

Learning Where to Be Flexible: Using Environmental Cues to Regulate Cognitive Control

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Cognitive flexibility refers to a mental state that allows efficient switching between tasks. While deciding to be flexible is often ascribed to a strategic resource-intensive executive process, people may also simply use their environment to trigger different states of cognitive flexibility. We developed a paradigm where participants were exposed to two environments with different task-switching probabilities, followed by a probe phase to test the impact of environmental cues. Our results show that people were more efficient at switching in a high-switch environment. Critically, we observe environment-specific triggering of cognitive flexibility after a 4-day training period (Experiment 2, $N = 51$), but not after a 1-day training period (Experiment 1, $N = 52$). Together, these findings suggest that people can associate the need for cognitive flexibility with their environment, providing an environmental triggering mechanism for cognitive control.

Public Significance Statement

Cognitive flexibility, the ability to adapt to changing task demands, is essential for goal-directed behavior. While the regulation of cognitive flexibility is traditionally ascribed to resource-intensive mental processes, contemporary theories of cognitive control have suggested that cognitive flexibility can also be triggered by stimuli in the environment that have been associated with the application of control. Using a novel task-switching paradigm and multiday training, we bring important evidence for these theories, showing that participants can indeed learn to bind (the need for) cognitive flexibility to environmental cues, but only after longer sessions of learning. Our findings support recent theories of cognitive control and meta-learning while also offering important recommendations for cognitive training research and practice.

Keywords: cognitive flexibility, cognitive control, context, task-switching, associative learning

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Humans are often faced with challenges that require them to either maintain task focus or adapt to new task demands. This reflects a trade-off between cognitive stability and flexibility (Dreisbach & Fröber, 2019; Goschke & Bolte, 2014), sometimes referred to as meta-control (Goschke, 2000; Musslick & Cohen, 2021). This regulation of cognitive stability versus flexibility is often ascribed to a supervisory, resource-intensive mental process (Diamond, 2013). However, others have suggested that deciding to be flexible could also rely on basic learning mechanisms that associate required

cognitive flexibility with contextual features, such as cues, stimuli, response modalities, or features in our environment (e.g., Abrahamse et al., 2016; Braem & Egner, 2018; Egner, 2014; Lieder et al., 2018; Verguts & Notebaert, 2009), essentially making it a form of meta-learning (or learning to learn, Wang, 2021). Similar to how the meta-learning literature traditionally focuses on the learning of task structure knowledge or abstract models of the environment (e.g., Griffiths et al., 2019), we here wanted to investigate whether people can learn environment-specific levels of cognitive flexibility.

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Cognitive flexibility is often investigated by measuring the switch cost (for reviews, see Kiesel et al., 2010; Monsell, 2003), which is the performance difference between task alternations (i.e., the current task is different from the previous) and task repetitions (i.e., the current task is the same as the previous). A smaller switch cost is often interpreted as a higher level of cognitive flexibility. While the switch cost has been related to other measures of cognitive flexibility (Pan & Yu, 2018; Zhang et al., 2020), other studies have also shown distinct roles (Beck et al., 2016; Howlett et al., 2021; Ophir et al., 2009), consistent with the idea that “cognitive flexibility” can be measured at different levels of information processing, rather than be considered a core cognitive skill shared across those levels (Arán Filippetti & Krumm, 2020; Ionescu, 2012). Importantly, when measured through switch costs, it has also been shown that humans can adjust their cognitive flexibility. For instance, the switch cost is attenuated as a function of an increasing frequency of task alternations (Bonnin et al., 2011; Dreisbach & Haider, 2006; Monsell & Mizon, 2006), showing different levels of cognitive flexibility in response to varying environmental demands.

In line with the meta-learning framework mentioned above, we propose that cognitive flexibility can also be bound to contextual features in one’s environment, and then retrieved by this environment in a manner of stimulus–control associations. In this way, humans can partially off-load cognitive processing to their environment, since features in that environment can act as predictive cues to signal the generally required level of cognitive flexibility. For instance, when facing a high proportion of task switching, humans prepare more for potential task switches, showing smaller switch costs. If this high-switch condition is further paired with a contextual feature, we hypothesize that this feature will become predictive of this high need for flexibility via associative learning, and helps induce a corresponding control state when humans encounter the same contextual feature again. However, while several have argued for such forms of learning (for similar perspectives in developmental psychology or neuroscience, see Doebel, 2020; Eisenreich et al., 2017), there is currently no scientific evidence for this idea.

First while other studies have shown that people can learn to associate specific tasks or task stimuli (task-relevant features) with a higher need for cognitive flexibility (e.g., Chiu & Egner, 2017; Siqu-Liu & Egner, 2020; Whitehead et al., 2020), no study has shown that people can also use (nominally task-irrelevant) features in their environment to regulate cognitive flexibility, that is, deciding where to be flexible. Second, it is important for our purposes to dissociate stimulus–control associations, which describes a mapping of a certain control state to a specific stimulus (e.g., an environmental cue), from stimulus–action associations, which describe a mapping of an action to a specific stimulus. However, previous studies that tried to show how people can allocate different levels of cognitive flexibility to the spatial location of their task (Crump & Logan, 2010; Leboe et al., 2008, Experiment 2), were likely driven by a stimulus–action instead of stimulus–control contingency learning (Schmidt & Liefoghe, 2016), and did not use an independent probe phase (e.g., Bugg, 2012).

To investigate whether changes in cognitive flexibility can be induced by nominally task-irrelevant contextual features in people’s environment, we developed a novel task-switching paradigm applicable to a large item set to discourage item-specific or task-(sequence-) specific learning, and adopted the use of a probe phase with unbiased switching probabilities (e.g., Braem et al.,

2019). Learning about these higher-level control states may be cognitively and time-demanding. Therefore, we trained participants to learn about two different switching probabilities in two different environments for 1 (Experiment 1) or 4 (Experiment 2) days. We expect to see that participants are able to bind the need for control to the environmental cues and use this knowledge to decide where (i.e., at which cue) to be more flexible, which should be reflected in lower switch costs in high-switching environments, even in phases where the probabilities are the same (i.e., probe phase). Whether the association between contextual features and the need for control constitutes explicit or implicit knowledge is a controversial topic (for a review, see Kunde et al., 2012) and beyond the scope of this study. Therefore, we did not have any prior assumptions on the role of awareness in this process.

Experiment 1

Method

Transparency and Openness

We report our task design, data collection strategy, and analysis plans in Experiments 1 and 2, which were preregistered before data collection. All analyses were done using RStudio (Version 2022.07.1+554) and JASP (Version 0.16.2, JASP, 2022). All pre-registration forms, data files, and analysis scripts in this study can be accessed at <https://osf.io/ajbpt/>.

Participants

We first recruited 60 university students. Because the proportion of participants who detected the context difference was low (see the [online supplemental materials](#)), we did not apply exclusion on this measure. Participants with an accuracy lower than 75% or below $Q1 - 1.5 \times \text{interquartile range (IQR)}$ of the mean group accuracy were removed from analyses. Participants were also removed if, after data exclusion (see criteria below), the remaining data were less than 50% of all trials or below $Q1 - 1.5 \times \text{IQR}$ of group data remaining rate. Five participants were removed due to a severe data missing issue (more than one fourth of their performance was not saved due to either software failure or participants’ internet connection) and another three because of too little remaining data after exclusion, leaving a total sample size of 52 participants (46 females, six males, age ranging from 17 to 31, $M = 18.88$, $SD = 2.12$ for 51 participants, one participant did not report age information). We stopped sampling after these 52 participants because we reached our Bayesian stopping rule¹ ($BF_{10} = 0.10$, calculated by JASP). All participants received course credits for their participation. This study was approved by the ethical committee of the Faculty of Psychology and Educational Sciences of Ghent University.

Material and Procedure

We developed a new cued task-switching paradigm with five categorization tasks: clothing, electronics, foods, liquids, and tools. In the experiment, participants were asked to evaluate whether an object

¹ The alternative hypothesis in all experiments used for the Bayesian stopping rule was that, in the probe phase, the switch costs are smaller in the high-switch condition than the low-switch condition. See the Method and Procedure for experimental setup information.

belonged to the cued category or not. Task-switching occurs when participants moved from one task (e.g., a clothing task) on the previous trial to another one (e.g., a tool task) in the current trial. The experiment script was programmed using JsPsych (de Leeuw, 2015; <https://www.jspsych.org>). Task stimuli consisted of 240 randomly selected object pictures from the Bank of Standardized Stimuli (Brodeur et al., 2010, 2014), which were always presented in the center of the screen with a size of 120×120 pixels. The two environments were indicated by a supermarket or airport security picture as a background, with the peripheral area being blurred (Figure 1A). We used pictures (and the corresponding cover story) of the supermarket and airport security because the categorization tasks we used can be considered appropriate in these two environments. This way, the two environments looked less arbitrary compared to other traditional choices such as colors (Lehle & Hübner, 2008), location on the screen (Crump & Logan, 2010), or shapes (Schoupppe et al., 2014). We only used one picture for each environment, to reduce the burden of this type of abstract learning and help promote this learning process.

The experiment consisted of a learning and a probe phase, divided into blocks. In the learning phase, each environment was associated with either a high probability of having to switch tasks from one trial to the next (90%), or a low-switch probability (30%), respectively (picture–probability associations were counterbalanced across participants). In the probe phase, the actual task-switching probability was set to 60% in both environments. These switching probabilities were chosen as a compromise between the base rate (80% with five tasks) and feasibility (to ensure enough repetitions for analyses in the high-switch probability blocks).

The experiment started with two practice blocks of 20 trials in each environment, where participants needed to reach 80% accuracy before moving on to the experiment. Otherwise, they needed to practice again until they reached the criterion. Next, participants were presented with six experimental runs, each run consisting of eight blocks of 30 trials. The entire session lasted around 60 min. Two environments were alternated block by block, and the order was counterbalanced among participants.

In each run, we ensured that each task was presented equally often in all conditions (i.e., per run, each task was presented 47–49 trials; the difference that any task occurred in one vs. the other environment was always smaller than seven trials; the difference that any task would occur more on switching or repetition trials was always smaller than five trials), and no stimulus was presented twice within a run. In the last four runs (Figure 1B), two out of eight blocks were actually probe blocks (one for each environment), which were randomly determined for each block separately. After the experimental blocks, participants needed to answer a question about whether they noticed the environment-specific frequency manipulation in the experiment (see the [online supplemental materials](#)).

In all blocks, each trial started with a fixation cross that was presented for 600 ms, followed by a task cue that was displayed for 300 ms. The task cue consisted of a “task list,” where all five categories were presented, and the relevant category was highlighted by a red tick mark (Figure 1B). The position of categories and cued category was randomly assigned on each trial, with the exception that no category or red tick reoccurred on the same position on three successive trials, which ensured that cue processing was not easier on task repetition than on task switch trials (Forstmann et al., 2007; Schneider & Logan, 2006). To further distinguish both environments, a Times New Roman letter font was used for the task cues

presented in the airport security environment and Arial was used for the supermarket environment. After the task cue, an object image appeared, and participants needed to press “F” or “J” to answer if the object belonged to the cued category or not (the key-response mapping was fixed within but counterbalanced across participants, and the proportion of each response was 50%). The trial ended when participants pressed one of the two keys or when the response deadline of 2,000 ms had passed. There was no feedback presentation.

Data Analysis

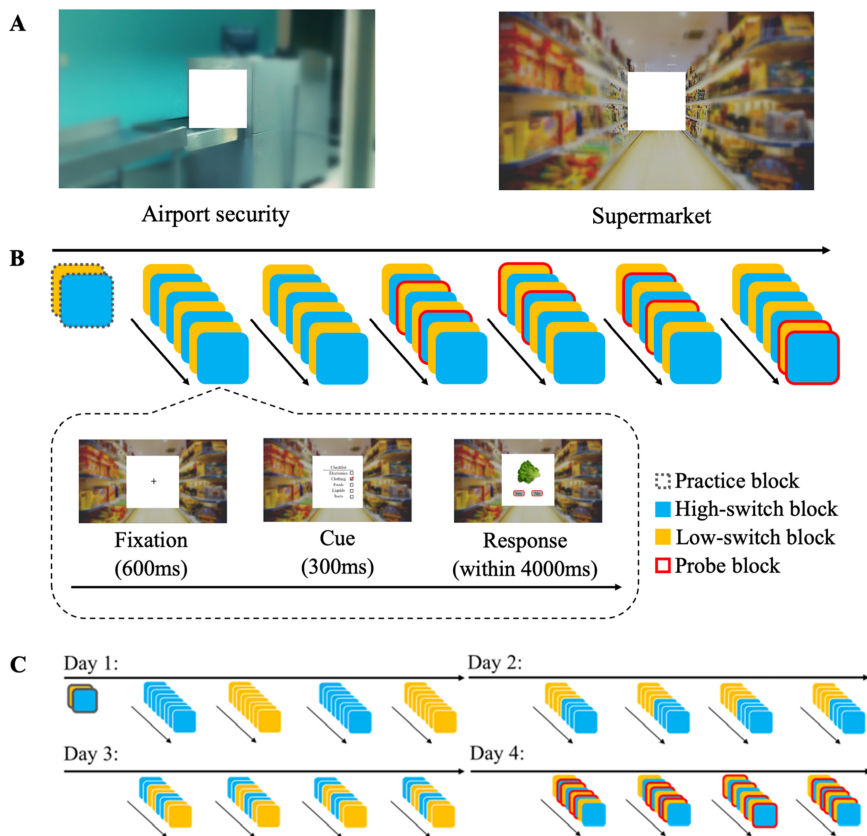
The main dependent variables were reaction time (RT) and accuracy. To measure task-switching, we removed the first trial of each run, because it is not possible to define it as neither task-switching nor task repetition. In a similar vein, we also removed all trials following an error, as the classification of task switch or task repetition is problematic if participants did not process the correct task on the previous trial. Although this is standard practice in task-switching research, we also note that there was not any significant difference in the numbers of posterror trials across different conditions (i.e., context, phase, or their interaction) in Experiment 1 or Experiment 2 (all F s < 3.10, p s > .084, η_p^2 s < 0.05). For RT analyses, we further removed errors, fast RTs (<200 ms), and RT outliers (defined as out of $Q3 + 1.5 \times IQR$ or $Q1 - 1.5 \times IQR$ after removing fast RT trials). To test our main hypothesis, we ran a $2 \times 2 \times 2$ repeated-measures analysis of variance (rmANOVA) with the within-participants factors environment (high- vs. low-switch probability), task sequence (repetition vs. switch), and phase (learning vs. probe) on correct RTs and error rates. We expected a two-way interaction between the environment and task sequence indicating a larger switch cost in the low-switch probability environment. Irrespective of whether or not there was a three-way interaction with the phase, we also evaluated this effect for the learning versus probe phase separately. Finally, we also evaluated our hypotheses using a mixed effect model to ensure our effects could not be attributed to individual differences, task identity, or specific items (see the [online supplemental materials](#)).

Results

Descriptive statistics are displayed in [Table S1 in the online supplemental materials](#). The RT showed a significant main effect of the task sequence, $F(1, 51) = 58.70$, $p < .001$, $\eta_p^2 = 0.54$, indicating a task switch cost. We did not observe a main effect of environment, $F(1, 51) = 0.20$, $p = .658$, $\eta_p^2 < 0.01$, nor did environment interact with task sequence, $F(1, 51) = 2.44$, $p < .13$, $\eta_p^2 = 0.05$. There was a significant main effect of phase, $F(1, 51) = 147.37$, $p < .001$, $\eta_p^2 = 0.74$, showing that RT was slower in the learning relative to the probe phase. Note that the probe phase was only administered in the last four out of six blocks, so the effect of the phase was likely a training effect. Importantly, we also observed a significant three-way interaction between environment, task sequence, and phase, $F(1, 51) = 9.55$, $p = .003$, $\eta_p^2 = 0.16$. Finally, neither of the other two-way interactions were significant, Environment \times Phase: $F(1, 51) = 0.29$, $p = .866$, $\eta_p^2 < 0.01$; Phase \times Task Sequence: $F(1, 51) = 0.79$, $p = .378$, $\eta_p^2 = 0.02$.

Separate rmANOVAs for the learning and probe phases indicated that the main effect of task sequence was significant in the learning, $F(1, 51) = 74.00$, $p < .001$, $\eta_p^2 = 0.59$, and probe phase, $F(1, 51) = 21.60$, $p < .001$, $\eta_p^2 = 0.298$; while the environment was not in both the learning phase, $F(1, 51) = 0.06$, $p = 0.81$, $\eta_p^2 < .01$, and probe

Figure 1
Experimental Material and Procedure in Experiments 1 and 2



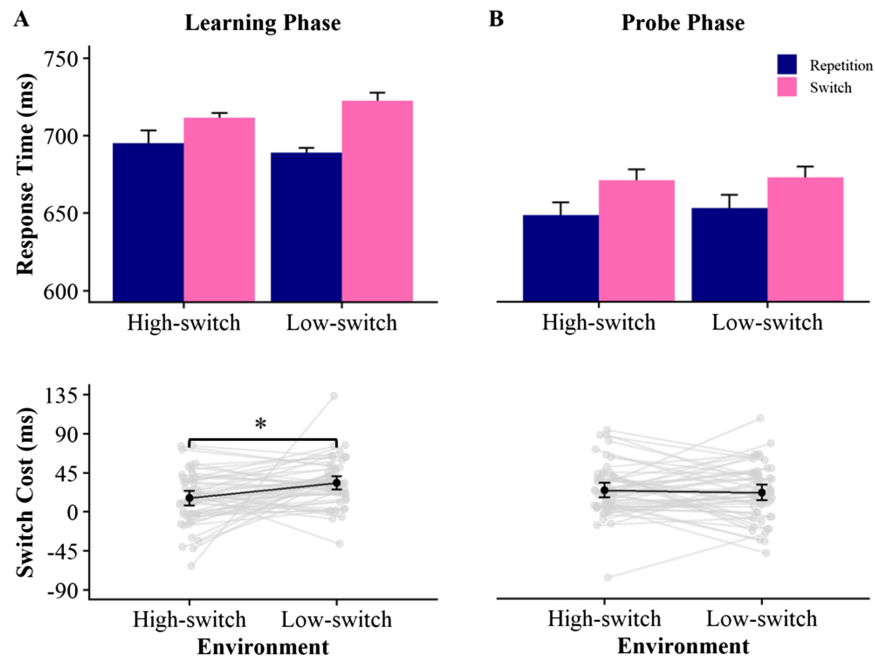
Note. (A) Two context pictures, airport security and supermarket (due to a copyright issue, we present here another two example context pictures which are close to the ones used in our experiments. Two example pictures are adapted from *Security Equipment at Gustavo Rizo Airport*, by Sarang, 2019, https://commons.wikimedia.org/wiki/File:Gustavo_Riezo_security_equipment.jpg. In the public domain; *SAS Supermarket—Interior*, by Kirakosyan, 2019 https://commons.wikimedia.org/wiki/File:SAS_Supermarket_-_interior-7.jpg. In the public domain. (B) Overview of experimental blocks and trials (within the dashed box) in Experiment 1. On each trial, after fixation, participants were informed by the task cue (the target category was cued by a red tick) about which category they need to judge, then pressed “F” or “J” to respond. High-switch and low-switch blocks were learning blocks, where participants needed to learn the task switch probabilities. In the probe blocks, the actual task switch probability was the same in both environments (background pictures were still different in two environments). Participants started the experiment with two practice blocks, where feedback was provided at the end of each trial (no feedback in experimental blocks). (C) An overview of experimental blocks in Experiment 2. First 3 days were training days and consisted only of learning blocks. Day 4 was the testing day, where both learning and probe blocks were presented. The trial setup was the same as Experiment 1, but with a response deadline of 2,000 ms. See the online article for the color version of this figure.

phase, $F(1, 51) = 0.12, p = .730, \eta_p^2 < 0.01$. Crucially, the interaction between task sequence and the environment was significant in the learning phase (Figure 2A), $F(1, 51) = 10.80, p = .002, \eta_p^2 = 0.18$, but not in the probe phase (Figure 2B), $F(1, 51) = 0.34, p = .565, \eta_p^2 = 0.01$. More specifically, in the learning phase, participants were faster on the switch trials, $t(51) = 2.97, p = .005$, Cohen’s $d_z = 0.41$, but slower on the repetition trials, marginally significant, $t(51) = 1.87, p = .067$, Cohen’s $d_z = 0.26$, in the high-switch environment. This suggests that people were more prepared to switch in the high-switch environment, despite not knowing which task they would switch to (before the task cue). However, there was no

difference in the probe blocks, $t_s < 0.77, p_s > .447$, Cohen’s $d_zs < 0.11$. No other RT effects from the main rmANOVA or post hoc tests were significant, all $F_s < 2.44, p_s > .125, \eta_p^2s < 0.05$. Results from the mixed effect model further corroborated these findings (see the online supplemental materials). Accuracy results are provided in the online supplemental materials.

Discussion

The results from Experiment 1 showed reduced RT switch costs during experiment phases with higher likelihoods of task alternations,

Figure 2*Experiment 1 Mean RT and RT Switch Costs in the Learning (A) and Probe (B) Phases*

Note. Dots in the lower panel of (A) and (B) are individual RT switch cost in each condition and paired for each participant. All error bars stand for the 95% confidence intervals. RT = reaction time. See the online article for the color version of this figure.

in line with results from several previous studies (Bonnin et al., 2011; Dreisbach & Haider, 2006; Monsell & Mizon, 2006). Notably, these previous studies on task-switching used only two tasks. Therefore, observed modulations of switch costs under different switching probabilities may actually reflect the learning of specific task sequences, rather than a generally enhanced preparation for switching. Importantly, this was virtually impossible in our five-task design. In addition, a certain level of cognitive flexibility can be also bound to a specific stimulus (e.g., Chiu & Egner, 2017) or task (e.g., Siqi-Liu & Egner, 2020). In our design, and different from most previous designs, no stimulus was ever repeated within a run, and the number of presentations of each task was balanced in each run to avoid potential confounds from stimulus- or task-related factors. Finally, we also used varying cues on each trial to avoid confounding cue repetition effects (Mayr & Kliegl, 2003; Schmidt & Liefoghe, 2016). Thus, the observed environment-specific cognitive flexibility in the learning phase provides unequivocal evidence that humans are able to adjust their cognitive flexibility depending on local task-switching probabilities. However, we could not observe a contextual difference in the switch cost in the probe phase. Therefore, there was no evidence that people also use environmental cues (i.e., supermarket or airport context information) to guide their cognitive flexibility.

Experiment 2

In Experiment 1, we observed environment-specific cognitive flexibility in the learning phase but found no such regulation in

the probe phase. Learning about such higher-level control states may be cognitively demanding (Bugg, 2014). Therefore, it is likely that humans will first need to overcome the need to explore local contingencies, even though this is discouraged by our five-task design with a new stimulus on each trial (per run). Consequently, humans may only start learning about relations between environmental features and higher-level control states, after they improve their motor skills (e.g., the training effect in the probe phase) and explore lower-level contingencies (such as between stimuli and actions, or stimuli and different tasks). This would be concordant with the idea that action regulation strategies are biased toward lower-level representations at first (Vallacher & Wegner, 1987), and that control regulation is a last resort for hierarchical learning of action and control settings (Bugg, 2014). Similarly, learning a higher-level latent structure (i.e., environment-control associations in our study) is thought to occur on a slower timescale than lower-level stimulus-action contingencies in computational models of hierarchical learning (e.g., Mathys et al., 2014). Together, these theories and models suggest that the association of task-irrelevant environmental cues and the need for cognitive flexibility may require a longer time to learn. In addition, sleeping may also play a critical role in this learning process. It is long suggested that sleeping is not only crucial for human motor skills consolidation (Walker & Stickgold, 2006) but also essential for the consolidation and retrieval of newly acquired knowledge (Tamaki et al., 2020; Wagner et al., 2004). Therefore, in Experiment 2, we decided to train participants for 4 days before

testing whether the contextual cues can trigger the need for cognitive flexibility.

Method

Participants

Our data collection strategy for Experiment 2 was highly similar to Experiment 1. But different from Experiment 1, because this was a multiday experiment, a data quality check was done after the first day's session to decide whether a participant should be allowed to the following sessions. The inclusion criteria were an overall accuracy of over 75%; and after removing the first trial of each block, fast RT (<200 ms) trials, error trials, and posterror trials, the data remaining rate should be over 50%. Participants who passed the inclusion criteria were allowed to proceed to Day 2. Among the participants who completed all sessions, the same exclusion criteria as in Experiment 1 were applied. Additionally, participants who did not start the session within 24 ± 3 hr after their previous session (i.e., they started too early or too late), were removed from analyses. We recruited 98 participants; among these participants, nine did not pass the Day 1 check, 25 did not return on either the second, third, or fourth day, five had severe missing data issues, and two did not start the experiment on time. In the valid data set, four more participants were removed due to overall low accuracy (below $Q1 - 1.5 \times IQR$), and another two participants were removed because of low remaining data percentage (below $Q1 - 1.5 \times IQR$), leaving a final sample size of 51 participants (43 females, eight males, age ranged from 18 to 26 with $M = 18.76$, $SD = 1.45$). With 51 participants, we reached our Bayesian criterion ($BF_{10} = 6.20$, calculated on the RT data on Day 4). Participants received two course credits for their participation. This study was approved by the ethical committee of the Faculty of Psychology and Educational Sciences of Ghent University.

Material and Procedure

Experiment 2 was a multiday experiment (Figure 1C), where the first 3 days were training days, including only learning blocks, and the fourth day was a testing day, including both learning and probe blocks. We used the same context pictures, tasks, and task stimuli as Experiment 1. To promote the feeling of switching, the switching probabilities fluctuated randomly in each of the two switching conditions: 80%–90% in the high-switch condition with an average of 85%, and 30%–40% in the low-switch condition with an average of 35%. The switching probability in the probe phase was still 60%. Moreover, to make the environment more salient, the background was displayed for another 3,000 ms at the beginning of each block to indicate to participants which environment they were in. We also used different font colors in the checklists of each environment, blue for the supermarket and lime green for the security. On each training day, participants completed four learning runs, each consisting of eight blocks of 30 trials, totaling 960 trials per day. On the first training day, two additional practice blocks were given prior to the main learning blocks to let participants become familiar with the general procedure. Moreover, because blocked training might be more efficient than interleaved training for learning uninstructed, environment-specific task settings (e.g., Flesch et al., 2018), we gradually interleaved the two environments over the course of the different days (see Figure 1C). Specifically, on

Day 1, each run consisted of eight blocks in only one environment, with odd versus even runs taking place in the high-switch versus low-switch environments (counterbalanced across participants). On Day 2, the first half of each run consisted of blocks in a high-/low-switch environment, and the second half in the other environment. On Day 3, the environments were interleaved quarter by quarter. Finally, on Day 4, the testing day, the environments interleaved on a block-by-block basis (as in Experiment 1). Just like the other 3 days, there were still four runs of eight blocks, but half of the blocks were probe blocks where the switching probability was 60% (i.e., 18 switch trials) which allowed for a direct comparison of the switch cost associated to, and triggered by, the two environments. We also asked participants about their feelings of environment-related task switch frequencies at the end of the experiment on the last day (see the [online supplemental materials](#)).

Data Analysis

The analysis plan was close to Experiment 1. Different from Experiment 1, we also ran rmANOVAs for RT and accuracy (see the [online supplemental materials](#)) on the first three training days consisting of the factors, environment (high-switch vs. low-switch probability), task sequence (repetition vs. switch), and day (Days 1–3). The Greenhouse–Geisser correction was applied if the assumption of sphericity was violated. Finally, similar to Experiment 1, we again ran a mixed effect model to account for individual differences, task identity, and item identity (see the [online supplemental materials](#)).

Results

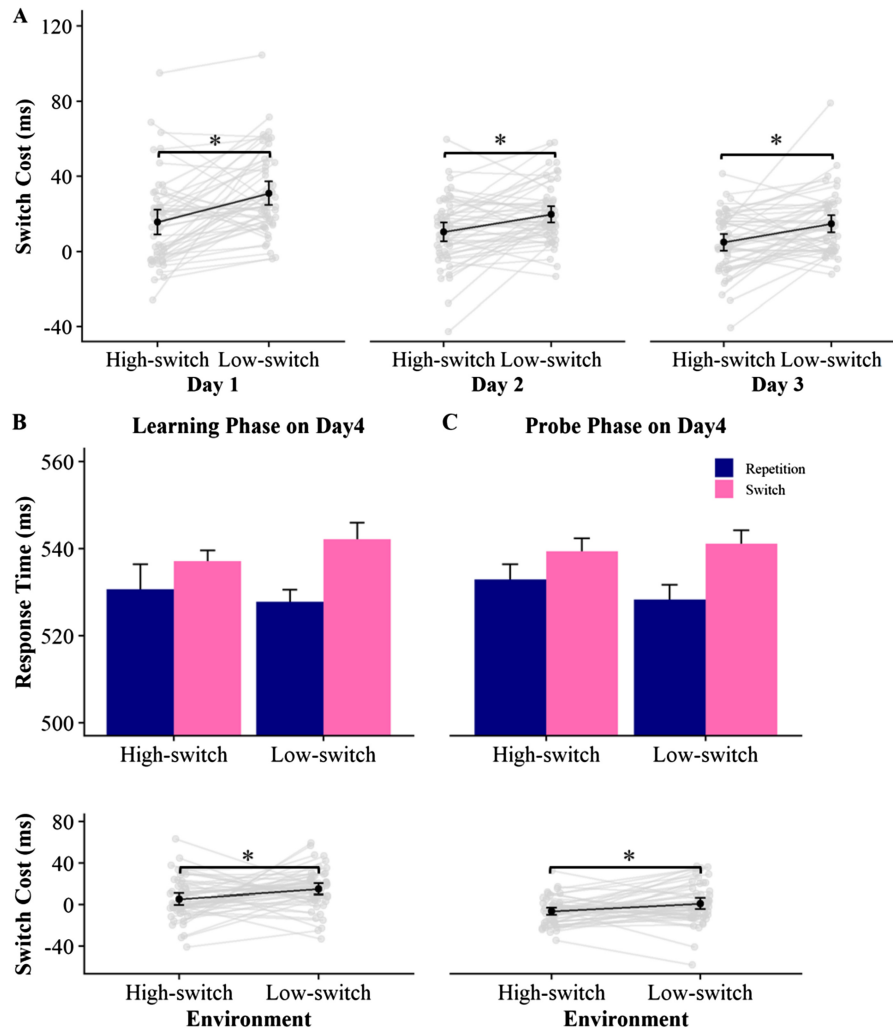
Descriptive statistics of data on training days and testing days can be found in [Tables S2 and S3 in the online supplemental materials](#). RT results can be found in [Figure 3](#).

Training Day

Across training days, participants gradually improved their task performance on RT, indicated by the main effect of day, $F(1.49, 74.33) = 205.62$, $p < .001$, $\eta_p^2 = 0.80$. The improved performance was also found on the RT switch cost (Figure 3A), indicated by the interaction between day and task sequence, $F(1.68, 83.77) = 13.88$, $p < .001$, $\eta_p^2 = 0.22$. The main effect of the task sequence, $F(1, 50) = 97.23$, $p < .001$, $\eta_p^2 = 0.66$, and the interaction between the task sequence and environment, $F(1, 50) = 68.97$, $p < .001$, $\eta_p^2 = 0.58$, showed that participants were slower on switch trials and had higher RT switch costs in the low-switch environment. These effects also reached significance when tested on each training day separately ($F_s > 13.1$, $p_s < 0.01$, $\eta_p^2_s > 0.21$). In addition, we observed a significant main effect of environment, $F(1, 50) = 4.73$, $p = .034$, $\eta_p^2 = 0.09$, indicating that RT was faster in the high-switch condition. In line with Experiment 1, participants were consistently faster on switch trials in the high- relative to the low-switch environment; Day 1, $t(50) = 1.93$, $p = .060$, Cohen's $d_z = 0.27$; Day 2, $t(50) = 4.69$, $p < .001$, Cohen's $d_z = 0.66$; Day 3, $t(50) = 4.12$, $p < .001$, Cohen's $d_z = 0.58$, but there was no significant difference on the repetition trials between the two environments, $t_s < 1.14$, $p_s > .259$, Cohen's $d_zs < 0.16$.

Figure 3

Experiment 2 RT Switch Costs Over Training Days (A) and Mean RT and RT Switch Costs in Learning (B) and Probe (C) Blocks on the Testing Day



Note. Dots in (A) and the lower panel of (B) and (C) are individual RT switch costs in each condition and paired for each participant. All error bars stand for the 95% confidence intervals. RT = reaction time. See the online article for the color version of this figure.

Testing Day

The three-way rmANOVA revealed a significant main effect of the task sequence, $F(1, 50) = 60.47, p < .001, \eta_p^2 = 0.55$, indicating that the RT switch cost persisted after three training days, in both run types, learning, $F(1, 50) = 28.70, p < .001, \eta_p^2 = 0.37$; probe, $F(1, 50) = 36.70, p < .001, \eta_p^2 = 0.42$. The main effect of environment, $F(1, 50) = 0.03, p = .864, \eta_p^2 < 0.01$, and phase, $F(1, 50) = 0.09, p = .762, \eta_p^2 < 0.01$, were not significant. The three-way interaction between environment, task sequence, and phase was not significant either, $F(1, 50) = 0.45, p = .506, \eta_p^2 < 0.01$. However, the interaction between environment and task sequence was significant, $F(1, 50) = 12.47, p < .001, \eta_p^2 = 0.20$, again suggesting a smaller switch cost in the high-switch environment. Crucially, central to

our hypothesis, this modulation was now significant in both the learning phase, $F(1, 50) = 5.95, p = .018, \eta_p^2 = 0.11$, and the probe phase, $F(1, 50) =$

$6.75, p = .012, \eta_p^2 = 0.12$ (Figure 3B and C). Interestingly, in the learning phase participants were faster on the switch trials, $t(50) = 2.30, p = .026$, Cohen's $d_z = 0.32$, but no difference was observed on the repetition trials, $t(50) = 0.89, p = .376$, Cohen's $d_z = 0.13$, in the high-switch condition. In contrast, in the probe phase, participants were slower on the repetition trials in the high-switch condition, $t(50) = 2.28, p = .027$, Cohen's $d_z = 0.32$, but there was no significant difference on the switch trials, $t(50) = 0.39, p = .696$, Cohen's $d_z = 0.05$. However, these phase-specific manifestations of the switch cost modulation should be interpreted with caution as we saw no effect of, or interaction with, the factor

phase in the overarching rmANOVA. Those effects were again corroborated in the mixed effect model after accounting for individual differences, task identity and item identity (see the [online supplemental materials](#)).

To validate our findings are independent of our exclusion criteria, we further explored the effect we found with different exclusion strategies. When not using any participant exclusion criteria ($N = 57$), the rmANOVA still yielded a significant interaction between environment and task sequence in the probe phase, $F(1, 56) = 4.96$, $p = .030$, $\eta_p^2 = 0.08$. Similarly, we also evaluated within-participant data exclusion criteria such as those used for RT outliers. When defining outliers as those falling outside three standard deviations from the mean, the interaction effect of environment and task sequence in the probe phase remained significant, $F(1, 50) = 6.00$, $p = .018$, $\eta_p^2 = 0.11$. Also when not removing RT outliers at all, $F(1, 50) = 4.23$, $p = .045$, $\eta_p^2 = 0.08$, or when using median instead of mean RT, $F(1, 50) = 9.49$, $p = .003$, $\eta_p^2 = 0.16$, the results revealed the same significant interaction of environment and task sequence in the probe phase. Together, these results show that our main observation is robust over different participants and data exclusion criteria.

Discussion

The results of Experiment 2 showed a consistent training effect on participants' task performance, with RT, accuracy, and switch costs improving across the three training days. We also observed a consistent context-specific adjustment of cognitive flexibility on all three training days, in line with the results of the learning phase of Experiment 1. Importantly, this time we also observed the same contextual effect in the probe phase, on Day 4, offering the first empirical support for the regulation of cognitive flexibility through environmental cues.

General Discussion

We aimed to investigate whether the regulation of cognitive flexibility can be achieved through environmental cues. To this end, we used a newly developed task-switching design where participants learned to associate two environments with two different switching probabilities, after which they were tested in an unannounced probe phase. Across two experiments, we found that humans are able to adjust their level of cognitive flexibility, robustly showing smaller switch costs in response to a higher switching probability. Critically, in Experiment 2, we show first evidence that, after 3 days of training, a specific level of cognitive flexibility can also be bound to, and then triggered by, the environment. We further show that this main finding was robust to different participant exclusion criteria or RT removal procedures and that trials used for analysis were not affected by our standard, preregistered trial selection criteria (see Method section).

Our findings suggest that humans can learn to associate the need for cognitive flexibility to features of a nominally task-irrelevant environment, and strategically benefit from these learned environment-control associations by triggering appropriate, environment-specific levels of cognitive flexibility. Importantly, multiday training seems to play an essential role in the formation of environment-control associations, suggesting that the learning process of higher-level control and task-irrelevant contextual features may occur on a longer timescale. Our results support recent theories of cognitive control

suggesting that humans rely on associative learning to decide their levels of cognitive control, supporting a learning-based control regulation (Abrahamse et al., 2016; Braem & Egner, 2018; Lieder et al., 2018; Otto et al., 2022).

More broadly, the here-observed learning of environment-specific cognitive flexibility also fits with hierarchical architectures of cognitive control (Badre, 2008; Badre & Nee, 2018; Fuster, 2001; Hunt & Hayden, 2017; Koechlin et al., 2003; Nee & D'Esposito, 2016). According to these theories, cognitive control is organized hierarchically, from simple stimulus-action pathways all the way up to abstract goal representations, which are arguably the most abstract action plans (Fine & Hayden, 2022). Also in these theories, contextual signals are thought to help decide which control scheme to apply. For instance, in continental Europe, the driver needs to enter a car through the left door. Instead, in the United Kingdom, people drive on the right side of the road, and therefore car designs are mirrored and car drivers sit on the right side of the car. Supporting such theories, Collins and Frank (2013) found that humans indeed tend to spontaneously infer latent task structures and decide between task strategies according to the context they were in. However, their work focused on the control of task-set selection. Our results now show that such learning can also occur at an even higher level of control, namely the act of switching between task sets.

Previous studies have argued that stimulus-control associations can be formed quickly, even with just a single exposure (Whitehead et al., 2020). In contrast, our study suggested that learning an association between nominally task-irrelevant contextual features in one's environment and cognitive flexibility settings might take multiple days of training. Crucially, previous studies only focused on stimulus-control associations involving task-relevant target stimuli. This suggests that humans are likely biased to learn from the most concrete levels of information processing first (e.g., task-relevant features), and only move to higher levels of abstraction (e.g., involving task-irrelevant features) when lower-level information cannot further help optimize behavior (Bugg, 2014; Vallacher & Wegner, 1987). Similarly, in a hierarchical control framework, the higher-order structure is formed by abstracting the lower-level features and their mutual relationships, which requires time. From these perspectives, it is not surprising that higher levels of learning occur more slowly (as also implemented in artificial intelligence models, e.g., Çatal et al., 2021). An open question is whether they occur serially, or can occur in parallel but with different learning efficacies.

Our findings, we believe, also have implications for studies of cognitive training. Here, participants are typically trained on specific cognitive control tasks to promote the training of the putatively underlying control functions, aimed at improving people's ability for cognitive control (von Bastian et al., 2022), or even general well-being (e.g., Grol et al., 2018; Koster et al., 2017). Interestingly, however, several authors have shown and argued that such forms of cognitive training rarely transfer to related tasks or other domains (e.g., Aben et al., 2019; Kassai et al., 2019; Melby-Lervåg et al., 2016; Sala & Gobet, 2019; Simons et al., 2016). Indeed, context and task similarity have been considered two critical factors for successful transfer (for a review, see Klahr & Chen, 2011). This suggests that humans probably need to be cued by a familiar context feature, to allow for transfer after cognitive training. Consistent with our findings, we believe that humans do not improve control functions as if

they were generalizable “cognitive muscles” during these training programs, but likely learn associations between the training context (e.g., a specific environment or task) and the required level of cognitive control. Consequently, when tested in another environment or task setting, the corresponding control status is no longer triggered, showing no enhanced performance.

Long-term exposure to hierarchical control problems is common in daily life (consider the car example above). Therefore, it is likely that these forms of learning also happen over multiple days in real life, with different learning speeds. Here, we showed that longer training allowed to infer and learn about higher-order task structures. According to the two-stage model of memory consolidation (Born & Wilhelm, 2012; McClelland et al., 1995), newly encoded information in the temporary store (fast-learning) is gradually, across several days, integrated into a long-term store (slow-learning) via consolidation. Sleeping has been widely proven to be essential for this memory consolidation process and subsequent processes of abstraction, inference, and insight (for reviews, see Diekelmann & Born, 2010; Rasch & Born, 2013). During the slow wave sleep period, the brain repeatedly reactivates newly encoded memory in the temporary store (i.e., the hippocampus), which in turn triggers their corresponding representations in the long-term store (i.e., the neocortex) and integrates them into preexisting long-term memories (a process called active system consolidation). This integration is further stabilized in the following rapid eye movement period. Yet, future research should determine whether reactivation and consolidation during sleep play a determinant role (e.g., Walker & Stickgold, 2006), or whether instead the total number of training trials or blocked nature of our training regime were more critical (e.g., Flesch et al., 2018).

Another open issue is the role of awareness. Intriguingly, exploratory analyses in Experiment 2 suggest that the learning effect in the probe phase was stronger when participants were aware of the environmental difference (see the online supplemental materials). However, we believe that, rather than awareness per se, it was the length of training and the experience-based learning that were critical in establishing environment-triggered cognitive flexibility. Namely, in Experiment 1, we found no relation between awareness and environment-specific flexibility (see the online supplemental materials). Second, and perhaps more importantly, we also ran another experiment (see Experiment 1B in the online supplemental materials) where we explicitly informed participants about the environment-specific task-switching probabilities in a single-day session (i.e., similar experimental settings as Experiment 1). Just like Experiment 1, the results from Experiment 1B showed context-specific switch costs in the learning phase, but no (transfer) context-specific switch costs in the probe phase.

Taken together, we believe our study brings important first evidence that people can learn environment–control associations, suggesting that the exertion of top-down cognitive control can be bound to, and then triggered by, task-irrelevant contextual features in one’s environment. These results provide important support for central assumptions in recent theories of cognitive control, open new avenues for investigating the roles of sleep and awareness in the context-specific learning of abstract control parameters, and help clarify the conditions of transfer in cognitive training.

Constraints on Generality

Our main goal was to document whether people can learn environment-specific cognitive control settings. However we cannot

make hard claims about which feature of Experiment 2’s training regime was critical in forming this abstract environment-specific knowledge. Future studies should investigate the individual roles of sleep awareness specific environmental features (e.g. background pictures or font color) and learning regime (e.g. block length and number of trials) in shaping this learning process and the generalizability to other populations. With our behavioral paradigm we aimed at minimizing the influence of arbitrary settings in a laboratory environment by using a large number of various stimuli and making our tasks closer to real-life scenarios (e.g. various stimuli realistic tasks and familiar environments). Therefore we hypothesize that our findings should generalize to other tasks and environments also believe that using these more realistic task–environment relations could turn out to be an important facilitator in learning these environment-specific control settings. Second although our study mostly consisted of relatively young adult female Belgian university students we believe our findings should generalize to all healthy adults.

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