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Switching Between Cognitive Control States? No, Thank You

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Cognitive control enables adaptive behavior by flexibly adjusting information processing to meet changing environmental demands. While extensive research has examined the costs of switching between different tasks, it remains unclear whether switching between different control states (relaxed and focused) within a single task carries intrinsic costs. We hypothesized that internally switching between control states while performing a single task incurs a subjective effort cost and, therefore, that people avoid such demands. Additionally, we examined the trade-off between this cost and interference demands. We conducted a series of experiments using the Demand Selection Task, where participants chose between different options that yielded lists of Stroop trials. Participants consistently avoided options with high demands for internal control-state switching (Experiment 1, $N = 64$) or interference (Experiment 2, $N = 86$). Regarding the potential trade-off, Experiment 3 ($N = 120$) tested whether internal control-state switching demands modulate interference costs when both are present. There was no evidence for this modulation, but this could be because the structure of the Stroop task only allowed for relatively weak manipulations of control-state switching and interference. To address this limitation, we developed a novel parametric flanker task (Experiment 4, $N = 50$) that enables strong simultaneous manipulation of both demands. With these stronger manipulations, both control-state switching and interference costs influenced selections, revealing trade-offs between these competing demands (Experiment 5, $N = 130$). Our findings refine models of control regulation, highlighting that not only exerting but also adjusting control carries intrinsic costs.

Public Significance Statement
Our minds constantly shift between different mental states even while doing the same activity, such as switching between relaxed and intense focus while driving. This study reveals that these internal mental shifts, even within the same task, require effort and that people try to avoid them when possible. We found that people prefer to be in environments that do not require frequent switching between focused and relaxed states of attention, and that this tendency trades off with other forms of demand, such as the amount of distracting information. These findings help explain why tasks requiring frequent attention adjustments can be mentally taxing, and switching between different levels of focus carries a mental effort cost. Understanding the cost of mental flexibility could inform better strategies for managing cognitive effort in educational, workplace, and digital environments.

Keywords: cognitive control, internal control states, demand avoidance, Demand Selection Task, control-state switching

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Switching between tasks is essential for adaptive behavior, enabling us to navigate dynamic environments and to deal with shifting priorities. Yet, it comes with an objective cost of slower and less accurate responding (Monsell, 2003; Wylie & Allport, 2000), which people actively avoid when given the choice (Kool et al., 2010). Most research has focused on externally cued transitions, where participants are instructed to switch between tasks (e.g., parity vs. magnitude judgments of digits). Far less is known about the costs of internally shifting between cognitive *control states* while performing the same task. For instance, while proofreading an article, one might transition from skimming for general flow (a relaxed state) to closely inspecting for typos (a focused state) without explicit external cues. These internally generated shifts in control may impose costs akin to externally cued switches yet remain largely unexplored.

In the present research, we define a control state as a configuration of attentional parameters that determines the degree to which goal-relevant information is prioritized relative to competing goal-irrelevant information (Blais et al., 2007; Botvinick & Cohen, 2014; Botvinick et al., 2001; Gheza & Kool, 2025; Miller & Cohen, 2001). A *focused* control state involves strong suppression of distractors and/or prioritization of task-relevant information, whereas a *relaxed* control state involves relatively weaker suppression of distractors and/or prioritization of task-relevant information.

Control states can be viewed as a component of the broader task set. Task sets encompass the complete configuration of task-relevant stimuli and responses, stimulus–response (S-R) mappings, and the control parameters needed to perform a task (Kiesel et al., 2010; Logan & Gordon, 2001; Monsell, 2003, 2017). Adjusting control states involves reconfiguring control parameters, strengthening or weakening attention, without changing the task goal or S-R associations.

This distinction separates control-state adjustments from traditional task switching. In traditional task-switching paradigms, participants change the goal they are pursuing (e.g., switching from a color judgment to a shape judgment), typically in response to an external cue and involving a complete reconfiguration of task-related representations. In contrast, control-state adjustments occur within a single task. Here, participants maintain the same goal but adjust how intensely they filter distracting information or prioritize goal-relevant information. This type of control-state switching is often internally generated and self-initiated, responding to changes in perceived task demands or internally generated cues rather than explicit instructions. As such, these adjustments provide a unique window into cognitive flexibility and the subjective experience of cognitive effort (i.e., a "subjective intensification of mental activity"; Eisenberger, 1992; Inzlicht et al., 2018).

A canonical example of control-state adjustment occurs in Stroop tasks, such as the picture-word Stroop task, where the goal is to name a picture while ignoring a superimposed word. Here, when picture and word are congruent (e.g., a bird picture with the word BIRD), participants engage in a more relaxed control state compared to incongruent trials (e.g., a bird picture with the word DOG). This distinction is captured by conflict monitoring theory (Botvinick et al., 2001), according to which cognitive control is deployed dynamically in response to detected conflict (Botvinick et al., 1999, 2001; Carter et al., 1998, 2000; Egner & Hirsch, 2005; Kerns et al., 2004; MacDonald et al., 2000). When conflict is detected, control mechanisms bias processing toward task-relevant stimulus properties and/or away from task-irrelevant, distracting properties. These adjustments can happen both based on conflict experienced in previous (Botvinick et al., 1999; Egner, 2014, 2007; Gratton et al., 1992) and current trials (Cavanagh et al., 2011; Resulaj et al., 2009; Scherbaum et al., 2010, 2011). As a result, switching between congruent and incongruent trials within the same task may induce shifts between relatively more relaxed and relatively more focused control states.

A substantial body of research demonstrates that people flexibly regulate their control settings in response to changing task demands, incentives, and performance goals (Botvinick & Braver, 2015; Botvinick et al., 2001; Braem et al., 2019; Braver, 2012; Cohen, 2017; Danielmeier & Ullsperger, 2011; Egner, 2017; Forstmann et al., 2010; Leng et al., 2021; Miller & Cohen, 2001; Parro et al., 2018; Schevernels et al., 2014). However, this work largely treats these adjustments as adaptive but does not focus on whether these control adjustments are themselves effortful. More recent work suggests that adjusting control states can impair performance, suggesting that reconfiguration may carry intrinsic processing costs (Grahek et al., 2022, 2025). However, these studies involve explicit shifts in task goals or priorities (e.g., speed–accuracy trade-offs), making it difficult to isolate the costs of control-state adjustments from broader strategic changes. Moreover, this prior work leaves unanswered the question of whether people seek to avoid the cost of control-state adjustments.

The present study addresses this question directly. Rather than inducing switches between tasks or explicit goals, we manipulate the frequency with which choice options require shifts between control states, while holding task and goal constant. This allows us to examine whether switching between relatively relaxed and focused control states registers as costly, that is, whether people actively avoid environments that require frequent internal reconfiguration of control.

If switching internal control states is effortful, people should prefer actions minimizing these demands (Kool & Botvinick, 2014, 2018; Kool et al., 2010; Kurzban et al., 2013; Lieder & Griffiths, 2020). To test this hypothesis, we designed a series of experiments in which control-state switching was induced through a picture–word Stroop task (Bugg et al., 2011) and the cost of control-state switching was assessed in a Demand Selection Task (DST; Kool et al., 2010). In the DST, participants chose between options (here, decks of cards) that varied in their demands, with no explicit indication of which option was more effortful, enabling us to examine participants' demand avoidance. This paradigm has been used to demonstrate that people avoid a variety of cognitive effort demands (Bustos et al., 2024; Cameron et al., 2019; Kool et al., 2010; Patzelt et al., 2019; Sayalı & Badre, 2019) and these are traded off against other costs (Devine & Otto, 2022; Sayalı et al., 2023; Vogel et al., 2020). By embedding a control-state switching manipulation within the DST, we tested whether frequent control-state switching is subjectively costly and therefore avoided.

Understanding the costs of internal control-state transitions is critical, as they shape how we regulate attention. Current theories of cognitive control emphasize control costs associated with the maintenance of focused control states (Kurzban et al., 2013; Shenhav et al., 2013, 2017). Here, however, we ask whether costs also arise from dynamically adjusting cognitive control states within a task. This allows us to connect to broader questions about the optimization of cognitive resources. For example, the Expected Value of Control framework (Shenhav et al., 2013, 2017) proposes that people allocate control based on the expected costs and benefits of different control configurations. However, if state transitions themselves are costly, this suggests that control systems face another trade-off: The ability to adapt to changing demands must be balanced against the effort required for reconfiguration. Incorporating control adjustment costs into these models could provide a more complete picture of how people optimize cognitive resource allocation. Moreover, because cognitive control is often deployed in response to multiple forms of demand, a key question is how people balance the costs of internal control-state transitions with competing demands. It remains unclear how internal control-state switching—an underexplored yet ubiquitous demand—compares to established sources of cognitive effort, such as interference (the need to resolve conflict from competing or irrelevant information). We address these gaps by asking (a) whether internally shifting between control states carries a cost and (b) how individuals trade off this cost against interference demands.

In a series of experiments, we first demonstrated that participants avoid switches between internal control states (Experiment 1), suggesting internal control-state switching is effortful. Next, we sought to examine whether people trade off these control-state switching demands against interference demands. After establishing that participants avoid environments with frequent interference (Experiment 2), we investigated how people navigate trade-offs between these demands (Experiments 3–5). Together, these experiments reveal the cost of internal control-state switching and show how internal control-state switching and interference jointly shape demand avoidance.

It is important to distinguish our investigation of internal control-state switching from the well-established congruency sequence effect (CSE; Braem et al., 2019; Gratton et al., 1992). The CSE demonstrates that people adjust control settings in response to recent conflict, showing a reduced congruency effect following incongruent trials. While this effect reflects within-task control adjustments, it captures an obligatory, reactive form of adaptation that occurs regardless of participant preferences or strategic choices (Van Gaal et al., 2010). Our investigation addresses a fundamentally different question: Do people actively choose to avoid implementing within-task control adjustments? This distinction is critical, allowing us to examine the subjective effort associated with control-state transitions. In studies investigating the CSE, participants cannot choose the sequence of trials they encounter. In contrast, our DST explicitly gives participants agency over their environment, allowing us to measure their preferences and reveal the subjective costs of different control demands. This methodological difference has important theoretical implications. While the CSE tells us that control systems adapt to changing demands on a trial-by-trial basis, it does not reveal whether these adaptations are subjectively effortful or whether people prefer to avoid them. We bridge this gap by examining not just whether control states can be flexibly adjusted, but whether people find such adjustments aversive enough to avoid them.

Experiment 1

We used a DST to investigate whether participants avoid switching between relaxed (congruent trials) and focused (incongruent trials) control states. Participants repeatedly chose between two options (decks) that differed in their congruency-switch rate; that is, the frequency with which trial congruency (congruent vs. incongruent) changed from one trial to the next. One deck produced low-switch trial sequences and the other high-switch sequences, while the proportion congruence (PC, i.e., interference) was held constant at 50% by presenting an equal number of congruent and incongruent trials in each deck. Participants were not informed of this difference, requiring them to rely on their experiences with the options to guide choice. We reasoned that if switching control states is effortful, participants should favor the low-switch option.

Method

Transparency and Openness

No aspects of this experiment were preregistered. All study materials, data, and analysis scripts are publicly available (https://osf.io/8vhcw/files/osfstorage, Ileri-Tayar et al., 2025). In all experiments, the data were analyzed using R, Version 4.1.1 (R Core Team, 2021). We describe the methods used to determine our sample size, detail any data exclusions, outline all manipulations, and list all measures included in the study.

Participants

We recruited 75 students from Washington University in St. Louis, who participated as a partial requirement of psychology courses. The sample size was chosen based on practical considerations, including the time available for data collection within the academic semester and the typical sample sizes used in similar cognitive control studies (approximately 60–80 participants), as this was an initial study without preregistration (see the Supplemental Materials for a preregistered replication with a sample size determined via power analysis). In all experiments, all participants were native English speakers with normal or corrected-to-normal vision. We excluded two participants due to extreme error rates (more than 20%). We also excluded four participants who selected one of the decks less than 2% of the time, indicating they did not follow instructions to explore decks at the beginning of the experiment.[1] Finally, we excluded five participants who took five or more hours to complete the experiment, which was designed to take 1–1.5 hr (note that this issue has been addressed in Experiments 3–5 by adding a deadline for deck selection and preventing participants from waiting too long). We analyzed the remaining 64 participants (34 reporting "female," 28 reporting "male," two reporting "nonbinary"; $M_{\text{age}} = 19.97$, $SD = 1.27$). The Institutional Review Board at Washington University in St. Louis approved all experiments, and all participants provided informed consent.

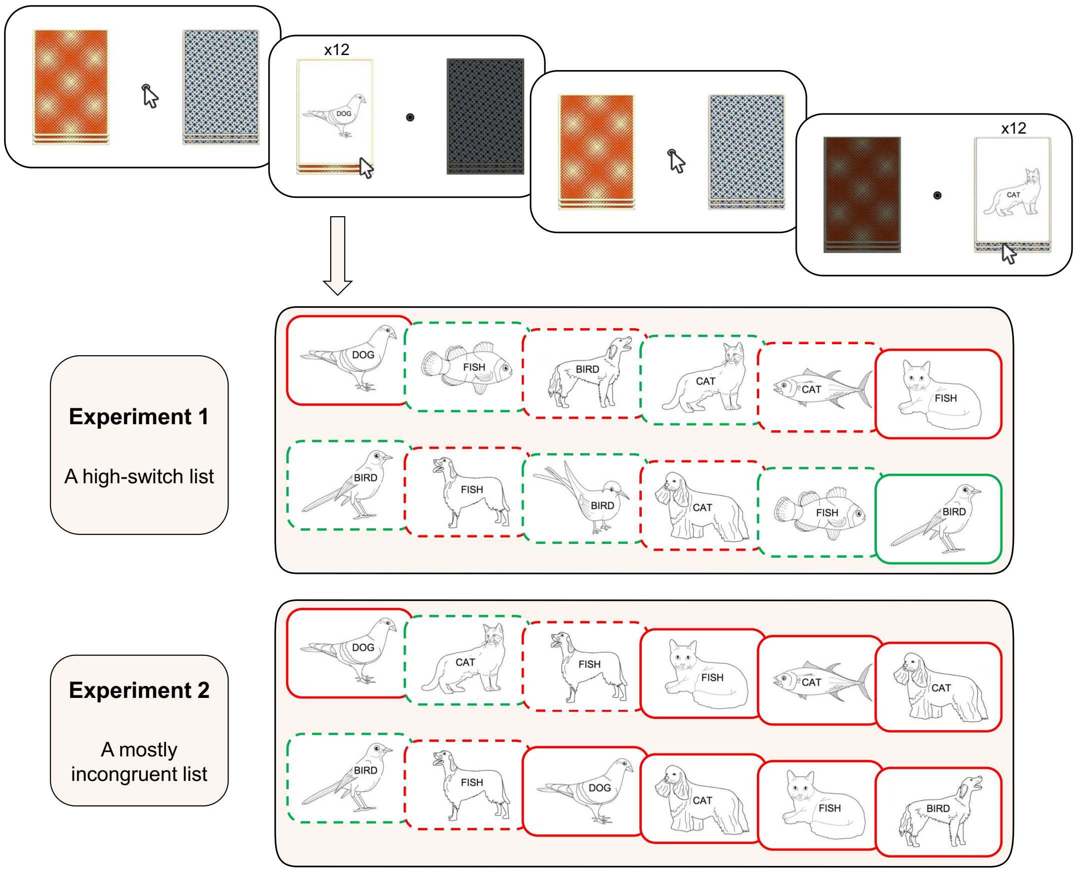
Materials and Procedure

We programmed all experiments using jsPsych (De Leeuw, 2015) so that they could be run in web browsers on participants' computers. Participants were instructed to select between two deck options on the screen (see Figure 1). The two images representing these decks were randomly selected from a set of four, each with

[1] This criterion (preregistered for the replication of Experiment 1) ensured participants selected each deck at least four times (from 180 total selections) to gain adequate experience with each option.

Figure 1

Trial Sequence of the Main Choice Phase in Experiments 1 and 2



Note. Participants selected one of two choice options represented by decks of cards, one of which was assigned to be the low-demand option (low-switch for Experiment 1 and mostly congruent for Experiment 2) and the other the high-demand option (high-switch for Experiment 1 and mostly incongruent for Experiment 2). After they made their choice, the nonselected deck was darkened, and 12 Stroop stimuli were presented on the selected deck. Participants responded to each trial using previously learned stimulus–response mappings. Next, both decks lit up again, signaling participants to make another selection. Participants made 180 selections in total. An example sequence of a high-switch list (for Experiment 1) and an example sequence of a mostly incongruent list (for Experiment 2) following the selection of the high-demand option are shown in the bottom panel (the presentation of stimuli in the second row follows the presentation of stimuli in the first row). In the figure, congruent and incongruent trials are shown with a green or red frame, respectively, and a dashed frame indicates switch trials. See the online article for the color version of this figure.

different colors and patterns. One deck was randomly assigned as the low-switch option and the other as the high-switch option. This deck assignment remained constant throughout the experiment (i.e., the same deck always corresponded to either low-switch or high-switch conditions), allowing participants to learn the association between deck appearance and congruency-switch rate. The low-switch option yielded sequences of 12 Stroop trials with only two switches in congruency, while the high-switch option yielded nine such switches. Importantly, both conditions comprised 50% congruent trials in each single selection.

To select a deck, participants had to first move their mouse cursor over a circle at the center of the screen to ensure it was equally distanced from both options. Then, they could select either deck by moving the cursor over one of them. After selecting, participants performed 12 Stroop trials resulting from that deck choice (Figure 1). Each Stroop trial involved identifying the animal depicted in a picture–word stimulus presented at the center of the selected deck. The stimuli consisted of line drawings of animals (birds, cats, fish, and dogs) with superimposed animal names (BIRD, CAT, FISH, DOG). The stimuli pool included four exemplars per animal category (e.g., four different pictures of dogs), which were randomly selected for each trial (Bugg et al., 2011). Importantly, the same animal category was not presented consecutively to avoid repetition effects in any of the experiments. Participants responded to the Stroop stimulus (i.e., responding to the picture while ignoring the word) by pressing one of four keys on the keyboard (“U,” “I,” “O,” and “P,” randomly assigned to “bird,” “cat,” “fish,” and “dog” responses).

The stimulus remained on the screen until participants responded or until a deadline of 5,000 ms elapsed.[2] When participants responded within the deadline, the next stimulus was displayed after a 300-ms interstimulus interval. If they did not respond within the deadline, a red "X" was presented on the deck for 1,000 ms, followed by the next stimulus.

At the start of the experiment, participants were taught the correct (randomly determined) S-R mappings. They were also required to pass a threshold of 90% accuracy in a block of practice trials and to correctly identify the goal of the picture–word Stroop task. Participants were instructed to use their right hand for responding to the Stroop stimuli and their left hand for using the mouse for deck selection. Over the course of the experiment, participants made 180 deck selections and responded to 2,160 Stroop trials.

Following prior work (Kool et al., 2010), participants were instructed to initially choose between decks randomly, allowing them to explore both options. However, they were also informed that they may notice differences between the two decks and that if they developed a preference, they should feel free to choose one deck more than the other. Participants were asked to avoid simple choice rules such as alternating back and forth between the decks, and instead try to make a deliberate, informed decision on every deck selection (Kool et al., 2010). This design allowed participants to explore the decks while engaging in the Stroop task, providing an opportunity to observe differences between the deck conditions and adapt their decision making accordingly.[3]

Results

In all experiments, we excluded trials slower than 3,000 ms or faster than 200 ms from reaction time (RT) and error rate analyses and excluded error trials from the RT analyses.[4] Table 1 displays the mean RTs for low- and high-demand options for Experiments 1 and 2. To assess participants' Stroop task performance, we fit a linear mixed-effects model with fixed effects of deck type (low-switch vs. high-switch) and trial type (congruent vs. incongruent) and subject-level random intercepts and slopes for both factors.[5,6,7] This analysis allowed us to determine whether there were differences in performance across decks and trial types while accounting for individual variability.[8] Note that, for all experiments, RT analyses for the Stroop task (flanker task in Experiments 4 and 5) are reported in the main text, and error rate analyses, which qualitatively mirrored the RT analyses, in the Supplemental Materials.[9]

Finally, to test whether participants preferred the low-switch option, we analyzed deck choices using a logistic mixed-effects

Table 1
Mean Reaction Times in Experiments 1 and 2

Experiment	Trial type	Low-demand	High-demand
Experiment 1	Incongruent	947 (186)	947 (184)
	Congruent	874 (168)	885 (166)
	Stroop effect	73[a] (52)	62[a] (43)
Experiment 2	Incongruent	922 (163)	893 (152)
	Congruent	831 (138)	831 (131)
	Stroop effect	91[a] (57)	62[a] (58)

Note. Standard deviations are presented in parentheses. Low-demand represents low-switch options in Experiment 1 and mostly congruent options in Experiment 2. High-demand represents high-switch options in Experiment 1 and mostly incongruent options in Experiment 2.
[a] Significant Stroop effects.

[2] The 5-s response window was included as a technical safeguard specific to online data collection to prevent participants from leaving the browser open without responding. In our laboratory-based Stroop tasks, we typically allow unlimited response time and trim RTs exceeding 3,000 ms. To maintain comparability with prior studies, we applied the same 3,000-ms trimming threshold here. The 5-s limit was therefore not intended as a response deadline to influence participants' behavior but merely as a precaution to ensure the experiment did not stall if a participant became inactive.

[3] Across all experiments, participants' awareness of deck contingencies was assessed via postexperiment questionnaires. Results are reported in the Supplemental Materials and discussed in the General Discussion section.

[4] After data trimming, the proportions of trials retained for RT analyses were high and comparable across conditions. In Experiment 1, 93% of low-switch-congruent, 92% of low-switch-incongruent, 93% of high-switch-congruent, and 92% of high-switch-incongruent trials were included. In Experiment 2, 93% of MC-congruent, 90% of MC-incongruent, 93% of MI-congruent, and 93% of MI-incongruent trials were retained. In Experiment 3, 91% of MC-low-switch-congruent, 90% of MC-low-switch-incongruent, 90% of MI-low-switch-congruent, and 90% of MI-low-switch-incongruent trials, as well as 91% of MC-high-switch-congruent, 90% of MC-high-switch-incongruent, 91% of MI-high-switch-congruent, and 90% of MI-high-switch-incongruent trials, were included in the analyses. In Experiment 4, 98% of zero- to four-distractor trials and 97% of five- to eight-distractor trials were retained. In Experiment 5, 97% of MC-low-switch-congruent, 95% of MC-low-switch-incongruent, 95% of MI-low-switch-congruent, and 94% of MI-low-switch-incongruent trials, as well as 96% of MC-high-switch-congruent, 95% of MC-high-switch-incongruent, 94% of MI-high-switch-congruent, and 94% of MI-high-switch-incongruent trials, were included in the analyses.

[5] For all mixed-effects analyses, we initially specified the maximal random-effects structure justified by the design (Barr et al., 2013). When models exhibited singular fits or convergence failures, we simplified the random-effects structure by removing random slopes for interaction terms or other predictors until a stable solution was obtained. Random intercept-only models were used only when no random slopes could be retained without singularity. This approach balances model stability with appropriate accommodation of participant-level variability. Importantly, resolving singularity and convergence issues through model simplification did not change the results; the statistical conclusions for all fixed effects were identical across maximal and simplified models.

[6] Because deck selection was voluntary, the number of observations differed across conditions. Across all participants, the RT analyses for Experiment 1 included 36,789 congruent trials from low-switch decks, 36,222 incongruent trials from low-switch decks, 26,929 congruent trials from high-switch decks, and 26,493 incongruent trials from high-switch decks.

[7] We additionally reran this analysis excluding trials that occurred before participants had experienced both decks at least once. This exclusion removed very few trials and did not qualitatively change any of the results.

[8] Mixed-effects models were fitted using the *lmer* function from the *lme4* package with Satterthwaite's method for estimating degrees of freedom, using the optimizer "bobyqa" (lmerTest package; Kuznetsova et al., 2017). To maintain interpretability and comparability with prior work, results are reported in an analysis of variance–like format derived from mixed-effects models, which provides Satterthwaite-approximated F and p values for fixed effects. We additionally report the complete model output tables in Supplemental Tables S5–S10 to allow readers to examine specific contrasts and effect sizes for all model parameters.

[9] To address concerns about the nonnormality of RT distributions, we log-transformed RTs and reran all main RT analyses in all experiments. Additionally, we reran the analyses after excluding trials that immediately followed an error. In both cases, the pattern of results remained qualitatively unchanged, and all key effects remained consistent, supporting the robustness of the original findings reported in the article.

model with subject-level random intercepts, using the *glmer* function from the *lme4* package. The dependent variable indicated whether the low-switch deck was selected on a given trial (coded as 1 vs. 0).

Stroop Task Performance

We observed a significant Stroop effect for RTs, signaled by a main effect of trial type, $F(1, 62) = 170.06$, $p < .001$, indicating that participants responded faster to congruent trials ($M = 879$ ms) than incongruent trials ($M = 947$ ms), $b = 66$ ms, 95% CI [56, 76]. The Stroop effect was significant both for low-switch deck ($M = 73$ ms), $F(1, 64) = 124.97$, $p < .001$, $b = 74$ ms, 95% CI [61, 87], and high-switch deck ($M = 62$ ms), $F(1, 50) = 197.50$, $p < .001$, $b = 56$ ms, 95% CI [48, 64]. The Stroop effect was significantly smaller in high-switch decks compared to low-switch decks ($b = 16$ ms, 95% CI [6, 25]), as indicated by the interaction between trial type and deck type, $F(1, 39537) = 10.88$, $p < .001$. Importantly, participants' overall RTs did not differ between the decks (low-switch = 910 ms, high-switch = 916 ms), indicated by the absence of a main effect of deck type, $F(1, 49) = 0.49$, $p = .488$.

Demand Selection

We assessed participants' preferences with a logistic mixed-effects model with subject-level random intercepts. Participants preferred the low-switch option more than the high-switch option ($M = 57.80\%$, Figure 2). In this model, the intercept represents the log-odds of selecting the low-switch option, with a value of zero corresponding to selection at the chance level (50%). The intercept was significantly greater than zero ($\beta = .39$, $SE = .11$, $z = 3.50$, $p < .001$), indicating that participants selected the low-switch deck more often than expected by chance.

One potential alternative explanation for participants' preference for low-switch decks is that they learned the trial structure and strategically shifted to word reading after the final switch in low-switch decks (when remaining trials were all congruent). Such a strategy shift, rather than sensitivity to control-state switching per se, could reduce cognitive effort and motivate deck selection.[10] To test this possibility, we compared RTs before versus after the last switch within each low-switch deck. We excluded 14 decks (0.12% of 11,520 total deck selections) that contained no switch trials after applying accuracy and RT exclusion criteria, as these cases precluded identification of a "last switch."

To test the possibility that participants strategically disengaged from picture naming and switched to word reading after the final congruency switch, we analyzed RTs using a mixed-effects model with phase (before vs. after the last switch) and trial type (congruent vs. incongruent) as fixed effects, and random intercepts and slopes for phase, trial type, and their interaction by subject. The model revealed a significant main effect of phase, $F(1, 65) = 10.37$, $p = .002$, indicating general speeding after the last switch ($b = 15$ ms, 95% CI [6, 24]). Critically, the Phase × Trial Type interaction was not significant, $F(1, 54) = 1.62$, $p = .209$, indicating that the magnitude of speeding from before to after the last switch did not differ between congruent and incongruent trials. Mean RTs showed modest and comparable improvements across both trial types (congruent: 874 ms before vs. 864 ms after; incongruent: 949 ms before vs. 929 ms after). This pattern is inconsistent with a strategic shift to word reading, which would predict selective speeding on congruent trials. Instead, parallel reductions in RT across trial types suggest general practice effects within decks rather than a qualitative change in processing strategy.

Additionally, we analyzed deck selection separately for participants who sped ($N = 37$) and those who slowed ($N = 27$) on congruent trials after the last switch trial to test if this factor could explain deck selection. Both groups of participants preferred the low-switch over the high-switch option (57.58% and 58.11%, respectively) and showed above-chance selection of the low-switch option ($\beta = .40$, $SE = .16$, $z = 2.49$, $p = .013$; $\beta = .37$, $SE = .15$, $z = 2.57$, $p = .010$, respectively). These results indicate that deck preferences reflect sensitivity to control-state switching demands rather than strategic exploitation of trial structure.

Discussion

We found that people tended to select lists of Stroop trials with fewer switches between congruency types, providing the first evidence that people actively avoid the demands of internally switching between relaxed and focused control states within a single task. We replicated these results with a preregistered study with stronger power (according to an a priori power analysis), reported in the Supplemental Materials, reinforcing the robustness of our novel finding.

Experiment 2

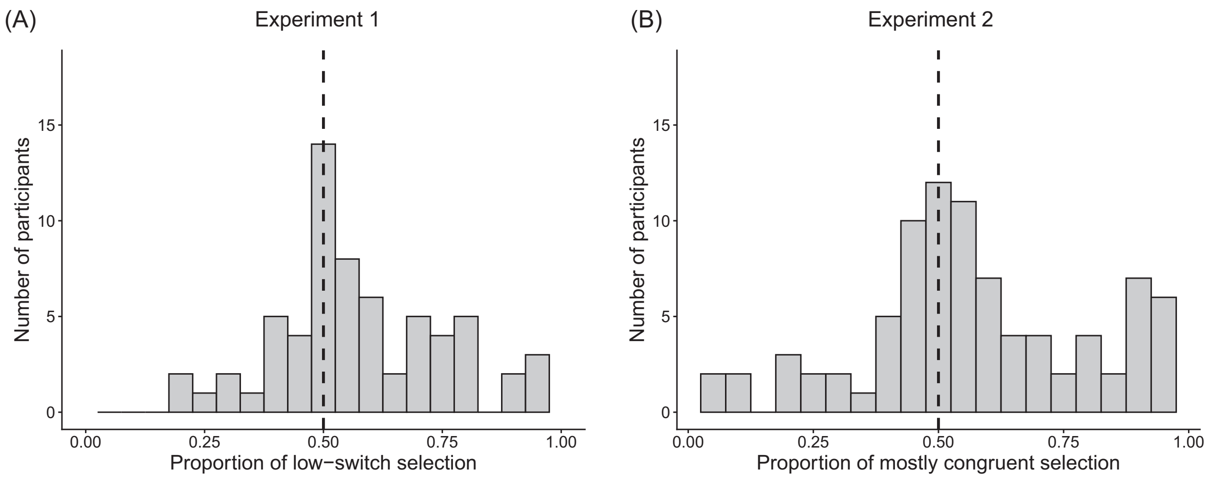
One of our key aims was to examine whether internal control-state switching demands trade off against downstream demands, such as interference. Therefore, we first needed to demonstrate that individuals avoid interference, using the same DST paradigm. Resolving interference in the Stroop task, dealing with conflicting information from the word and the picture on individual trials, is a canonical form of cognitive demand. Indeed, a great deal of research shows that incongruent trials yield increased RTs and error rates compared to congruent trials (MacLeod, 1991). Consequently, one could expect that as the number of conflicting (incongruent) trials increases in a list, subjective effort demands would also increase.

The list-wide proportion congruence (LWPC) paradigm investigates behavioral performance change in response to an increase in the frequency of conflicting trials (Bugg & Crump, 2012; Logan & Zbrodoff, 1979). In this paradigm, participants experience mostly congruent (MC; e.g., 80% of the trials are congruent, and 20% of the trials are incongruent) and mostly incongruent (MI; e.g., 20% of the trials are congruent, and 80% of the trials are incongruent) lists. This leads to the highly stable LWPC effect (Bugg, 2014, 2017; Bugg & Crump, 2012; Bugg et al., 2008; Gonthier et al., 2016; Spinelli et al., 2019), with smaller Stroop effects observed in the MI compared to the MC lists. This finding is assumed to reflect heightened control in the MI lists to deal with frequent interference and relaxed control in the MC lists, where distracting information interferes less frequently with optimal performance. Taken together, this suggests that the MI lists require a higher level of effort compared to the MC lists;

[10] We thank an anonymous reviewer for raising this possibility.

Figure 2

The Distribution of the Proportion of Low-Switch Option Selection in Experiment 1 (A) and Mostly Congruent Option Selection in Experiment 2 (B)



Note. We found that 41 out of 64 participants selected the low-demand option (low-switch for Experiment 1 and mostly congruent for Experiment 2) more than the high-demand option (high-switch for Experiment 1 and mostly incongruent for Experiment 2) in Experiment 1, and 54 participants out of 86 in Experiment 2.

however, direct evidence for this is lacking. Even though there is evidence that people avoid frequent interference associated with specific items (stimuli, Bustos et al., 2024) or contexts (Schouppe et al., 2014), it has not been demonstrated that people tend to avoid lists with a high proportion of incongruent trials.

In Experiment 2, we aimed to investigate such avoidance of frequent interference as a foundational step for our subsequent experiments. We tested this by manipulating the LWPC between options in the DST. One option yielded MC lists with infrequent interference, and the other yielded MI lists with frequent interference. Critically, to isolate the effects of interference, internal control-state switching demands were kept equal between the lists. We reasoned that if MI lists are more effortful to process, participants should prefer MC lists; but if not, MC selection should not exceed chance.

Method

Transparency and Openness

The hypotheses, methods, and analysis plan were preregistered (https://osf.io/gawj5/) prior to data collection. All study materials, data, and analysis scripts are publicly available (https://osf.io/8vhcw/files/osfstorage).

Participants

We performed an a priori power analysis identical to that in the replication of Experiment 1 (see the Supplemental Materials), using Cohen's method with G*Power (Faul et al., 2007). This analysis indicated that 85 participants would provide .95 power ($\alpha = .05$) for detecting an effect of $d = .37$. We specified that we would stop at 100 participants unless the number of usable participants fell below the target sample size.[11] We recruited 95 students from Washington University in St. Louis to take part in the study as a partial requirement for psychology courses. We excluded one participant due to an extreme error rate (more than 20%), four for not following the instructions (i.e., selecting one of the decks less than 2% of the time and not exploring decks at the beginning of the experiment), and four for taking more than 5 hr, resulting in 86 usable participants (65 reporting "female," 19 reporting "male," two did not prefer to answer; $M_{\text{age}} = 18.98$, $SD = 1.12$).

Materials and Procedure

The materials and procedure closely followed Experiment 1. However, the low-demand deck now yielded lists of trials that were MC, and the high-demand deck yielded lists that were MI. Specifically, when the MC deck was selected, 10 congruent and two incongruent picture–word Stroop stimuli were presented in a random order. When the MI deck was selected, two congruent and 10 incongruent picture–word Stroop stimuli were presented in a random order (Figure 1). Congruency-switch rate was not directly manipulated, and the sequence of trials was randomly determined, but because the ratio of trial types was identical between decks (8:2), the expected congruency-switch rate was identical between them.

Results

To analyze Stroop task performance, we fit a linear mixed-effects model with fixed effects of deck type (MC vs. MI) and trial type (congruent vs. incongruent) and subject-level random

[11] The stopping rule establishes a preregistered maximum for participant recruitment, serving two purposes: (a) preventing optional stopping (i.e., continuing data collection to achieve significance) and (b) accounting for anticipated exclusions to ensure the target sample is reached. The final number of valid data sets may fall between the target and this maximum depending on exclusions and recruitment practicalities.

intercepts and slopes for both factors.[12,13,14] To test whether participants preferred the MC option over the MI option, we analyzed deck choices using a logistic mixed-effects model with subject-level random intercepts. The dependent variable indicated whether the MC deck was selected on a given trial (coded as 1 vs. 0).[15]

Stroop Task Performance

We observed a significant Stroop effect for RTs, indicated by a main effect of trial type, $F(1, 83) = 252.36$, $p < .001$. Participants responded faster on congruent trials ($M = 831$ ms) compared to incongruent trials ($M = 908$ ms), $b = 76$ ms, 95% CI [67, 86]. The Stroop effect was significant both for the MC deck, $F(1, 88) = 242.32$, $p < .001$, $b = 94$ ms, 95% CI [82, 105], and the MI deck, $F(1, 70) = 160.46$, $p < .001$, $b = 56$ ms, 95% CI [47, 65]. As expected, the Trial Type × Deck Type interaction was significant, indicating that the Stroop effect was significantly smaller for MI decks ($M = 62$ ms) compared to MC decks ($M = 91$ ms), $F(1, 8465) = 37.94$, $p < .001$, demonstrating an LWPC effect, $b = 33$ ms, 95% CI [22, 43]. Finally, there was a main effect of deck type, $F(1, 77) = 8.51$, $p = .005$, indicating that participants were slower in MC decks ($M = 876$ ms) compared to MI decks ($M = 862$ ms), $b = 13$ ms, 95% CI [4, 21].

Demand Selection

We assessed participants' preferences with a logistic mixed-effects model with subject-level random intercepts. Participants preferred the MC option over the MI option: The mean proportion of MC option selection throughout the experiment was 56.63% (see Figure 2). In this model, the intercept represents the log-odds of selecting the MC option, with a value of zero corresponding to a selection at the chance level (50%). The intercept was significantly greater than zero ($\beta = .35$, $SE = .13$, $z = 2.62$, $p = .009$), indicating that participants selected the MC deck more often than expected by chance.

Discussion

In Experiment 2, participants preferred MC options, avoiding the higher demands of frequent interference in MI options. This sets the stage for the question posed in the remainder of this article: Do people trade off internal control-state switching costs against the costs of resolving interference?

Experiment 3

The experiments so far have uncovered independent evidence that people avoid frequent internal control-state switching (Experiment 1) and interference (Experiment 2). However, in many real-world contexts, cognitive control is often challenged by multiple, potentially competing, cognitive demands, requiring individuals to navigate trade-offs rather than simply avoiding one cost in isolation. Thus, the goal of Experiment 3 was to examine how internal control-state switching and interference jointly shape demand avoidance and whether the preference for infrequent interference options can be flexibly modulated by the demands associated with control-state switching. For instance, when presented with a choice between a high-switch MC option and a low-switch MI option, how do participants negotiate between their preferences to minimize switching between control states and encounter less interference?

To test this, we independently manipulated congruency-switch rate and PC, creating four deck types that varied in either control-state switching demand, interference demand, or both. This design creates situations where the deck with less interference demand (MC) may carry higher control-state switching demands, and vice versa. On each trial, participants made choices between two of these options. Our primary question was whether the well-established preference for infrequent interference options (consistently observed in prior work—Bustos et al., 2024; Schouppe et al., 2014—and in our Experiment 2) is modulated by the amount of control-state switching. If people integrate both demands, then preferences for MC over MI should vary depending on the congruency-switch rate, indicating that interference avoidance is flexible and revealing a trade-off between avoiding interference and avoiding frequent control-state switches. In other words, we tested whether the preference for the option with low interference decreases when it requires frequent control-state switching.

Method

Transparency and Openness

The hypotheses, methods, and analysis plan were preregistered (https://osf.io/u6zky/) prior to data collection. All study materials, data, and analysis scripts are publicly available (https://osf.io/8vhcw/files/osfstorage).

Participants

We used the same a priori power analysis as Experiment 2. Since we have more variables in this study compared to it, we aimed for a sample size of 120 to be more conservative. We specified that we would stop at 130 participants unless the number of usable participants fell below the target sample size. We recruited 128 students from Washington University in St. Louis to take part in the study as a partial requirement of psychology courses. We excluded eight participants due to extreme error rates (more than 20%), resulting in

[12] Note that, in the original preregistration, Stroop task data (and flanker task data for Experiment 5) were to be analyzed using repeated-measures analysis of variance. In line with reviewer recommendations, we instead report mixed-effects analyses for consistency across all experiments and to better capture participant-level variability. Importantly, the pattern of results and statistical conclusions were qualitatively identical across both methods and did not affect any reported conclusions.

[13] Across all participants, the RT analyses for Experiment 2 included 81,557 congruent trials from MC decks, 15,773 incongruent trials from MC decks, 12,551 congruent trials from MI decks, and 62,372 incongruent trials from MI decks.

[14] We additionally reran this analysis excluding trials that occurred before participants had experienced both decks at least once. This exclusion removed very few trials and did not qualitatively change any of the results.

[15] This analysis deviates from the preregistered plan of Experiment 2 (and the replication of Experiment 1), which specified a one-sample Wilcoxon signed-rank test on participants' choices. Following a reviewer suggestion, we analyzed choice behavior using mixed-effects models. Importantly, the mixed-effects analysis yields the same inferential conclusion as the Wilcoxon test for both Experiments 1 and 2. Therefore, we reported the mixed-effects model results since the mixed-effects models have the advantage of using all available trial-level data while appropriately accounting for within-subjects dependency.

120 usable participants (88 reporting "female," 28 reporting "male," four reporting "nonbinary"; $M_{\text{age}} = 19.27$, $SD = 1.08$).

Materials and Procedure

Similar to Experiments 1 and 2, participants completed an experiment involving deck selection and a subsequent picture–word Stroop task. However, in the present experiment, participants were presented with four deck types reflecting the crossing of the congruency-switch rate and PC manipulations. The experiment comprised a high-switch MC, a high-switch MI, a low-switch MC, and a low-switch MI deck. For each participant, one of four distinct shapes (diamond, triangle, cross, and circle) was randomly assigned to each deck type. On each trial, participants saw two of these shapes, selected one, and then completed a list of Stroop trials.

This experiment featured some modifications to allow for the simultaneous manipulation of congruency-switch rate and PC. Participants performed 16 Stroop trials per deck selection instead of 12, providing greater flexibility to manipulate both factors. The MC decks included 10 congruent and six incongruent trials, while the MI decks included six congruent and 10 incongruent trials. The low-switch decks involved five switches between congruent and incongruent trials, whereas the high-switch decks included 11 switches. For instance, selecting a high-switch MI deck resulted in 10 congruent and six incongruent trials with 11 switches between them. All trial sequences were randomized while maintaining these conditions. Note that the combined manipulation of PC and congruency-switch rate inherently reduced the strength of each manipulation, compared to Experiments 1 and 2.

To ensure participants gained sufficient experience with each deck, a learning phase was introduced before the main choice phase (Devine & Otto, 2022; Sayalı & Badre, 2019). Participants practiced a list of trials from each deck eight times in a random order without making any selections. At the beginning of each list, the shape representing the deck was displayed, indicating which deck they would practice next. Participants then completed a list of 16 Stroop trials with the congruency-switch rate and PC associated with that deck. To facilitate learning of the association between deck and trial structure, the deck's symbol (e.g., diamond, triangle) was displayed above and below the Stroop stimuli during all trials, both in the learning and the main choice phases (Devine & Otto, 2022; Figure 3).

After completing the learning phase, participants proceeded to the main choice phase, during which they chose between pairs of decks. We presented all possible pairings, but the theoretically more important ones (low-switch MC vs. low-switch MI, low-switch MC vs. high-switch MI, high-switch MC vs. low-switch MI, high-switch MC vs. high-switch MI) were presented five times more often (50 times per pair) than the pairs with equal PC (10 times for both low-switch MC vs. high-switch MC and low-switch MI vs. high-switch MI), totaling 220 deck selections (see Treiman & Kool, 2024).

The procedure for the deck selection and Stroop trial presentation closely followed previous experiments. Unlike prior experiments that used four exemplars per animal category (e.g., four different pictures of dogs, four different pictures of cats), this experiment presented only one exemplar per category. This simplification reduced potential variability due to the increased number of deck types and does not affect the main manipulation, as the critical factors were the number of congruency-level switches and the total number of congruent and incongruent items, rather than the specific picture shown. To increase the number of deck selections while controlling for the overall duration of the experiment, deck selections were followed by the Stroop trials with a 30% probability. With 70% probability, participants did not see Stroop trials after the deck selection, and a new pair of decks was presented. Participants were explicitly informed:

> In some trials, you will only make a deck selection, but this selection will not be followed by animal pictures. You will not be able to know if the deck selection will be followed by animal pictures because it is random. However, you should make each deck selection as if animal pictures were shown after the deck selection.

This instruction emphasized that each choice should be treated as consequential, to ensure that participants engaged meaningfully with the task on every trial. A final difference was that participants were instructed to use their left hand to respond to Stroop stimuli and their right hand to make deck selections.

Results

We excluded choice trials where participants did not make a selection within the deadline (i.e., 5,000 ms). In such cases, a deck was randomly assigned, and participants were required to complete the Stroop trials associated with that deck. Due to minor timing inaccuracies in JavaScript—likely caused by screen refresh rates, processing delays, or variability in event handling—a very small number of deck selections (16 out of 26,400) were recorded slightly beyond the 5,000-ms deadline. These late responses were manually excluded from analyses to maintain consistency. We excluded the Stroop trials from analyses if the corresponding deck was excluded from the demand selection analysis. Table 2 displays the mean RTs for different deck types in the main choice phase of Experiments 3 and 5. To assess participants' Stroop task performance, we conducted a linear mixed-effects model with fixed factors congruency-switch rate (low-switch vs. high-switch), PC (MC vs. MI), and trial type (congruent vs. incongruent). The model included subject-level random intercepts and random slopes for all main effects, allowing individual differences in the effects of congruency-switch rate, PC, and trial type.[16] This analysis allowed us to determine whether there were differences in performance across decks (according to congruency-switch rate and PC) and trial types.

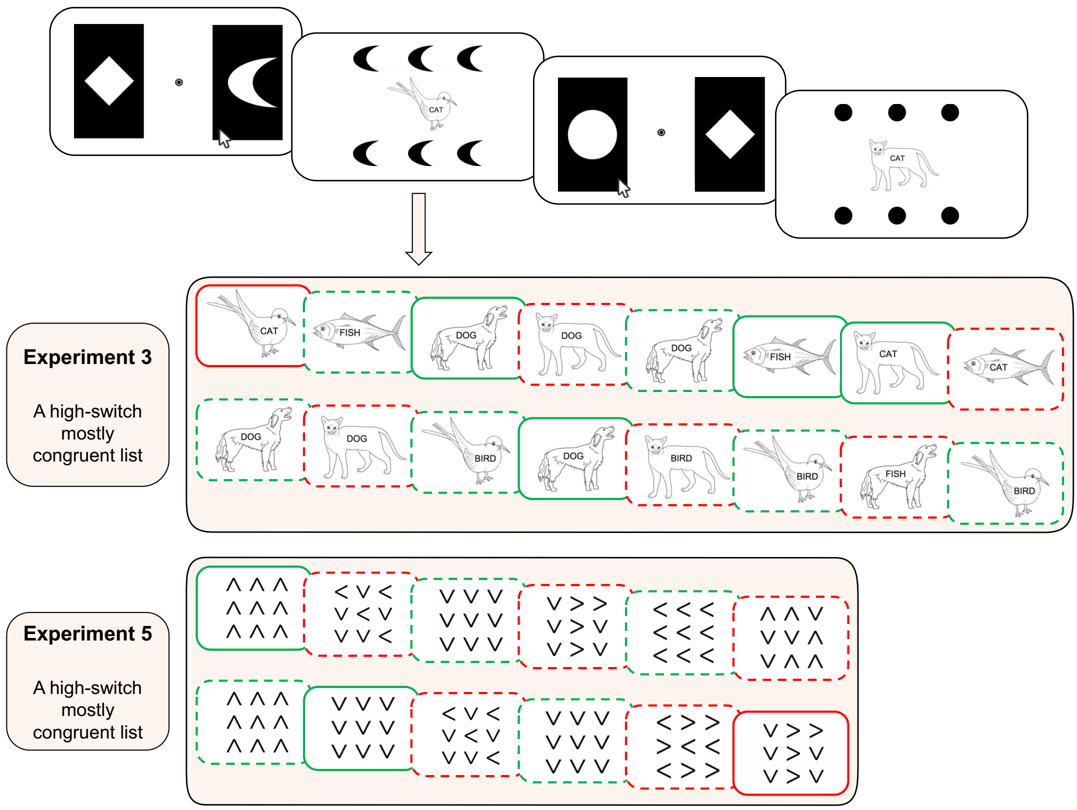
Finally, to evaluate how the preference for MC over MI options changes with congruency-switch rates, we analyzed choice trials involving one MC and one MI deck (see Figure 4).[17] We ran a

[16] Across all participants, the RT analyses for Experiment 3 included 18,732 congruent trials from low-switch MC decks, 11,141 incongruent trials from low-switch MC decks, 10,594 congruent trials from low-switch MI decks, 17,559 incongruent trials from low-switch MI decks, 18,095 congruent trials from high-switch MC decks, 10,730 incongruent trials from high-switch MC decks, 10,300 congruent trials from high-switch MI decks, and 17,089 incongruent trials from high-switch MI decks.

[17] The remaining two deck pairings (high-switch MC vs. low-switch MC and high-switch MI vs. low-switch MI) were not included in the analyses because our focus was on understanding how preferences between MC and MI options change as a function of congruency-switch rate. These "same-interference" pairings cannot inform this question since both decks have identical interference levels. For this reason, these pairings were presented less frequently (10 trials each) compared to the theoretically relevant "cross-interference" pairings (50 trials each).

Figure 3

Trial Sequence of the Main Choice Phase in Experiments 3 and 5



Note. Participants selected one of two choice options, each of which represented a combination of congruency-switch rate and proportion congruence. After they made their choice, 16 Stroop stimuli for Experiment 3 and 12 flanker stimuli for Experiment 5 were presented on the center of the screen with symbols of the selected deck surrounding the stimulus. Participants responded to each trial using previously learned stimulus–response mappings. After responding to all stimuli, a new trial started. Participants made 220 selections in total in Experiment 3 and 160 selections in Experiment 5. An example sequence of a high-switch mostly congruent list following the selection of the associated option is shown in the bottom panel for Experiments 3 and 5 separately. In the figure, congruent for Experiment 3 (or fully congruent for Experiment 5) and incongruent for Experiment 3 (or moderately incongruent for Experiment 5) trials are shown with a green or red frame, respectively, and a dashed frame indicates switch trials. Note that four unique decks (diamond, moon, circle, and triangle for Experiment 3, and diamond, moon, circle, and cross for Experiment 5) were randomly assigned to the four possible combinations of congruency-switch rate and proportion congruence (i.e., low-switch mostly congruent, high-switch mostly congruent, low-switch mostly incongruent, high-switch mostly incongruent). See the online article for the color version of this figure.

logistic mixed-effects model with subject-level random intercepts and random slopes for the log ratio of congruency-switch rates (MI/MC). The model predicted the selection of the MC option based on the log ratio of congruency-switch rates (MI/MC). The intercept captures the baseline preference for MC when congruency-switch rates were equal, while the slope (i.e., the effect of the log ratio of congruency-switch rates [MI/MC]) captures how this preference changed as the MI option involved more switches than the MC option.

Stroop Task Performance

We observed a significant Stroop effect for RTs, indicated by a main effect of trial type, $F(1, 113) = 252.65$, $p < .001$. Participants responded faster to congruent trials ($M = 824$ ms) than incongruent trials ($M = 872$ ms), $b = 47$ ms, 95% CI [41, 52]. There were no main effects of congruency-switch rate, $F(1, 122) = 0.59$, $p = .443$, and PC, $F(1, 112) = 0.95$, $p = .332$, indicating that RTs did not differ between high-switch ($M = 848$ ms) and low-switch ($M = 848$ ms) decks or between MC ($M = 848$ ms) and MI ($M = 848$ ms) decks. There was an interaction between congruency-switch rate and trial type, $F(1, 70087) = 5.08$, $p = .024$, as the Stroop effect was smaller in high-switch lists ($M = 43$ ms) than in low-switch lists ($M = 53$ ms), $b = 10$ ms, 95% CI [1, 19]. The two-way interaction between congruency-switch rate and PC was also significant, $F(1, 14048) = 9.70$, $p = .002$, as the difference between RTs for low-switch lists and high-switch lists differed across MC (low-switch = 852 ms, high-switch = 843 ms) and MI (low-switch = 843 ms, high-switch = 853 ms) lists, $b = 15$ ms, 95% CI [6, 24]. There was an overall LWPC effect indicated by a PC × Trial Type interaction, $F(1, 22016) = 58.83$, $p < .001$. The Stroop effect was smaller in MI lists compared to MC lists ($b = 36$ ms, 95% CI [27, 45]). However, the LWPC effect did not interact with congruency-switch rate, $F(1, 66759)$

Table 2
Mean Reaction Times in Experiments 3 and 5

Experiment	Congruency-switch rate	Trial type	MC	MI
Experiment 3	High-switch	Incongruent	873 (156)	866 (162)
		Congruent	813 (150)	839 (166)
		Stroop effect	60[a] (60)	27[a] (57)
	Low-switch	Incongruent	888 (156)	861 (148)
		Congruent	817 (144)	825 (151)
		Stroop effect	71[a] (64)	36[a] (61)
Experiment 5	High-switch	Relatively incongruent	642 (126)	644 (126)
		Relatively congruent	604 (123)	630 (125)
		Congruency effect	38[a] (32)	14[a] (40)
	Low-switch	Relatively incongruent	645 (128)	638 (125)
		Relatively congruent	602 (121)	629 (131)
		Congruency effect	43[a] (40)	9[a] (31)

Note. Standard deviations are presented in parentheses. MC = mostly congruent; MI = mostly incongruent.
[a] Significant Stroop effects.

< 0.01, $p = .955$, indicating that the LWPC effect was similar in high-switch lists ($M = 33$ ms) and low-switch lists ($M = 35$ ms).

When the MC and MI options were compared across low-switch conditions, there was a significant LWPC effect indicated by a PC × Trial Type interaction, $F(1, 117) = 20.61$, $p < .001$. The Stroop effect was smaller in MI lists ($M = 36$ ms) compared to MC lists ($M = 71$ ms), $b = 35$ ms, 95% CI [20, 50]. The Stroop effect was significant for both low-switch MC decks, $F(1, 106) = 146.82$, $p < .001$, $b = 69$, 95% CI [58, 81], and low-switch MI decks, $F(1, 101) = 44.77$, $p < .001$, $b = 34$, 95% CI [24, 45]. Similarly, when the MC and MI options were compared across high-switch conditions, there was a significant LWPC effect indicated by a PC × Trial Type interaction, $F(1, 7795) = 29.17$, $p < .001$.[18] The Stroop effect was smaller in MI lists ($M = 27$ ms) compared to MC lists ($M = 60$ ms), $b = 36$ ms, 95% CI [23, 48]. Again, the Stroop effect was significant for both high-switch MC decks, $F(1, 93) = 143.30$, $p < .001$, $b = 59$, 95% CI [49, 69], and high-switch MI decks, $F(1, 27269) = 23.62$, $p < .001$, $b = 23$, 95% CI [14, 33].[19]

Demand Selection

To assess how people balance the costs of internal control-state switching and interference, we analyzed choices on trials that involved one MC and one MI deck. Specifically, we ran a logistic mixed-effects model that predicted whether the participant selected the MC option as a function of the log of the congruency-switch rate ratio between the two options (MI/MC), including both random intercepts and random slopes for subjects (i.e., allowing the effect of the log congruency-switch rate ratio to vary across individuals). In this model, the intercept indicates the preference for MC options over MI options when both options have the same congruency-switch rate (i.e., when the log ratio [MI/MC] is 0). The slope reflects how the preference for MC options over MI options changes as the MI option involves more switches than the MC option (i.e., as the log ratio [MI/MC] increases).

Neither the intercept ($\beta = .11$, $SE = .09$, $z = 1.30$, $p = .195$) nor the slope was significant ($\beta = .05$, $SE = .13$, $z = .39$, $p = .700$). That is, participants did not show a preference for MC decks over MI decks when congruency-switch rates were equal and their preference for MC decks did not change as a function of the congruency-switch rate ratio (MI/MC) between two options (see Figure 5).

Discussion

When control-state switching demands were equal across options, participants showed no preference for options with less frequent interference, suggesting interference alone did not drive demand avoidance. Even when options differed in control-state switching demands, the preference did not differ between options.

While this pattern may reflect genuine insensitivity to control demands in this context, we interpret the findings cautiously in light of some design limitations. First, and most importantly, the joint manipulation of congruency-switch rate and PC restricted the design space, limiting the strength and independence of each manipulation. Specifically, because PC and congruency-switch rate were both defined based on the same binary trial type (congruent vs. incongruent), they were structurally coupled: Manipulating PC determines how many trials of each type are available, which in turn constrains the possible switch patterns. For example, strengthening the PC manipulation (i.e., increasing the number of congruent or incongruent trials) inherently reduces opportunities to manipulate the congruency-switch rate. This happens because there are insufficient trials to create varied congruency-switch rate patterns, and vice versa, thereby weakening the strength of each factor. In short, this led to a weak manipulation for both PC and congruency-switch rate in Experiment 3.

Second, because only 30% of deck selections were followed by Stroop trials, participants may have been less motivated to track and avoid specific types of control demand, even though instructions emphasized making each selection as if it would lead to the Stroop task. Together, these factors likely contributed to the absence of robust effects in this experiment. In the remainder of this article, we address this limitation with a novel interference paradigm (Experiment 4), allowing us to employ stronger manipulations and tighter decision–outcome couplings in a DST (Experiment 5).

Experiment 4

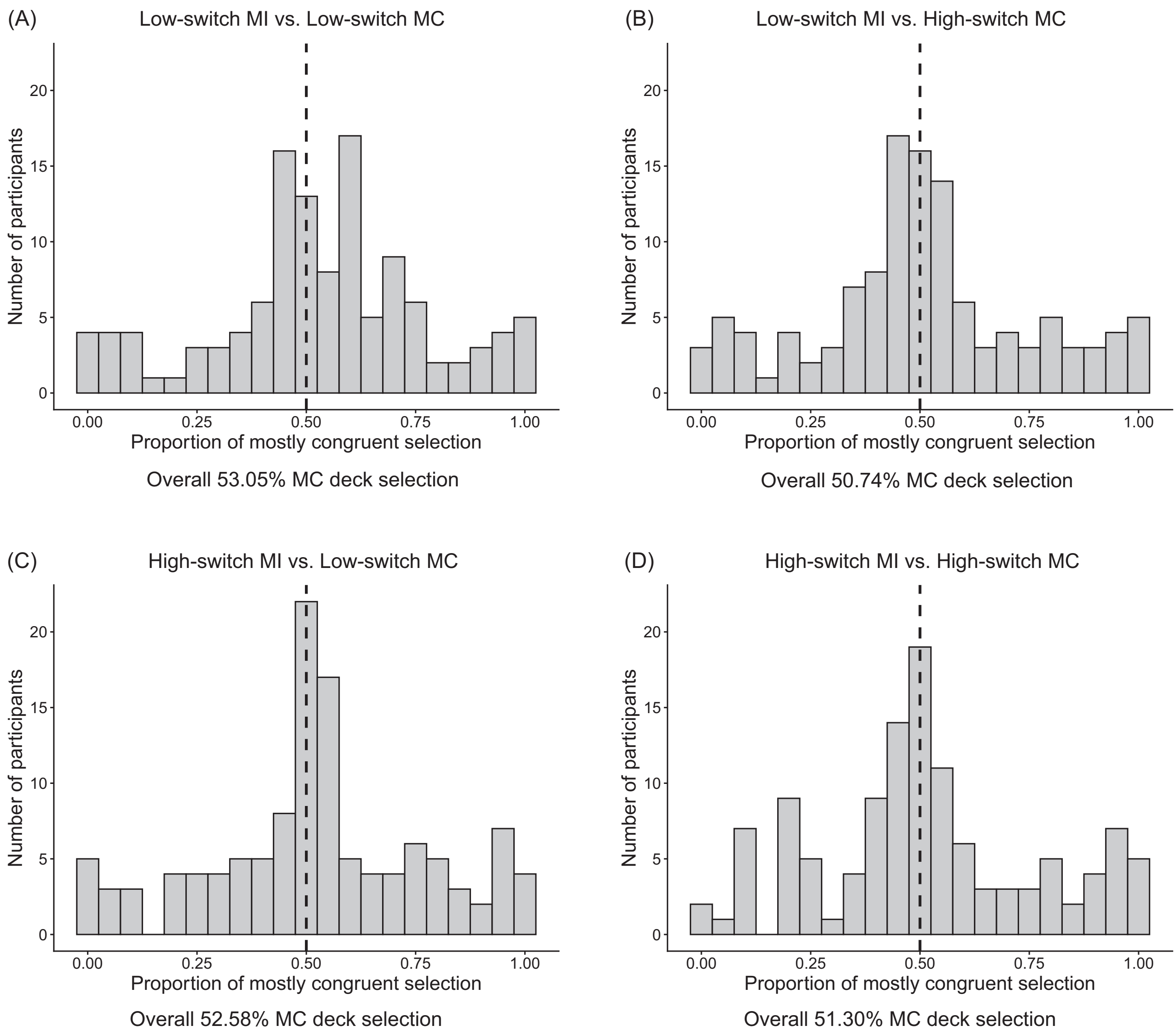
In Experiment 3, we did not observe an influence of control-state switching or interference on selections when they were

[18] This model included a random slope for trial type; adding a random slope for PC resulted in a singular fit.

[19] The model assessing the Stroop effect in high-switch MI decks did not include any random slopes due to singularity issues.

Figure 4

The Distribution of the Proportion of MC Option Selection in Experiment 3



Note. The figure shows the distribution of the proportion of MC option selection rates across participants for (A) low-switch MI versus low-switch MC, (B) low-switch MI versus high-switch MC, (C) high-switch MI versus low-switch MC, and (D) high-switch MI versus high-switch MC deck pairings in Experiment 3. MC = mostly congruent; MI = mostly incongruent.

manipulated simultaneously. However, a key limitation was that our ability to isolate these effects was constrained by the structure of the Stroop task. Because congruency-switch rate and PC were manipulated simultaneously, the strength of one manipulation was inherently limited by the other. For example, for lists of 16 Stroop trials with a relatively high PC (say 75% congruent), the highest possible congruency-switch rate is quite low (eight switches out of 16 trials).

This raises the concern that the lack of demand avoidance for control-state switching or interference in Experiment 3 was due to weak congruency-switch rate and PC manipulations. To address this, we developed a novel parametric flanker task, which manipulates experienced congruency within instead of across individual trials. Participants identified the orientation of a center arrow within a grid while the number of incongruent flankers varied, creating a parametric scale of within-trial congruence. Before using this task to reexamine the trade-offs, we validated that it produces increased RTs as incongruence increases.

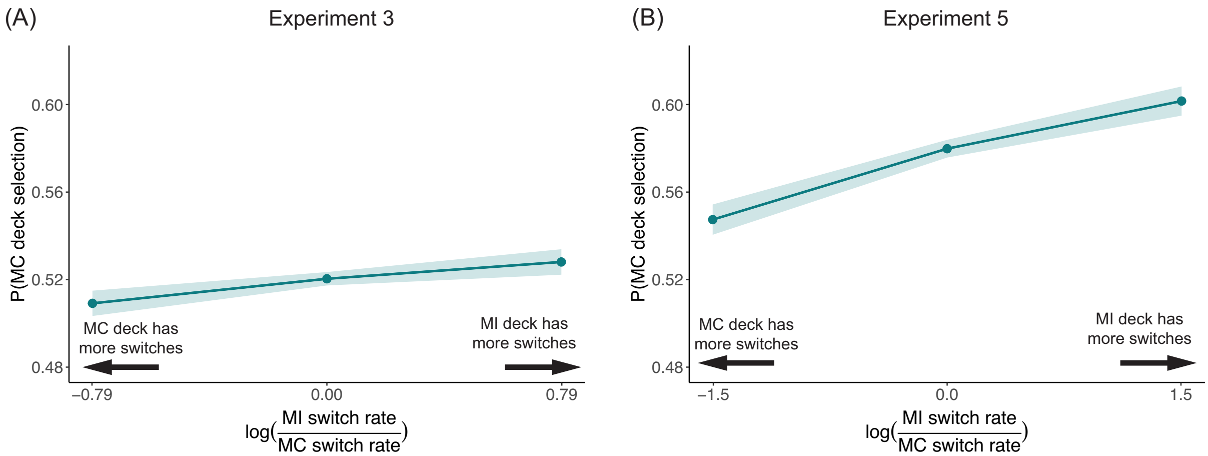
Method

Transparency and Openness

No aspects of this experiment were preregistered. All study materials, data, and analysis scripts are publicly available (https://osf.io/8vhcw/files/osfstorage).

Figure 5

The Probability of Selecting the MC Option by Log Ratio of Congruency-Switch Rates in Experiments 3 (A) and 5 (B)



Note. The figure shows the model-predicted probability of selecting the MC deck based on the log ratio of MI versus MC congruency-switch rates from mixed-effects logistic regression models with random intercepts and slopes for Experiment 3 (A) and Experiment 5 (B). The shaded area around the line represents the 95% confidence interval for the predicted probabilities, indicating the uncertainty in the estimated probabilities at each log ratio (MI/MC) level. MC = mostly congruent; MI = mostly incongruent. See the online article for the color version of this figure.

Participants

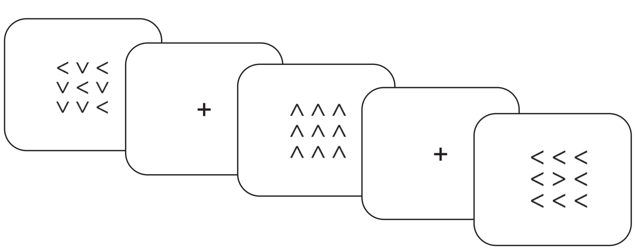
We did not run an a priori power analysis since it was a novel task, and we did not have a study to base the effect size on. We recruited 50 participants from Prolific to take part in the study for a compensation of \$5 (24 reporting "female," 23 reporting "male," three reporting "nonbinary"; $M_{age} = 26.46$, $SD = 2.93$). There were no exclusions because none of the participants had an extreme error rate (more than 20%).

Materials and Procedure

As shown in Figure 6, a parametric flanker stimulus comprising a 3 × 3 grid of arrows pointing left, right, up, or down appeared on each trial. The number of incongruent flankers varied from 0 to 8, and all of them pointed in the same direction. Participants were instructed to indicate the orientation of the central, target arrow while ignoring surrounding flanker arrows. Participants responded using four naturally mapped keys on the keyboard: "G" for left, "H" for right, "Y" for up, and "B" for down. Stimuli remained on screen until a response was given or 5,000 ms elapsed. Across trials, the orientations of the target and flanker arrows were randomized, ensuring that each target arrow appeared 90 times in total without repetitions to avoid repetition effects. The flanker array was generated on-the-fly for each trial, randomly determining the orientation and spatial position of incongruent flankers according to the designated congruency condition. The interstimulus interval was 300 ms, during which time a fixation cross was presented centrally. However, if no response was made within the deadline, a red "X" was displayed for 1,000 ms before proceeding to the next stimulus. Before the main experiment started, participants practiced the S-R mappings and performed 36 practice trials. The main experiment consisted of 810 flanker trials.

Figure 6

Trial Sequence in Experiment 4



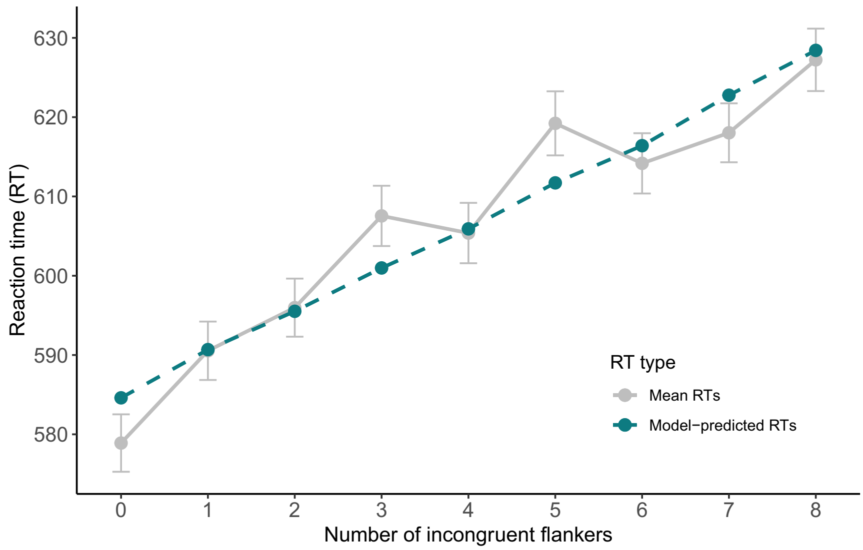
Note. Participants responded to stimuli presented one at a time at the center of the screen by pressing the key corresponding to the orientation of the center arrow. After responding to the stimulus, a fixation cross was presented, followed by the next stimulus. The first, second, and third stimuli represent sample trials with 5, 0, and 8 incongruent flanker arrows, respectively.

Results

To evaluate the effectiveness of the novel parametric flanker task, we tested whether RTs systematically increased with the number of incongruent flankers. We modeled RTs as a function of congruency levels using a linear mixed-effects model with subject-level random intercepts and slopes to account for variability in individual responses to congruency levels. This analysis allowed us to test whether RTs parametrically increased with the number of incongruent flankers, validating the task's sensitivity to graded cognitive demands. Figure 7 depicts the change in RTs as a function of congruency levels, and Supplemental Table S4 provides the mean RTs and error rates for each congruency level.

The model revealed a significant effect of congruency levels on RTs ($\beta = 5.37$, $SE = .49$, $z = 10.95$, $p < .001$). RTs increased as

Figure 7
Reaction Times by Congruency Level in Experiment 4



Note. The figure shows the mean RTs across congruency levels (gray, solid line) and model-predicted values (green, dashed line) in Experiment 4. Error bars represent the within-subjects standard error of the mean RTs. The model accounts for individual variability by including random slopes for congruency level. RTs = reaction times. See the online article for the color version of this figure.

the number of incongruent flankers increased, consistent with the hypothesis that incongruence increased response speed parametrically.

As an additional validation of our parametric flanker task, we examined whether incorrect responses in incongruent trials reflected distractor interference rather than random error. On trials where participants failed to identify the center arrow correctly while experiencing incongruent flankers (i.e., at least one incongruent flanker), 55% of incorrect responses matched the distractor orientation. A logistic mixed-effects model with subject-level random intercepts tested whether this probability exceeded chance (1/3, reflecting three possible incorrect responses). The intercept was significantly greater than zero, indicating that distractor-consistent errors occurred more frequently than expected by chance ($\beta = .99$, $SE = .11$, $z = 8.92$, $p < .001$). This pattern suggests that the distractors' orientation competed with the target orientation during response selection. We further examined how the probability of responding according to the distractor orientation changed across different levels of congruency. A logistic mixed-effects regression model was fit to predict the probability of responding according to the distractor orientation as a function of the congruency level, using subject-level random intercepts for the congruency level.[20] The number of incongruent flankers significantly predicted the likelihood of a distractor-based response ($\beta = .15$, $SE = .03$, $z = 4.32$, $p <$ .001), further validating that the parametric congruency levels in this novel task captured varying levels of sensitivity to distraction.

Discussion

Experiment 4 validated our parametric flanker task, with RTs increasing with more incongruent flankers. This demonstrates it yields graded cognitive demand and sets the stage for Experiment 5.

Experiment 5

In Experiment 5, we used the parametric flanker task to conceptually replicate Experiment 3 but with a key difference: The ability to manipulate congruency within trials allowed us to create stronger, independent manipulations of internal control-state switching and interference. In this paradigm, instead of binary congruent versus incongruent trials, the number of incongruent flankers parametrically varies, allowing us to define "high interference" and "low interference" lists on a continuum. With this design, lists can be created that differ in overall level of interference while using equal numbers of relatively congruent and relatively incongruent trials. Because this introduces sufficient trials at each interference level, we were no longer constrained by the scarcity of one trial type when manipulating the congruency-switch rate. This approach allowed for a cleaner assessment of how interference frequency and control-state switching demands separately influence demand avoidance.

Here, MC lists included six fully congruent trials (zero incongruent flankers) and six moderately incongruent trials (five incongruent flankers). MI lists included six moderately congruent trials (three incongruent flankers) and six fully incongruent trials (eight incongruent flankers, see Figure 3). While the overall level of interference was higher in MI lists than MC lists due to differences in interference magnitude across trial types (since MI lists included fully incongruent and moderately congruent trials, whereas MC lists included fully congruent and moderately incongruent trials), the equal number of congruent (fully or moderately) and incongruent (fully or moderately) trials across MC and MI lists allowed for a strong manipulation of control-state switching: two or nine switches between decks. Crucially, this design still required switching

[20] This model did not include any random slopes due to singularity issues.

between relatively relaxed and relatively focused internal control states, allowing us to examine how internal control-state switching and interference jointly shape choice.[21]

Method

Transparency and Openness

The hypotheses, methods, and analysis plan were preregistered (https://osf.io/9zpb6/) prior to data collection. All study materials, data, and analysis scripts are publicly available (https://osf.io/8vhcw/files/osfstorage).

Participants

Following the same a priori sample size rationale as Experiment 3, we aimed for a sample size of 120. We specified that we would stop at 150 participants unless the number of usable participants falls below the target sample size. We recruited 137 participants from Prolific to take part in the study for a compensation of $11. We excluded seven participants due to extreme error rates (more than 20%), resulting in 130 usable participants (58 reporting "female," 71 reporting "male," one reporting "nonbinary"; $M_{\text{age}} = 24.86$, $SD = 3.38$).

Materials and Procedure

Experiment 5 closely followed Experiment 3. The main difference between the experiments was that we employed stronger manipulations in Experiment 5 by leveraging the within-trial congruency manipulation of our novel parametric flanker task. MC decks (low and high switch) included six fully congruent (i.e., zero incongruent flankers) and six moderately incongruent (i.e., five incongruent flankers) trials, while the MI decks included six moderately congruent (i.e., three incongruent flankers) and six fully incongruent (i.e., eight incongruent flankers) trials. The low-switch decks included two switches between these trial types, whereas the high-switch decks included nine switches (Figure 3). Another difference from Experiment 3 was that the deck selections were followed by flanker trials with a 70% probability rather than 30% probability to increase the influence of the experience with decks on selections. We also removed the deck with a triangle (since it may be suggestive of an "up" response) and instead used a cross shape. Participants made 160 deck selections in total. The procedure of the deck selection closely followed the previous experiments, and the flanker trial presentation was as described in Experiment 4.

Results

The exclusion criteria and analysis plan were identical to Experiment 3 (see Table 2 for mean RTs in the choice phase and Figure 8 for preference for MC over MI options across different deck pairings), with the exception that trial type has parametric levels of congruence depending on the deck type and there was an additional exclusion based on very fast choice selections (<25 ms).[22] During the implementation of Experiment 5, we identified a potential issue where the program could misregister deck selections, possibly due to screen refresh timing causing the cursor's position to be misidentified relative to the deck. Occasionally, this led to a selection being detected before the deck selection screen had fully updated in our pilot tests. To prevent this, we introduced a 25-ms "hold time" before responses were recorded, ensuring that selections were only registered after the display stabilized. This precaution aimed to eliminate the issue without affecting participants' ability to make deliberate choices. Due to minor timing inaccuracies in JavaScript—likely stemming from screen refresh rates, processing delays, or event-handling variability—a very small number of trials (12 out of 20,800) had selection RTs faster than 25 ms. These trials were manually excluded from analyses to ensure consistency.[23]

Flanker Task Performance

We observed a significant congruency effect for RTs, indicated by a main effect of trial type, $F(1, 128) = 351.78$, $p < .001$. Participants responded faster to congruent trials (i.e., fully congruent and moderately congruent trials, $M = 617$ ms) than incongruent trials (i.e., fully incongruent and moderately incongruent trials, $M = 642$ ms), $b = 24$ ms, 95% CI [22, 27]. There was no main effect of congruency-switch rate, $F(1, 114) = 1.27$, $p = .262$, as participants' overall RT was not different across high-switch ($M = 630$ ms) and low-switch ($M = 629$ ms) decks. There was a main effect of PC, $F(1, 129) = 34.66$, $p < .001$, as participants were faster in MC decks ($M = 623$ ms) compared to MI decks (635 ms), $b = 13$ ms, 95% CI [8, 17]. There was no interaction between congruency-switch rate and trial type, $F(1, 84358) < 0.01$, $p = .937$, and between congruency-switch rate and PC, $F(1, 10839) = 0.44$, $p = .509$. There was an overall LWPC effect indicated by a PC × Trial Type interaction, $F(1, 9987) = 128.21$, $p < .001$, as the congruency effect was smaller in the MI decks ($M = 11$ ms) compared to the MC decks ($M = 40$ ms), $b = 27$ ms, 95% CI [22, 32]. Please note that, due to the structure of the novel flanker task, trial types differed across decks: In the MC deck, the congruency effect reflects the RT difference between moderately incongruent and fully congruent trials, whereas in the MI deck, it reflects the difference between fully incongruent and moderately congruent trials. This should be considered when interpreting the LWPC effect in this task. The LWPC effect also interacted with congruency-switch rate, $F(1, 88066) = 5.74$, $p = .017$, indicating that it was smaller in high-switch lists ($M = 24$ ms) compared to low-switch lists ($M = 34$ ms), $b = 11$ ms, 95% CI [2, 21].

When the MC and MI options were compared across low-switch conditions, there was a significant LWPC effect indicated by a PC × Trial Type interaction, $F(1, 6886) = 94.08$, $p < .001$.[24] The congruency effect was smaller for MI decks ($M = 9$ ms) compared to MC decks ($M = 43$ ms), $b = 33$ ms, 95% CI [26, 39]. Also, we confirmed that the

[21] In the flanker task, focused versus relaxed control states can be potentially understood as differences in the spatial extent of attentional allocation, ranging from narrower focus on the central target to broader inclusion of flanking information (cf. the attentional zoom-lens metaphor; Eriksen & St. James, 1986).

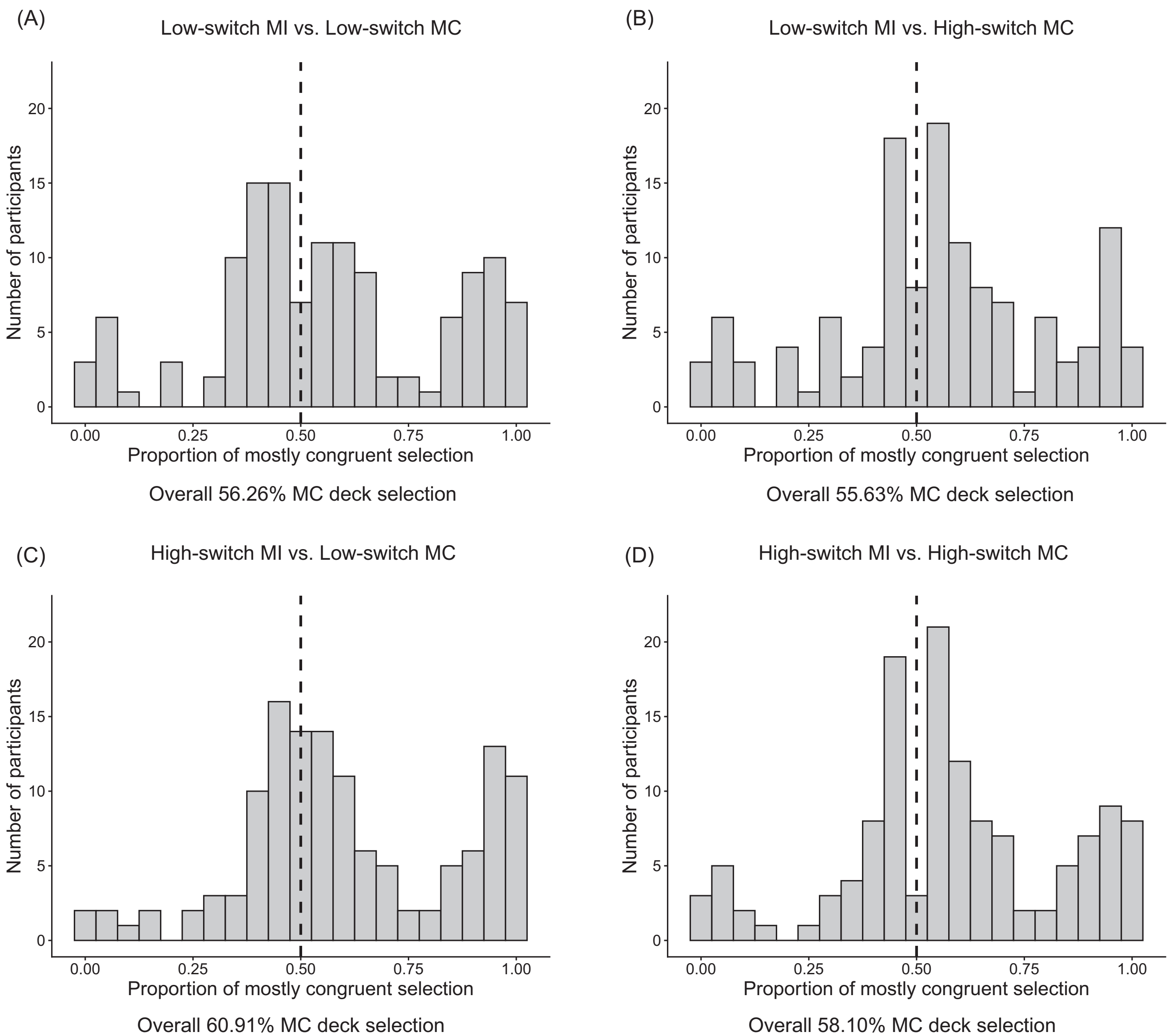
[22] Across all participants, the RT analyses for Experiment 5 included 24,175 relatively congruent trials from low-switch MC decks, 23,868 relatively incongruent trials from low-switch MC decks, 18,793 relatively congruent trials from low-switch MI decks, 18,621 relatively incongruent trials from low-switch MI decks, 23,870 relatively congruent trials from high-switch MC decks, 23,628 relatively incongruent trials from high-switch MC decks, 17,240 relatively congruent trials from high-switch MI decks, and 17,111 relatively incongruent trials from high-switch MI decks.

[23] Since this issue was not controlled in previous experiments, we reran the analyses for all experiments, excluding selections faster than 25 ms and additionally applying the 5,000-ms response deadline (which removes less than 1% of deck selections). These adjustments did not qualitatively change the results in any experiment.

[24] This model included a random slope for trial type; adding a random slope for PC resulted in a singular fit.

Figure 8

The Distribution of the Proportion of MC Option Selection in Experiment 5



Note. The figure shows the distribution of the proportion of MC option selection rates across participants for (A) low-switch MI versus low-switch MC, (B) low-switch MI versus high-switch MC, (C) high-switch MI versus low-switch MC, and (D) high-switch MI versus high-switch MC deck pairings in Experiment 5. MC = mostly congruent; MI = mostly incongruent.

congruency effect was significant for both low-switch MC decks, $F(1, 107) = 183.34$, $p < .001$, $b = 42$ ms, 95% CI [36, 48], and low-switch MI decks, $F(1, 37286) = 10.00$, $p = .002$, $b = 8$ ms, 95% CI [3, 13].[25] Similarly, when the MC and MI options were compared across high-switch conditions, there was a significant LWPC effect indicated by a PC × Trial Type interaction, $F(1, 81595) = 39.62$, $p < .001$.[26] The congruency effect was smaller for MI decks (M = 14 ms) compared to MC decks (M = 38 ms), $b = 21$ ms, 95% CI [15, 28]. Again, the congruency effect was significant for both high-switch MC decks, $F(1, 105) = 266.13$, $p < .001$, $b = 35$ ms, 95% CI [31, 39], and high-switch MI decks, $F(1, 34220) = 25.05$, $p < .001$, $b = 14$ ms, 95% CI [8, 19].[27]

Demand Selection

As in Experiment 3, we ran a hierarchical logistic regression model with both random intercepts and random slopes for the log ratio of congruency-switch rates (MI/MC). In contrast to Experiment 3, the intercept of the hierarchical logistic regression was significant

[25] The model assessing the congruency effect in low-switch MI decks did not include any random slopes due to singularity issues.

[26] This model included a random slope for PC; adding a random slope for trial type resulted in a singular fit.

[27] The model assessing the congruency effect in high-switch MI decks did not include any random slopes due to singularity issues.

($\beta = .48$, $SE = .11$, $z = 4.29$, $p < .001$), indicating that there was a preference for selecting MC options over MI options when controlling for congruency-switch rate. Critically, the log ratio of congruency-switch rates (MI/MC) also significantly predicted the preference of MC options over MI options ($\beta = .12$, $SE = .06$, $z = 2.15$, $p = .032$; see Figure 5). Here, the preference for the MC options increased as the MI option involved relatively more switches than the MC option (i.e., the log ratio of congruency-switch rates increased).

Discussion

Experiment 5 extended the findings of Experiment 3 with stronger manipulations of control-state switching and interference. We found that both internal control-state switching and interference shaped participants' preferences. When congruency-switch rates were equal, participants exhibited a clear preference for MC environments. However, this preference diminished when MC options required more frequent state switches, revealing a trade-off between avoiding frequent control-state switches and minimizing interference. Across all experiments and contrasts, participants preferred MC over MI decks, suggesting that interference demands were stronger in this task. Critically, however, control-state switching demands attenuated, but did not eliminate, this avoidance for interference. These results demonstrated that this novel cost associated with control-state switching can flexibly modulate the well-established avoidance for high interference frequency.

General Discussion

Cognitive control is pivotal for goal-directed behavior but carries inherent costs. We examined how the cost of internally switching between relaxed and focused control states within a single task shapes demand avoidance. Experiment 1 demonstrated, for the first time, that participants avoid frequent internal control-state switches, highlighting their cognitive cost. Experiment 2 also provided the first direct evidence that participants avoid lists including frequent interference, extending prior item-based (Bustos et al., 2024) and context-based (Schouppe et al., 2014) demand-avoidance findings. Experiment 3 was designed to test for a trade-off between internal control-state switching costs and interference costs when both demands are present, but did not provide such evidence, most likely because the manipulations were weak due to the constraints of the experimental design. However, using a novel parametric flanker task (Experiment 4), which enabled stronger manipulations of both factors, Experiment 5 demonstrated both interference and control-state switching influences on decisions, with a trade-off: The preference for MC environments decreased when they required more internal control-state switches.

These findings advance models of control regulation, such as the Expected Value of Control framework (Shenhav et al., 2013, 2017), by showing that individuals incur costs not only when increasing control intensity (Kool et al., 2010) but also when adjusting it. They also build on prior work that shows that reconfiguration costs slow people down when adjusting control settings in response to external cues (e.g., speed vs. accuracy or low vs. high reward; Grahek et al., 2025; Kukkonen et al., 2025) or when voluntarily shifting between cognitive strategies (Weis & Kunde, 2024). However, our results show that internal control-state switching is a distinct, effortful process that systematically shapes control allocation: Even in environments with frequent interference, participants aimed to minimize control-state switching. By identifying internal control-state switching as a critical determinant of cognitive effort, our study highlights an often-overlooked factor in control allocation.

If switching between control states is effortful, then why do participants not adopt a single, stable, "intermediate" control state that is strong enough to handle occasional interference but not so rigid as to be unnecessarily effortful on congruent trials? Such a strategy would minimize the costs of dynamic control-state switching. Of course, our results suggest that participants do not default to a uniform (intermediate) control setting, as this would have led to no preference for low-switch decks. Instead, they avoided frequent control-state switching, suggesting that participants engaged in trial-by-trial control modulation and thus incurred a cost of reconfiguration of control when choosing the high-switch deck. This apparent conundrum may be explained by considering that strategy selection is rarely driven by a single decision. For example, participants may have been motivated to achieve a baseline level of performance, making it worthwhile to reconfigure control states even if reconfiguration is associated with a cost. Such cost–benefit analyses have been argued to lie at the core of effort-based decision making (Botvinick & Braver, 2015; Kool & Botvinick, 2018), explaining why people exert cognitive control even if it registers as costly (Inzlicht et al., 2025).

Are Participants Aware of the Control Demands They Are Balancing?

One important question raised by the findings is whether participants are aware of the control-state switching demands when making their choices in the DST. Therefore, we assessed participants' awareness of the deck contingencies using open-ended questions at the end of Experiments 1, 2, 3, and 5 (e.g., "How did you choose between the decks?"; "Did you notice anything about the decks?"; "Did you develop a preference for one of the decks?"). Across experiments, participants showed very limited explicit awareness of the deck contingencies, particularly the manipulation of congruency-level switching (see the Supplemental Materials for the results). Most participants could not articulate why they preferred certain decks but indicated they "felt easier" or "felt smoother," suggesting that subjective impressions rather than accurate knowledge about the options guided their preferences. Importantly, participants who reported no awareness still showed comparable deck preferences to those who reported awareness, indicating that demand avoidance emerged largely in the absence of explicit knowledge.

This pattern has important theoretical implications. First, it suggests that avoidance of control-state switching does not depend on consciously recognizing the underlying contingencies, consistent with prior work showing that demand avoidance can arise through implicit learning rather than conscious strategy formation (Kool et al., 2010), and it supports the interpretation that participants were not intentionally optimizing based on explicit structural knowledge of the decks. Second, the finding that control-state switching demands were difficult to detect explicitly, despite reliably influencing behavior, suggests that the abstract, sequential structure that governs control-state switching is learned implicitly through subjective experience rather than explicit reasoning. More broadly, this pattern underscores the need for future work investigating the mechanisms by which people track the costs of control-state reconfiguration and how these implicit learning processes shape decision making in complex environments.

Assessing the Role of Performance Differences in Demand Avoidance

Even though we found robust evidence that people avoid switching between control states, it is possible that this preference was driven by factors not directly related to these control demands. One intuitive explanation for the avoidance of control-state switching is that participants aimed to avoid the variability in performance that results from frequent control-state switching. Specifically, high-switch decks may elicit increased performance fluctuations (i.e., more variance in RT) or stronger trial-by-trial control adjustments (i.e., larger CSE), which may be effortful and lead to demand avoidance. In other words, people may avoid not the demands of control-state switching per se, but an outcome associated with control-state switching. To evaluate this possibility, we conducted two complementary analyses.

First, we examined trial-by-trial RT variability (using both standard deviation [*SD*] and coefficient of variation [CoV]) within low- and high-switch decks in Experiment 1. Contrary to the idea that the demand avoidance may be driven by performance variability, there was no difference in RT variability between deck types (Experiment 1, low-switch deck $SD = 387$ ms, high-switch deck $SD = 385$ ms, low-switch deck $\text{CoV} = 0.42$, high-switch deck $\text{CoV} = 0.42$), suggesting that participants did not experience more inconsistent performance in high-switch decks (see the Supplemental Materials for the full report of these analyses, as well as the same analyses for the preregistered replication of Experiment 1).

Second, to test whether differences in CSEs explained choice behavior, we analyzed whether the congruency effect was modulated by the congruency of the previous trial, and more importantly, whether this adjustment effect differed across low- and high-switch decks in Experiment 1 (see the Supplemental Materials for the full report of these analyses, as well as the same analyses for the preregistered replication of Experiment 1). There was no significant CSE in Experiment 1, as the congruency effect was comparable following congruent ($M = 77$ ms) and incongruent ($M = 64$ ms) trials, and critically, the CSE did not differ between high-switch ($M = 8$ ms) and low-switch ($M = 16$ ms) decks. If anything, the CSE was numerically smaller in the high-switch decks, possibly reflecting participants' improved ability to manage frequent transitions. Thus, CSE was absent overall and numerically smaller in high-switch decks, suggesting that participants may become better at handling congruency transitions under higher control-state switching demands. In other words, participants may increase cognitive flexibility in high-switch contexts. This interpretation aligns with findings from task-switching studies, which show that when forced-choice and free-choice trials are intermixed, switch rates during forced-choice trials influence participants' behavior on free-choice trials (Fröber & Dreisbach, 2017). Specifically, people are more likely to choose to switch between tasks when they have experienced frequent task switching in forced-choice trials. These findings indicate that being in a high-switch context can shift overall control settings, leading individuals to adopt a more flexible mode even when task choices are voluntary.

Together, these findings argue against the idea that participants avoided high-switch decks because of increased performance variability or increased trial-by-trial adjustment effects. Neither RT variability nor CSE was greater in high-switch conditions. Instead, the observed avoidance likely reflects the subjective effort associated with control-state switching demands, rather than objective performance differences between options.

Relation to Voluntary Task-Switching Research

The present work is related to research on voluntary task switching (VTS), which examines how people choose between tasks when given control over task selection. Thus, both examine how people choose between options under varying internal and external constraints (Arrington & Logan, 2004; Kiesel et al., 2010; Mittelstädt et al., 2018). However, VTS studies generally focus on choices between distinct task sets that differ in task goals and S-R mappings. In this literature, choice patterns and repetition biases are primarily explained by the costs of switching between task sets.

In contrast, our paradigm isolates a more fine-grained within-task adjustment: switching between control states that modulate attentional focus while the task goal, stimuli, and S-R mappings remain constant. Thus, whereas VTS decisions reflect the costs of switching task sets more broadly, our findings reveal that people also avoid switching subcomponents of a task set, such as the attentional weighting parameters that determine target and distractor processing. This distinction shows that switching costs are not limited to goal-level or mapping-level reconfiguration; even within a stable task set, adjustments of control-state parameters carry subjective costs that influence choice. Our results therefore complement the VTS literature by demonstrating that subjective switching costs emerge even at this finer representational level, suggesting that the cognitive system tracks and avoids adjustment demands not only at the task-set level but also within the internal control configuration for a single task.

Limitations and Future Directions

One limitation of the present work is that the observed effects on choice behavior, while statistically significant, were relatively modest. This is a common feature of data from DSTs, which are intentionally designed with minimal instruction and no overt incentives to bias participants' decisions (see Kool et al., 2010; Treiman & Kool, 2024, for discussion). As a result, participants are free to select options based on a range of factors, some of which may be unrelated to cognitive effort (e.g., preferences for the visual appearance or location of a deck). This openness increases variability in choice behavior and introduces noise that can attenuate real effects. In the original DST, Kool et al. (2010) measured demand preference across many blocks involving decks that appeared in different locations and with different appearances. This attenuates the influence of choices based on factors other than effort. However, because each choice in the present studies yielded a list of trials, such a design was impossible. Importantly, despite these sources of noise, we consistently observed robust and replicable effects across experiments, suggesting that sensitivity to control demands reliably influences choice behavior even in the absence of strong external constraints. Future work could create DST paradigms with incentive-based designs to potentially observe stronger effort avoidance.

We specifically tested whether control-state switching demands modulate the well-established preference for avoiding interference.

Indeed, Experiments 3 and 5 were designed to test this preregistered directional hypothesis, oversampling choices where the options differed in PC. As a result, the reverse question (whether interference demands modulate preference for control-state switching) was not the focus of the present study. Future work could more directly examine whether the relationship between control-state switching and interference demands is bidirectional.

Although congruent and incongruent trials do not constitute separate tasks in the traditional sense, since the task goal remains constant, it is still possible that participants implicitly categorize them as different contexts (e.g., "easy" vs. "hard" contexts) that feel task-like. Such experiential categories may function as distinct control states, raising the question of whether people sometimes treat control-state variations as if they were different tasks.[28] Future work could directly investigate how participants represent these contexts and whether control-state representations share mechanisms with task representations in classic task-switching paradigms.

Internal control-state switching costs have implications for understanding individual differences in cognitive control. People vary considerably in their willingness to exert cognitive effort (Westbrook & Braver, 2015) and their cognitive flexibility (Hommel & Colzato, 2017). If internal control-state switching is indeed effortful, then individual differences in sensitivity to control-state switching costs could help explain why some people are more cognitively flexible than others. While exploratory analyses to test individual differences (e.g., relating individual congruency effects to demand avoidance) are intuitively appealing, we chose not to pursue them for several reasons. First, difference scores such as the congruency effect are known to have low reliability (Draheim et al., 2021; Kałamała et al., 2020; Paap et al., 2020; Rey-Mermet et al., 2018; but also see Moretti et al., 2026), which can obscure true relationships. Second, our studies were not powered or designed to support robust individual differences analyses. Another limitation concerns the nonindependence of task performance and choice behavior in our experiments. Because performance metrics (e.g., RT, accuracy, congruency effects) are measured within the decks that participants selected, selection and performance are inherently linked. This limits the interpretability of relationships between performance and selection. For example, participants that selected the MI deck more often necessarily had more practice with incongruent trials, likely leading to a reduced interference effect. Here, an individual differences approach may lead one to falsely conclude that those participants preferred the MI deck because they showed smaller interference effects. Future work could more directly address these questions by using independent task batteries to assess individual differences in task performance (e.g., Stroop or flanker task) before participants engage in demand selection. Such investigations would offer valuable insights into how control-state switching costs contribute to individual profiles of cognitive flexibility and effort allocation.

In the present work, we operationalized the cost of cognitive control through participants' choices between options. We use the term "subjective effort cost" following conventions in the demand-avoidance and effort-based decision-making literatures (e.g., Kool et al., 2010; Westbrook & Braver, 2015), where "subjective" refers to the internal valuation that participants assign to different control demands, inferred indirectly from revealed preferences. This usage does not imply direct access to participants' introspective experience. Indeed, our study does not include self-report measures, and therefore our estimates reflect an indirect, behavioral index of how participants value (or devalue) exerting control. Critically, across experiments, participants reliably preferred options involving fewer control-state switches even when these options did not have objective performance advantages. In particular, we observed no RT or accuracy differences between low- and high-switch options in any experiment, indicating that congruency-switching frequency itself did not impose measurable performance costs. Moreover, we did not observe differences in RT variability or congruency sequence effects between low- and high-switch options, further ruling out performance-based instability as a driver of choice. We acknowledge two important nuances. First, in Experiment 2, there was a main effect of PC on RT; however, this effect ran in the opposite direction of the observed preferences (i.e., participants were slower in MC decks) and therefore cannot explain the avoidance of MI decks. Second, in Experiment 5, participants were faster in MC decks, which may have contributed to the preference in that experiment. However, because an MC preference was observed consistently across all experiments, including those with no performance advantage, objective performance differences cannot fully account for the pattern of choices.

To avoid confusion, we emphasize that "subjective" in this context denotes that the cost is constructed by the participant and is not determined by external performance metrics. As a limitation and direction for future work, incorporating explicit self-report measures or psychophysiological indices (e.g., pupillometry, Kahneman & Beatty, 1966; cardiovascular markers, Wright, 1996) would allow a more direct characterization of subjective effort experience alongside choice behavior.

Conclusion

Even though cognitive control is critical for goal-directed behavior, people often fail to successfully implement it. Current theories explain these failures as the result of a motivated withholding of the required amount of control (Botvinick & Braver, 2015). Our findings, however, reveal a novel source: the cost of adjusting control processes themselves. Across five experiments, we showed that people actively avoid switching between internal control states and that these control-state switching costs and interference demands jointly shape control allocation. Indeed, our framework opens up the intriguing possibility for understanding cognitive impairments in psychopathology (Dajani & Uddin, 2015) and aging (Peltz et al., 2011) in terms of control adjustment costs. Thus, our work uncovers a critical variable for understanding how humans regulate cognitive control in the pursuit of goals in dynamically changing environments.

Our findings suggest that switching between internal control states incurs meaningful cognitive costs, which has implications for designing real-life environments that support optimal performance. Specifically, our results suggest that environments that minimize the need for frequent control-state transitions or provide structural support for smoother transitions may help mitigate these costs. This has relevance for educational settings, workplace productivity, and technology design, where cognitive efficiency is essential. Some populations, such as older adults, may be especially sensitive to the costs of control-state switching, potentially leading to difficulty

[28] We thank Iring Koch for raising this interesting possibility.

adapting to changing demands. At the same time, this aversion to control-state switching may be adaptive in stable, single-task contexts, where maintaining a consistent control state can reduce the burden of frequent adjustments. By recognizing the cognitive costs of internal control-state switching, interventions and tools can be better tailored to balance flexibility and the cost of flexibility, ultimately enhancing performance across a variety of real-world domains.

Constraints on Generality

Our participant samples consisted of young adults recruited from Washington University in St. Louis and Prolific, which may not represent the full range of individual differences in cognitive control and effort sensitivity across age groups and populations. Also, the choice-based methodology, while revealing preferences, required participants to make explicit decisions about cognitive effort, which does not fully capture how the costs of internal control-state switching influence choice in naturalistic settings.

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