are. Here we address these questions by pairing specific stimuli with a need to update task-sets either frequently or rarely. In all 3 experiments, we observe robust item-specific switch probability (ISSP) effects as revealed by smaller switch costs for frequent switch items than for rare switch items. By including a neutral condition, we also show that the ISSP effect is primarily driven by S-C learning reducing switch costs in frequent switch items. Furthermore, by employing 3 tasks in Experiment 3, we establish that the ISSP effect reflects an enhancement of general switch readiness, rather than of the readiness to switch to a specific alternate task. These results firmly establish that switch readiness is malleable by item-specific S-C learning processes, documenting that a generalizable state of cognitive flexibility can be primed by a bottom-up stimulus.

Public Significance Statement

A hallmark of human behavior is cognitive flexibility, the ability to give different responses to the same stimulus, depending on our current task. Flexibly shifting between tasks is effortful, however, as reflected in “switch costs”: responses are slower when changing than when repeating tasks. Here we tested whether we could improve people’s ability to switch by presenting particular visual stimuli (e.g., a picture of a dog) more often in a context where people were cued to switch tasks than to repeat tasks. Over three experiments, we found that people learned to associate these stimuli with a greater “switch readiness,” as reflected in reduced switch costs when having to shift tasks in the context of such frequent switch stimuli than of stimuli that were only rarely paired with the need to switch. This suggests that, through learning, particular stimuli can come to serve as external triggers of cognitive flexibility.

Keywords: task-switching, cognitive control, memory, associative learning

Cognitive control refers to a collection of processes that allow us to flexibly coordinate thoughts and actions according to internal goals, and is thought to be required in situations where we need to override well-learned stimulus-response (S-R) associations or habits. Key cognitive control functions include the ability to maintain task-relevant information (a task set) to guide information processing (Desimone & Duncan,

1995), the ability to shield ongoing task sets from interference by minimizing task-irrelevant processing (conflict-control; Botvinick, Braver, Barch, Carter, & Cohen, 2001) as well as the ability to update task sets in response to changing goals (task-switching; Monsell, 2003). Although controlled processing is considered effortful and time-consuming in comparison to habitual or automatic processing, it grants the flexibility to connect stimuli and responses in novel, context-dependent ways. The costs and benefits of controlled versus automatic processing are juxtaposed in classic laboratory tasks such as Stroop tasks. For example, in the color-word Stroop task (Stroop, 1935), participants are asked to identify the print color of color-words while suppressing habitual word reading. The cost of exerting control over habitual processing is revealed by the Stroop interference or conflict effect: Slower correct color-naming responses for incongruent items (e.g., the word “RED” in green ink) as compared to congruent ones (e.g., the word “BLUE” in blue ink).



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Our ability to efficiently exert control varies over time and situations, and is thought to adapt strategically to changing demands (Botvinick et al., 2001; Braver, 2012; Bugg, Jacoby, & Chanani, 2011; Egner, 2014). For example, when control is required frequently during a particular temporal frame (e.g., a block of trials), people seem to up-regulate control accordingly (Dreisbach & Haider, 2006; Jiang, Beck, Heller, & Egner, 2015; Lindsay & Jacoby, 1994; Logan & Zbrodoff, 1979). In the Stroop task, this is reflected in the proportion congruence (PC) effect: Mean Stroop inference tends to be considerably smaller in a block where 75% of the trials are incongruent (frequently incongruent) as compared to a block where only 25% of the trials are incongruent (rarely incongruent). The PC effect is typically interpreted as reflecting a strategic adaptation whereby, in response to experiencing a preponderance of trials that require suppression of the automatic word reading process, participants enhance their attentional focus on the color naming process in a preparatory fashion to minimize the interference from automatic word reading (Lindsay & Jacoby, 1994).

Bottom-Up Priming of Conflict Control

In contrast to this type of strategic, anticipatory adjustment of top-down control, recent work suggests that control regulation can also be driven by bottom-up priming via learned associations between a contextual cue or a particular stimulus and appropriate control states (Egner, 2014; Spapé & Hommel, 2008; Verguts & Notebaert, 2008). Such stimulus-control state (S-C) associations allow appropriate control states (e.g., increased attentional selectivity) to become directly associated with, and retrieved by, exogenous stimuli such as contextual cues (e.g., locations, colors, or sensory modalities) or task-relevant stimuli. For example, by linking the proportion of (in)congruent stimuli to being presented at a particular location (which serves as a contextual cue), interference is reduced for trials appearing in the location (e.g., left side of the screen) where incongruent trials are frequent compared to a location where incongruent trials are rare (Crump, Gong, & Milliken, 2006; King, Donkin, Korb, & Egner, 2012; King, Korb, & Egner, 2012).

Similar to this “location-specific” PC effect, an “item-specific” PC effect has also been demonstrated (Bugg & Hutchison, 2013; Bugg et al., 2011; Chiu, Jiang, & Egner, 2017; Jacoby, Lindsay, & Hessels, 2003). In these studies, a smaller mean Stroop interference effect was found for a task-irrelevant stimulus feature (e.g., the word “BLUE”) that predicts incongruent trials 75% of the time as compared to a stimulus (e.g., the word “GREEN”) that predicts incongruent trials 25% of the time. As argued in these studies, the item-specific PC effect cannot be attributed to anticipatory top-down control adjustments because participants do not know which stimulus will be presented on a given trial. Instead, the item-specific PC effect must arise from associative mechanisms whereby the appropriate control state is linked to, and subsequently retrieved by, the predictive stimulus in a rapid, bottom-up manner (Bugg et al., 2011; Jacoby et al., 2003). However, a confounding factor in the original item-specific PC design (Jacoby et al., 2003) has led some researchers to conclude that this effect is driven by an alternative contingency learning mechanism whereby the participants learned to predict the correct response based on direct S-R associations, for instance, the word “GREEN”

predicting a “green” response most of the time (Schmidt & Besner, 2008). However, a more recent experimental design proposed by Bugg and colleagues (Bugg & Hutchison, 2013; Bugg et al., 2011) has de-confounded S-C from S-R learning and established an item-specific PC effect that is exclusively driven by S-C learning. The notion of S-C learning is important because it breaks down the traditional dichotomy of mutually counteracting top-down versus bottom-up drivers of attention (Awh, Belopolsky, & Theeuwes, 2012) and represents a means to combine the merit of the speed of automatic processing with the generalizability and flexibility of controlled processing (Egner, 2014).

Bottom-up Priming of Task-Set Updating

While S-C learning has thus been demonstrated in the context of conflict-control, it is not known whether the control operation of task-set updating can also be linked to specific stimuli and be subsequently retrieved to modulate task-switching performance. In task-switching paradigms, participants are typically asked to switch between two tasks that can be performed on a single set of stimuli (Allport, Styles, & Hsieh, 1994; Jersild, 1927; Meiran, 1996; Rogers & Monsell, 1995), such as judging whether a number is odd or even and whether a number is smaller or bigger than five. The general finding is that when switching tasks, participants’ responses are slower and more error-prone than when repeating the same task, and these switch costs (response time [RT] of switch vs. repeat trials) are used to index the control requirements of task-set updating in response to a change in behavioral goals. Previous studies have shown that associative learning interacts with task-set updating processes and modulates switch costs. In particular, learning of S-R associations or stimulus-task associations reduces switch costs by activating specific responses or specific task sets directly (Koch & Allport, 2006; Waszak, Hommel, & Allport, 2003, 2004, 2005). Furthermore, contextual cues, such as stimulus location that signal switch probability have also been shown to modulate switch costs (Crump & Logan, 2010; Leboe, Wong, Crump, & Stobbe, 2008), similar to the location-specific PC effect in the Stroop task. It is therefore plausible that direct stimulus-control state (S-C) associations, akin to the item-specific PC effect in the Stroop task, could modulate task-switching performance.

This idea has received partial support from a recent experiment by Leboe and colleagues (Leboe et al., 2008). In that experiment, participants switched between judging whether a word stimulus referred to a land animal versus a sea animal (habitat judgment) and judging whether the word referred to a big animal versus a small animal (size judgment). While keeping the overall probability of switching at 50%, unbeknownst to the participants, half of the words appeared on switch trials 75% of the time and the other half of the words appeared on switch trials 25% of the time. In other words, half of the words were frequently paired with a need to update task sets whereas the other half of the words were rarely paired with that requirement. Similar to the item-specific PC effect in the Stroop task, Leboe et al. (2008) observed a significant item-specific switch probability (ISSP) effect: switch costs were smaller for the frequent switch items as compared to the rare switch items. To our knowledge, this is the only previous study to demonstrate an ISSP effect. It is therefore not clear yet whether this effect is reliable and generalizable to other task-switching paradigms. Furthermore, in that experiment, the response keys for

the two tasks did not overlap with each other (i.e., two sets of keys were designated for the two tasks, respectively). This means that, when participants switched tasks, they were also required to switch response keys. Therefore, it is unclear whether the smaller switch costs in the frequent switch items were driven by item-specific associations with the process of task-set updating or instead with the process of response-key shifting. Moreover, due to the lack of a neutral condition (50% switch likelihood) in Leboe et al.'s (2008) task design, it was impossible to gauge whether the ISSP effect was driven by improved task-switching performance on high-probability switch items or by impaired task-switching performance on low-probability switch items (or a mixture of the two). Finally, and perhaps most importantly, when employing a design with only two tasks, it is impossible to infer whether an ISSP effect reflects an increased readiness to switch to a *specific* alternate task set or whether this effect might reflect a more profound phenomenon; namely, that a bottom-up stimulus can prime a *generalizable* state of cognitive flexibility that would facilitate switching to *any* other task.

The Current Experiments

Here, we present three experiments aimed at resolving these questions. The goal of the first experiment was to replicate Leboe et al.'s (2008) finding of a basic ISSP effect while controlling for potential biases in response shifting. We used two simple cognitive tasks involving digit categorizations. To isolate the influence of the task-set updating control process within the item-specific switch probability manipulation, we employed an overlapping response set for the two tasks. This manipulation also resulted in half of the stimuli cueing compatible responses (i.e., the same key) for the two tasks and the other half cueing incompatible responses (i.e., two different keys) for the two tasks. For stimuli that cued compatible responses, participants could in theory generate the correct response by learning direct S-R associations and bypassing the controlled processes of task-set retrieval and digit categorization (cf., Dreisbach, Goschke, & Haider, 2007). If this were the case, the ISSP manipulation would have no effect on the response compatible stimuli because S-C learning would not occur for these stimuli. Thus, using an overlapping response set for the two tasks allowed us to control for potential cueing of response shifts and examine the possibility of differential learning for stimuli that cue incompatible responses for the two tasks (pure S-C learning) compared to those that cued compatible responses, thus facilitating S-R learning.

In the second experiment, we examined the exact source of the ISSP effect. In both Leboe et al.'s (2008) study and our first experiment, the effect represents an interaction between the factor of switch probability (high vs. low) and the factor of task switch condition (repeat vs. switch). This means that the ISSP effect could be a result of reduced switch costs in the frequent switch items, or a result of increased switch costs in the rare switch items, or a combination of the two. Therefore, from those experiments alone, it remains unclear what the mechanism is by which S-C learning modulates switch costs. We adjudicated between these hypotheses in Experiment 2. To this end, we included a set of stimuli that were associated with an equal probability of switch and repeat trials. The inclusion of a 50% switch probability condition provided us a baseline condition with which to compare switch costs in the 75%

and 25% switch probability conditions. Finally, as the first two experiments, as well as the Leboe et al.'s (2008) study, employed only two tasks, when an item predicted a switch, it not only predicted the need to update a task set but also predicted the specific alternative task that subjects needed to switch to (e.g., judging the parity of a digit). In Experiment 3, we therefore tested whether the ISSP effect reflects the association of specific stimuli with a generic readiness for task-set updating or whether it just reflects enhanced readiness to switch to a particular alternative task set. In order to do so, we included a third task and ensured that the item-specific switch probability manipulation was not tied to a specific task. In other words, a particular stimulus would only predict the probability of a switch but not the specific task that it would switch to/from.

Experiment 1

Method

Participants. Sixty-five Amazon Mechanical Turk workers provided informed consent, which was approved by the Duke University Institutional Review Board. Participants were compensated with \$3. The sample size was determined by a power calculation based on the effect size of an item-specific PC study in our lab (Chiu et al., 2017). With a power of .8 and a Type I error of .05, the estimated sample size was 60. We recruited five additional participants in anticipation of poor performance or data loss (i.e., participants who finished the experiment but did not successfully upload their data to our server). Data from two participants were excluded as they made more than 35% of errors in the study. Sixty-three participants (Mean age = 34, $SD = 7$; 32 males) were included in the analysis.

Stimuli. The stimulus set consisted of the digits 1–4 and 6–9, rendered in Helvetica font with a font size of 60 pixels (0.63-in in height and 0.36-in in width), and displayed in either blue or red color (Figure 1A).

Design and procedure. On each trial, subjects were cued, via the color of the stimulus, to perform one of two possible tasks, a parity task (deciding whether the digit was even or odd) or a magnitude task (deciding whether the digit was smaller or larger than five). The color-to-task mappings were counterbalanced across participants. While keeping the probability of switching versus repeating a task to 50% at the experiment level, we manipulated this probability at the item level. Unbeknownst to the participants, half of the stimuli were associated with 25% task-switch trials and the other half were associated with 75% task-switch trials. Specifically, trials were generated with a randomization procedure with the following constraints: (a) the incidence of the two tasks was equated, (b) the incidence of each stimulus was equated, (c) exactly α percentage of trials of a stimulus were switch trials for items in the α switch probability condition while the remaining trials were repeat trials ($\alpha = 25, 75$), and (d) the difference in trial count for each stimulus appearing in one task versus the other task was less than four trials.

The assignment of digits to switch probabilities was balanced across the different possible stimulus categories defined by digit magnitude and parity. Specifically, given this stimulus set, there are two digits falling into each of the categories of even and <5 , odd and <5 , even and >5 , odd and >5 . For each subject, one digit



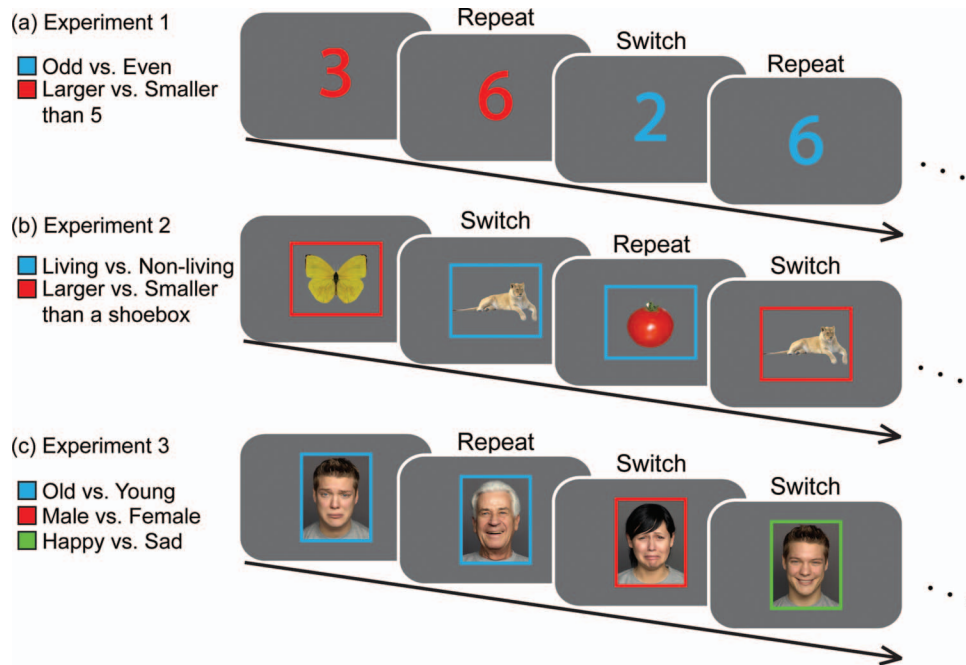


Figure 1. Task-switching procedure for Experiments 1–3 (a–c). In all three experiments, on each trial, participants were cued to perform one of two (or three) categorization tasks on the presented stimulus. Stimuli in Experiment 1–3 were color digits, color photographs of objects (reproduced, with permission from Moreno-Martínez & Montora, 2012) and color photographs of faces (reproduced, with permission from Ebner, Riediger and Lindenberger, 2010), respectively. In Experiment 1, the task to perform was cued by the color of the stimuli, whereas in Experiments 2 and 3, the task was cued by the color of a frame placed around the stimuli. See the online article for the color version of this figure.

from each of these categories was randomly assigned to the 25% switch probability condition and the other one to the 75% switch probability condition. This ensured that stimulus categories/responses were equated across the two switch probability conditions (75% vs. 25%). In other words, since the factor of task switch condition (repeat vs. switch) is orthogonal to the currently cued task, as well as to the response required of the participant, the switch probability manipulation was neither confounded by biased stimulus-task associations nor by biased stimulus-response associations.

Two keys on a standard QWERTY keyboard (*G* and *J*) were used to indicate the category of the stimulus. Stimulus category-to-response key mappings were counterbalanced across participants. As we used overlapping response keys for the two tasks, half of the stimuli cued incompatible responses and the other half cued compatible responses for the two tasks. We included the factor of response compatibility in the analysis to compare potential benefits derived from S-R learning (which is possible for response-compatible stimuli) with those derived from pure S-C learning (for response-incompatible stimuli).

There were 144 trials per block for eight blocks with the first block designated as a burn-in to familiarize participants with the item-specific switch probability manipulation. Each trial started with a fixation for 300 ms, following by a colored digit for 1,200 ms appearing at the center of the screen, during which participants could make a response, and after which they received written feedback (correct or incorrect) for 300 ms. Participants were

instructed to respond as fast as possible without sacrificing accuracy before the task stimuli disappeared (i.e., within 1,200 ms). Nevertheless, their responses were recorded even if they occurred after the task stimulus disappeared. Before the main task, participants familiarized themselves with the assigned stimulus-response mapping in one or more practice blocks (to reach an 80% accuracy criterion) in which the switch probability for all stimuli was kept at 50%. See Figure 1A for example trials.

Analysis. To address the question of whether subjects can learn item-specific control over task-switching processes, both accuracy and response time (RT) data were subjected to a 2 (task-switch probability: 75%, 25%) \times 2 (trial type: switch, repeat) repeated measures analysis of variance (ANOVA). In addition, we performed a 2 (response compatibility: compatible, incompatible) \times 2 (task-switch probability: 75%, 25%) \times 2 (trial type: switch, repeat) ANOVA to examine whether participants displayed differential learning for stimuli that cue incompatible responses for the two tasks (pure S-C learning) compared to those that cued compatible responses, thus facilitating possible S-R learning. Analyses involving RT data excluded trials with incorrect responses (incorrect button press or no button press) and trials with excessively fast or slow responses (<150 ms or $>1,200$ ms; $M = 0.23\%$). We performed post hoc paired *t* tests to follow up on a significant interaction effect. We report means (i.e., response time, accuracy) along with their 95% confidence intervals (CIs). CIs were calculated using the bootstrap resample ($N = 50,000$) with replacement technique. For effect-size measures, we report η_p^2 for

ANOVAs and Cohen's d (mean difference score/standard deviation of the difference score) for paired t tests.

Results

As expected, participants took longer to respond on switch trials than on repeat trials, $F(1, 62) = 200.45$, $\eta_p^2 = .76$, $p < .001$. The mean response times (RT) did not differ between the items associated with a high probability of switching and those with a low probability of switching, $F(1, 62) = .03$, $\eta_p^2 = .001$, $p > .05$. However, the magnitude of switch costs (switch trial RT vs. repeat trial RT) was different between items in the two switch probability conditions (75% vs. 25%), as reflected in a significant Switch Probability \times Trial Type interaction, $F(1, 62) = 5.39$, $\eta_p^2 = .08$, $p = .02$. Specifically, switch costs were smaller for items associated with a high probability of switching ($M = 60$, 95% CI [51, 69]) as compared to those associated with a low probability of switching ($M = 66$, 95% CI [57, 75]), $t(62) = 2.28$, $p < .05$, Cohen's $d = 0.29$ (Figure 2A). These results demonstrate that switch costs can be modulated by an item-specific switch probability manipulation, thus replicating Leboe et al.'s (2008) findings while controlling for response shift probability effects. Our data thus support the hypothesis that participants acquired stimulus-control state associations to optimize performance in a task-switching context.

In addressing whether response compatibility might interact with the switch probability manipulation in modulating switch costs, we performed an additional ANOVA involving response compatibility. We observed an expected main effect of response compatibility ($F[1, 62] = 457.07$, $\eta_p^2 = .88$, $p < .001$) on RT, as well as a significant Response Compatibility \times Trial Type interaction ($F[1, 62] = 26.63$, $\eta_p^2 = .30$, $p < .001$), as reflected in the larger switch costs for response incompatible items as compared to response compatible ones (response compatible: $M = 55$, 95% CI [47, 64]; response incompatible: $M = 74$, 95% CI [64, 83]). However, the factor of response compatibility did not interact with switch probability, $F(1, 62) = 1.88$, $\eta_p^2 = .03$, $p > .05$. The three-way interaction was also not significant, $F < 1$, $p > .1$. These results suggest that S-C learning was similar for all stimuli regardless of their response compatibility. Therefore, the item-specific switch benefit reported above was not reliant on the response compatible items where S-R learning was possible.

While the overall accuracy was high ($M = 89$, 95% CI [87, 90]), participants responded less accurately on switch trials than on repeat trials, $F(1, 62) = 51.09$, $\eta_p^2 = .45$, $p < .001$. However, no other effects on accuracy were observed (main effect of switch probability and Switch Probability \times Trial Type interaction, F 's < 1 , p 's $> .05$). Complete descriptive statistics are shown in Table 1.

Experiment 2

The findings of an item-specific modulation of switch costs in Experiment 1 could be due to S-C learning reducing the task switch costs in the frequent switch items, or increasing switch costs in the rare switch items, or a combination of the two. We adjudicated between these hypotheses in Experiment 2. Therefore, Experiment 2 conceptually replicated Experiment 1, but with the addition of stimuli that were associated with an equal probability of switch and repeat trials. The inclusion of a 50% switch probability condition provided a baseline condition with which to compare the switch costs in the 75% and 25% switch probability conditions. In order to have unique stimuli in each of the switch probability conditions, we changed the stimulus set from single digits to images of objects (Figure 1B).

Method

Participants. Sixty-five Amazon Mechanical Turk workers provided informed consent to the study, which was approved by the Duke University Institutional Review Board. Participants were compensated with \$3. Target sample size was the same as that in Experiment 1. Data from 10 participants were excluded, two due to data loss and eight due to a mean error rate higher than 35%. Fifty-five participants (mean age = 32, $SD = 8$; 25 males) were included in the analysis.

Stimuli. We used 40 color photographs of objects from Moreno-Martínez & Montoro (2012), with 10 objects belonging to each of four categories: living objects smaller than a shoebox, living objects larger than a shoebox, nonliving objects smaller than a shoebox and nonliving objects larger than a shoebox. For each participant, a total of 12 images were used in the experiment (three from each stimulus category, randomly selected). See Figure 1B for example trials. A different set of eight images was used for practice exclusively.

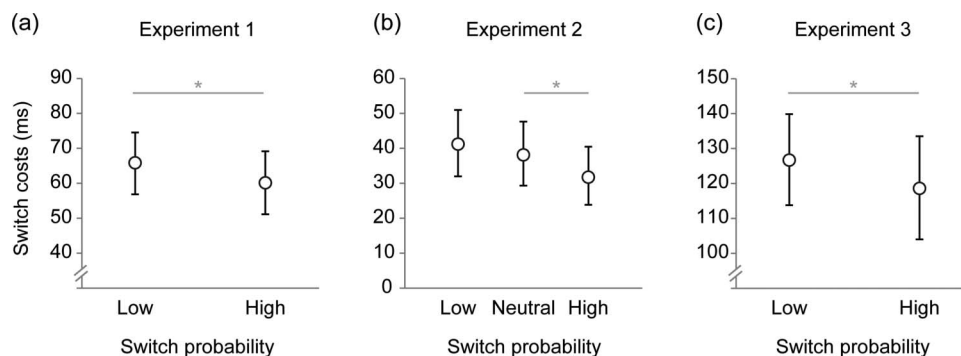


Figure 2. Mean switch costs as a function of item-specific switch probability for Experiments 1–3 (a–c). Error bars indicate 95% confidence intervals (calculated using the bootstrap resample with replacement technique). * $p < .05$.

Table 1
Mean and 95% Confidence Intervals for Task-Switching Performance

| | Rare Switch Items | | Neutral Items | | Frequent switch items | |
|--------------------|----------------------|----------------------|----------------------|----------------------|-----------------------|----------------------|
| | Repeat | Switch | Repeat | Switch | Repeat | Switch |
| Experiment 1 | | | | | | |
| Accuracy (%) | 91.0 [89.5, 92.4] | 87.6 [85.6, 89.4] | | | 90.7 [89.0, 92.2] | 87.7 [85.5, 89.6] |
| Response Time (ms) | 640.7 [628.9, 652.5] | 706.2 [691.7, 720.8] | | | 643.1 [631.2, 654.9] | 703.2 [688.6, 717.6] |
| Experiment 2 | | | | | | |
| Accuracy (%) | 91.0 [89.5, 92.4] | 88.6 [86.8, 90.3] | 91.3 [89.9, 92.6] | 89.0 [87.2, 90.8] | 91.1 [89.4, 92.6] | 89.3 [87.5, 91.0] |
| Response Time (ms) | 660.7 [645.5, 675.9] | 701.9 [681.5, 722.2] | 664.6 [649.3, 679.4] | 702.4 [683.2, 721.0] | 668.9 [652.5, 685.3] | 700.7 [681.0, 720.0] |
| Experiment 3 | | | | | | |
| Accuracy (%) | 92.0 [90.5, 93.4] | 89.5 [87.6, 91.2] | | | 92.6 [91.1, 94.0] | 89.4 [87.6, 91.0] |
| Response Time (ms) | 784.7 [768.2, 801.9] | 911.2 [891.4, 930.9] | | | 785.1 [767.0, 803.8] | 903.6 [883.8, 923.2] |

Design and procedure. Participants categorized stimuli either as living versus nonliving or as larger versus smaller than a shoebox, based on the color of a frame placed around the stimuli, which was presented simultaneously with the stimuli. As in Experiment 1, the incidence of the two tasks and trial types (switch/repeat) were both equated. Unbeknownst to the participants, four of the stimuli were associated with 75%, four with 50%, and four with 25% chance of switching. The stimulus categories/responses were equated across the three switch probability conditions. As in Experiment 1, trials were generated with a randomization procedure with the following constraints: (a) the incidence of the two tasks was equated, (b) the incidence of each stimulus was equated, (c) exactly α percentage of trials of a stimulus were switch trials for items in the α switch probability condition while the remaining trials were repeat trials ($\alpha = 25, 50, 75$), and (d) the difference in trial count for each stimulus appearing in one task versus the other task was less than four trials. The probability of a stimulus appearing in one of the two tasks was also roughly equated. Therefore, the switch probability manipulation was not confounded by biased stimulus-task or biased stimulus-response associations.

There were 192 trials per block for nine blocks, with the first block serving as a burn-in to familiarize participants with the item-specific switch probability manipulation. The colors were randomly assigned to each task in each participant. Trial timing was the same as that in Experiment 1. Before the main task, participants familiarized themselves with the assigned stimulus-response mapping in one or more practice blocks (to reach an 80% accuracy criterion) in which the switch probability for all stimuli was kept at 50%. Two keys, *G* and *J*, were used to indicate the category of the stimulus. Stimulus category-to-response key mappings were counterbalanced across participants. See Figure 1B for example trials.

Analysis. Data analysis and reporting procedures were exactly the same as in Experiment 1 except for the following modifications. First, the factor of switch probability involved three levels (75%, 50%, 25%) instead of two. Second, to further investigate the mechanism by which S-C learning reduces switch costs, we first calculated the switch costs in each switch probability condition, and then directly compared the switch costs in the 75/25% switching condition with the switch costs in the 50% switching condition, respectively, using paired *t* tests. RT outliers in Experiment 2 were handled in the same way as those in Experiment 1 ($M = 0.10\%$ excluded).

Results

Participants again took longer to respond on switch trials than on repeat trials, $F(1, 54) = 71.90$, $\eta_p^2 = .57$, $p < .001$. Mean RT did not differ between stimuli associated with different switch probability conditions, $F(2, 108) = 1.79$, $\eta_p^2 = .03$, $p > .05$. However, the switch probability \times trial type interaction was highly significant, $F(2, 108) = 5.75$, $\eta_p^2 = .10$, $p < .005$. Replicating Experiment 1, these results demonstrate a robust ISSP effect in task-switching, and support the hypothesis that S-C learning modulates switch costs.

Unpacking this interaction, post hoc *t* tests showed that switch costs for items associated with 75% switch probability ($M = 32$, 95% CI [24, 40]) were significantly smaller than for items associated with 50% switch probability ($M = 38$, 95% CI [29, 47]), *t*

(54) = 2.42, $p < .05$, Cohen's $d = .30$. On the other hand, switch costs for items associated with 25% switch probability ($M = 41$, 95% CI [32, 51]) were not different from those associated with 50% switch probability, $t(54) = 1.30$, $p > .05$, Cohen's $d = .18$ (Figure 2B). These direct comparisons suggest that S-C learning modulates switch costs primarily by reducing the task switch costs in the frequent switch items rather than by increasing switch costs in the rare switch items.

Similar to Experiment 1, we did not find any evidence suggesting that the item-specific switch benefit was driven specifically by the response compatible items, where S-R learning was possible. Again, although the main effect of response compatibility and the Response Compatibility \times Trial Type interaction were highly significant (main effect: $F(1, 54) = 390.04$, $\eta_p^2 = .88$, $p < .001$; interaction: $F(1, 54) = 25.10$, $\eta_p^2 = .32$, $p < .001$), the Response Compatibility \times Switch Probability interaction was not significant, and the three way interaction involving response compatibility, switch probability and trial type was also not significant, F 's < 1.5 , p 's $> .1$.

Overall accuracy was high ($M = 89$, 95% CI [87, 90]) and the mean accuracy was similar across conditions with different switch probability, $F < 1$, $p > .05$. Participants showed a typical switch cost in accuracy: Lower accuracy on switch trials than on repeat trials, $F(1, 54) = 29.99$, $\eta_p^2 = .36$, $p < .001$. The interaction of switch probability by trial type was not significant, $F < 1$, $p > .05$. Complete descriptive statistics are shown in Table 1.

Experiment 3

The first two experiments successfully demonstrate the possibility of S-C learning in task-switching and suggest that switch costs can be reduced by a learned linkage between a particular bottom-up stimulus and the control operation of task-set updating. Importantly, in these two experiments, we ensured that the ISSP effect was driven by stimulus-control state (task-set updating) associations and could not be driven by stimulus-task or stimulus-response shift associations. However, as there was only one alternative task in the first two experiments, it is unclear whether the learned stimulus-switch associations reflected a primed readiness to shift to the specific alternative task set or whether it is possible for a stimulus to cue a more generic form of cognitive flexibility, facilitating updating of the current task set to any other task set. Therefore, Experiment 3 conceptually replicated Experiments 1 and 2, but with the addition of a third task. In order to have three different categorization rules applicable to a single stimulus, we used faces as stimuli and instructed participants to switch between categorizing gender, age and emotional expression of the faces (Figure 1C).

Method

Participants. Seventy-five Amazon Mechanical Turk workers provided informed consent to the study, which was approved by the Duke University Institutional Review Board. We increased the sample size by 10 as we anticipated a potentially decreased effect size due to the addition of a third task. Participants were compensated with \$3. Data from 1 participant were excluded due to a mean error rate higher than 35%. Seventy-four participants (Mean age = 35, $SD = 11$; 26 males) were included in the analysis.

Stimuli. We used 32 color photographs of faces with unique identity from Ebner, Riediger, and Lindenberger (2010). Each face image belonged to one of the eight categories resulting from binary categorizations of three face features: Age (young, old), gender (male, female) and emotion (happy, sad). For each participant, a subset of 16 images was used in the experiment (two from each stimulus category, randomly selected). See Figure 1C for example trials. A different set of 12 faces was used for practice exclusively.

Design and Procedure. Participants categorized stimuli according to one of the three features—age (old vs. young), gender (male vs. female) and emotional expression (happy vs. sad), based on the color of a frame placed around the stimuli, which was presented simultaneously with the stimuli. As in Experiments 1 and 2, the incidence of each task was equated. As a result, there were 2/3 (66%) of switch trials and 1/3 of repeat trials. Unbeknown to the participants, half of the stimuli were associated with a high probability of switching ($\sim 89\%$), and half with a low probability ($\sim 33\%$) of switching. These frequencies correspond to an 80:20% item-level switch probability in a two-task design. As there were more switch trials, the stimuli associated with a high probability of switching did appear more frequently than those associated with a low probability of switching. However, as switch costs were calculated within each switch probability condition, the difference in the stimulus frequency should not influence the detection of the magnitude difference in switch costs between the two switch probability conditions (89% vs. 33%). The stimulus categories/responses were equated across switch probability conditions. As in Experiments 1 and 2, trials were generated with a randomization procedure with the following constraints: (a) the incidence of the two tasks was equated; (b) each stimulus in the 33% switch probability condition appeared on 51 ± 1 trials, and each stimulus in the 89% switch probability condition appeared on 75 ± 1 trials; (c) roughly α percentage of trials of a stimulus were switch trials for items in the α switch probability condition, while the remaining trials were repeat trials ($\alpha = 33, 89$); and (d) the difference in trial count for each stimulus appearing in one task versus the other task was less than four trials. Therefore, the switch probability manipulation was not confounded by biased stimulus-task, or biased stimulus-response associations.

There were 112 trials per block for nine blocks with the first block serving as a burn-in to familiarize participants with the item-specific switch probability manipulation. The task colors were randomly assigned to each task in each participant. Each trial started with a fixation for 400 ms, followed by a face stimulus for 1,500 ms appearing at the center of the screen, during which participants could make a response, and after which they received written feedback (correct or incorrect) for 600 ms. Before the main task, participants familiarized themselves with the assigned stimulus-response mapping in one or more practice blocks (to reach $>80\%$ accuracy) in which the switch probability for all stimuli was kept at 50%. Two keys, *G* and *J*, were used to indicate the category of the stimulus. Stimulus category-to-response key mappings were counterbalanced across participants. See Figure 1C for example trials.

Analysis. Data analysis and reporting procedures were exactly the same as in Experiment 1. RT outliers in Experiment 3 were handled in the same way as those in Experiment 1 ($M = 0.05\%$ excluded).

Results

Participants took longer to respond on switch trials than on repeat trials, $F(1, 73) = 318.45$, $\eta_p^2 = .81$, $p < .001$. Despite differences in stimulus frequency, mean RT did not differ between stimuli associated with different switch probability conditions, $F(1, 73) = 2.76$, $\eta_p^2 = .04$, $p > .05$. Importantly, the Switch Probability \times Trial Type interaction was significant, $F(1, 73) = 4.46$, $\eta_p^2 = .06$, $p < .05$. Unpacking this interaction, the post hoc t test showed that switch costs for items associated with a high switch probability ($M = 119$, 95% CI [104, 133]) were significantly smaller than in items associated with a low switch probability ($M = 126$, 95% CI [114, 139]), $t(73) = 2.11$, $p < .05$, Cohen's $d = .25$ (Figure 2C). The finding of an ISSP effect in the context of switching between three tasks suggests that the frequent switch items have come to be associated with a general readiness to update task set that is not limited to a context where there is only one alternative task to switch to/from.

Similar to Experiments 1 and 2, we did not find any evidence suggesting that the item-specific switch benefit was driven by the response compatible items. In Experiment 3, while the main effect of response compatibility and the main effect of trial type were significant (response compatibility: $F[1, 73] = 141.52$, $\eta_p^2 = .66$, $p < .001$; trial type: $F[1, 73] = 325.16$, $\eta_p^2 = .82$, $p < .001$), no other effects or interactions were significant, p 's $> .05$.

Overall accuracy was high ($M = 90$, 95% CI [88, 91]) and the mean accuracy was similar across conditions with different switch probability, $F < 1$, $p > .05$. Participants showed a typical switch cost in accuracy: Lower accuracy on switch trials than on repeat trials, $F(1, 73) = 50.31$, $\eta_p^2 = .41$, $p < .001$. The interaction of switch probability by trial type was not significant, $F(1, 73) = 2.02$, $\eta_p^2 = .03$, $p > .05$. Complete descriptive statistics are shown in Table 1.

General Discussion

While there have been several studies investigating the possibility of item-specific stimulus-control state (S-C) learning in the context of conflict-control, to our knowledge only a single previous study has examined whether S-C learning can associate specific items with the control operation of task-set updating (Leboe et al., 2008). Here, in three experiments, we demonstrated robust item-specific switch probability (ISSP) effects and furthermore examined the underlying mechanism as well as the generalizability of such modulation of switch costs by S-C learning. In the first experiment, we employed a common task-switching protocol involving participants switching between two simple digit categorization tasks with equal probability of task switches and repetitions but a biased distribution of switch and repeat trials over specific digits. We found clear evidence of S-C learning as the magnitude of switch costs was reduced for the frequent switch items as compared to the rare switch ones. In the second experiment, we added a set of items with 50% switch probability that served as the baseline condition to which to compare the switch costs for items with a high versus low switch probability, thus identifying the exact source(s) of the ISSP. Results showed that the ISSP effect was primarily driven by S-C learning reducing switch costs in the frequent switch items rather than increasing switch costs in the rare switch items. Finally, in the third experiment, we demonstrated an ISSP effect in a task-switching experiment involving three equally

probable tasks. This result suggests that S-C learning enables the linking of a particular bottom-up stimulus with a generalizable (or abstract) task-set updating process as opposed to simply enhancing the readiness to switch to one particular alternative task.

Our results are consistent with an emerging perspective suggesting that cognitive control is dynamically adjusted to task demands (Goschke, 2000; MacDonald, Cohen, Stenger, & Carter, 2000; Tornay & Milán, 2001) by incorporating multiple levels of learning to produce goal-coherent actions (Bugg, 2012; Egner, 2014). In the context of task-switching, previous studies have demonstrated that this adjustment can be linked to, and subsequently retrieved by, a temporal context (e.g., a block of trials) (Dreisbach & Haider, 2006; Schneider & Logan, 2006b), a task-irrelevant spatial context (e.g., Crump & Logan, 2010; Leboe et al., 2008), or an explicit trial-by-trial cue (Dreisbach & Haider, 2006; Dreisbach, Haider, & Kluwe, 2002). For instance, when given a trial-by-trial cue of the likelihood of a task switch, participants appear to proactively adjust their control settings such that switch costs are reduced for cues that predict a high probability of switch on the upcoming trial (Dreisbach & Haider, 2006). Our demonstration of an ISSP effect significantly extends this line of work by showing that such control adjustment can also be triggered reactively by the task-relevant stimulus itself, such that switching is facilitated in a bottom-up fashion by stimuli that have been frequently paired with a need to update task sets. However, one caveat to the scope of our conclusions is that it is possible that the ISSP effect reflects a greater ease of the cue-retrieval (interpretation) process rather than the updating of the task set per se (Mayr & Kliegl, 2003). As our study used only one cue per task, the processes of cue and task-set retrieval were not dissociable. Nonetheless, findings from a study by Crump and Logan (2010), which employed two cues per task to examine location specific switch probability effect, suggest that the switch probability manipulations modulate the task-set retrieval (or re-configuration) process rather than the cue interpretation process. Future work could employ two cues per task in an ISSP design in order to examine whether this effect is also primarily driven by priming of the task-set retrieval process.

The binding of a control state with a specific stimulus in the shape of an episodic memory representation dovetails with the theory of "event files," which are transient bindings of different stimulus features and responses based on co-occurrences of these features (Hommel, 2004; Waszak et al., 2003). S-C learning further extends this idea by suggesting that internal control states (e.g., task-set updating) that are operative at the time of stimulus presentation can also be bound into the stimulus-response event file (Verguts & Notebaert, 2008, 2009). A subsequent encounter of the same stimulus then triggers the retrieval of that memory representation, which includes the associated control states (Abrahamse, Braem, Notebaert, & Verguts, 2016; Egner, 2014). The neural substrates mediating this binding of bottom-up stimulus features with abstract control states have been localized to the hippocampus and the dorsal striatum in the context of conflict processing (Chiu et al., 2017; Jiang, Brashier, & Egner, 2015; Jiang, Beck et al., 2015). These two structures appear to play complementary roles in mediating S-C learning. On the one hand, the hippocampus has been found to represent an integration of event features including the stimulus and response features as well as the control demands, as opposed to other brain regions that represent only single event features (Jiang, Brashier et al., 2015).

On the other hand, the caudate nucleus has been identified to continuously track and update associations between stimuli and control demands across time (Chiu et al., 2017; Jiang, Beck et al., 2015). The neural substrates supporting the ISSP effect are currently unknown. It would therefore be very interesting for future studies to probe whether associations between specific stimuli and switch readiness as captured by the ISSP effect are mediated by a similar neural circuitry.

How does the item-specific switch proportion effect we report here relate to other learning-based reductions in switch cost? One related phenomenon is the list-wide switch probability effect, the finding that switch costs are smaller in blocks of trials where switch trials are frequent than in blocks where they are rare (Dreisbach & Haider, 2006). It is possible that this type of effect could arise from cumulative item-level learning effects, but it is more commonly assumed that appropriate control states in the list-wide case are linked to a temporal context (e.g., a block with 75% switch trials) instead of being linked to a specific predictive stimulus or item. In the case of item-level S-C learning, like in the current study, the application of the appropriate control state can be said to be “reactive,” as opposed to “proactive” or anticipatory (Braver, 2012), because participants cannot know in advance which stimulus will occur next. By contrast, in the case where control states are linked to a temporal context (a block of trials), participants are thought to strategically adjust controlled processing “proactively” before the stimulus appears (Botvinick, Braver, Barch, Carter, & Cohen, 2001). It is therefore likely that the exact form of S-C learning may differ between item and list-wide effects (Gonthier, Braver, & Bugg, 2016).

Another way in which learning has been shown to reduce switch costs is through predictable task sequences, as shown, for example, by studies of “hierarchical control” in task-switching (De Jong, 1995; Lien & Ruthruff, 2004; Schneider & Logan, 2006a), where participants are explicitly memorizing particular task sequences (e.g., AABB, ABBA). This might raise the question whether the ISSP effect may perhaps also be related to predictable (i.e., probabilistically biased) local task transitions, rather than reflecting stimulus-triggered cognitive flexibility. We believe this is highly unlikely. First, recall that in our experiments items are linked to switch-likelihood but they are not associated with a particular task or a specific response. Second, particular task transition sequences (e.g., AABB, ABBA) were neither instructed nor likely to occur any more or less frequent than others, as the trial sequences were generated randomly. We also subsequently verified that the randomization produced the expected frequencies of possible first-, second-, and third-order task transitions. Thus, while by its very nature a “frequent switch item” is more likely to occur second in AB and BA task sequences than in AA and BB sequences, since the occurrence of these local sequences was unpredictable, a participant would not know what the transition is until after it has occurred. Finally, a specific switch probability condition (e.g., a frequent switch item) was equally likely to be preceded by a frequent or a rare switch item. Thus, the ISSP effects reported here cannot be accounted for by learning about local item or task transitions.

As mentioned in the introduction, the possibility of S-C learning was first supported by the item-specific PC effect in the Stroop task (Jacoby et al., 2003). However, Jacoby et al. (2003)’s original design confounded S-C with possible S-R associations, which led

to the counterclaim that the item-specific PC effect was driven by participants learning to predict responses rather than control states (Schmidt & Besner, 2008). While these design issues can be circumvented (Bugg et al., 2011; Chiu et al., 2017), an inherent upside of the ISSP effect is that it does not suffer from possible S-R learning confounds, because the correct response for a particular stimulus varies as a function of the task set. Thus, even though a frequent switch stimulus predicted a high probability of switching, it was associated neither with a specific task nor with a particular response. Therefore, participants could not rely on stimulus-response (or stimulus-task) associations alone to figure out the appropriate response. As shown in both Experiments 1 and 2, even for stimuli that cued the same response in both tasks, participants did not seem to rely on direct stimulus-response (S-R) associations to generate the correct response. Instead, participants appeared to acquire associations between a specific stimulus and a context-appropriate control state, and later retrieved that control state to facilitate the generation of task-appropriate responses. Therefore, the ISSP effect implicates S-C learning as a stand-alone learning mechanism for optimizing behavior, independently of S-R learning.

In conclusion, the demonstration of the ISSP effect in task-switching along with the item-specific PC effect in the context of conflict processing suggest that S-C learning is a common mechanism that can be employed to link bottom-up cues to specific but generalizable control operations. While top-down control-driven responding can ensure accuracy in the face of conflicting cues, memory-guided routine actions are less effortful and faster. S-C learning thus provides a means whereby memory can guide control and alleviate lengthy processing time and effort. Goal-directed actions therefore cannot be considered as a pure product of cognitive control, but instead a joint contribution of controlled and associative processes.

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