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Conflict-based regulation of control in language production

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Abstract

Is language production dynamically regulated by cognitive control? If so, how domain-general is this process? In two experiments, we studied conflict adaptation, or conflict-driven adjustments of control, in two paradigms: Picture-Word Interference (PWI), which induces linguistic conflict, and Prime-Probe (PP), which induces visuospatial conflict. Exp. 1 tested within-task conflict adaptation separately in PWI and PP. Exp. 2 tested cross-task adaptation by alternating the two tasks in a task-switching paradigm. We found reliable within-task conflict adaptation in both PWI and PP, but neither an analysis of individual differences (Exp. 1), nor a direct manipulation of between-task conflict (Exp. 2) revealed cross-task adaptation. We further report a robust 2-back within-task adaptation in Exp. 2 to refute alternative accounts of null cross-task adaptation. These findings support models of dynamic, top-down control in language production that posit at least some degree of domain-specificity.

Keywords: language production; cognitive control; domain-generality; conflict adaptation; picture-word interference

Introduction

Much research has shed light on the nature and levels of representation in language production, but less has explored how language production is controlled. A recent theory posits that language production is monitored via mechanisms similar to those that monitor other cognitive operations (Nozari, Dell, and Schwartz, 2011). Electrophysiological and neuroimaging studies support this claim by showing a similar ERP negativity (the Error-related Negativity, ERN) as well as common cortical regions involved in monitoring of linguistic and non-linguistic tasks (e.g. Gauvin, De Baene, Brass, & Hartsuiker, 2016; Piai, Roelofs, Acheson, & Takashima, 2013; Riès, et al., 2011). More specifically, this account proposes that the amount of conflict generated between target and competing representations in the production system signals the need for increased control (which, when following an error, often manifests as error detection), and the subsequently-recruited control helps resolve this conflict (see also Yeung, Botvinick, & Cohen, 2004).

While Nozari et al.'s (2011) conflict-based account is domain-general in the sense that it proposes conflict as a signal that is monitored in both linguistic and non-linguistic systems, it does not commit to a unitary neural or cognitive system that monitors for conflict regardless of domain. In fact, the authors provided evidence from computational modeling and individuals with brain damage, showing that the consequences of conflict detection (e.g. detecting errors) are specific to the source of conflict: the amount of conflict between lexical representations (e.g. cat and dog) only predicted the ability to detect semantic errors, while the amount of conflict between phonological representations (e.g. /k/ and /d/) only predicted detection of phonological errors. Importantly, increased conflict at the lexical level did not lead to better detection of phonological errors and vice versa. This specificity arises because each layer of the production system generates conflict independently of other layers and presumably of other cognitive systems, and it is the internal dynamics of these conflict generators that determine the strength of the conflict signal. Thus, the model poses a domain-specific component to the monitoring process. This notion of domain-specificity has been supported by ERP studies showing that while detection of both linguistic and non-linguistic errors lead to ERNs, their magnitudes are not correlated between the two error types at the level of individuals (e.g. Acheson & Hagoort, 2014).

Ultimately, detection of conflict serves a purpose beyond signaling for errors—it helps regulate cognitive control to resolve future conflict and optimize task performance. This regulatory loop has been tested using the "conflict adaptation paradigm" (e.g. Weissman, Egner, Hawks, & Link, 2015). In a typical conflict adaptation paradigm, performance on the current trial is evaluated as a function of the level of conflict on the previous trial. The canonical finding is an interaction between the amount of conflict on the current and previous trials, such that performance is better on current incongruent (high conflict) trials if they are preceded by an incongruent compared to a congruent trial. For current congruent (low conflict) trials, performance is better when preceded by another low conflict rather than high conflict trial. The effect is explained in terms of online regulation of cognitive control: when facing an incongruent trial, the control system increases

engagement by biasing processing away from the distractor. Thus, the next incongruent trial will be responded to more easily. This biasing works against congruent trials, where distractors aid performance.

A conflict adaptation paradigm is ideal for testing whether a cognitive system such as language production is subject to online regulatory control via top-down processes. This was tested as the first question of the current study. More importantly, a task-switching version of the conflict adaptation paradigm has been used to test whether increased conflict in one domain helps with recruitment of control that resolves conflict in another domain (e.g. Egner, 2008; Kan et al., 2013). For example, Kan et al. (2013) interleaved trials from the color-word Stroop task with those of a task in which participants passively viewed a Necker cube (a perceptually bistable figure), which can induce visuospatial conflict. These authors showed that a high-conflict Necker cube trial improved performance on subsequent incongruent button-press Stroop, and interpreted this finding as evidence for a domain-general control system that encompasses verbal and visuospatial domains. Other studies, however, have found no evidence of cross-domain adaptation (Egner, Delano, & Hirsch, 2007; Forster & Cho, 2014; Wühr, Duthoo, & Notebaert, 2015).

Three methodological issues make reconciliation of these contradicting findings difficult. First, some of the studies employed factorial combinations of the two tasks (e.g. Stroop and Simon), where each stimulus is simultaneously congruent or incongruent with respect to each task. Participants always perform Stroop, but the position of the button could be congruent or incongruent with the location of stimulus presentation (Simon). While this design avoids a switch cost, it potentially dilutes the effect of conflict, as an incongruent Simon is paired with a congruent Stroop in a single trial. This dilution could lead to a weaker recruitment of control and thus a weaker adaptation effect (Kan et al., 2013). Second, some of these studies did not control for low-level learning and memory confounds that can obscure the adaptation effect (see Schmidt, 2013 for a review). Finally, all but one of these studies used non-verbal Stroop tasks with arbitrary response mappings, which poses additional demands on working memory. The one study that did use verbal Stroop (Wühr et al., 2015) was designed for a different purpose and had an imbalanced design (i.e. a ratio of 8:2 of task A to task B) that is known to obscure the adaptation effect (Freitas & Clark, 2015). Our design addresses these issues.

The current study was designed to answer two questions: (1) is language production subject to dynamic, top-down regulation? (2) Does conflict in language production serve to regulate performance in a non-linguistic task and vice versa? Conflict adaptation in language production was tested in the well-established Picture-Word Interference (PWI) paradigm (e.g. Schriefers, Meyer, & Levelt, 1990), which avoids arbitrary stimulus-response associations and allows for much more stimulus variability than Stroop. Conflict adaptation in a non-linguistic domain was tested using the visuospatial

Prime-Probe task (PP), which uses an optimal design to avoid low-level learning and memory confounds and employs meaningful response mappings (see Weissman et al., 2015). Exp. 1 tested whether each task in isolation showed evidence of adaptation. Finding adaptation in PWI would support models that claim language production is regulated online via top-down control.

The second question was tested in two ways. In Exp. 1, we conducted an analysis of individual differences that investigated the correlation between the size of conflict adaptation in PWI and PP tasks. A positive and reliable correlation would support a domain-general control process. This test, however, is subject to limitations of a correlational analysis (e.g. Redick et al., 2013). Thus, in Exp. 2 we directly tested whether conflict in one task regulated control in the other task, by using a task-switching adaptation paradigm. We alternated PWI and PP trials and assessed the response to conflict in one task as a function of conflict in the other. If PWI shows conflict adaptation as a function of PP conflict (and vice versa), we can conclude a domain-general process of control regulation. The absence of cross-task adaptation would support some level of domain-specificity in the control system.

Finally, Exp. 2 was designed to allow us to test conflict adaptation as a function of conflict on two trials prior (2-back; e.g. Forster & Cho, 2014). Given the alternating nature of the design, the 2-back conflict adaptation provides a second test of within-task conflict adaptation. Importantly, this test also ensured that any potential null effects in the cross-task adaptation were not due to other confounds. Collectively, the results of the two experiments inform us about whether language production is subject to dynamic regulation and whether a fully domain-general model or a partially domain-specific model (e.g. Nozari et al., 2011) is better supported by the evidence.

Experiment 1

Methods

Participants. Forty-eight native English speakers recruited from the Johns Hopkins University community (32 women; mean age = 21.2 years) participated for payment. All participants gave informed consent under a protocol approved by the Institutional Review Board of Johns Hopkins Neurology.

Materials. For the PWI task, a list of 120 target-distractor word pairs was compiled to form the incongruent PWI stimuli. Target and distractor words were semantically related (e.g. target = *bus;* distractor = *car*; as suggested by norms on Mechanical Turk), were matched in length and frequency, and had minimal phonological overlap. Then, 120 300x300 pixel black-and-white line drawings corresponding to each target word were selected from Google images. The word (the distractor for incongruent and the target for congruent stimuli) was overlaid in the center of each image

in black uppercase 36 point Helvetica, to create 120 congruent and 120 incongruent stimuli. Four experimental conditions (*cC, iC, iI, cI;* N = 60 in each) were constructed, where lowercase "*c*" and "*i*" denote the congruent and incongruent status of the *previous* trial respectively, and capital "*C*" and "*I*", the congruent and incongruent status of the *current* trial. To avoid cumulative semantic interference (e.g. Schnur, 2014) semantically-related pictures were spaced by at least 12 unrelated items. Each stimulus appeared in all four conditions across participants. Final stimuli comprised two blocks of 120 trials, prepared in four different orders to avoid systematic order effects.

For the PP task, the materials were identical to Weissman et al. (2015), and consisted of the outlines (in black, on white background) of large arrows (primes) and small arrows (probes, or targets; 75% smaller than the large arrows) pointing in the four cardinal directions. Similar to PWI, four conditions (*cC, iC, iI, cI;* N = 96 in each) were constructed, with congruent trials having primes and probes pointing in the same directions, and incongruent trials in opposite directions. Final stimuli comprised four blocks of 96 trials, prepared in four different orders to avoid systematic order effects.

Procedures. The experiment was run in E-Prime 2.0 software (Psychology Software Tools, Pittsburgh, PA). Stimuli were displayed at the center of a 15 x 12 inch Dell monitor approximately 25 inches in front of the participants. Response times (RTs) for PWI were registered using an Audio-Technica microphone connected to the E-Prime's SRBOX. Responses were also recorded digitally and transcribed offline for the identification of errors. RTs for the PP were registered using a Dell keyboard.

First, participants silently reviewed a slideshow containing labeled images of all PWI targets in the experiment. Next, they completed a 10-item practice block of the PWI paradigm, followed by the two experimental blocks in counterbalanced order. Each trial began with a centrally presented fixation for 900 ms followed by a 50 ms blank screen. Stimuli were then presented for 3000 ms or until a response was registered. The next trial started after a 50 ms blank screen.

The PP task was conducted after PWI. Following a 48-item practice block, participants completed four blocks of 96 items in counterbalanced order. PP trials consisted of the presentation of a prime of either horizontal or vertical orientation, followed by a probe oriented along the same dimension. Participants indicated the direction of the target by pressing one of the four arrows on the keyboard corresponding to the correct direction, with index and middle fingers of left and right hands.

Results

Figure 1 shows the conflict adaptation pattern in RTs and errors for PWI and PP tasks. As can be seen, the pattern of RTs and errors in both tasks is compatible with conflict

adaptation. Due to the small number of errors, statistical analyses focused on the RT data.

PWI. Erroneous responses (5%), microphone problems (2%), and trials following these errors (to avoid post-error slowing effects; 7%) were excluded from the RT analysis. RTs were log-transformed prior to analysis in order to better approximate a Gaussian distribution and outliers (<1%) were removed using QQ Plots (e.g. Schmidt & Weissman, 2015). Data were analyzed using linear mixed-effect models in R v3.2.3 with the lmerTest package (e.g. Schmidt & Weissman, 2015). Fixed-effect structures included the main effects of previous-trial congruency, current-trial congruency, and their interaction (i.e. the canonical test of conflict adaptation). For random effect grouping factors of subject and item, structures included random intercepts as well as random slopes of previous-trial congruency, current-trial congruency, and their interaction.
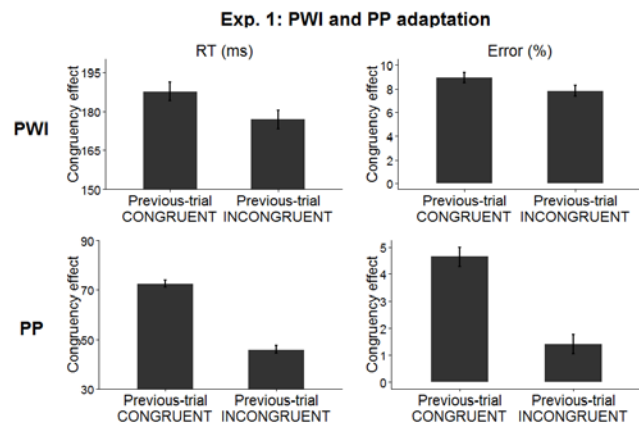

Figure 1: Conflict adaptation in RT and errors in PWI (upper panel) and PP (lower panel). Each bar reflects the subtraction of current congruent from current incongruent (*I−C;* means of participant means ± SE). Conflict adaptation predicts that this difference should be larger for the previous-congruent (left bars) than the previous-incongruent (right bars), a pattern reflected in all four diagrams.

Analysis of log-transformed RTs revealed significant conflict adaptation through an interaction between previous-trial congruency and current-trial congruency (β = −0.005, t = −2.0, p = 0.049). Post-hoc tests revealed a reliable effect on current congruent trials: RT was slower on *iC* trials (M = 760, SE = 4.34 ms) relative to *cC* (M = 743, SE = 5.21; β = 0.02, t = 4.35, p < 0.001), but *iI* (M = 937, SE = 5.9 ms) and *cI* (M = 930, SE = 4.89 ms) trials were not significantly different (β = 0.004, t = 0.53, p = 0.60).

PP. Response errors and subsequent trials each accounted for 3% of the data and were excluded. Mixed-level models were specified using procedures analogous to those used in PWI analysis. Analysis of log-transformed RT revealed a robust effect of conflict adaptation (β = −0.09, t = −9.98, p < 0.001), replicating previous results (Weissman et al., 2015). Pairwise

comparisons indicated that this adaptation effect emerged in both current congruent trials, as a slowing of reaction time in iC trials (M = 301, SE = 1.91 ms) relative to cC (M = 283, SE = 2.1 ms; β = 0.06, t = 9.27, p < 0.001) and in current incongruent trials, as a speeding of reaction time in iI trials (M = 347, SE = 2.08 ms) relative to cI (M = 356, SE = 2.04 ms; β = −0.02, t = −4.54, p = 0.01).

Cross-task Correlation of Adaptation. To probe whether the size of conflict adaptation in one task was predictable from the size of adaptation in the other task, we conducted an analysis of individual differences. Each participant's adaptation effect was calculated as (cI − cC) − (iI − iC), once for RTs and once for errors. Adaptation size in one task was not predictive of adaptation size in the other[1] (R = 0.06, p = 0.7).

Discussion

When tested in isolation, conflict adaptation was found in both PWI and PP, demonstrating that, similar to spatial tasks, language production is subject to dynamic, top-down regulation. While both congruent and incongruent trials showed the effect in PP, the effect was only reliable on the congruent trials in PWI. This pattern is not uncommon in adaptation studies (Duthoo et al., 2014; Kan et al., 2013; Weissman et al., 2015), but could also reflect a lack of power in detecting a reliable effect in post-hoc tests that use only half of the materials. To address this issue, we doubled the number of trials in Exp. 2.

Additionally, we found no correlation between the size of conflict adaptation in PWI and PP at the level of individuals. This could imply that the two tasks indeed use different regulatory mechanisms, but could also reflect the problems associated with using the correlational method. Internal consistency is known to be low for effects calculated as subtractions (e.g. Redick et al., 2013), and when internal consistency of measures is low (as was the case here), correlations between measures are unreliable. Exp. 2 addressed this problem by directly manipulating cross-task conflict adaptation by interleaving the two tasks.

Experiment 2

Methods

Participants. Thirty-two native English speakers recruited from the Johns Hopkins University community (24 women; mean age = 24.8 years) participated for payment. None had participated in Exp. 1.

Materials. The same materials as Exp. 1 were used with a minor addition: we created eight new PWI stimuli to bring the number of PWI trials to a multiple of 16 (as was

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[1] To ensure that outliers did not influence these results, we also calculated non-parametric correlations, which returned similar results to the parametric test (Spearman's rho = 0.06; P = 0.7).

necessitated by the PP design, in which four targets were balanced across four conditions). The design of Exp. 1 was changed in two ways: (1) we interleaved PWI and PP trials within each experimental block in an alternating ABAB pattern to minimize switch costs. (2) We duplicated the number of blocks, so that the same PWI target appeared in all four conditions within each participant in counter-balanced order. This change was made to increase the power to detect conflict adaptation in PWI, as well as in a potential cross-task adaptation effect. Each PWI target occurred once before each PP condition for each participant, and each PP condition occurred equally often before each PWI target for each participant. This design ensured that any item-specific effects of PWI on PP or vice versa would be balanced between our conditions of interest. This also led to a balanced design for analyzing the 2-back conflict-adaptation. The final materials consisted of four blocks, each containing 256 experimental trials prepared in four different orders to avoid systematic order effects.

Procedures. Procedures were similar to those used in Exp. 1. Following the familiarization of PWI stimuli, participants completed three practice blocks. The first was a 10-trial PWI block, the second was a 48-trial PP block, and the third was a 20-trial task-switching block. They then completed the four experimental blocks in counterbalanced order.
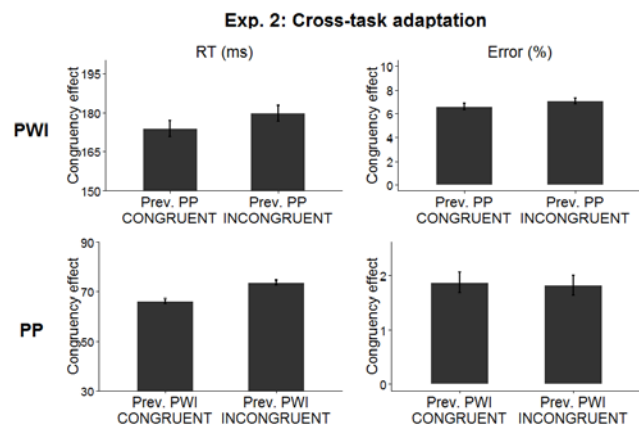

Figure 2: Performance in PWI (upper) and in PP (lower) as a function of previous task congruency. Graphs depict means of participant means for RTs and error rates (± SE). No evidence for conflict adaptation was found. PP showed a reliable anti-adaptation effect.

Results and Discussion

Figure 2 shows the results of cross-task conflict adaptation for PWI (as a function of PP conflict; upper panel) and PP (as a function of PWI; lower panel) on RTs and errors. Neither pattern is compatible with cross-task conflict adaptation. The effects on RTs were tested using similar mixed-level models

as Exp. 1 with maximally specified random-effects. Fifteen and 11% of responses were excluded from the PWI and PP respectively, due to errors, post-error status, microphone malfunction, or outlier status.

Cross-task Adaptation. In PWI, the congruency of the previous PP trial did not interact with the congruency of PWI (β = 0.006, t = 0.89, p = 0.38). In PP, the congruency of the previous PWI trial interacted significantly with the congruency of PP (β = 0.02, t = 2.8, p = 0.03), but the direction of this interaction is the opposite of what conflict adaptation predicts, that is, congruent PWI trials (compared to incongruent ones) caused slightly *longer* RTs on subsequent congruent PP trials (*cC:* M = 316, SE = 3.24; *iC:* M = 309, SE = 2.72, while incongruent PP RTs were similar regardless of previous-trial conflict (*iI:* M = 383, SE = 2.96; *cI:* M = 382, SE = 2.75).
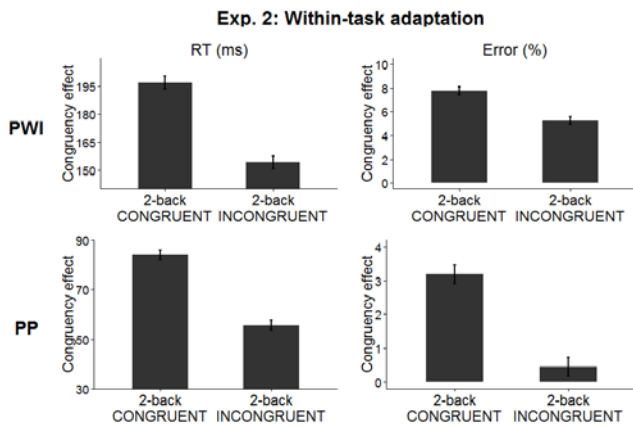

Figure 3: Performance in PWI (upper) and PP (lower) as a function of the n-2 trial congruency. Graphs depict means of participant means for RTs and error rates (± SE). Robust evidence for within-task conflict adaptation was found.

These results suggest that the ability to resolve PWI conflict was not improved by recent experience of PP conflict (and vice versa), in line with predictions of domain-specificity. But, two alternative hypotheses must first be refuted: (1) the null effect may simply reflect the lack of statistical power to detect a significant effect. (2) More theoretically-interesting, is the possibility that interference between task representations in working memory prevented sustained activation of those representations from one trial to the next (Braem, Abrahamse, Duthoo, & Notebaert, 2014). The latter could mask the operation of a domain-general regulatory loop due to excessive task demands. To address these concerns, we examined higher-order sequence effects in our data, namely adaptation as a function of conflict occurring *two* trials before. Due to the ABAB task-switch pattern, the 2-back trial would allow for assessment of within-task conflict adaptation. If robust adaptation is found in PWI and PP as a function of conflict on two trials before, we can reject these alternative hypotheses.

2-back Within-task Adaptation. Within-task adaptation was assessed with the interaction between 2-back congruency and current-trial congruency. This interaction was significant for both PWI (β = −0.01, t = −5.77, p < 0.001), and for PP (β = −0.09, t = −5.96, p < 0.001). Moreover, post-hoc tests in PWI revealed significant effects on both current congruent trials (*iC:* M = 742, SE = 4.28; *cC:* M = 722, SE = 3.96; β = 0.04, t = 5.69, p < 0.001) and current incongruent trials (*iI:* M = 896, SE = 4.68; *cI:* M = 919, SE = 4.61; β = −0.02, t = −2.41, p = 0.02). The same was true for PP: both current congruent (*iC:* M = 320, SE = 2.58; *cC:* M = 307, SE = 3.53; β = 0.05, t = 4.84, p = 0.001) and current incongruent trials (*iI:* M = 375, SE = 2.52; *cI:* M = 391, SE = 3.49; β = −0.04, t = −3.69, p = 0.02) showed a reliable adaptation effect in the predicted direction. These analyses refuted low power and limitations of working memory as alternative explanations for the absence of cross-task adaptation between PWI and PP.

General Discussion

Our first question was whether language production is regulated online through similar mechanisms that regulate non-linguistic tasks. The sequence effects observed in PWI in both experiments took the classic pattern of adaptation, demonstrating dynamic, top-down regulation of language production. While often discussed as "conflict adaptation," alternative accounts have been proposed (e.g. Lamers & Roelofs, 2011). Importantly though, all accounts agree that the pattern reflects online adjustments of top-down control based on the bottom-up requirements of the task.

We then asked whether the conflict-monitoring and control loop is shared between language production and a task that does not involve linguistic representations (Exp. 2). The most general view of conflict monitoring would predict that an increase in conflict in any domain would lead to increased control in other domains. To this end, we first replicated conflict adaptation using the spatial PP task (Weissman et al., 2015), then tested whether interleaving trials from PWI and this task would lead to cross-task conflict adaptation. We found no evidence in support of adaptation in one task as a function of conflict in the other, in either analysis of individual differences (Exp. 1) or in cross-task adaptation (Exp. 2). Demonstration of robust 2-back (within-task) adaptation in both PWI and PP allowed us to reject low statistical power and limitations of working memory as alternative explanations for the null cross-task adaptation effect. Thus, the current results convincingly refute a fully domain-general control system in which control is insensitive to the nature of conflict.

Our results are in agreement with several other studies also concluding at least some specificity in the process of control regulation (Egner et al., 2007; Forster & Cho, 2014; Wühr et al., 2015). One prominent exception is Kan et al. (2013), which found evidence for cross-task adaptation in button-press Stroop and passive viewing of the Necker cube. One difference between Kan et al.'s study and the current study is response modality (button press vs. oral), but since Stroop and PWI conflict most likely occur at a similar level (Piai,

Roelofs, & Schriefers, 2014), this difference is unlikely to have caused the discrepancy.

A more salient difference is the level of forced engagement of control in the spatial tasks. In the PP task, a speeded response is required on each trial, forcing the spatial control system to engage in all participants. But, viewing a Necker cube does not force engagement of control. Incidentally, cross-task adaptation in Kan et al. (2013) was only found in a subset of participants (N = 14) with a large number of switches between the two percepts of the cube, while no effect was found in other participants with fewer switches. It is difficult to determine what characteristic of the participants (e.g. motivation, effort, etc.) led to cross-task adaptation, but the effect was limited to a select group.

The current findings, along with prior evidence for domain-specificity in the monitoring-control system (Egner et al., 2007; Forster & Cho, 2014; Wühr et al., 2015), invite caution in interpreting the results of Kan et al. (2013) as strong support for a fully domain-general control system. Instead, our findings favor accounts that posit at least some level of domain-specificity, even when allowing for contribution of domain-general processes (e.g. Nozari et al., 2011; Gauvin et al., 2016).

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