4-18-2024

Objects are Subordinate to Spatial Features as Cues for Control

Abhishek Dey
Washington University in St. Louis

WASHINGTON UNIVERSITY IN ST. LOUIS

Department of Psychological and Brain Sciences

Dissertation Examination Committee:
Julie Bugg, Chair
Richard Abrams
Todd Braver
Jason Hassenstab
Wouter Kool

Objects are Subordinate to Spatial Features as Cues for Control

by
Abhishek Dey

A dissertation presented to
Washington University in St. Louis
in partial fulfillment of the
requirements for the degree
of Doctor of Philosophy

May 2024
St. Louis, Missouri

Table of Contents

List of Figures

List of Tables

Acknowledgments

I would like to thank my advisor, Dr. Julie Bugg, for her all her unwavering support during my time as her student. I would also like to thank my committee members, Dr. Richard Abrams, Dr. Todd Braver, Dr. Jason Hassenstab, and Dr. Wouter Kool for their advice, time, and feedback on my dissertation.

I also thank all members of the Cognitive Control and Aging Laboratory for supporting me through graduate school.

Last, but not least, I would like to thank my parents for giving me the opportunity to follow my passions.

Abhishek Dey

Washington University in St. Louis

May 2024

Dedicated to my parents.

ABSTRACT OF THE DISSERTATION

Objects are Subordinate to Spatial Features as Cues for Control

by

Abhishek Dey

Doctor of Philosophy in Psychology

Washington University in St. Louis, 2024

Professor Julie Bugg, Chair

It is well documented that people can engage in flexible adjustments of attention using context cues from the environment. The most prolific cue used to assess such context-specific attentional control in the literature has been location (or spatial features). Context-specific attentional control using object features has been less well researched and some of the most conclusive evidence of object-based attentional control comes from designs where location and spatial features cannot be used to guide attentional control. Across five experiments, I investigate the interplay between object and spatial features in their usefulness to guide attentional control when both are available as cues for control. The bulk of the evidence suggested that when spatial features could be leveraged for attentional control, object features were disregarded. In only one experiment was there weak evidence for object-based control due to an increase in the variability of object features. Overall, object features were subsumed by spatial features as guides for attentional control.

Chapter 1: Introduction

A well-established finding within the realm of cognitive control is that context can serve as a signal for the control of attentional states. That is, people can apply different attentional states (e.g., focused versus relaxed attention) in a context-specific manner. One of the more common contexts used to assess context-specific control is location (Corballis & Gratton, 2003; Crump et al. 2006; Crump & Milliken, 2009; Pickel et al. 2019, Weidler & Bugg, 2016; Weidler et al. 2018). Typically, researchers assess *location-specific control* using a selective attention task (e.g., flanker task) comprising congruent (e.g., > > > > >) and incongruent (e.g., < < > > >) trials and present mostly incongruent trials in one location (e.g., above a central fixation point) and mostly congruent trials in another location (e.g., below a central fixation point). Participants exposed to such manipulations show smaller compatibility effects (i.e., response time and error rate differences between incongruent and congruent trials) in mostly incongruent locations relative to mostly congruent locations, indicating reduced interference from the distractor. In other words, they demonstrate a *location-specific proportion congruence* (LSPC) effect.

1.1 An Event-Files Account of Control and Transfer

Prominent theories of control claim that for a given event (e.g., a trial in an experiment) a snapshot of the event is produced and stored in memory such that concrete features such as stimuli and responses as well as abstract control states are bound together (Crump & Milliken, 2009; Dignath et al., 2019; Egner, 2014; Frings et al., 2020). As it relates to location-specific control, these accounts assert that event-files are created wherein mostly incongruent and mostly congruent locations become bound together with focused and relaxed control states, respectively, because over the course of the experiment those states were used when responding to trials at

those locations. The binding of such states to locations then triggers retrieval of those attentional states when stimuli are presented in those locations in subsequent trials. These location-bound attentional states are reflexively retrieved producing LSPC effects as indicated by reduced compatibility effects for locations that host mostly incongruent trials relative to those that host mostly congruent trials (Crump & Milliken, 2009).[1]

An important question researchers tackle when investigating location-specific control is what do we mean when we say *locations* are bound in the event-file (Weidler et al., 2022)? For example, at an extremely granular level, location could mean the specific coordinate on the display screen or a specific point in the visual field. A telling finding is that participants exhibit differential attentional states not only for locations that host mostly congruent or mostly incongruent trials, termed inducer locations, but also for novel locations that host equally congruent and incongruent trials and that are adjacent to inducer locations (Pickel et al. 2019; Weidler & Bugg, 2016; Weidler et al. 2018). Inducer locations are statistically *informative* because they host different proportions of congruent and incongruent trials thereby allowing the cognitive system to (over time) expect either mostly congruent or mostly incongruent trials and adopt different attentional states. However, because *diagnostic* novel locations host trials that have an equal chance of being incongruent or congruent, by themselves they are statistically uninformative and cannot guide the control system to adopt one state or another. Despite this, *transfer* of location-specific control is evidenced in these locations because the diagnostic locations near mostly incongruent inducer locations show reduced compatibility effects relative to diagnostic locations near mostly congruent locations. The transfer of location-specific control

[1] These accounts differ from traditional event-file accounts (Hommel, 2004) which limit the scope of features that can be stored to only concrete features of the trial (i.e., perceptual or action features and not abstract attentional states).

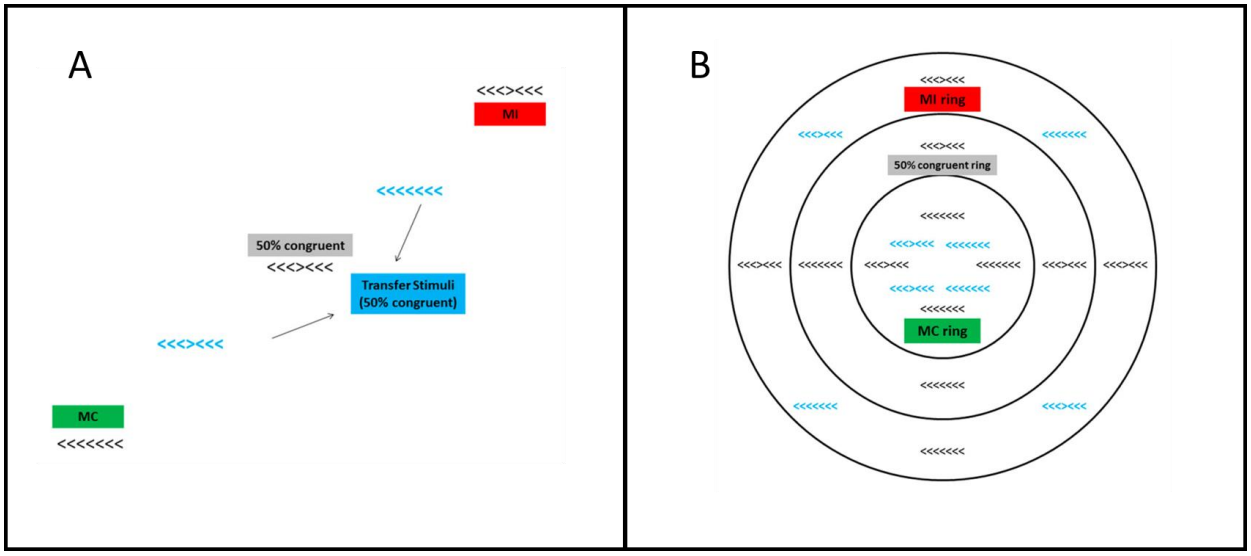
to these diagnostic locations tells us that these locations are treated as if they *are* informative and speaks against granular representations of locations in event-files because a specific coordinate representation (or another equally granular representation that would only treat inducer locations as informative) would not allow attentional states to transfer to separate sets of coordinates (see also Diede & Bugg, 2016 for evidence against extreme granularity of location representation without the use transfer locations).

The first empirical work providing evidence of the transfer of location-specific control came from two experiments in Weidler and Bugg (2016). Figure 1A and 1B, taken from Weidler and Bugg, illustrates their designs. In Experiment 1 of Weidler and Bugg arrow flanker trials were presented in five possible locations on screen along a diagonal. The locations at the endpoints of the diagonal were inducer locations and were either mostly incongruent or mostly congruent. The central location on the diagonal hosted trials that had an equal chance of being incongruent or congruent. Only these first three locations (i.e., the inducer locations at the endpoints and the central location) were initially presented in an induction phase. In a subsequent diagnostic phase, two novel diagnostic locations (color coded blue in Figure 1A) were introduced. These diagnostic locations were at the midpoints between the central location and the inducer locations. Critically, these diagnostic locations hosted trials that had an equal chance of being incongruent or congruent. The authors found that participants produced an LSPC effect for inducer locations and showed transfer of the LSPC effect to the diagnostic locations. That is, they found reduced compatibility effects (in both RTs and error rates) for the trials in diagnostic locations appearing near the mostly incongruent inducer location relative to trials in diagnostic locations appearing near the mostly congruent inducer location. This finding demonstrated the transfer of previously trained location-control associations to new adjacent locations.

Experiment 2 of Weidler and Bugg (2016) provided further evidence for the transfer of location-specific control with the added benefit of demonstrating that the representation of locations in the event-file need not be simple spatial categories such as the left or right of space but could also be non-linear in form. Specifically, the authors used a three-ring bullseye pattern (seen in Figure 1B) such that the inner and outer rings contained inducer locations which were either mostly incongruent or mostly congruent, and the ring in between contained trials that had an equal chance of being incongruent or congruent. Reproducing their findings from Experiment 1, novel diagnostic locations (also color-coded blue in Figure 1B) introduced in the diagnostic phase of the experiment produced transfer effects.

Figure 1

Experiments from Weidler and Bugg (2016)



Note. **Panel A** depicts Experiment 1 from Weidler and Bugg (2016). The inducer locations – mostly incongruent (MI) and mostly congruent (MC) were located on opposite endpoints of an invisible diagonal on the screen. A central location on the diagonal hosted flanker trials that had an equal chance of being incongruent or congruent. Subsequent a training phase, novel transfer locations (colored blue in the figure for purposes of illustration) were introduced at the midpoint between the central location and endpoints. These transfer locations also hosted trials with an equal chance of being congruent or incongruent. **Panel B** depicts Experiment 2 from Weidler and Bugg (2016). All locations were contained with a visible three-ring bullseye pattern. Inducer

locations (MI and MC) were in the inner most or outermost rings. Transfer locations are again colored blue for the purposes of illustration.

1.2 Using Objects versus Spatial Features for the Transfer of Control

While Weidler & Bugg (2016) adequately demonstrated that transfer, and consequently how location is represented in event files, precludes a coordinate-only account of control, they did not definitively answer how locations *are* represented and subsequently how transfer operates. For example, the results of Experiment 2 in Weidler and Bugg could have been the result of two potential mechanisms that support the transfer of control – an *object* mechanism based on grouping within the inner or outer ring object[2] or a *visual angle* -mechanism based on visual angle from the center of the display. To clarify, the diagnostic locations in their experiment were both within the same ring object as inducer locations *and* had the same or similar visual angle from center to inducer locations within the ring. Because they had either the same or similar visual angles, the distance participants needed to saccade to from the fixation point was either the same or similar. By contrast, we can imagine a different design where inducer and diagnostic locations share a common object but have very different visual angles from the center. This change in the design would allow for different saccade distances from fixation for inducer and diagnostic locations *and* keep them within the same object. While it can be argued that the object-based mechanism is more parsimonious, the visual angle mechanism posits that LSPC

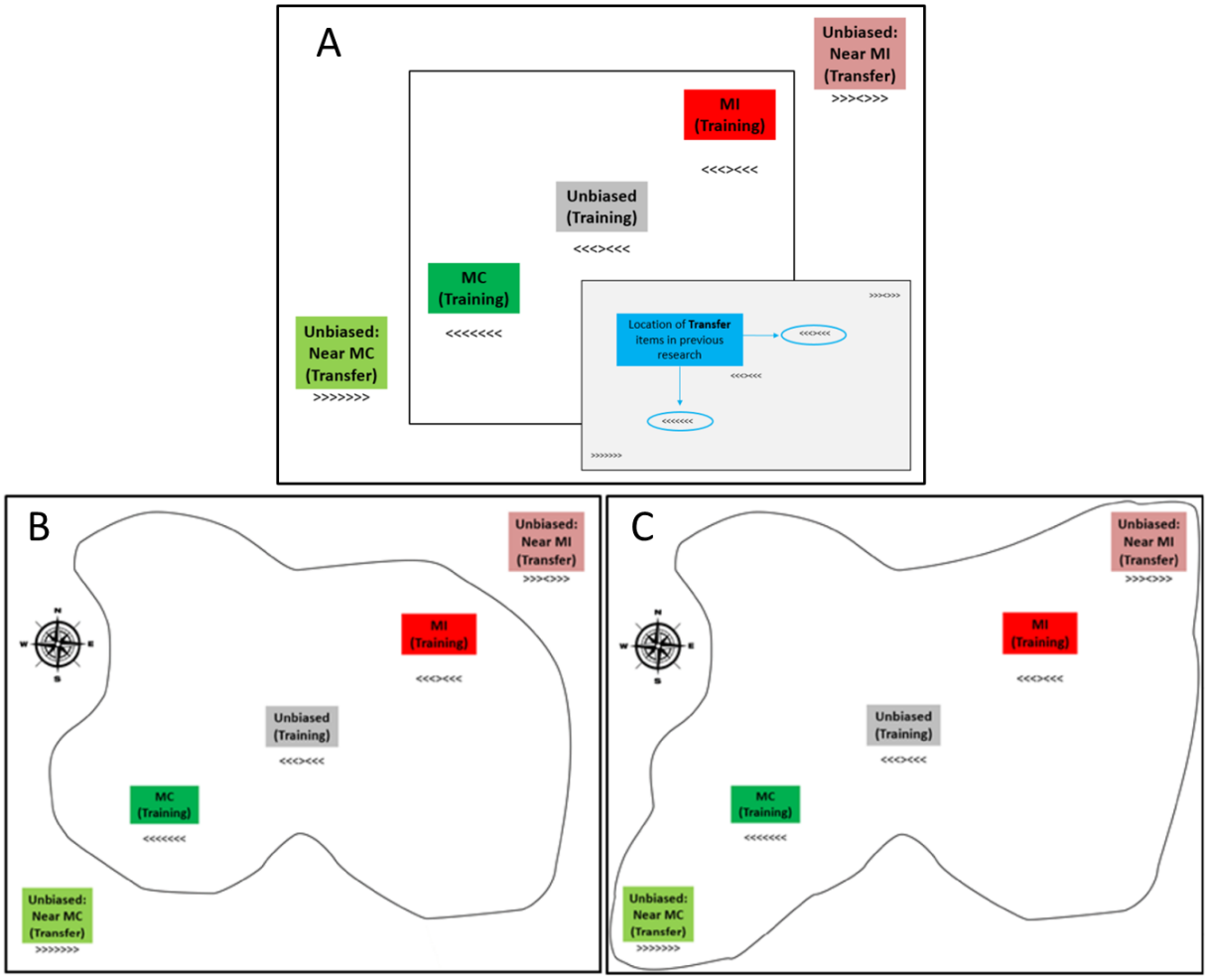
[2] In a review by Chen (2012), the author reminds researchers that defining visual objects is a difficult endeavor because it depends not only on the features of a group of stimuli, but also on task goals. As such, following their recommendation and previous research (Chen, 2012; Goldsmith, 1998; Kimchi et al. 2007), I define an object as the set of visual features that can be grouped by one or more Gestalt principles. For the purposes of this study, a key requirement for an object is that it must have a visual boundary that defines space that is within and outside the object. Without this additional requirement, proximity alone (one of the Gestalt principles) could be used to claim that the group of inducer and proximal locations forms an object.

effects transferred because diagnostic locations had same/similar visual angles from the center to one type of inducer location. To resolve this ambiguity, in the next section I detail three published studies and one unpublished study whose results provide mixed evidence for an object-based mechanism.

Weidler et al. (2018) followed up on the novel transfer findings described above and examined whether line drawn boundaries that formed an object (Experiments 2 and 3) could mitigate the transfer of attentional states to diagnostic locations. Compared to Weidler and Bugg (2016), they swapped the locations of inducer locations with the diagnostic locations such that the diagnostic locations were relocated to the endpoints of the diagonal and the mostly incongruent and mostly congruent inducer locations were relocated more towards the central portions of the diagonal. See Figure 2 for an illustration. Importantly, this allowed an object (i.e., a box in Experiment 2 and an "island" in Experiment 3) to be drawn around both types of inducer locations present during an induction phase (Figure 2A and 2B). In Experiment 2, the diagnostic phase of the experiments introduced diagnostic locations outside of the box but proximal to mostly incongruent or mostly congruent inducer locations. In Experiment 3, half of the diagnostic phase introduced diagnostic locations outside of the "island" (Figure 2B) and half of the diagnostic phase extended the boundaries of the "island" object to contain the diagnostic location (Figure 2C).

Figure 2

Experiments from Weidler et al. (2018)



Note. **Panel A** displays Experiment 1 from Weidler et al. (2018). The inducer locations – mostly incongruent (MI) and mostly congruent (MC) were located within a centrally presented visible box near its corners. A central location on the diagonal hosted flanker trials that had an equal chance of being incongruent or congruent. Subsequent a training phase, novel diagnostic locations were introduced outside the box but proximal to the inducer locations. These diagnostic locations also hosted trials with an equal chance of being congruent or incongruent. **Panel B** displays one condition from Experiment 2 from Weidler et al. (2018). The box from Experiment 1 was replaced with a line-drawn shape that participants were told represented an "island". **Panel C** displays a second condition from Experiment 2 from Weidler et al. (2018). All locations, including diagnostic locations, were contained within the "island".

These two experiments from Weidler et al. (2018) allowed for a preliminary answer to whether object-based control could be observed in designs where both an object was presented and also control could be guided by spatial features (i.e., the specific coordinate on the screen, or a category of space on the screen). The object was essentially ignored in these experiments and its presence did not mitigate transfer of LSPC effects to adjacent diagnostic locations. To be clear, an object-based control mechanism would produce no LSPC effects either for inducer or diagnostic locations because both types of inducer locations were within the object. Thus, if participants grouped such locations together in event-files due to the object features, there would be no need to vary attentional states because locations within the object hosted trials that had equal chances of being congruent or incongruent. In addition, the object did not mitigate spatial-based transfer of LSPC effects because transfer was evident even when locations were outside the object bounds. That is, not only was there no evidence of an object-based control mechanism, there was also no evidence that object-features were considered at all when transferring LSPC effects to diagnostic locations.[3]

From this experiment, one might conclude that objects are not used by the control system to guide control in selective attention tasks. And yet, ranging from early empirical work by Duncan (1984) and Egly et al. (1994) to more recent reviews by Chen (2012), object-based attentional processes (e.g., visual attention selection) have been well documented and studied for over thirty years. While this does not in and of itself imply the existence of object-based control,

[3] An interesting caveat from these experiments pertains to the error rate analyses. Typically, in such designs the error rates approach a floor effect and the critical interaction indicating the transfer of control in error rates is often absent even when it is present for response times (and the means are typically in the same direction as response time [i.e., there is no evidence of speed-accuracy tradeoffs]. However, an intriguing pattern emerged in the "island" experiment in Weidler et al. wherein the error rate analyses did indicate transfer of control, but only when the transfer locations were within the bounds of the island object. While this could in no way be classified as conclusive evidence because the critical interaction that would indicate the transfer of control for error rates did not emerge in Experiment 2, it provided a spark of hope for the object-based account. Ultimately, however, the preponderance of evidence indicated that object-based control lost.

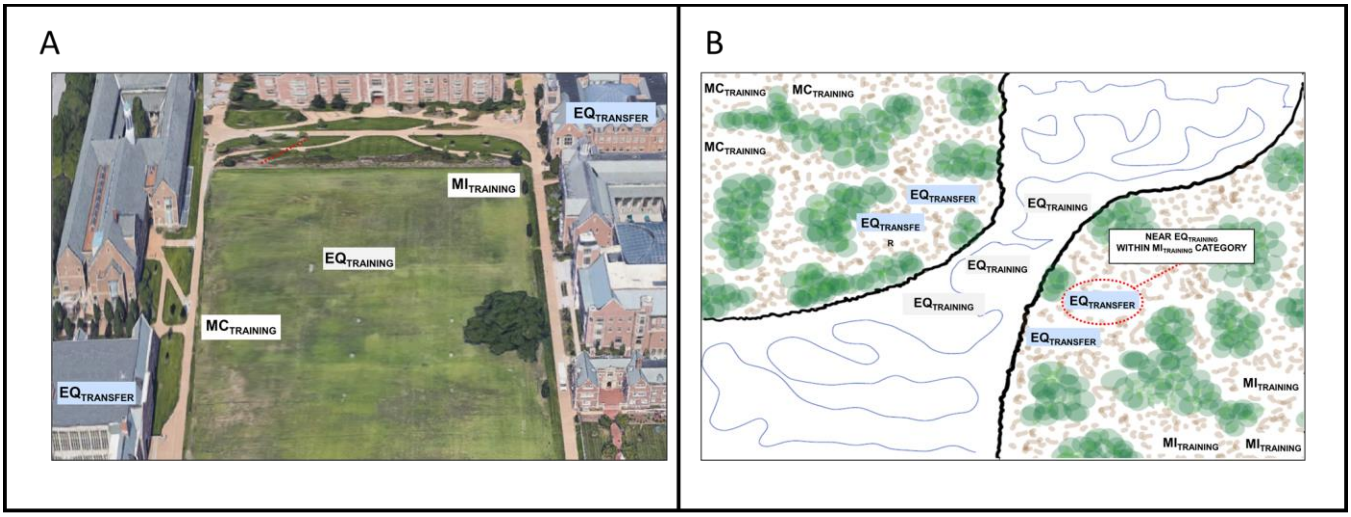
it seems unlikely that objects are inconsequential in this facet of attention. In addition, under the larger umbrella of context-specific control, many other context-features such as color (Lehle & Hübner, 2008), font (Bugg, Jacoby, & Toth, 2008), and stimulus valence (Dreisbach et al., 2019) have all been found to trigger control states. It is thus unclear why objects would be excluded as a potential trigger of context-specific control. One possibility is that because objects are typically situated in space, it is difficult to remove the influence of alternative spatially-related mechanisms like visual angle or proximity-based control when investigating object-based control and it may be that these alternative mechanisms are overpowering any potential effects of object-based control.

To demonstrate just such a possibility and to try to shore up evidence of object-based control, Colvett and Bugg (2021) ran an experiment to extend Weidler et al. (2018). Most of the key elements of their design remained the same as in Weidler et al. However, to promote object-based disruption of the transfer of the LSPC effect based on spatial features, they opted to increase the meaningfulness and salience of the displayed object(s). Illustrated in Figure 3A, they used a picture of the Washington University in St. Louis quad to denote an enclosed area that contained mostly incongruent or mostly congruent locations. Transfer locations fell outside the quad area. The participants of this study were undergraduates from Washington University in St. Louis, and so the display presumably had greater meaning to them than line drawn objects. In other words, this display was intended to accentuate object-level features to mitigate proximity-based control. Their manipulation worked and the diagnostic locations outside of the quad boundary showed no transfer effects. This result was interpreted as successfully disrupting proximity-based control.

However, while the results of Colvett and Bugg (2021) tell us that object features can disrupt proximity-based transfer of LSPC effects, it could not provide evidence of object-based control because both types of inducer locations were within the bounds of the object as was the case in Weidler et al. (2018). To try to find positive evidence of object-based control, Dey and Bugg (unpublished) ran an experiment wherein evidence for object-based control would be evident if the transfer of LSPC effects emerged because mostly congruent and mostly incongruent inducer locations were within the bounds of two *different* objects. Figure 3B provides an illustration of the design.

Figure 3

Experiments from Colvett and Bugg (2021) and an unpublished Study from Dey and Bugg



Note. **Panel A** illustrates the stimuli and design of Experiment 1 from Colvett and Bugg (2021). The inducer locations – mostly incongruent (MI) and mostly congruent (MC) were located within photographic picture of a quad at Washington University in St. Louis. A central location within the quad hosted flanker trials that had an equal chance of being incongruent or congruent. Subsequent a training phase, novel transfer locations were introduced outside the quad but proximal in screen space to the inducer locations. These transfer locations also hosted trials with an equal chance of being incongruent or congruent. **Panel B** illustrates the stimuli and design of an unpublished study by Dey and Bugg. The hand-drawn display depicted two "landmasses" separated by a body of water. The corners of the landmasses contained MI and MC locations. Central locations in the body of water contained locations that hosted trials with an equal chance

of being incongruent or congruent. Subsequent a training phase, novel transfer locations were introduced within the landmasses by proximal to the central locations in the body of water.

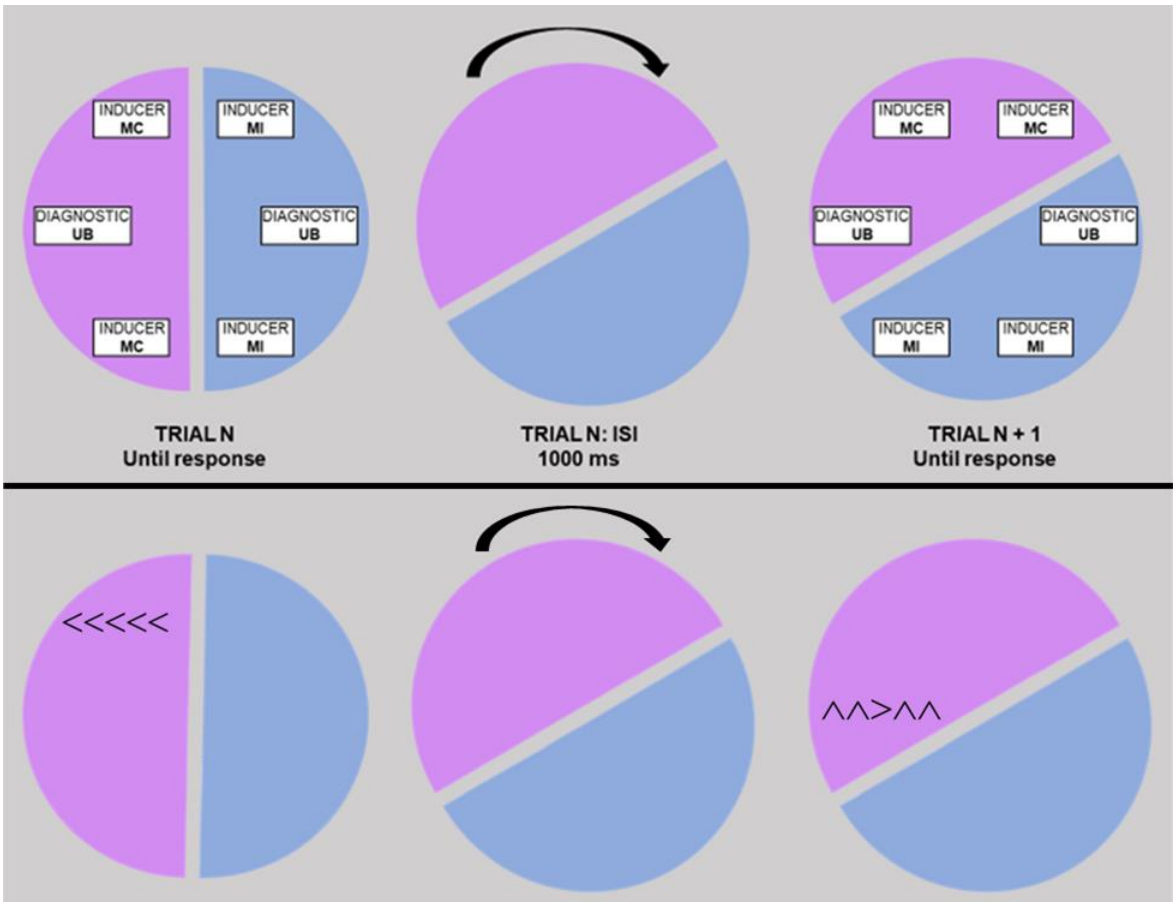
Harkening back to the design from Weidler and Bugg (2016), Dey and Bugg moved the mostly incongruent and mostly congruent locations back to the endpoints of a diagonal. Importantly, this allowed for the creation of two "landmass" objects, one of which contained mostly incongruent locations in one corner and another which contained mostly congruent locations in the opposite corner. Diagnostic locations were contained within the landmasses but were proximal to central inducer locations that hosted trials that had an equal chance of being incongruent or congruent. Consequently, the transfer of LSPC effects to diagnostic locations would support object-based control whereas the absence of transfer would support proximity-based control. Adding another blow to object-based theories of control, the critical interaction for response times failed to emerge. That is, the transfer of the LSPC effect was not observed in response times. However, paralleling Weidler et al. (2018), the error rate analysis (see Footnote 3) yielded a different conclusion. Specifically, the critical interaction for error rates did emerge and the transfer of LSPC effects was found in error rates. This once again provided a modicum of evidence for object-based mechanisms. However, historically, in the types of designs I have detailed, the presence or absence of the transfer of control has depended on response time data and error rate analyses have been less conclusive due to floor effects. As such, although intriguing, the conclusions based on error rate analyses from these studies should be taken with a grain of salt.

In a more recent study, Colvett et al. (2023) demonstrated positive evidence of object-based control with response time data. In their study, they used a moving object paradigm with two semicircular objects that rotated clockwise from trial to trial (see Figure 4). Colvett et al. had

four inducer locations, and importantly, the proportion congruence (mostly congruent vs. mostly incongruent) of these locations was determined by what semicircular object the inducer locations were superimposed on. On half the trials a given inducer location would be superimposed on one object (e.g., the purple semicircle) and for the other half of trials they would be superimposed on the other object (e.g., the blue semicircle). As such, from a coordinate-based perspective, inducer locations were equally likely to be congruent and incongruent. That is, spatial features of the event were uninformative for guiding attention.

Figure 4

Experiment from Colvett et al. (2023)



Critically, Colvett et al. (2023) employed the use of two diagnostic locations. These locations were always uninformative and were equally likely to host congruent and incongruent

trials. Thus, evidence of transfer of proportion congruence effects to these locations would establish that attention was guided based on the object and not on any spatial features whether they be granular representations like coordinates or be more disperse representations like spatial category (i.e., proximity-based). Colvett et al. found exactly this. Diagnostic locations showed transfer of proportion congruence effects conclusively demonstrating object-based control.

1.3 Current Study

Taking stock, the evidence from studies described above indicates that both spatial features and objects can be used to guide attention with one important caveat. Namely, conclusive evidence of object-based control was observed only when spatial features (i.e., coordinates or spatial proximity) were not informative for guiding attention. From the lens of event file accounts of control, it seems that objects do not become bound to attentional states when both objects and spatial features are informative and available for binding. As such, the goal of this study was to investigate if it was possible to observe object-based control in a design where both objects and spatial features could be used to guide attention.

In Experiment 1, I took inspiration from Colvett and Bugg (2021) and Colvett et al. (2023) and attempted to maximize the salience of object features in an effort to find evidence for object-based control in a design where proximity-based control is typically expected. Specifically, I used concrete (i.e., as being analogous to real-world items) and visually complex pictures of objects (Colvett and Bugg, 2021). I also used an object counting manipulation (Colvett et al., 2023; see also Brosowsky & Crump, 2021; Crump et al., 2008 for counting manipulations for other contexts) to maximize the possibility that object features would be attended and become bound to attentional states. In addition, and differing from the studies describe above, I used two sets of diagnostic locations for each inducer type – a *proximal* and a

distal *diagnostic* location. Both proximal and distal diagnostic locations were close to either mostly congruent or mostly incongruent inducer locations, but distal diagnostic locations were farther away from their respective inducer type than the proximal diagnostic locations. Thus, proximity-based control could either take a concentrated form where only proximal diagnostic locations showed transfer of LSPC effects, or it could take on a more diffuse form where both proximal and distal diagnostic locations showed transfer. Either type of proximity-based control (i.e., either concentrated or diffuse) is consistent with what prior research has referred to as a form of control guided by spatial categories (Weidler et al. 2018). Importantly, object-based control would only be evidenced if distal diagnostic locations and not proximal diagnostic locations showed transfer of LSPC effects (see Table 1).

To foreshadow, in Experiment 1, rather than object-based control, a concentrated form of proximity-based control was observed as there was transfer of LSPC effects to proximal diagnostic locations but not to distal diagnostic locations. This result pushed me to design Experiments 2 and 3 where I sought to investigate the transfer pattern of LSPC effects for both proximal and distal diagnostic locations in cases where either only object features (Experiment 2) or only spatial features (Experiment 3) were informative as means to guide attention. Finally, Experiments 4a and 4b were designed to assess if changing attributes of object features such as varying the number of object exemplars (Experiment 4a) or increasing the abstractness and reducing the visual complexity of objects (Experiment 4b) would allow for either object-based control, diffuse proximity-based control, or if they would replicate the results of Experiment 1 and show only concentrated proximity-based control.

1.4 Transparency and Openness

All experiments were approved by the Washington University in St. Louis institutional review board. These studies and the treatment of participants were in accordance with established ethical guidelines. Participants were all undergraduates from Washington University in St. Louis and as such were from a majority WEIRD (Western, educated, industrialized, rich, and democratic) sample. All raw data files and analysis scripts (R; R Core Team, 2023) are made available here: https://osf.io/m7sgn/.

Chapter 2: Experiment 1

Experiment 1 allowed participants to use either object-based or proximity-based control and sought to determine which prevailed. The design of Experiment 1 was heavily inspired by Experiment 1 from Weidler and Bugg (2016) and had four key changes. First, pictures of objects (a picture of a donut and a gear) were presented on the background accompanying the presentation of flanker trials. That is, whenever a flanker trial appeared on the screen it was always accompanied by the presentation of either a donut or a gear (see Figure 5b. for an example from one counterbalance). A second change was that participants were exposed to four (instead of two) inducer locations during both the induction and diagnostic phases. Two inducer locations were present within each object type. The rationale behind including two inducer locations instead of just one was to signal to participants that the object as a whole, and not just a particular location in the object, could be used to trigger context-specific control. The third key change was that the central location on the diagonal was removed to limit a concern that comes with having too many locations at which a flanker trial can appear. Diede and Bugg (2019) found that having 10 locations at which flanker trials could appear seemed to prevent context-specific control. A reason why many locations could impair context-specific control is that the system could become "overloaded" by having to keep track of many locations.[4] Removing the central location from the design allowed me to compensate and include additional diagnostic locations that made it possible to assess whether object- or proximity-based control was at play.

[4] This explanation is, however, less persuasive when accounting for the results of Experiment 2 in Weidler and Bugg (2016) which had 20 possible locations and found location-specific control. Still, an alleviating factor in Weidler and Bugg could be that the 20 locations appeared within a static image with distinct spatial categories (rings of a bulls eye) allowing the system to keep track of the locations more easily than if there were no such image as in Diede and Bugg (2019).

Table 1

Predicted Results and Implications for the Type of Control Observed

Experiment	Location-specific Proportion Congruence Effect			Type of Control
	Inducer Locations	Proximal Locations	Distal Locations	
Experiments 1 and 4b	Yes	No	No	Coordinate-based
	Yes	**Yes**	**No**	**Proximity-based (concentrated)**
	Yes	Yes	Yes	Proximity-based (diffuse)
	Yes	No	Yes	Object-based
Experiment 2	Yes	No	No	Object + Coordinate-based
	Yes	Yes	No	Object + Proximity (concentrated)
	Yes	**Yes**	**Yes**	**Object + Proximity (diffuse)**
	Yes	No	Yes	Object-based
Experiment 3	Yes	No	No	Coordinate-based
	Yes	**Yes**	**No**	**Proximity-based (concentrated)**
	Yes	Yes	Yes	Proximity-based (diffuse)
Experiment 4a	Yes	No	No	Coordinate-based
	Yes	Yes	No	Proximity-based (concentrated)
	Yes	Yes	Yes	Proximity-based (diffuse)
	Yes	**No**	**Yes**	**Object-based ***

Note. The pattern of results and types of control that are bolded are those that were observed for a given experiment. Proximity-based (concentrated) = proximity-based control but with limited scope (i.e., transfer is limited only to the nearest location). Proximity-based (diffuse) = proximity-based control with a wide scope (i.e., transfer spreads to both the nearest and second nearest location). For Experiment 2, Object + = control guided by a conjunction of objects and spatial features (i.e., either coordinate or proximity). * = the interpretation in Experiment 4a indicates weak evidence of object-based control.

These additional diagnostic locations were the fourth and final change from the original Weidler and Bugg (2016) design. The diagnostic locations were either proximal to the inducer locations or distal from the inducer locations. Importantly, the proximal diagnostic locations were "outside" the objects (in the center of the donut/gear hole) and the distal diagnostic locations were within the objects (on the side of donut/gear furthest from the inducer locations). These two different sets of diagnostic locations gave me the opportunity to assess whether participants used either object-based or proximity-based control during the flanker task. Table 1 outlines the various potential results and their implication for the form of control at play for Experiment 1 (in addition to Experiments 4a and 4b, which will be introduced in detail later).

2.1 Methods

2.1.1 Participants

To maintain sufficient power and in line with prior research described above, I initially intended to recruit 60 younger adult undergraduate participants from Washington University in St. Louis. While collecting data, descriptive analyses revealed that I had underestimated the effect-size of interest. To adjust for this, I ran a power analysis with simulated data and found that I needed around 80 participants to maintain a power above .90 to find an effect size that was approximately 15 ms (R script can be found here: https://osf.io/m7sgn). In total, 84 participants were collected, and two were removed due to their overall accuracy being <.70 (mean accuracy excluding these two participants was .98). As a result, data from a total of 82 participants (mean age = 19.9; men = 21, women = 60, gender not reported = 1) were collected and analyzed. For all subsequent experiments, I used 82 participants as the stopping rule to maintain consistency with Experiment 1.

2.1.2 Design and Stimuli

Figure 5a provides an illustration of the stimuli that I used for Experiment 1. On a given trial, two objects, a donut and a gear, were presented in the bottom left or top right areas of the display screen. After 1000 ms, one of the objects was removed from the screen at random and a flanker stimulus appeared on one of four locations associated with the remaining object. The flanker stimuli consisted of five arrows that could point in one of four directions – up, down, left, and right. Participants were tasked with responding to the direction of the central target arrow amidst the other four flanking arrows. Flanker stimuli (and trials) could either be congruent wherein all arrows pointed in the same direction (e.g., > > > > >) or incongruent where in the center target arrow pointed in one direction and the four flanking arrows pointed in another direction (e.g., < < > > >).

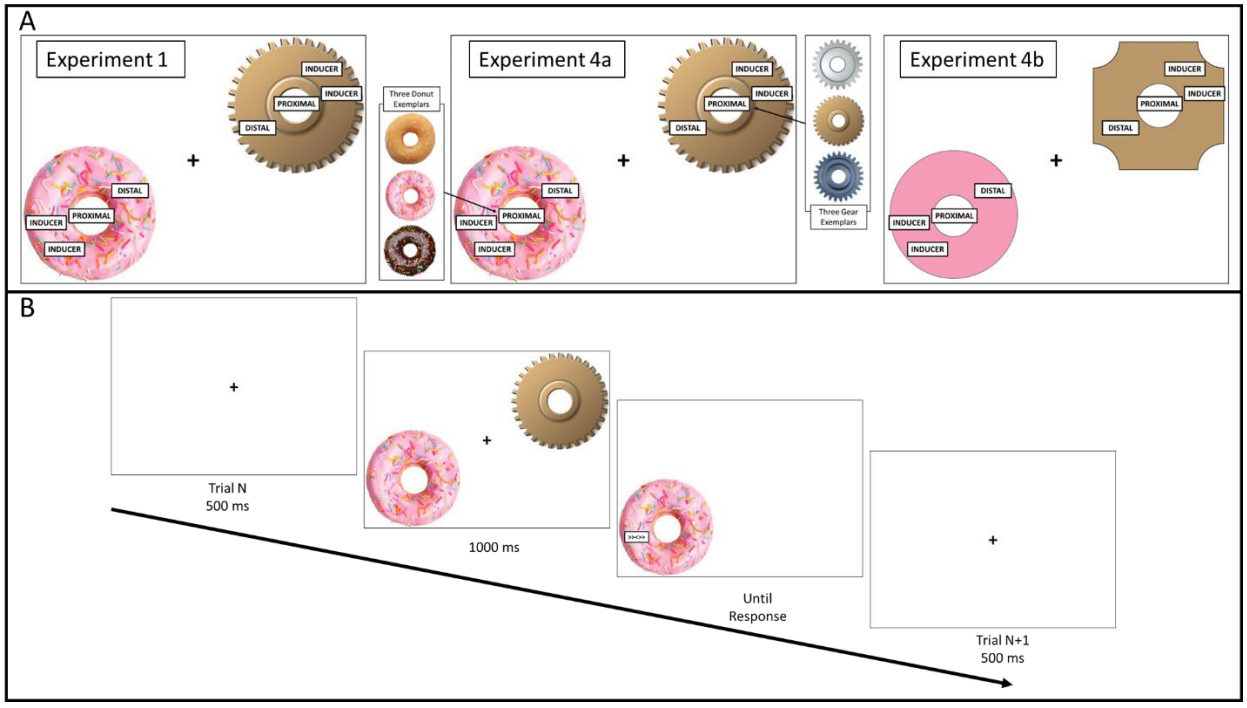
The experiment consisted of two phases. In the first phase, called the induction phase, flanker trials could only appear in the inducer locations. Flanker trials in these locations had a proportion congruence (PC) of either 88% or 12%. Inducer locations that were 88% congruent will henceforth be called *mostly congruent* locations, and those that were 12% congruent will be called *mostly incongruent* locations. In the second phase, called the diagnostic phase, flanker trials could appear in the inducer locations and in both types of diagnostic locations (proximal and distal). Flanker trials in the diagnostic locations had an equal probability of being congruent or incongruent. In other words, the flanker trials in the diagnostic location had a PC of 50% (PC50).

The inducer, proximal diagnostic, and distal diagnostic locations all had a 2 x 2 design. Inducer locations had a 2(PC: mostly congruent or mostly incongruent) x 2 (trial type: congruent or incongruent) design. Both the proximal and distal diagnostic locations had a (near PC: near

mostly congruent or near mostly incongruent) x 2 (trial type: congruent or incongruent) design.

Figure 5

Stimuli and Trial Structure of Experiments 1, 4a, and 4b



Note. **Panel A** shows the stimuli used for Experiments 1, 4a, and 4b. Inducer locations were either in the two most bottom-left locations, or in the two most top-right locations. Proximal locations were outside the object boundaries in the center hole. Distal locations were within the object boundaries but more central to the overall screen. **Panel B** shows the trial structure used for Experiments 1, 4a, and 4b.

2.1.3 Procedure

Participants were tested individually in the lab. Participants were first given a consent form to read and were instructed to only continue with the experiment if they gave informed consent. Participants then completed a brief demographic survey. Participants were instructed to respond with the direction of a central arrow amidst an array of flanker arrows. Participants used the left, right, down, and up arrow keys on a keyboard to make their response. Figure 5b illustrates the procedure at the trial level. Each trial began with a black fixation cross presented centrally for 500 ms. Following this, the donut and gear appeared on the bottom left and top right

of the screen along with the fixation cross for an additional 1000 ms. The two objects appeared prior to the flanker trials so that their presentation could not cue the location of where the flanker trial would be on that trial and to allow the appearance of the upcoming flanker stimulus to capture their visual attention so they could saccade to it directly. Next, one of the objects and the fixation cross disappeared. A flanker trial appeared in one of the four available locations associated with the object that remained on screen. For example, if the bottom right object remained on the screen, a flanker trial would appear in one of the four bottom right locations (see Figure 5b). On congruent trials, the flanker trial had all five arrows pointed in the same direction. On incongruent trials, the flanker trial had the four flanking arrows pointing in a different direction from the central arrow. The flanker trial and the object remained on the screen until a response was detected.

Participants first completed two practice blocks of trials. The first practice block asked participants to respond to flanker trials presented in the center of the screen without any associated objects on screen. The first practice block consisted of 24 trials. The second practice block was more closely aligned with the design of the main experiment. On a given trial, flanker trials were presented with accompanying objects in one of the eight potential locations (the four inducer locations and four diagnostic locations). The second practice block consisted of eight trials.

The main experiment was separated into two phases. The induction phase had flanker trials appear in one of four possible inducer locations with one of the corresponding objects. In the induction phase, flanker trials appeared 48 times in each of the four inducer locations. To reiterate, the inducer locations were either mostly congruent (i.e., PC 88) or mostly incongruent (PC 12). The induction phase also had 16 catch trials where flanker trials would appear with an

object but not in any of the inducer or diagnostic locations. These locations were further away from the objects and the purpose of including the catch trials was to dissuade participants from habituating to focusing on only the bottom left or top right of the screen during the induction phase. In total, the induction phase had 208 trials. Participants were given a short break in between this induction phase and the upcoming diagnostic phase.

In the diagnostic phase flanker trials appeared in one of eight possible locations (i.e., the four inducer and four diagnostic locations) with one of the corresponding objects. In the diagnostic phase, flanker trials appeared 48 times in each of the four inducer locations and 48 times in each of the four diagnostic locations. The diagnostic locations were all PC 50 (i.e., they had an equal chance of the flanker trials being congruent or incongruent). In total, the diagnostic phase had 384 trials.

A counting manipulation was included in the second practice block, the induction phase, and diagnostic phases of the experiment. Participants were asked to count how many times a flanker trial appeared with one of the two objects. For example, some participants were asked, "how many times did the donut appear with a flanker array?" Participants then input a number via a keyboard and were asked to reset their count back to 0. Participants were asked to answer with their counts every 24 trials.

2.2 Data Analysis

2.2.1 General Data Analysis Approach and Model Selection

Bayesian hierarchical linear models (Bayesian HLM) were used to model response times (RT) and error rates for all experiments. These models were used for two primary reasons. One, in order to leverage trial level data, I elected to forgo traditional repeated measures ANOVA and chose to use a hierarchical model approach to increase power to detect effects and to account for

subject-level variability. Two, traditional generalized hierarchical linear models using the lme4 package in R (Bates, et al. 2015) had model convergence issues. To circumvent this, Bayesian models using the brms package (Bürkner, 2017) were used instead. Bayesian HLM model outputs differ from that of traditional HLM and repeated measures ANOVA in that they do not produce *p*-values. Instead, in line with suggestions from Makowski et al. (2019) I report posterior distributions for all effects of interest (i.e., mean and HDI) along with their probability of direction (*pd*) and, for RT analyses, full-ROPE (region of practical equivalence) percentages. For those unfamiliar with how to interpret these indices, I include more detail in **Appendix A: Interpreting Bayesian Indices**. For those familiar with frequentist approaches, Makowski et al. (2019) demonstrate that *pd* is akin to and strongly correlates with *p*-values and has a direct mathematical conversion given here:

$$p - \text{value}_{two-tailed} = 2 \times (1 - pd)$$

For quick reference, a *pd* of .95, .975, and .995 would convert to *p*-values of .10, .05, and .01, respectively (Makowski et al. 2019). For RT analyses, the ROPE range for the compatibility effect was set at [-50 ms, 50 ms]. For PC effects and location-specific PC effects ROPE was set at [-5 ms, 5 ms].[5]

For RT, using data from the inducer locations in Experiment 1, I compared three different families of distributions to select the most appropriate underlying distribution – Gaussian, ex-Gaussian, and shifted-log normal. Details of those comparisons are included in **Appendix B:**

[5] ROPEs were set based on what I would consider effect magnitudes that are for all practical purposes equivalent to null results. I concede that ROPE ranges may differ based on criteria set by other researchers. I note, however, that standards suggested by Kruschke (2014) guide researchers to use ROPE ranges between -.1 and .1 standard deviations of the response variable. The primary effect of interest in this study, the LSPC effect, is a difference between compatibility effects. As such, the response variable to base the ROPE range on would be the compatibility effect. Of note, the range of -5 ms to 5 ms is a stricter criterion (i.e., a wider range) than setting the range between -.1 and 1 standard deviations of the compatibility effect for all experiments reported here. In addition, having a fixed ROPE range that does not vary between experiments allows for easier interpretation of the differences in the magnitude of effects between experiments.

Model Comparison for Inducer Locations in Experiment 1. The model that assumed an ex-Gaussian distribution fit the data best as indicated by visual inspection of posterior-predictive checks and quantifiable ELPD differences (an index of Bayesian model comparison; see Appendix B for more detail). To maintain consistency, for all subsequent analyses of RT, I used models which assumed an ex-Gaussian distribution. For error rates, a Bayesian logistic HLM was used to model data with a Bernoulli distribution and a logit link function.

All Bayesian HLM analyses included proportion congruence (either MC-MI for inducer locations, or near MC – near MI for diagnostic locations), stimulus type (Congruent – Incongruent), and their interaction (PC x stimulus type) as fixed effects. In addition, PC, stimulus type, and the PC x stimulus type interaction were also included as random effects.

2.2.2 Data Pre-Processing and Trimming

Catch trials were discarded prior to all analyses. The data were then trimmed according to previous cutoffs used for flanker tasks (see Weidler & Bugg, 2016; Weidler et al. 2018). Only trials with RT greater than 200 ms and less than 2000 ms were included. For RT analyses, only accurate trials were included.

Location-specific proportion congruence effects[6] were analyzed separately for RT and error rates for each location type (i.e., inducer, proximal diagnostic, and distal diagnostic). RT differences between mostly congruent and mostly incongruent locations are the preeminent measure of location-specific proportion congruence effects in designs such as the experiments reported here because error rates run into floor effects (which was true for all experiments reported here as well). As such, my interpretations mainly rest on the set of RT analyses

[6] The use of term location-specific here is appropriate because I assessed control for inducer and both types of diagnostic locations separately. As per Table 1, the overall pattern indicating in which locations control is evident would then speak to the type of control observed (i.e., coordinate, proximity, or object).

reported. For completeness, model estimated error rates for all experiments are given in Table 2. I used the brmsmargins package in R in order to back-transform the log-odd estimates from the models to probability values listed in Table 2 (Wiley & Hedeker, 2022). More detailed error rate analyses are included in **Appendix C: Error Rate Analyses**.

2.2.3 RT Analyses

Inducer Locations. A robust flanker compatibility effect (Incongruent – Congruent) was observed, μ = 119 ms, $HDI_{95\%}$ = [112 ms, 127 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (833ms) than incongruent trials (952ms). There was no effect of PC (MI – MC), μ < 1 ms, $HDI_{95\%}$ = [-17 ms, 19 ms], $pd_{positive}$ = .513, full-ROPE = 41.45%. Most importantly, there was a robust location-specific PC effect ($MI_{\text{compatibility effect}} - MC_{\text{compatibility effect}}$), μ = -35 ms, $HDI_{95\%}$ = [-45 ms, -26 ms], $pd_{negative}$ > .999, full-ROPE <.02%, indicating that the compatibility effect for MI locations (101 ms) was attenuated compared to MC locations (136 ms). See Figure 6 top row. For interested readers, I include Table 3 which shows the model estimated RTs for each condition (PC and stimulus type) for Experiment 1 (and all other experiments in this study).

Table 2

Model Estimated Error Rates

	MC (or Near MC)			MI (or Near MI)			
	Congruent	Incongruent	CE	Congruent	Incongruent	CE	LSPC *pd*
Experiment 1							
Inducer	.006 (.001)	.040 (.006)	.034	.002 (.001)	.027 (.003)	.025	.916
Proximal Diagnostic	.006 (.002)	.030 (.006)	.024	.004 (.002)	.021 (.004)	.017	.502
Distal Diagnostic	.006 (.002)	.031 (.006)	.025	.004 (.002)	.017 (.004)	.013	.762
Experiment 2							
Inducer	.004 (.001)	.029 (.006)	.025	.003 (.001)	.025 (.003)	.022	.812
Proximal Diagnostic	.006 (.002)	.030 (.005)	.024	.005 (.002)	.017 (.004)	.012	.894
Distal Diagnostic	.009 (.002)	.042 (.009)	.033	.007 (.002)	.033 (.007)	.026	.551
Experiment 3							
Inducer	.009 (.001)	.040 (.006)	.031	.006 (.002)	.031 (.003)	.025	.614
Proximal Diagnostic	.008 (.003)	.048 (.006)	.040	.009 (.003)	.034 (.006)	.025	.860
Distal Diagnostic	.008 (.002)	.043 (.007)	.035	.005 (.002)	.034 (.007)	.029	.666
Experiment 4a							
Inducer	.004 (.001)	.034 (.006)	.030	.003 (.001)	.016 (.002)	.013	.838
Proximal Diagnostic	.006 (.002)	.035 (.006)	.029	.003 (.001)	.030 (.005)	.027	.520
Distal Diagnostic	.007 (.002)	.022 (.005)	.015	.004 (.002)	.016 (.004)	.012	.717
Experiment 4b							
Inducer	.005 (.001)	.040 (.006)	.035	.003 (.001)	.024 (.003)	.021	.518
Proximal Diagnostic	.004 (.002)	.031 (.006)	.027	.003 (.001)	.014 (.001)	.011	.909
Distal Diagnostic	.003 (.001)	.023 (.005)	.023	.002 (.001)	.020 (.005)	.018	.752

Note. Mean errors and standard errors of the means (in parentheses) were computed from back-transforming log-odd estimates from Bayesian hierarchical logistic models. Compatibility effects = CE, were computed by taking the difference (Incongruent – Congruent) of these back-transformed model estimates. LSPC *pd* (an index of the existence of a location-specific PC

effect; see Appendix A) for each experiment and location type was computed directly from their respective log-odd model estimates. In all experiments reported here, no error rate LSPC effects were observed (i.e., there were no LSPC *pds* > .975).

Table 3

Model Estimated RT in Milliseconds

	MC (or Near MC)			MI (or Near MI)			
	Congruent	Incongruent	CE	Congruent	Incongruent	CE	LSPC *pd*
Experiment 1							
Inducer	824 (9)	960 (10)	136	842 (9)	943 (10)	101	>*.999**
Proximal Diagnostic	783 (9)	911 (10)	128	795 (9)	902 (10)	107	>*.999**
Distal Diagnostic	769 (8)	885 (9)	116	778 (8)	887 (9)	109	.885
Experiment 2							
Inducer	728 (8)	839 (9)	111	742 (9)	837 (10)	95	>*.999**
Proximal Diagnostic	701 (8)	788 (8)	87	716 (9)	789 (9)	73	*.996**
Distal Diagnostic	755 (8)	884 (8)	129	773 (9)	889 (9)	116	*.991**
Experiment 3							
Inducer	755 (7)	869 (8)	114	761 (7)	858 (7)	97	>*.999**
Proximal Diagnostic	715 (7)	824 (8)	109	721 (7)	821 (8)	100	*.979**
Distal Diagnostic	695 (7)	815 (7)	120	698 (7)	810 (7)	112	.962
Experiment 4a							
Inducer	833 (11)	965 (12)	132	842 (11)	945 (11)	103	>*.999**
Proximal Diagnostic	787 (11)	902 (12)	115	791 (11)	897 (12)	106	.953
Distal Diagnostic	783 (11)	895 (11)	112	791 (10)	891 (11)	100	*.989**
Experiment 4b							
Inducer	807 (7)	951 (8)	144	820 (9)	926 (9)	106	>*.999**
Proximal Diagnostic	773 (7)	897 (8)	124	778 (8)	884 (9)	106	>*.999**
Distal Diagnostic	753 (7)	869 (8)	116	760 (8)	870 (8)	110	.888

Note. LSPC *pd* >.975 indicates that the LSPC effect for that location likely exists (see Table 5 in Appendix A for more detailed interpretations of *pd*).

Diagnostic Proximal Locations (Outside Object). A robust flanker compatibility effect (Incongruent – Congruent) was observed, μ = 118 ms, $HDI_{95\%}$ = [110 ms, 125 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (789 ms) than incongruent trials (907 ms). There was no observable effect of near PC (near MI – near MC), μ = 1 ms, $HDI_{95\%}$ = [-18 ms, 20 ms], $pd_{positive}$ = .529, full-ROPE = 38.57%. Importantly, there was a location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -21 ms, $HDI_{95\%}$ = [-34 ms, -10 ms], $pd_{negative}$ > .999, full-ROPE = 0.33%, indicating that the compatibility effect for near MI locations (107 ms) was attenuated compared to near MC locations (128 ms). See Figure 6 middle row.

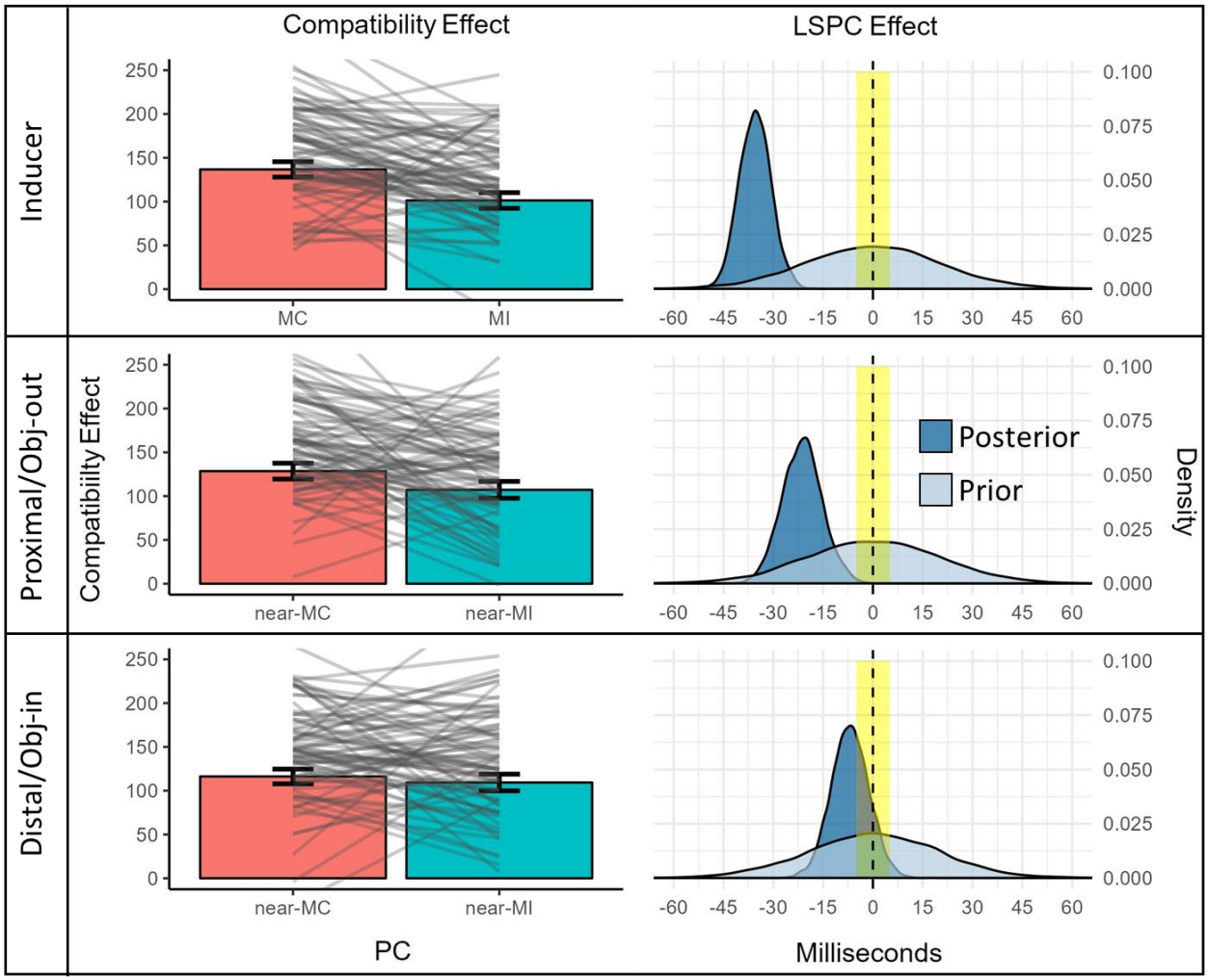
Diagnostic Distal Locations (In Object). A robust flanker compatibility effect (Incongruent – Congruent) was observed, μ = 113 ms, $HDI_{95\%}$ = [106 ms, 120 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (773 ms) than incongruent trials (886 ms). There was no observable effect of near PC (near MI – near MC), μ = 5 ms, $HDI_{95\%}$ = [9 ms, 21 ms], $pd_{positive}$ = .746, full-ROPE = 40.55%. Importantly, there was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -7 ms, $HDI_{95\%}$ = [-18 ms, 4 ms], $pd_{negative}$ = .885, full-ROPE = 34.67%.

Three-way Interaction for Diagnostic Locations. An additional model on all diagnostic locations was run investigating the three-way interaction of the LSPC effect by diagnostic location type (i.e., near PC x stimulus type x diagnostic location type). For brevity, I report just the three-way interaction results here (and not the other subordinate two-way interactions and main effects). The three-way interaction revealed a likely difference in the LSPC effect between

proximal diagnostic and distal diagnostic locations ($LSPC_{Proximal} - LSPC_{Distal}$), μ = -16 ms, $HDI_{95\%}$ = [-32 ms, 0 ms], $pd_{negative}$ = .981, full-ROPE = 7.52%.

Figure 6

Compatibility and Location-specific Proportion Congruence Effects for Experiment 1



Note. **Left panel** shows model estimated compatibility effects (ms) for inducer, proximal diagnostic, and distal diagnostic locations. Grey lines show individual participants' compatibility effects calculated through simple mean differences. **Right panel** shows the location-specific proportion congruence effect for inducer, proximal diagnostic, and distal diagnostic locations. The dark-blue distribution is the posterior distribution, i.e., the model estimated LSPC effect given the data. The light-blue distribution is the weakly informative prior distribution. The yellow region indicates the ROPE (region of practical equivalency) which is used to calculate

full-ROPE percentages. For the LSPC effect, the ROPE range was set at [-5 ms, 5 ms] (see footnote 5).

2.3 Discussion

The main takeaway from Experiment 1 was that an LSPC effect was observed for the MC and MI inducer locations (PC88 and PC12, respectively), and transfer of the LSPC effect was found for the proximal diagnostic locations (PC50) but not the distal diagnostic location (also PC50). Referring to Table 1, this would indicate concentrated proximity-based control was observed and not object-based control. The pattern of results aligns with prior research by Weidler et al. (2018). Recall that in Weidler et al. (2018) transfer of LSPC effects from inducer locations to diagnostic locations occurred despite the inclusion of background objects that led to clear boundaries between inducer and diagnostic locations. In other words, objects features were ignored and did not disrupt proximity-based transfer of LSPC effects. While the transfer effects in Experiment 1 of this study did not spread out to distal diagnostic locations, the transfer effects found in proximal locations speak to the notion that boundaries provided by objects do not disrupt proximity-based control when spatial features are informative. This result does not, however, align with Colvett and Bugg (2021) in which more meaningful boundaries from visually complex and concrete objects (i.e., those that have real-world analogous objects) disrupted proximity-based control.

The results of Experiment 1 raised two important questions. The first question is, can object features be used as a guide for control at all with the current set of stimuli (i.e., visually complex pictures of a donut and a gear)? That is, while object features were ignored when spatial features were informative, would object features continue to be ignored if spatial features were uninformative? If it is the case that these objects are simply ignored no matter the statistical

informativeness of spatial features, then when only objects are informative and not spatial features, we ought to see no LSPC effects in inducer locations and no transfer of LSPC effects to diagnostic locations. By contrast, when locations alone cannot be relied on to guide control, participants may default to using objects features to guide control producing LSPC effects for inducer and diagnostic locations as was observed in Colvett et al. (2023). The second question is, are objects limiting the spread of proximity-based control to only the proximal locations and would the removal of objects allow transfer of LSPC effects to distal diagnostic locations indicating more diffuse proximity-based control? That is, if objects were removed from Experiment 1, would distal diagnostic locations show LSPC effects? Or would their removal have no effect and transfer would still only be observed in proximal locations. Experiments 2 and 3 were designed to investigate the above outlined questions, respectively.

Chapter 3: Experiment 2

As noted above, the goal of Experiment 2 was to determine if in the absence of informativeness of locations, object-based control could emerge. As such, for Experiment 2, I changed the design such that instead of having objects appear in either the bottom-left or top-right of the screen, they only appeared in the center of the screen. See Figure 7 for an illustration. This meant that there were only four locations where flanker trials could appear. Three locations were within the objects and one location was outside the objects in the center hole of the objects. Two of the locations within the objects were inducer locations and the third was a distal diagnostic location. The location in the center hole of the objects was a proximal diagnostic location. Importantly, the inducer locations were either mostly congruent or mostly incongruent when paired with a specific object. For example, for one counterbalance, the inducer locations were PC88 when presented with a donut and PC12 when presented with a gear. In this way, a strict proximity-based account of control, ignoring the object, would yield no observable effect in either the inducer or diagnostic locations because averaging across both objects the inducer locations would be PC50 (recall that diagnostic locations are always PC50). The inducer locations would only be informative when paired with a specific object. For differences in attentional control to emerge, participants would need to leverage the informativeness of the object that is presented with the flanker trials. Table 1 outlines the various potential results of Experiment 2 and their implication for the form of control at play.

3.1 Methods

3.1.1 Participants

Eighty-four undergraduate participants from Washington University in St. Louis were collected. Two were removed due to their overall accuracy being <.70 (mean accuracy excluding these two participants was .97). As a result, data from a total of 82 participants (mean age = 19.8; men = 20, women = 62, non-binary = 0) were used for the analyses below.

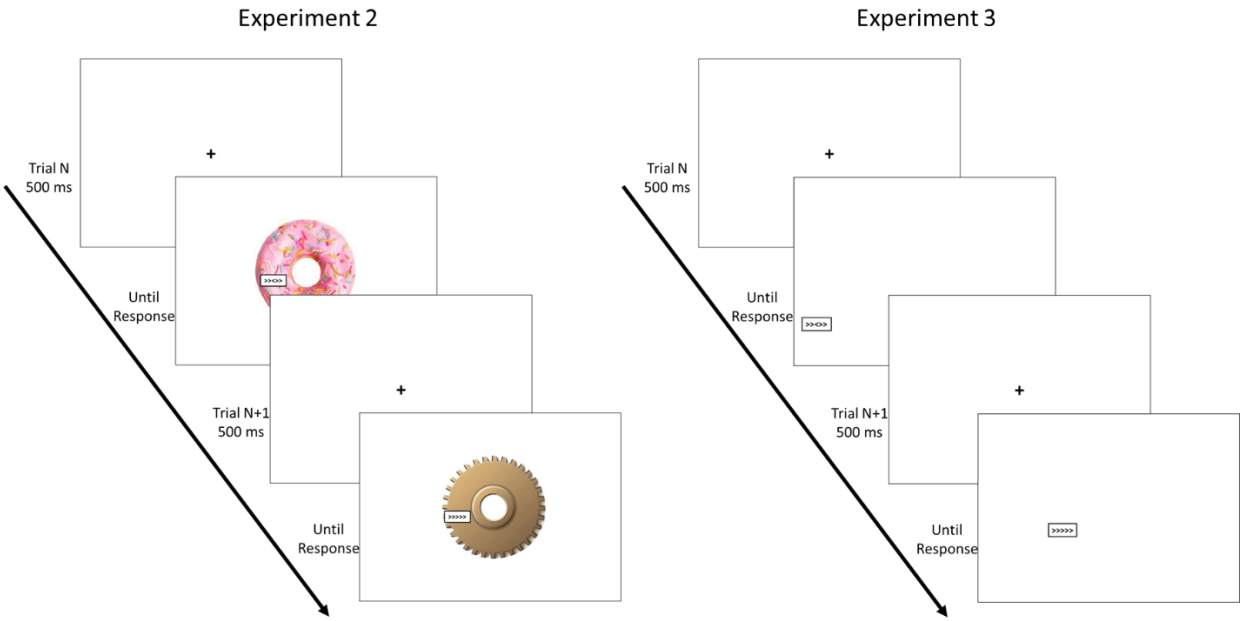
3.1.2 Design and Stimuli

Figure 7 (left panel) provides an illustration of the stimuli that I used for Experiment 2. For the main experimental task, one of two objects, either a donut or a gear, was presented in center of the display screen accompanied by a flanker trial in one of four locations. All other stimuli and design elements were the same as in Experiment 1.

As a result, mimicking Experiment 1, the inducer, proximal diagnostic, and distal diagnostic locations all had a 2 x 2 design. Inducer locations had a 2(PC: mostly congruent or mostly incongruent) x 2 (trial type: congruent or incongruent) design. Both the proximal and distal diagnostic locations had a (near PC: near mostly congruent or near mostly incongruent) x 2 (trial type: congruent or incongruent) design.

Figure 7

Trial Structure for Experiments 2 and 3



Note. **Left panel** shows the trial structure of Experiment 2. The objects were moved to the center of the screen and flanker stimuli were presented in only four locations. For one counterbalance, the inducer locations were in the bottom-left of the object boundaries. Proximal locations were outside the object boundaries in the center hole. Distal locations were in the object boundaries but on the opposite diagonal of the inducer locations. In another counterbalance the inducer locations were in the top-right of the object boundaries and the distal locations were appropriately relocated to the opposite diagonal of the object. **Right panel** shows the trial structure used for Experiment 3. No objects were presented in this experiment.

3.1.3 Procedure

The procedure was the same as in Experiment 1 with the following exceptions. On a given trial, a fixation cross was presented for 500 ms and subsequent to this an object appeared centrally on the screen accompanied by a flanker trial in one of four locations (see Figure 7). The next trial did not begin until a response was detected.

Participants first completed the same two practice blocks of trials as in Experiment 1. The second practice block consisted of eight trials. On each trial in the the second practice block objects now appeared in the center of the screen as opposed to the bottom-right and top-left of

the screen following the main design change of Experiment 2. As such, on a given trial, flanker trials were presented with accompanying objects in one of four (instead of eight) potential locations. In addition, participants were asked to count how many times a flanker trial appeared with either a donut or a gear. As was the case in Experiment 1, the counting manipulation was continued for the main experiment.

The main experiment was separated into two phases. The induction phase had flanker trials appear in two possible inducer locations for each object. In the induction phase, flanker trials appeared 48 times in the two inducer locations for each object. The inducer locations were either mostly congruent (i.e., PC 88) or mostly incongruent (PC 12) only when presented with a specific object. Participants were given a short break in between this induction phase and the upcoming diagnostic phase.

In the diagnostic phase flanker trials appeared in any of the four possible locations (i.e., the two inducer and two diagnostic locations) for each object. In the diagnostic phase, flanker trials appeared 48 times in each of the two inducer locations and 48 times in each of the two diagnostic locations for each object. The diagnostic locations were PC50. All other elements of the procedure not detailed above remained the same (i.e., trial counts, the counting manipulation, etc.).

3.2 Data Analysis

All elements of data cleaning and analysis were kept constant with the data pipeline from Experiment 1.

3.2.1 RT Analyses

Inducer Locations. A robust flanker compatibility effect (Incongruent – Congruent) was observed, μ = 103 ms, $HDI_{95\%}$ = [97 ms, 109 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating

congruent trials were responded to faster (735 ms) than incongruent trials (838 ms). There was no effect of PC (MI – MC), μ = 6 ms, $HDI_{95\%}$ = [-3 ms, 15 ms], $pd_{positive}$ = .897, full-ROPE = 41.35%. As in Experiment 1, there was a location-specific PC effect ($MI_{compatibility\ effect}$ – $MC_{compatibility\ effect}$) , μ = -16 ms, $HDI_{95\%}$ = [-24 ms, -8 ms], $pd_{negative}$ > .999, full-ROPE <.02%, indicating that the compatibility effect for MI locations (95 ms) was attenuated compared to MC locations (111 ms). See Figure 8 top row.

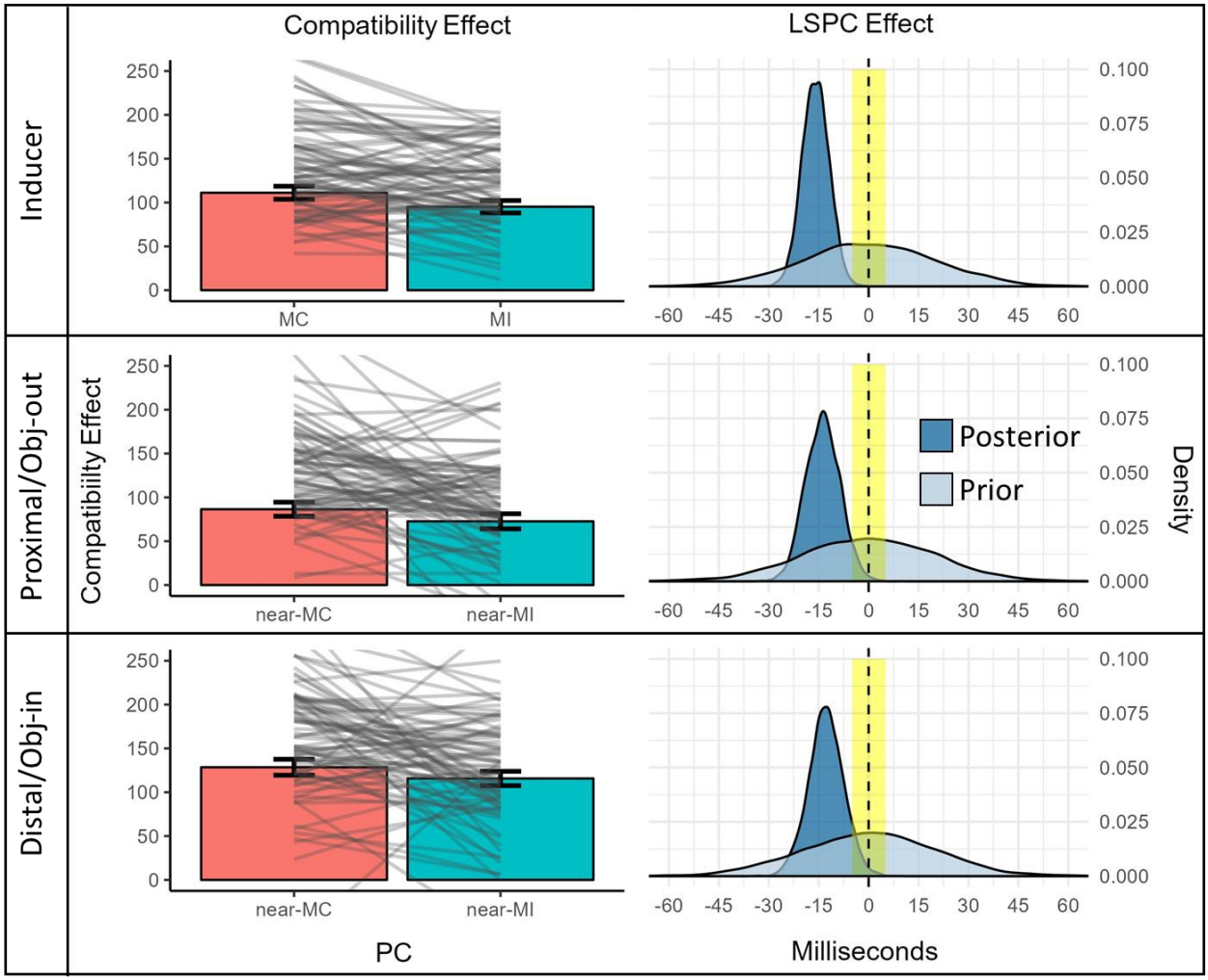
Diagnostic Proximal Locations (Outside Object). A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 80 ms, $HDI_{95\%}$ = [73 ms, 86 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (708 ms) than incongruent trials (788 ms). There was a possible effect of near PC (near MI – near MC), μ = 7 ms, $HDI_{95\%}$ = [-1 ms, 15 ms], $pd_{positive}$ = .960, full-ROPE = 29.43%. Most importantly, there was a location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -14 ms, $HDI_{95\%}$ = [-24 ms, -4 ms], $pd_{negative}$ = .996, full-ROPE = 4.68%, indicating that the compatibility effect for MI locations (73 ms) was attenuated compared to MC locations (87 ms). See Figure 8 middle row.

Diagnostic Distal Locations (In Object). A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 122 ms, $HDI_{95\%}$ = [115 ms, 129 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (765 ms) than incongruent trials (887 ms). There was an effect of near PC (MI – MC), μ = 11 ms, $HDI_{95\%}$ = [3ms, 19 ms], $pd_{positive}$ = .995, full-ROPE = 7.70%, indicating that flanker trials in MC locations (818 ms) were responded to faster than flanker trials in MI locations (829 ms). Importantly, contrary to Experiment 1, there was a location-specific PC effect ($MI_{compatibility\ effect}$ – $MC_{compatibility\ effect}$), μ = -13 ms, $HDI_{95\%}$ = [-23 ms, -2 ms], $pd_{negative}$ = .991, full-ROPE = 6.80%, indicating that the compatibility effect for

MI locations (116 ms) was attenuated compared to MC locations (129 ms). See Figure 8 bottom row.

Figure 8

Compatibility and Location-specific Proportion Congruence Effects for Experiment 2



Note. **Left panels** show model estimated compatibility effects (ms) for inducer, proximal diagnostic, and distal diagnostic locations. Grey lines show individual participants' compatibility effects calculated through simple mean differences. **Right panels** show the location-specific proportion congruence effect for inducer, proximal diagnostic, and distal diagnostic locations. The dark-blue distribution is the posterior distribution, i.e., the model estimated LSPC effect given the data. The light-blue distribution is the weakly informative prior distribution. The yellow region indicates the ROPE (region of practical equivalency) which is used to calculate full-ROPE percentages. For the LSPC effect, the ROPE range was set at [-5 ms, 5 ms].

Three-way Interaction for Diagnostic Locations. The three-way interaction revealed no difference in the LSPC effect between proximal diagnostic and distal diagnostic locations ($LSPC_{Proximal} - LSPC_{Distal}$), μ = -5ms, $HDI_{95\%}$ = [-17 ms, 8 ms], $pd_{negative}$ = .772, full-ROPE = 46.00%.

3.3 Discussion

Experiment 2 found LSPC effects for inducer locations and showed transfer of LSPC effects to both proximal and distal diagnostic locations. By themselves, all location types (i.e., inducer, proximal diagnostic, and distal diagnostic) were PC50 and thus uninformative as guides to control. Only when paired with specific objects were inducer locations either MC or MI. That participants showed LSPC effects for inducer locations reflects that they used the conjunction of objects and location types to guide control. In addition, the transfer of LSPC effects to both proximal and distal locations established that the objects were used in conjunction with spatial features as guides for control. In other words, Experiment 2 provided evidence of diffuse proximity-based control that leveraged object features. While it might be tempting to say that a more parsimonious interpretation is that it was simply object-based control at play, these results cannot be interpreted as such because we observed transfer of the LSPC effect to proximal locations that were outside a given object's form and boundary.

Chapter 4: Experiment 3

A remaining question from Experiment 1 is, did the objects disrupt the use of diffuse proximity-based control? Or was concentrated proximity-based control the only form of spatial control applicable in Experiment 1? To clarify this, Experiment 3 sought to establish whether diffuse proximity-based control would emerge once objects were removed from the design. The removal of objects of course meant that they could no longer be leveraged to induce object-based control. But more importantly, neither could they disrupt diffuse proximity-based control. Sans objects, if the results indicate participants still only use concentrated proximity-based control, I could conclude that objects had no effect on the type of control used in Experiment 1. On the other hand, if the results of Experiment 3 indicate that proximity-based control was used, I could conclude that the inclusion of objects in Experiment 1 disrupted diffuse proximity-based control.

4.1 Methods

4.1.1 Participants

Eighty-two undergraduate participants from Washington University in St. Louis were collected (mean age = 19.1; men = 22, women = 60).

4.1.2 Design and Stimuli

All design and stimuli elements were the same as in Experiment 1 except that objects were no longer presented in conjunction with the flanker stimuli.

As was the case in Experiment 1 and 2, inducer, proximal diagnostic, and distal diagnostic locations all had a 2 x 2 design. Inducer locations had a 2(PC: mostly congruent or mostly incongruent) x 2 (trial type: congruent or incongruent) design. Both the proximal and

distal diagnostic locations had a (near PC: near mostly congruent or near mostly incongruent) x 2 (trial type: congruent or incongruent) design.

4.1.3 Procedure

The procedure was the same as in Experiment 1 with the following exceptions. After a 500 ms fixation cross presentation, flanker stimuli appeared sans an object in one of eight locations (or 1 of 10 locations in the induction phase when including catch trials). See Figure 7 (right panel) for an illustration of the trial structure.

Before the experimental phase, participants completed two practice blocks of trials as in Experiment 1. The first practice block was the same as in Experiment 1. In lieu of practicing the object counting manipulation, the second practice block acclimatized participants to the main experimental procedure by presenting flanker arrays on a trial-by-trial basis in all inducer and diagnostic locations. The arrays were presented in each of the eight locations two times leading to a total of 16 practice trials.

Just as in all previous experiments, the main experiment was separated into two phases. The induction phase had flanker trials appear in four possible inducer locations and two other locations that were used for catch trials. In the induction phase, flanker trials appeared 48 times in each of the four inducer locations. Two of the inducer locations were mostly congruent and the other two were mostly incongruent (e.g., the bottom left inducer locations were PC 88 and the top right inducer locations were PC 12). In total, the induction phase consisted of 208 trials (16 of which were catch trials). Participants were afforded a short break in between the induction phase and the diagnostic phase.

In the diagnostic phase flanker trials appeared in any of the eight possible locations. In the diagnostic phase, flanker trials again appeared 48 times in each of the four inducer locations

and 48 times in each of the four diagnostic locations. The diagnostic locations were PC50. In total, the diagnostic phase consisted of 384 trials.

4.2 Data Analysis

All elements of data pre-processing and analysis were kept constant with the data pipeline used in Experiment 1.

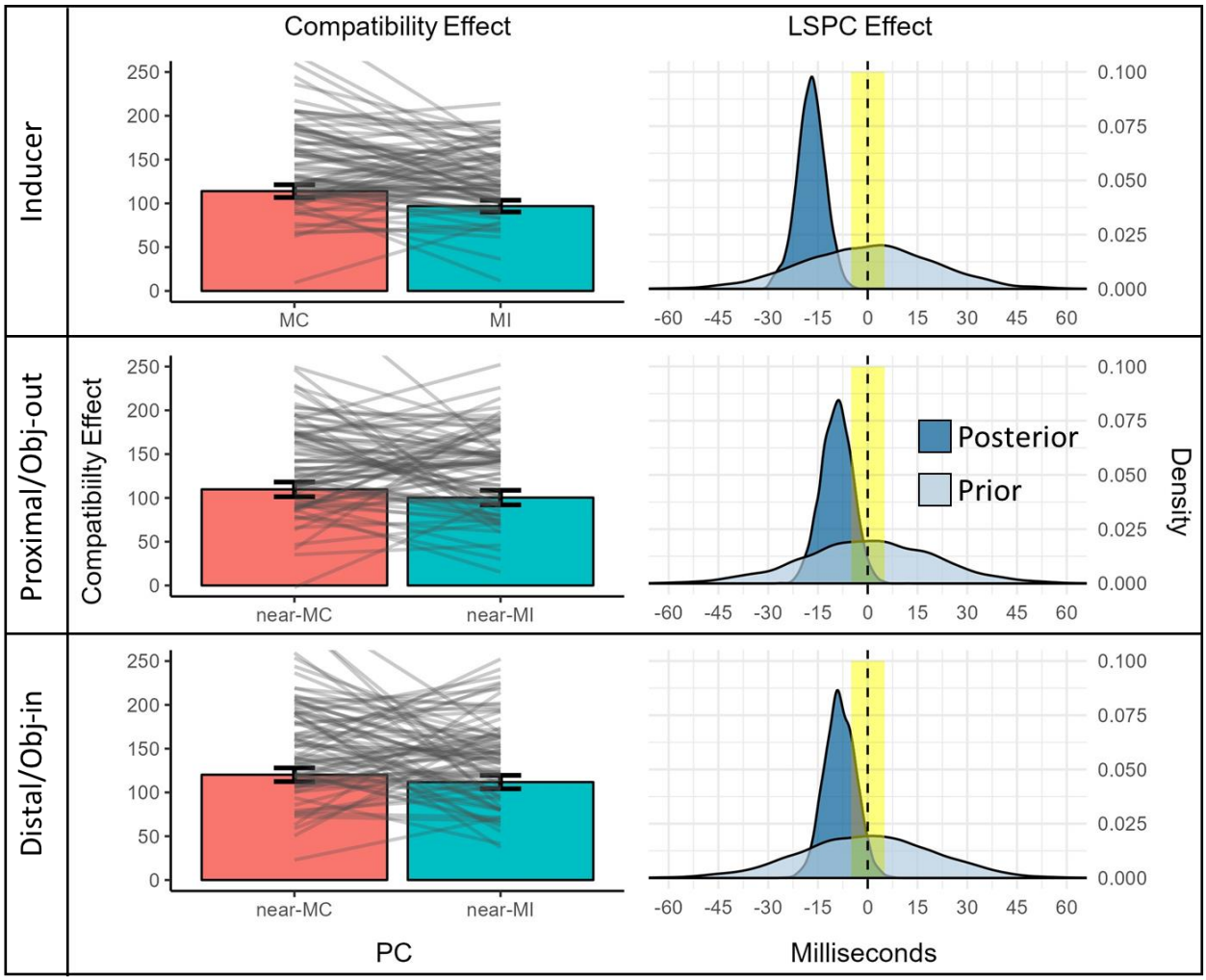
4.2.1 RT Analyses

Inducer Locations. A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 105 ms, $HDI_{95\%}$ = [100 ms, 111 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (758 ms) than incongruent trials (863 ms). There was no observable effect of PC (MI – MC), μ = -2 ms, $HDI_{95\%}$ = [-9 ms, 5 ms], $pd_{negative}$ = .706, full-ROPE = 78.55%. Most importantly, there was a robust location-specific PC effect ($MI_{compatibility\ effect}$ – $MC_{compatibility\ effect}$), μ = -17 ms, $HDI_{95\%}$ = [-25 ms, -8 ms], $pd_{negative}$ > .999, full-ROPE <.02%, indicating that the compatibility effect for MI locations (97 ms) was attenuated compared to MC locations (114 ms). See Figure 9 top row.

Diagnostic Proximal Locations. A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 105 ms, $HDI_{95\%}$ = [98 ms, 112 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (718 ms) than incongruent trials (823 ms). There was no effect of near PC (near MI – near MC), μ = 1 ms, $HDI_{95\%}$ = [-5 ms, 8 ms], $pd_{positive}$ = .649, full-ROPE = 84.55%. Most importantly, aligning with Experiment 1 and Experiment 2 there was a small but likely location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -9 ms, $HDI_{95\%}$ = [-19 ms, -1 ms], $pd_{negative}$ = .979, full-ROPE = 17.48%. See Figure 9 middle row.

Figure 9

Compatibility and Location-specific Proportion Congruence Effects for Experiment 3



Note. **Left panels** show model estimated compatibility effects (ms) for inducer, proximal diagnostic, and distal diagnostic locations. Grey lines show individual participants' compatibility effects calculated through simple mean differences. **Right panels** show the location-specific proportion congruence effect for inducer, proximal diagnostic, and distal diagnostic locations. The dark-blue distribution is the posterior distribution, i.e., the model estimated LSPC effect given the data. The light-blue distribution is the weakly informative prior distribution. The yellow region indicates the ROPE (region of practical equivalency) which is used to calculate full-ROPE percentages. For the LSPC effect, the ROPE range was set at [-5 ms, 5 ms].

Diagnostic Distal Locations. A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 116 ms, $HDI_{95\%}$ = [110 ms, 123 ms], $pd_{positive}$ > .999, full-ROPE <.02%,

indicating congruent trials were responded to faster (696 ms) than incongruent trials (812 ms). There was no effect of near PC (near MI – near MC), μ = -1 ms, $HDI_{95\%}$ = [-7 ms, 5 ms], $pd_{positive}$ = .513, full-ROPE = 89.32%. In addition, there was a possible small location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = -8 ms, $HDI_{95\%}$ = [-17 ms, 1 ms], $pd_{negative}$ = .962, full-ROPE = 23.93%. See Figure 9 bottom row.

Three-way Interaction for Diagnostic Locations. The three-way interaction revealed no difference in the LSPC effect between proximal diagnostic and distal diagnostic locations ($LSPC_{Proximal}$ – $LSPC_{Distal}$), μ = -1 ms, $HDI_{95\%}$ = [-14 ms, 12 ms], $pd_{negative}$ = .540, full-ROPE = 56.72%.

4.3 Discussion

Experiment 3 showed reliable LSPC effects for inducer locations and a small but likely effect of transfer of the LSPC effect for proximal diagnostic locations. In addition, a possible effect was observed in distal diagnostic locations. Taken together, the pattern of results suggests participants used proximity-based control but whether this was in its concentrated form or diffuse form was inconclusive. A conservative interpretation would lead to the conclusion concentrated proximity-based control was indicated both in Experiment 1 and 3 and that objects were essentially ignored in Experiment 1.

Experiments 2 and 3 established that when only object features are informative, or when only spatial features are informative, transfer of LSPC effects emerge based on either a conjunction of object and spatial features or based only on spatial features. Importantly, when spatial features are informative, the objects were ignored as a means to guide control, and when spatial features were uninformative, the object features were used as conditional cues to allow

proximity-based control. To investigate how enduring the ineffectiveness of objects is on the use of proximity-based control I manipulated different attributes of the objects used in Experiment 1 to see if either object-based control or diffuse proximity-based control could be observed. In Experiment 4a I increased the variability of objects by increasing the number of exemplars for each object type to try and find a way to tip-the-scales in favor of object-based control. In Experiment 4b I used more abstract and less visually complex objects to see if changing the nature of objects would allow diffuse proximity-based control to emerge.

Chapter 5: Experiment 4a

Experiment 4a sought to explore the influence of object feature variability on the type of control used in Experiment 1. Specifically, I sought to find out if increasing the variability of objects promoted object-based control in lieu of concentrated proximity-based control found in Experiment 1. Prior work has demonstrated that increasing the variability on a given feature dimension increases binding of that feature to event files that contain other stimuli/response/control features of that experiment (George and Egner, 2022). As it relates to the event file theories of control, the logic would be that increasing the number of donuts or gears presented would promote attention toward object features. In turn, this would promote binding of those features to control states. In other words, increasing the variability of object features might more easily allow participants to use object-based control. However, another possibility is that increasing the variability of object features could overburden the cognitive system and reduce attention to object features. That is, rather than treating the different donuts or gears as part of one type of object (i.e., category of object), each donut or gear exemplar could be treated as a separate object exemplar. This may force the cognitive system to rely more on spatial features for two reasons. One, each object exemplar would be less informative compared to if it was treated as part of an object category because there would be fewer pairings between object features from a single exemplar and inducer locations as compared to pairings between object features from a category of object and inducer locations. Two, the cognitive system may have trouble keeping track of six unique objects and shy away from using object features in event files to guide attention. As a result, Experiment 4a could result in a similar pattern found in both Experiment 1 and Experiment 3 wherein proximity-based control was observed.

5.1 Methods

5.1.1 Participants

Data were collected from eighty-three undergraduate participants from Washington University in St. Louis. One participant was removed due to their overall accuracy being <.70 (mean accuracy excluding this participant was .98). As a result, data from a total of 82 participants (mean age = 19; men = 20, women = 61, non-binary = 1) were used for the analyses below.

5.1.2 Design and Stimuli

All elements of the design and stimuli were the same as in Experiment 1 except that there were three exemplars of each object type (i.e., donut and gear) as opposed to just one (see Figure 5a).

The inducer, proximal diagnostic, and distal diagnostic locations all had a 2 x 2 design. Inducer locations had a 2(PC: mostly congruent or mostly incongruent) x 2 (trial type: congruent or incongruent) design. Both the proximal and distal diagnostic locations had a (near PC: near mostly congruent or near mostly incongruent) x 2 (trial type: congruent or incongruent) design.

5.1.3 Procedure

The procedure was identical to that of Experiment 1 with the exception that on a given trial and for each object type there were three possible exemplars presented with a flanker array.

5.2 Data Analysis

All elements of data cleaning and analysis were kept constant with the data pipeline from Experiment 1.

5.2.1 RT Analyses

Inducer Locations. A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 118 ms, $HDI_{95\%}$ = [111 ms, 124 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (838 ms) than incongruent trials (956 ms). There was no effect of PC (MI – MC), μ = -5 ms, $HDI_{95\%}$ = [-26 ms, 13 ms], $pd_{negative}$ = .697, full-ROPE = 35.20%. Importantly, there was a robust location-specific PC effect ($MI_{\text{compatibility effect}}$ – $MC_{\text{compatibility effect}}$), μ = -29 ms, $HDI_{95\%}$ = [-38 ms, -20 ms], $pd_{negative}$ > .999, full-ROPE <.02%, indicating that the compatibility effect for MI locations (104 ms) was attenuated compared to MC locations (133 ms). See Figure 10 top row.

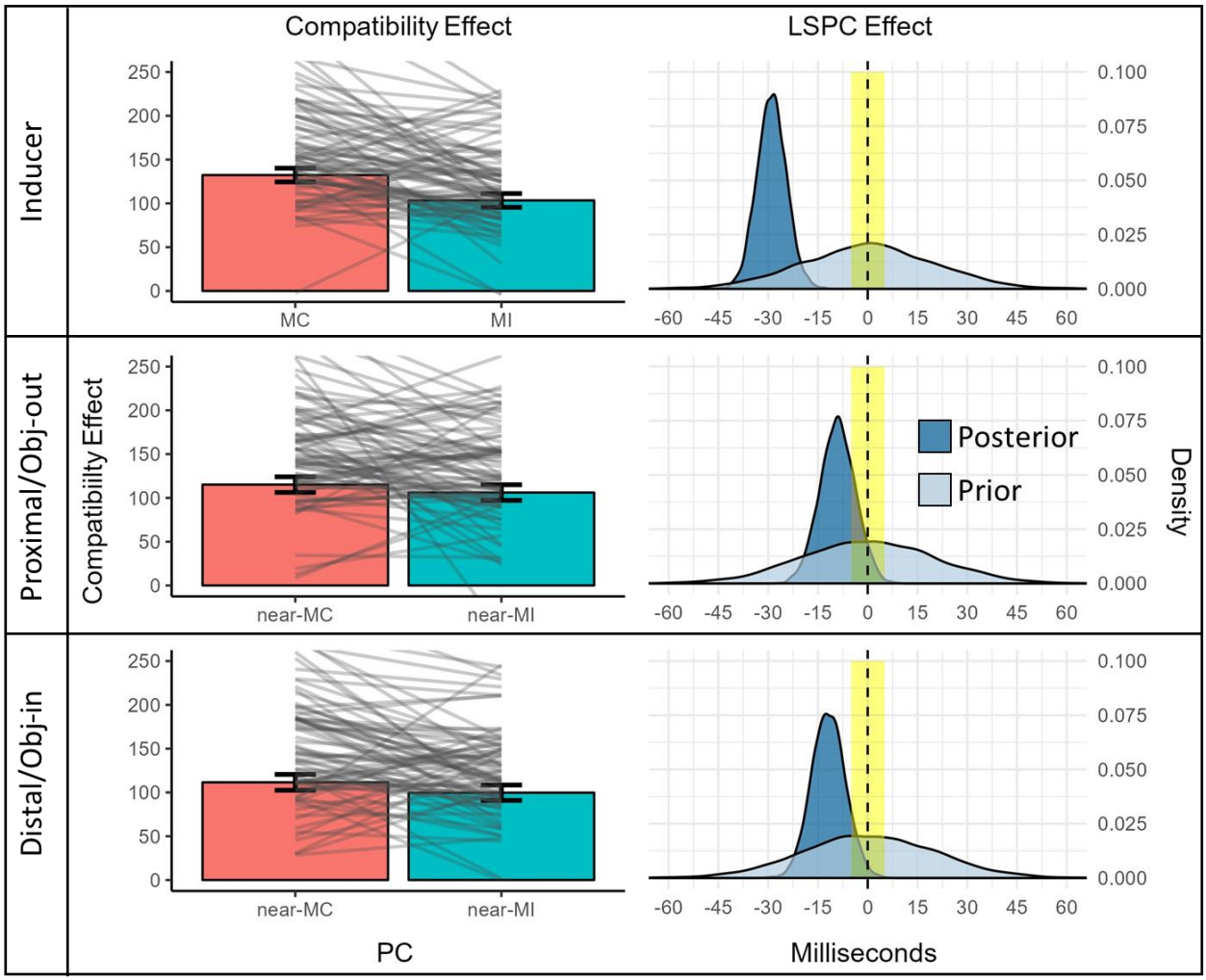
Diagnostic Proximal Locations (Outside Object). A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 111 ms, $HDI_{95\%}$ = [103 ms, 118 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (788 ms) than incongruent trials (899 ms). There was no effect of near PC (near MI – near MC), μ = 0 ms, $HDI_{95\%}$ = [-23 ms, 20 ms], $pd_{negative}$ = .513, full-ROPE = 34.95%. Most importantly, in contrast with Experiments 1, 2 and 3, there was only a possible location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = -9 ms, $HDI_{95\%}$ = [-19 ms, 1 ms], $pd_{negative}$ = .953, full-ROPE = 21.92%. See Figure 10 middle row.

Diagnostic Distal Locations (In Object). A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 106 ms, $HDI_{95\%}$ = [98 ms, 113 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (787 ms) than incongruent trials (893 ms). There was no effect of near PC (near MI – near MC), μ = 2 ms, $HDI_{95\%}$ = [-14 ms, 18 ms], $pd_{positive}$ = .596, full-ROPE = 43.03%. Importantly, there was a location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = -12 ms, $HDI_{95\%}$ = [-21.52 ms, -2 ms], $pd_{negative}$ =

.989, full-ROPE = 9.10%, indicating that the compatibility effect for MI locations (99 ms) was attenuated compared to MC locations (111 ms). See Figure 10 bottom row.

Figure 10

Compatibility and Location-specific Proportion Congruence Effects for Experiment 4a



Note. **Left panels** show model estimated compatibility effects (ms) for inducer, proximal diagnostic, and distal diagnostic locations. Grey lines show individual participants' compatibility effects calculated through simple mean differences. **Right panels** show the location-specific proportion congruence effect for inducer, proximal diagnostic, and distal diagnostic locations. The dark-blue distribution is the posterior distribution, i.e., the model estimated LSPC effect given the data. The light-blue distribution is the weakly informative prior distribution. The yellow region indicates the ROPE (region of practical equivalency) which is used to calculate full-ROPE percentages. For the LSPC effect, the ROPE range was set at [-5 ms, 5 ms].

Three-way Interaction for Diagnostic Locations. The three-way interaction revealed no difference in the LSPC effect between proximal diagnostic and distal diagnostic locations ($LSPC_{Proximal} - LSPC_{Distal}$), μ = 3 ms, $HDI_{95\%}$ = [-11 ms, 16 ms], $pd_{positive}$ = .669, full-ROPE = 49.50%.

5.3 Discussion

While there was an inducer LSPC effect, in contrast with Experiments 1, 2, and 3 there was only a possible transfer of the LSPC effect for proximal diagnostic locations. Coupled with the more robust evidence of transfer to distal diagnostic locations, Experiment 4a demonstrated the first hint of object-based control in the set of studies described. However, this conclusion must be tempered as transfer of the LSPC effect to proximal locations was still possible (albeit it was a marginal effect) given the data. In addition, the three-way interaction of the LSPC effects between proximal and distal locations revealed no differences. A point in favor of object-based control, however, is that in contrast with Experiment 1 (and Experiment 4b to foreshadow) the transfer of the LSPC effect to distal locations was reliable. As such, in aggregate the pattern of results from this experiment leads to weak evidence of the presence of object-based control.

Chapter 6: Experiment 4b

The objects used in Experiments 1, 2, and 4a could be described as concrete (i.e., representing real-world objects) and being visually complex. However, only in Experiments 1 and 4a were spatial features informative and those two experiments resulted in different interpretations for the type of control at play. In Experiment 1 proximity-based control was evidenced and in Experiment 4a object-based control was weakly evidenced. To replicate and extend Experiment 1, Experiment 4b kept spatial features informative but used more abstract and visually simple objects which is more consistent with stimuli used in prior research (Colvett et al., 2023; Weidler et al., 2018). Using more abstract and visually simple objects, could result in three different outcomes. One, it could reduce attention to objects and let participants more readily ignore object features, allowing for proximity-based control to operate as if there were no objects presented. This could result in a replication of the pattern of results from Experiment 3 wherein there was a hint of diffuse proximity-based control. Two, it is possible that abstract and visually simple objects could induce object-based control as was found in Experiment 4a. Perhaps less visually complex objects might allow those objects to be more easily bound to control states. But given the weight of evidence presented both in this study and in prior studies (Weidler et al., 2018), there is very little to support this possibility (but see Colvett et al., 2023 for evidence of control using abstract objects). Three, switching to less visually complex objects could yield no change in how object features are attended and thus used by the control system. This would result in no change in participants' propensity to exhibit concentrated proximity-based control and would replicate the pattern of results found in Experiment 1.

6.1 Methods

6.1.1 Participants

Eighty-two undergraduate participants from Washington University in St. Louis were collected (mean age = 19.4; men = 23, women = 58, gender not reported = 1).

6.1.2 Design and Stimuli

All elements of the design and stimuli were the same as in Experiment 1 except that the objects presented were abstract shapes. Specifically, they were a hollowed-out circle and a hollowed-out plaque (see Figure 5a).

6.1.3 Procedure

The procedure was identical to that of Experiment 1. See Figure 7 right panel.

6.2 Data Analysis

All elements of data cleaning and analysis were kept constant with the data pipeline from Experiment 1.

6.2.1 RT Analyses

Inducer Locations. A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 125 ms, $HDI_{95\%}$ = [119 ms, 132 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (813 ms) than incongruent trials (938 ms). There was no effect of PC (MI – MC), μ = -6 ms, $HDI_{95\%}$ = [-21 ms, 9 ms], $pd_{negative}$ = .764, full-ROPE = 36.85%. Importantly, there was a robust location-specific PC effect ($MI_{\text{compatibility effect}} - MC_{\text{compatibility effect}}$), μ = -38 ms, $HDI_{95\%}$ = [-46 ms, -29 ms], $pd_{negative}$ > .999, full-ROPE <.02%,

indicating that the compatibility effect for MI locations (106 ms) was attenuated compared to MC locations (144 ms). See Figure 10 top row.

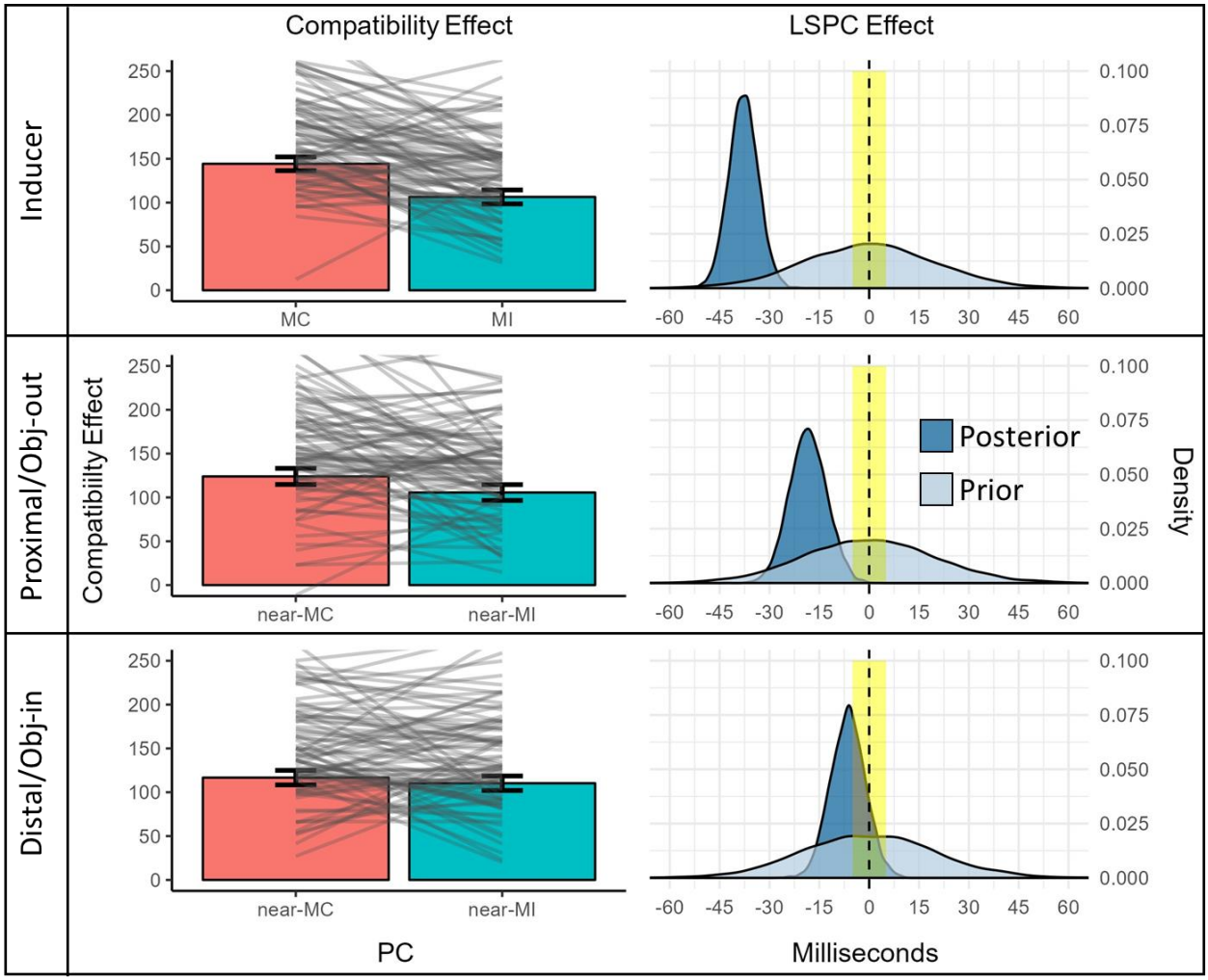
Diagnostic Proximal Locations (Outside Object). A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 115 ms, $HDI_{95\%}$ = [107 ms, 122 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (775 ms) than incongruent trials (890 ms). There was no effect of near PC (near MI – near MC), μ = -4 ms, $HDI_{95\%}$ = [-21 ms, 12 ms], $pd_{negative}$ = .703, full-ROPE = 40.28%. Most importantly, aligning with the results of Experiment 1, 2 and 3, there was a location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = -18 ms, $HDI_{95\%}$ = [-29 ms, -7 ms], $pd_{negative}$ > .999, full-ROPE = 0.70%. See Figure 10 middle row.

Diagnostic Distal Locations (In Object). A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 114 ms, $HDI_{95\%}$ = [107 ms, 120 ms], $pd_{positive}$ > .999, full-ROPE <.02%, indicating congruent trials were responded to faster (756 ms) than incongruent trials (870 ms). There was no effect of near PC (near MI – near MC), μ = 4 ms, $HDI_{95\%}$ = [-9 ms, 17 ms], $pd_{negative}$ = .735, full-ROPE = 45.87%. Importantly, there was no location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = -6 ms, $HDI_{95\%}$ = [-17 ms, 4 ms], $pd_{negative}$ = .888, full-ROPE = 37.57%. See Figure 10 bottom row.

Three-way Interaction for Diagnostic Locations. The three-way interaction revealed a possible difference in the LSPC effect between proximal diagnostic and distal diagnostic locations ($LSPC_{Proximal}$ – $LSPC_{Distal}$), μ = -12ms, $HDI_{95\%}$ = [-26 ms, 2 ms], $pd_{negative}$ = .956, full-ROPE = 15.00%.

Figure 11

Compatibility and Location-specific Proportion Congruence Effects for Experiment 4b



Note. **Left panels** show model estimated compatibility effects (ms) for inducer, proximal diagnostic, and distal diagnostic locations. Grey lines show individual participants' compatibility effects calculated through simple mean differences. **Right panels** show the location-specific proportion congruence effect for inducer, proximal diagnostic, and distal diagnostic locations. The dark-blue distribution is the posterior distribution, i.e., the model estimated LSPC effect given the data. The light-blue distribution is the weakly informative prior distribution. The yellow region indicates the ROPE (region of practical equivalency) which is used to calculate full-ROPE percentages. For the LSPC effect, the ROPE range was set at [-5 ms, 5 ms].

6.3 Discussion

Fully replicating Experiment 1 inducer locations showed a robust LSPC effect and a robust transfer of the LSPC effect to the proximal diagnostic location. No transfer of the LSPC effect was found for distal diagnostic locations. Introducing more abstract objects with less visual complexity resulted in the same pattern of results as in Experiment 1.

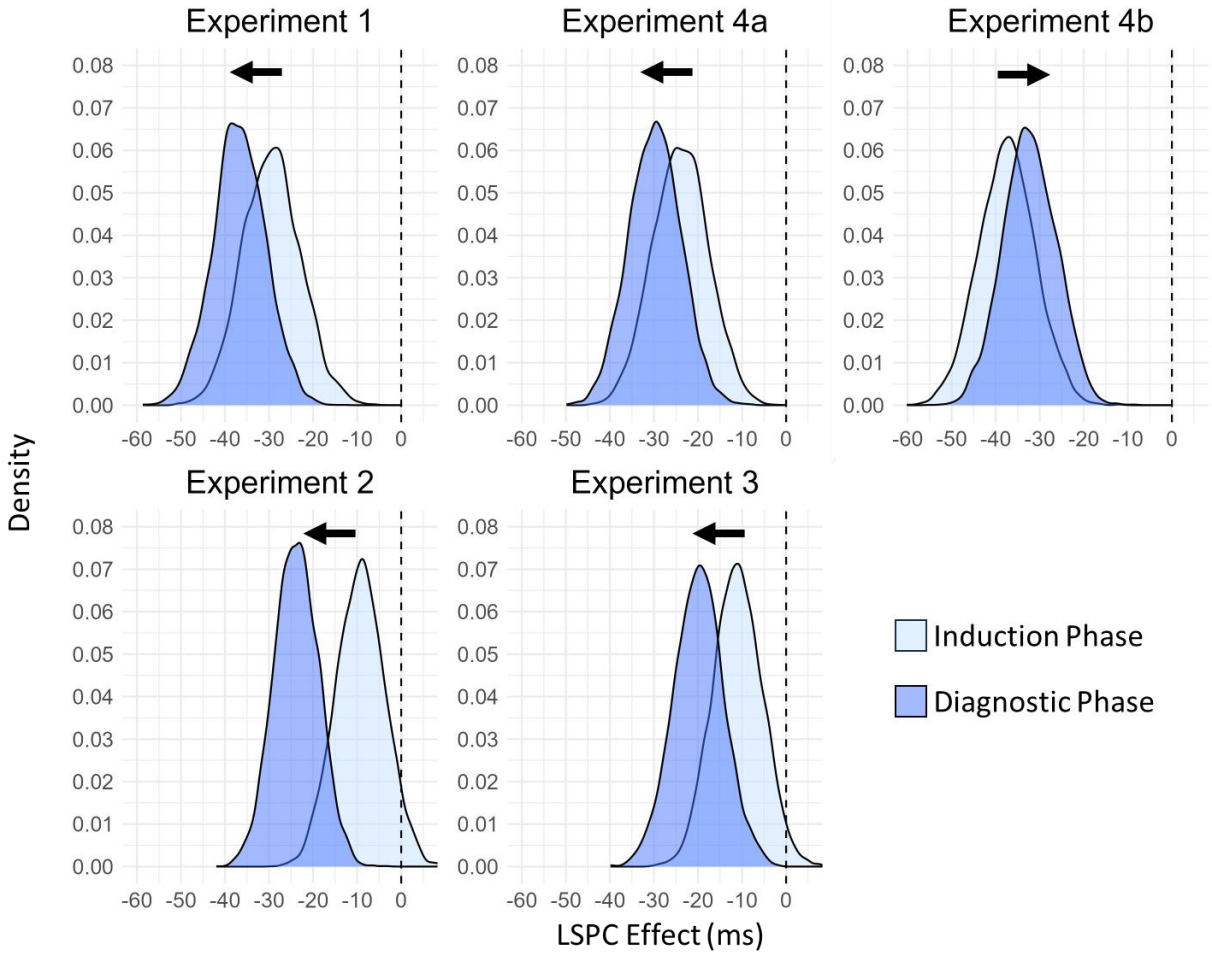
Chapter 7: Exploratory Analysis of Inducer Locations by Phases of Experiment

A lingering question from the current set of findings is by what means did proximity-based control emerge. One possible explanation is that the transfer of LSPC effects to diagnostic locations emerged because the cognitive system grouped inducer locations with diagnostic locations during the diagnostic phase of the experiments. If true, one possibility is that the LSPC effect for inducer locations would become weaker in the diagnostic phase relative to the induction phase because the grouping of locations would shift the PC of MC and MI "grouped locations" further towards PC 50 with the introduction of diagnostic locations (i.e., spatial grouping would shift MC conditions from PC 88 → PC 67 and MI conditions from PC 12 → PC 33). In other words, the presence of transfer of the LSPC effect for diagnostic locations could come at the expense of weaker LSPC effects for inducer locations in the diagnostic phase. To test this, I ran exploratory analyses on the inducer locations investigating their LSPC effects separately for the induction phase and diagnostic phase of each experiment. Figure 12 shows the results of this analysis.

Contrary to the idea that transfer in diagnostic locations comes at a cost of a weakening of LSPC effects for inducer locations during the diagnostic phase, the findings from all but one experiment reveal that the LSPC effect for inducer locations was nominally *strengthened* going from the induction phase to the diagnostic phase. This suggests that proximity-based control did not emerge because spatial grouping caused inducer locations to be less informative in the diagnostic phase.

Figure 12

LSPC Effect for Inducer Locations in the Induction and Diagnostic Phases of Experiments



Note. The arrows show the nominal direction of change for the inducer location LSPC effect going from the induction phase to the diagnostic phase. For all but one experiment, there was a nominal strengthening of the LSPC effect (more negative) for inducer locations.

Chapter 8: General Discussion

The goal of this study was to find positive evidence for object-based control in situations where spatial information, like coordinates and spatial proximity, could also be used to guide attentional control. At its inception, Experiment 1 was designed with the thought that if either object-based or proximity-based control was observed via transfer of LSPC effects, I could then parametrically change design features to establish ways to tip-the-scales in favor of or against object-based control. Aligning with a proximity-based account of control, transfer of LSPC effects to proximal diagnostic locations were found in Experiment 1. Experiment 2 ruled out the explanation that the particular objects used in this study were generally ineffective as cues for guiding control. In other words, when spatial features alone could not be used to guide attention, the object features used in this study in conjunction with spatial features were able to be leveraged to guide attention. In addition, the results of Experiment 3 showed that objects were essentially ignored as features for guiding control in Experiment 1 because their removal yielded mostly the same pattern of results.

The goal of Experiments 4a and 4b was to investigate if changing some attributes of object features, while maintaining the informativeness of spatial features, could push participants away from concentrated proximity-based control and onto either object-based (in Experiment 4a) or diffuse proximity-based control (in Experiment 4b). Varying the number of object exemplars (Experiment 4a) provided weak evidence of object-based control. By contrast, using abstract and less visually complex objects (Experiment 4b) yielded no change in the pattern of results from Experiment 1

8.1 Objects are Ignored when both Objects and Spatial Features are Informative

A natural question that arises is why are objects typically ignored as guides of attention, particularly when they are visually distinct from the rest of the environment? A prevailing explanation is that objects are less salient than spatial features in arrow flanker paradigms and are thus less able to bind to attentional states in event files. Much work has gone in to increasing object salience either via experimentally increasing attentional resources object features require (e.g., asking participants to count objects; Colvett & Bugg, 2023) or by introducing objects that might have more meaning for participants (e.g., using a picture of a quad that is frequented by participants of a particular university; Colvett & Bugg, 2021). In this study, weak evidence of object-based control was found only when multiple exemplars were used to try to increase the salience of object features. Colvett and Bugg (2023) found positive evidence of object-based attentional control when they used dynamically moving objects. All these approaches focus on improving the status of objects in event files by increasing their salience.

However, another way to approach the question may be to ask why do spatial features have such a special role in attentional control in these designs? One possibility is that the target response for flanker arrows is a direction, and this may benefit processing general spatial information thus allowing spatial features of the environment to be more easily leveraged for binding in event files. Pickel et al. (2019) demonstrated this when they extended the design from Weidler et al. (2018) but used tasks that were not arrow flanker selection tasks to assess proximity-based transfer of LSPC effects. The tasks they used were a regular Stroop task with color-word items, a Stroop task that used color patches and distractor words that were separated in space, and a spatial Stroop task that used direction words as targets and the distracting feature was the location on the screen where the target appeared. In all cases, inducer locations showed

robust LSPC effects, but only in the experiment that used the spatial Stroop task did they also observe proximity-based transfer of the LSPC effect. In other words, only when both the response target and the distractor features were spatial in nature did proximity-based control emerge. It should be noted that in the color-patch Stroop task, some spatial filtering was required because the color-patch and the distractor word were segregated in space, but this was not enough to induce proximity-based control.

In concert with this logic, perhaps using flanker stimuli that require participants to respond to target shapes amidst other flanking shapes may similarly benefit processing object information because both the target and the distractor would require processing of object features. This may, in turn, lead to more readily bound associations between object features and attentional states. In other words, rather than just increasing the status of object features in event files, this approach would seek to also degrade the status of spatial features for binding in event files by using a task that does not require as much processing of spatial features. Such an explanation would limit the interpretation of this study because the subservience of object features relative to spatial features as guides for control would then be task-dependent and not a general phenomenon.

8.2 Influence of Multiple Feature Dimensions in Event Files for Attentional Control

Recall that event file accounts of control suggest that a snapshot of events are created such that many different concrete and abstract dimensions become bound together. One criterion that is assumed to be used to determine whether a particular dimension is stored in event files is its informativeness for a given task. It is the parametric manipulation of the informativeness of specific dimensions that researchers have used to produce context-specific effects. However,

both in real-world settings and in some experimental settings, there can be a plethora of informative dimensions to access and store in event files. While the current study focuses on how two dimensions, object features and spatial features, influence one another in event-files, a broader perspective to address is how different dimensions in general influence each other in event files.

A few studies to date have looked specifically at how multiple informative dimensions influence one another to guide attentional control. For example, Experiment 2 of Bugg and Dey (2018), investigated how the dimensions of categories and exemplars influenced item-specific proportion congruence effects when they signaled opposing attentional states. In a follow-up study, Ileri-Tayar et al., (in prep) find that the exemplar dimension is subordinate to the category dimension barring a few exceptional cases. In other words, the informativeness of the exemplar dimension is not sufficient for its use in control when category dimensions are available. One might argue that there is a hierarchical relationship between categorical and exemplar dimensions in their study. In two other studies Bugg et al. (2020) and Bugg et al. (2022) showed locations were subordinate to picture items (e.g., pictures of dogs and cats) and colors items (e.g., the color blue and the color red) as cues for control in a picture-word Stroop task and a color-word Stroop task, respectively. These two studies further provide evidence that informative dimensions may be organized as hierarchies in event-files.

A study that hints at another type of organization of dimensions in event files is one by Gheza and Kool (2023; pre-print). Gheza and Kool investigated four different dimensions (color, shape, edge, and motion) and their influence on the congruency sequence effect, another measure of adaptive control. The upshot of their work was that the dimensions they experimentally manipulated worked in isolation such that informativeness on one dimension did not influence

another when modulating attention from trial to trial. They demonstrate this by comparing a connectionist model that assumed distractor-specific adaption (no crosstalk between dimensions) to a model that assumed global distractor adaption and found that distractor-specific adaptation best fit their data. Gheza and Kool also showed that behavioral measures of congruency-sequence effects (RT and error rates) were much more robust for within-dimension sequences compared to cross-dimension sequences. Taken together, their study found limited influence between informative[7] dimensions stored within event files.

In the current study, the relationship between the object dimension and the spatial dimension can be characterized as hierarchical in nature (as in Bugg et al., 2020, Bugg et al., 2022, and Ileri-Tayar et al., in prep) rather than independent (as in Gheza & Kool, 2023). Object features in this study were clearly subsumed by spatial features as guides for attentional control. However, this leaves an open question as to how and why some dimensions are hierarchical and others are independent and future research would do well in exploring relationships between dimensions in event files.

8.3 Limitations and Future Directions

Throughout this study, I have discussed the LSPC effects as an indicator of attentional control. However, the LSPC effects in this study may also reflect either stimulus-response contingencies or a combination of attentional control and stimulus-response contingencies (Schmidt and Lemercier, 2019). Specifically, for the mostly congruent inducer locations, the frequencies of the flanker items (see **Appendix D: Flanker Stimuli Frequencies**) could allow

[7] The dimensions in Gheza and Kool (2023) were informative on a trial-by-trial level as opposed to on the level of the entire experiment. That is, these dimensions did not have different proportion congruences as was the case in this study and in Ileri-Tayar et al. (in prep), rather differences in attentional states were observed when on a previous trial the dimension was either congruent or incongruent.

LSPC effects to emerge if participants associated distractor stimuli with the correct response. For example, in the mostly congruent locations, whenever participants saw that the distractors pointed up, they could have reflexively responded "up" regardless of what direction the target indicated. This type of contingency learning would then have to be paired with the location of the screen since those same distractors would not inform participants to respond in a particular direction in the mostly incongruent condition. In other words, rather than binding *location + attention state* (i.e., control) participants may have bound *location + distractor stimulus + response* (i.e., contingency) for the mostly congruent conditions. Importantly, these learned contingencies would be limited to just the mostly congruent locations because contingency learning cannot produce distractor-response associations in mostly incongruent locations as the frequency of target and distractor pairings does not allow any single response to be associated with a given distractor (see Table 7 in **Appendix D** under the MI condition).

Of note, previous work has demonstrated that contingency learning cannot explain LSPC effects in all designs, especially when diagnostic conditions use a different set of flanker stimuli than those of inducer locations (Weidler et al., 2022; see Braem et al., 2019 for review). And more importantly, the conclusions of this study are not dependent on whether attentional control or contingency learning is at play. The primary goal of this study was to investigate the interplay between object features and spatial features for binding in event files. Whether those features bind to attentional states, or whether they bind to learned contingencies is irrelevant for the purposes of this study. I elected to default to an interpretation of attentional control because I believe it is more parsimonious to assume location + attentional state bindings as compared to location + distractor stimulus + response bindings. In addition, interpretations of control have been used in prior literature with similar confounds (Crump & Milliken, 2009; Colvett & Bugg,

2021; Colvett et al., 2023; Pickel et al., 2019, Weidler & Bugg, 2016; Weidler et al. 2018). A worthwhile follow-up for researchers interested in definitively addressing if control or contingency is at play would be to replicate this work with separate sets of flanker items for inducer and diagnostic locations.

A limitation of this study that *does* impact its main conclusions relates to how objects were defined. Here, objects were defined as 2D visual pictures on a screen. For example, using Gestalt principles, perceptual boundaries formed by edges, colors, and shapes defined whether an object was a donut or a gear. In addition, these objects were somewhat unique (by design) because they had non-linear boundaries which allowed for diagnostic proximal locations to be outside the bounds of the object. However, objects are not just limited to being 2D pictures that are passively viewed. Objects are also things that are manipulable and can be acted upon (i.e., in reality we can eat donuts or rotate gears). In this way, the objects in this study were stripped of one of their main characteristics that define them. Of course, this was due to the limitation of the task at hand and to keep object features on a level playing field with spatial features. But perhaps objects cannot be on a level playing field with spatial features for object-based control to emerge when both dimensions are informative.

One area of research that illustrates the influence of the manipulability of objects for attentional processes is the literature on the *action-effect* (Weidler & Abrams, 2014). While perhaps research on the action-effect influencing perception is more well known (Bekkering & Neggers, 2002; Bloesch et al., 2012; Proffitt, 2008; Witt & Proffitt, 2005; Witt et al., 2005; Wykowska et al., 2009; but see Durgin et al., 2012; Firestone & Scholl 2016), its influence on attentional processes is a more recent avenue of research (Buttaccio & Hahn, 2011; Wang et al., 2017; Weidler & Abrams, 2014; but see Robinson et al., 2018 for evidence against action

influencing attention). The first evidence of the action effect influencing attention was demonstrated in Buttaccio and Hahn (2011). Buttaccio and Hahn found that response times on a visual search paradigm were faster when the to-be-searched item was within an object (a shape in their study) that was previously responded to in a go/no-go trial compared to objects that were not responded to. The simple action of pressing the spacebar for an object ramped up attentional resources for that object when it appeared on the next trial and for a completely different task. Weidler and Abrams (2014) replicated these findings and demonstrated that participants need not perceive a consequence of their action, nor do they need to process the acted-upon target to demonstrate an action-effect for attention.[8]

Revisiting the initial goal of this study, the action-effect may be leveraged to further shift attentional resources to objects and enable object-based control when both objects and spatial features are informative. One note, however, is that this was the very same logic used in Experiment 4a in this study. That is, introducing multiple exemplars for each object type was thought to perhaps increase attention to object features and would consequently lead to object-based control. That there was only weak evidence suggesting that object-based control was at play suggests that perhaps the action-effect, even if it does increase attention to the acted-upon object, may not be enough to tip-the-scales more conclusively in favor of object-based control. Alternatively, perhaps the manner by which attention towards objects is increased (i.e., perceptual/conceptual in Experiment 4a versus motor for the action-effect) would lead to more robust shifts towards object-based control.

[8] The attentional based action-effect is not, however, without controversy. Robinson et al., (2018) posit that the action-effect is actually an *attentional-template effect* wherein because attention is required to respond to objects, those attentional resources carry over to subsequent trials and are used in a task-general fashion.

A final limitation of this study I would like to address is a neglect of any investigation of individual differences for participants' propensity to use a given type of control. While this study was never meant to address such individual differences, I admit that there are likely systematic reasons that lead people to default to using spatial features for control and lead others to potentially use object features for control. For curiosity and to demonstrate this point, I ran an additional exploratory analysis labeling participants as having used one of four types of control – coordinate-based, proximity-based, object-based, or impaired (i.e., no apparent use of one of these types of control). This analysis is not meant to be robust, and participants were labeled according to whether they showed a nominal LSPC effect based on individual generalized linear models.[9] For example, if a participant had an LSPC effect greater than 5 ms (i.e., a coefficient of < -5 ms for the interaction term) for inducer locations but LSPC transfer effects less than 5 ms for both the diagnostic locations, they were labeled as using coordinate-based control. The logic of which label a participant was given was the same as described in the predictions given in Table 1. A participant was labeled as having impaired control if they did not show a nominal LSPC effect for inducer locations (irrespective of whether they showed transfer of LSPC effects in either the proximal or distal locations).

Because of the similarity of their design, I combined Experiments 1, 4a, and 4b (total n = 246) and Figure 13 shows a 3D scatter plot of participants' labeled control type for illustrative purposes. Table 4 shows the counts of the labeled control type for each experiment and for the combined pool as well. Of note, a large majority of participants displayed a form of control, but a not insignificant minority also showed impaired control. Additionally, across all three

[9] For visualization of the Bayesian model-estimated individual level variability of the LSPC effects for inducer locations and transfer of LSPC effects to diagnostic locations I include forest plots in Appendix E: Forest Plots of LSPC Effects.

experiments, a mere 36 participants displayed a nominal pattern consistent with object-based control, a large portion of whom were in Experiment 4a. Despite this relatively low proportion of "object-based controllers", this still hints at a substantial amount of individual variability in the propensity to use one form of control over another. In addition, this variability may also have been influenced by experimental contexts and thus implies how fruitful individual difference research may be on the type of control people lean toward.

Figure 13

3D Scatter Plot of Participants' LSPC Effects Labeled by the Nominal Type of Control Used

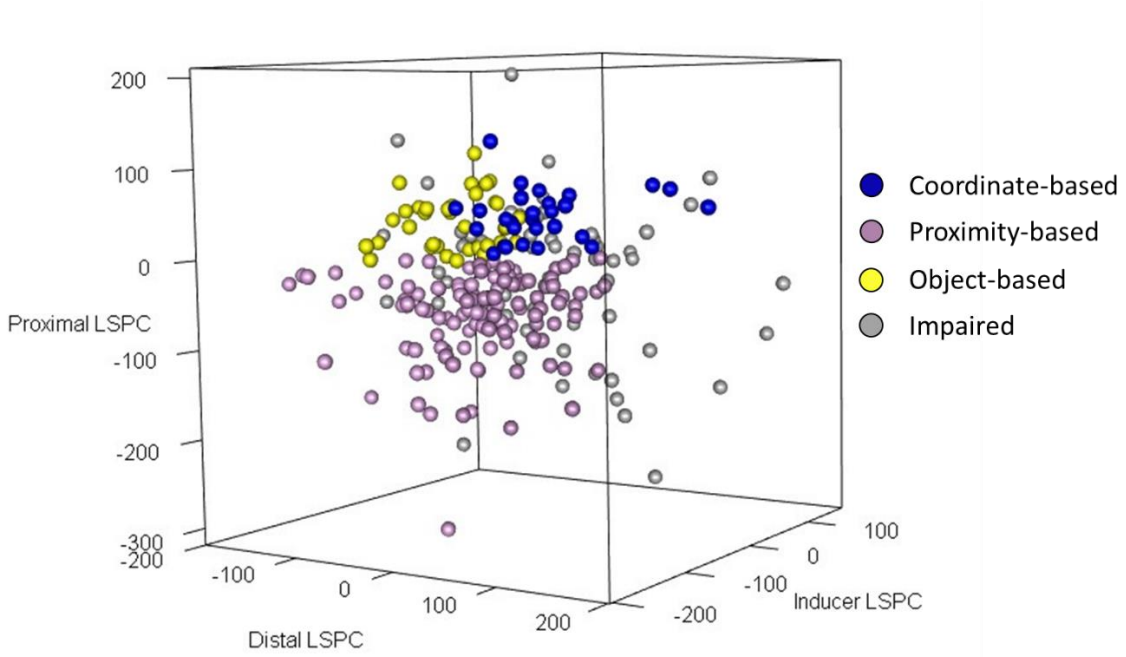


Table 4

Count of Participants' Nominal Type of Control Used for Experiments 1, 4a, and 4b

	Experiment			
Control Type	Exp 1	Exp 4a	Exp 4b	Combined
Coordinate-based	8	10	10	28
Proximity-based	45	36	37	118
Concentrated	17	13	17	47
Diffuse	28	23	20	71
Object-based	8	15	13	36
Impaired	21	21	22	64

Note. Proximity-based control is further subdivided into concentrated and diffuse. Concentrated = proximity-based control where transfer of LSPC effects only emerge in proximal locations. Diffuse = proximity-based control where transfer of LSPC effects emerge in both proximal and distal locations.

8.3 Conclusion

Across two out of three experiments, I demonstrated that when objects and spatial features were informative as guides for control, objects were subsumed by spatial features. Only in one out of three experiments where both feature types were useful for control was there weak evidence of object-based control. An additional two experiments ruled out alternative explanations for why objects were ignored in favor of spatial features. Specifically, Experiment 2 ruled out that the specific objects in this study were generally not useful as cues and Experiment 3 demonstrated that the inclusion of the objects in Experiment 1 yielded no difference in type of

proximity-based control at play. As a result, the overarching conclusion that can be reached is that objects are subordinate to spatial features as guides for attentional control.

References

Bates, D., Mächler, M., Bolker, B., & Walker, S. (2015). Fitting Linear Mixed-Effects Models Using lme4. *Journal of Statistical Software*, *67*, 1–48. https://doi.org/10.18637/jss.v067.i01

Bekkering, H., & Neggers, S. F. W. (2002). Visual Search Is Modulated by Action Intentions. *Psychological Science*, *13*(4), 370–374. https://doi.org/10.1111/j.0956-7976.2002.00466.x

Bloesch, E. K., Davoli, C. C., Roth, N., Brockmole, J. R., & Abrams, R. A. (2012). Watch this! Observed tool use affects perceived distance. *Psychonomic Bulletin & Review*, *19*(2), 177–183. https://doi.org/10.3758/s13423-011-0200-z

Braem, S., Bugg, J. M., Schmidt, J. R., Crump, M. J. C., Weissman, D. H., Notebaert, W., & Egner, T. (2019). Measuring Adaptive Control in Conflict Tasks. *Trends in Cognitive Sciences*, *23*(9), 769–783. https://doi.org/10.1016/j.tics.2019.07.002

Brosowsky, N. P., & Crump, M. J. C. (2021). Contextual recruitment of selective attention can be updated via changes in task relevance. *Canadian Journal of Experimental Psychology / Revue Canadienne de Psychologie Expérimentale*, *75*(1), 19–34. https://doi.org/10.1037/cep0000221

Bugg, J. M., & Dey, A. (2018). When stimulus-driven control settings compete: On the dominance of categories as cues for control. *Journal of Experimental Psychology: Human Perception and Performance*, *44*(12), 1905–1932. https://doi.org/10.1037/xhp0000580.supp

Bugg, J. M., Jacoby, L. L., & Toth, J. P. (2008). Multiple levels of control in the Stroop task. *Memory & Cognition*, *36*(8), 1484–1494. https://doi.org/10.3758/MC.36.8.1484

Bugg, J. M., Suh, J., & Colvett, J. S. (2022). The dominance of item learning in the location-specific proportion congruence paradigm. *Quarterly Journal of Experimental Psychology (2006)*, *75*(8), 1497–1513. https://doi.org/10.1177/17470218211055162

Bugg, J. M., Suh, J., Colvett, J. S., & Lehmann, S. G. (2020). What can be learned in a context-specific proportion congruence paradigm? Implications for reproducibility. *Journal of Experimental Psychology. Human Perception and Performance*, *46*(9), 1029–1050. https://doi.org/10.1037/xhp0000801

Bürkner, P. -C. (2017). brms: An R Package for Bayesian Multilevel Models Using Stan. *Journal of Statistical Software*, *80*(1), 1-28. https://doi:10.18637/jss.v080.i01

Buttaccio, D. R., & Hahn, S. (2011). The influence of action on visual search: Behavioral response toward stimuli modifies the selection process. *Attention, Perception, & Psychophysics*, *73*(5), 1453–1466. https://doi.org/10.3758/s13414-011-0121-y

Chen, Z. (2012). Object-based attention: A tutorial review. *Attention, Perception, & Psychophysics*, *74*(5), 784–802. https://doi.org/10.3758/s13414-012-0322-z

Colvett, J. S., & Bugg, J. M. (2021). Meaningful boundaries create boundary conditions for control. *Psychological Research*, *86*(5), 1615–1635. https://doi.org/10.1007/s00426-021-01580-9

Colvett, J. S., Weidler, B. J., & Bugg, J. M. (2023). Revealing object-based cognitive control in a moving object paradigm. *Journal of Experimental Psychology: Human Perception and Performance*, *49*(11), 1467–1484. https://doi.org/10.1037/xhp0001158.supp

Corballis, P. M., & Gratton, G. (2003). Independent control of processing strategies for different locations in the visual field. *Biological Psychology*, *64*(1), 191–209. https://doi.org/10.1016/S0301-0511(03)00109-1

Crump, M. J. C., Gong, Z., & Milliken, B. (2006). The context-specific proportion congruent Stroop effect: Location as a contextual cue. *Psychonomic Bulletin & Review*, *13*(2), 316–321. https://doi.org/10.3758/BF03193850

Crump, M. J. C., & Milliken, B. (2009). Short article: The flexibility of context-specific control: Evidence for context-driven generalization of item-specific control settings. *Quarterly Journal of Experimental Psychology*, *62*(8), 1523–1532. https://doi.org/10.1080/17470210902752096

Crump, M. J. C., Vaquero, J. M. M., & Milliken, B. (2008). Context-specific learning and control: The roles of awareness, task relevance, and relative salience. *Consciousness and Cognition*, *17*(1), 22–36. https://doi.org/10.1016/j.concog.2007.01.004

Diede, N. T., & Bugg, J. M. (2016). Spatial proximity as a determinant of context-specific attentional settings. *Attention, Perception, & Psychophysics*, *78*(5), 1255–1266. https://doi.org/10.3758/s13414-016-1086-7

Diede, N. T., & Bugg, J. M. (2019). Boundary conditions for the influence of spatial proximity on context-specific attentional settings. *Attention, Perception, & Psychophysics*, *81*(5), 1386–1404. https://doi.org/10.3758/s13414-019-01686-8

Dignath, D., Johannsen, L., Hommel, B., & Kiesel, A. (2019). Reconciling cognitive-control and episodic-retrieval accounts of sequential conflict modulation: Binding of control-states into event-files. *Journal of Experimental Psychology. Human Perception and Performance*, *45*(9), 1265–1270. https://doi.org/10.1037/xhp0000673

Dreisbach, G., Fröber, K., Berger, A., & Fischer, R. (2019). Unexpected conflict signals loom larger in a positive context: Evidence from context specific control adjustments. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *45*(8), 1398–1409. https://doi.org/10.1037/xlm0000662

Duncan, J. (1993). Similarity between concurrent visual discriminations: Dimensions and objects. *Perception & Psychophysics*, *54*(4), 425–430. https://doi.org/10.3758/BF03211764

Durgin, F. H., Klein, B., Spiegel, A., Strawser, C. J., & Williams, M. (2012). The social psychology of perception experiments: Hills, backpacks, glucose and the problem of generalizability. *Journal of Experimental Psychology. Human Perception and Performance*, *38*(6), 1582–1595. https://doi.org/10.1037/a0027805

Egner, T. (2014). Creatures of habit (and control): A multi-level learning perspective on the modulation of congruency effects. *Frontiers in Psychology*, *5*. https://doi.org/10.3389/fpsyg.2014.01247

Firestone, C., & Scholl, B. J. (2016). Cognition does not affect perception: Evaluating the evidence for "top-down" effects. *Behavioral and Brain Sciences*, *39*, e229. https://doi.org/10.1017/S0140525X15000965

Frings, C., Hommel, B., Koch, I., Rothermund, K., Dignath, D., Giesen, C., Kiesel, A., Kunde, W., Mayr, S., Moeller, B., Möller, M., Pfister, R., & Philipp, A. (2020). Binding and Retrieval in Action Control (BRAC). *Trends in Cognitive Sciences*, *24*(5), 375–387. https://doi.org/10.1016/j.tics.2020.02.004

Goldsmith, M. (1998). What's in a location? Comparing object-based and space-based models of feature integration in visual search. *Journal of Experimental Psychology: General*, *127*(2), 189–219. https://doi.org/10.1037/0096-3445.127.2.189

Gheza, D., & Kool, W. (2023). *Distractor-specific control adaptation in multidimensional environments* [Preprint]. Neuroscience. https://doi.org/10.1101/2023.09.04.556248

Kimchi, R., Yeshurun, Y., & Cohen-Savransky, A. (2007). Automatic, stimulus-driven attentional capture by objecthood. *Psychonomic Bulletin & Review*, *14*(1), 166–172. https://doi.org/10.3758/BF03194045

Kruschke, J. K. (2014). *Doing Bayesian data analysis: A tutorial with R and BUGS*. Elsevier, Academic Press.

Lehle, C., & Hübner, R. (2008). On-the-fly adaptation of selectivity in the flanker task. *Psychonomic Bulletin & Review*, *15*(4), 814–818. https://doi.org/10.3758/PBR.15.4.814

Makowski, D., Ben-Shachar, M. S., Chen, S. H. A., & Lüdecke, D. (2019). Indices of Effect Existence and Significance in the Bayesian Framework. *Frontiers in Psychology*, *10*. https://doi.org/10.3389/fpsyg.2019.02767

Pickel, L., Pratt, J., & Weidler, B. J. (2019). The transfer of location-based control requires location-based conflict. *Attention, Perception, & Psychophysics*, *81*(8), 2788–2797. https://doi.org/10.3758/s13414-019-01785-6

Peirce, J. W., Gray, J. R., Simpson, S., MacAskill, M. R., Höchenberger, R., Sogo, H., Kastman, E., Lindeløv, J. (2019). PsychoPy2: experiments in behavior made easy. *Behavior Research Methods*. https://doi.org/10.3758/s13428-018-01193-y

Proffitt, D. R. (2006). Embodied Perception and the Economy of Action. *Perspectives on Psychological Science*, *1*(2), 110–122. https://doi.org/10.1111/j.1745-6916.2006.00008.x

R Core Team (2023). _R: A Language and Environment for Statistical Computing_. R Foundation for Statistical Computing, Vienna, Austria. <https://www.R-project.org/>

Robinson, M. M., Clevenger, J., & Irwin, D. E. (2018). The action is in the task set, not in the action. *Cognitive Psychology*, *100*, 17–42. https://doi.org/10.1016/j.cogpsych.2017.11.005

Schmidt, J. R., & Lemercier, C. (2019). Context-specific proportion congruent effects: Compound-cue contingency learning in disguise. *Quarterly Journal of Experimental Psychology*, *72*(5), 1119–1130. https://doi.org/10.1177/1747021818787155

Wang, F., Sun, J., Sun, P., Weidler, B. J., & Abrams, R. A. (2017). Influence of simple action on subsequent manual and ocular responses. *Attention, Perception, & Psychophysics, 79*(2), 389–395. https://doi.org/10.3758/s13414-017-1280-2

Weidler, B. J., & Abrams, R. A. (2014). Decomposing the action effect: How simple actions affect subsequent perception. *Attention, Perception, & Psychophysics*, *76*(4), 1242–1252. https://doi.org/10.3758/s13414-014-0652-0

Weidler, B. J., & Bugg, J. M. (2016). Transfer of location-specific control to untrained locations. *Quarterly Journal of Experimental Psychology*, *69*(11), 2202–2217. https://doi.org/10.1080/17470218.2015.1111396

Weidler, B. J., Dey, A., & Bugg, J. M. (2020). Attentional control transfers beyond the reference frame. *Psychological Research*, *84*(1), 217–230. https://doi.org/10.1007/s00426-018-0984-9

Weidler, B. J., Pratt, J., & Bugg, J. M. (2022). How is location defined? Implications for learning and transfer of location-specific control. *Journal of Experimental Psychology: Human Perception and Performance*, *48*(4), 312–330. https://doi.org/10.1037/xhp0000989

Wiley J, Hedeker D (2022). _brmsmargins: Bayesian Marginal Effects for 'brms' Models_. R package version 0.2.0. https://CRAN.R-project.org/package=brmsmargins

Witt, J. K., & Proffitt, D. R. (2005). See the Ball, Hit the Ball: Apparent Ball Size Is Correlated With Batting Average. *Psychological Science*, *16*(12), 937–938. https://doi.org/10.1111/j.1467-9280.2005.01640.x

Witt, J. K., Proffitt, D. R., & Epstein, W. (2005). Tool Use Affects Perceived Distance, But Only When You Intend to Use It. *Journal of Experimental Psychology: Human Perception and Performance*, *31*(5), 880–888. https://doi.org/10.1037/0096-1523.31.5.880

Wykowska, A., Schubö, A., & Hommel, B. (2009). How you move is what you see: Action planning biases selection in visual search. *Journal of Experimental Psychology. Human Perception and Performance*, *35*(6), 1755–1769. https://doi.org/10.1037/a0016798

Appendices

Appendix A: Interpreting Bayesian Indices

The RT model indices reported here are from a set that Makowski et al. (2019) suggest should be reported when summarizing the results of Bayesian HLMs. Makowski et al. provide a thorough explanation of each index and provide ample reasoning for reporting these indices. A complete summary of their research is beyond the scope of this study and this section, but I highlight a few key points.

Makowski et al. (2019) make a distinction between indices of existence and indices that reflect significance. The probability of direction (*pd)* reported all throughout this study is an index of an effect's existence. Put simply, it describes the probability that an effect is greater than or less than 0. For example, if a compatibility effect has a $pd_{positive}$ = .990, this tells us that the probability that the compatibility effect is greater than 0 is .990. As another example, if a LSPC effect has a $pd_{negative}$ = .975, this tells us that the probability that the LSPC effect is less than 0 is .975. Through simulation analysis, Makowski et al., show that the *pd* value is most akin to *p*-values produced from frequentist approaches. This means that when frequentists interpret an effect being significant when $p < .05$, this is akin to the Bayesian interpretation that an effect likely exists.

Moving on to indices of significance, it is important to note that the meaning of *significance* changes when switching from a frequentist to a Bayesian approach. In Bayesian analysis, indices of significance, like the full-ROPEs reported in this study, are interpreted as how meaningful an effect is. In other words, it can be thought of as a measure of the effects magnitude or effect size. To understand in more detail what full-ROPEs mean, I first describe what ROPE is. ROPE (or region of practical equivalence) is a region set by the researcher and is

(hopefully) based on the researchers a-priori understanding of a range of effect sizes that are for all practical purposes akin to a null effect (see Kruschke, 2014 for a more a-theoretical method for setting ROPE ranges). For example, the ROPE range for the LSPC effects reported in this study was set between -5 ms to 5 ms. LSPC effects within this range are those I deemed equivalent to a null effect based on my prior knowledge of proportion congruence effects. For additional justification for this range see Footnote 6. Given this, a full-ROPE index is the percent of the *full* posterior distribution of an effect (as opposed to 89% or 95% of the posterior distribution of an effect) that falls within the ROPE range. Larger percentages imply the effect is likely null and smaller percentages imply that the effect is likely significant (in a Bayesian sense). Importantly, Makowski et al., also highlight that ROPE indices (full-ROPE or otherwise) should be viewed as continuous measures of significance and that dichotomizing effects as significant or not-significant under-utilizes the granularity of this index.

For each effect of interest, Makowski et al. (2019), thus suggest reporting a description of the effect's posterior distribution, an index of the effect's existence, and an index of the effect's significance. In this study, the posterior distribution is described with a measure of central tendency (μ) and the 95% highest-density interval around that central tendency ($HDI_{95\%}$; i.e., a credible interval). The *pd* is given as an index of an effect's existence and the full-ROPE percentage is given as an index of an effect's significance. For quick reference suggestions for how to interpret *pd* values are shown in Table 5 (adapted from Makowski et al., 2019), and a simple way to understand full-ROPE percentages is illustrated in Figure 14.

Table 5

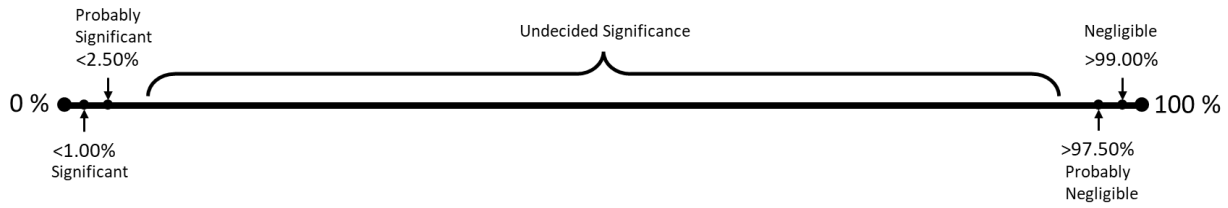
Interpretations for pd Values

pd	Two-tailed *p* equivalent	Interpretation
<=.950	>=.100	Uncertain if effect exists
>.950	<.100	Effect possibly exists
>.975	<.05	Effect likely exists
>.990	<.02	Effect probably exists
>.999	<.002	Effect certainly exists

Note. These interpretations are suggestions for convenience from Makowski et al., (2019).

Figure 14

Suggested Categorical Interpretations for Full-ROPE Percentages



Note. These are suggestions for those who wish for categorical interpretations of full-ROPE percentages. Makowski et al., (2019) advise using a continuous interpretation.

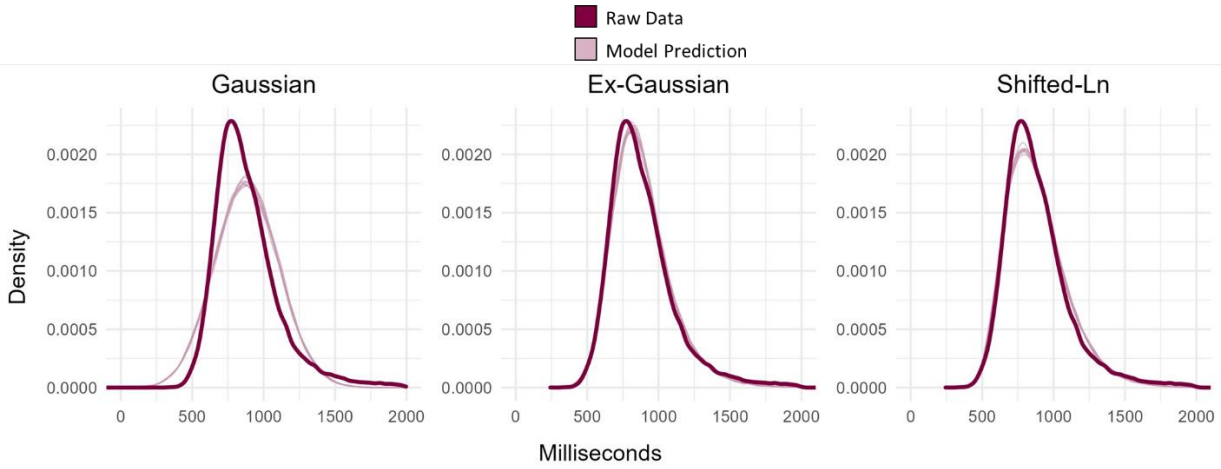
Appendix B: Model Comparison for Inducer Locations in Experiment 1

To set a standard for all RT analyses in this study, I compared Bayesian HLMs with different families of distributions as underlying assumptions for the inducer locations in

Experiment 1. Specifically, I compared models that assumed either a Gaussian, ex-Gaussian, or shifted-log normal distribution. Model comparisons were done by visual inspections of the posterior-predictive checks and by evaluating ELPD differences between the three model types. The posterior-predictive checks allow one to visually inspect how well model predicted values of the response variable match with the actual data. In this case, how well does the model fit the response time distribution of the actual data. Figure 15 shows these posterior-predictive checks for each model.

Figure 15

Posterior-predictive Checks from Models with Differing Distributional Assumptions



Note. The density plot of the raw data is in **darker purple**. In **lighter purple**, 10 separate posterior-predictive draws were sampled to create 10 model predicted density plots per model.

Visual inspection of the posterior-predictive checks reveal that both the ex-Gaussian and shifted-log normal distribution outperform the Gaussian distribution in their ability to track raw response time data. There is also a small but perceptible improvement of performance of the ex-Gaussian distribution over the shifted-log normal distribution. To verify that the ex-Gaussian model was indeed the best fit for the data, I also looked at another index of model fit, ELPD differences.

ELPD, or expected log-pointwise predictive density, is a measure of a model's ability to predict data (Vehtari et al., 2017). A higher ELPD indicates a better model fit. As such, models that perform worse than the best fitting model will have greater ELPD differences (more negative values) the worse they fit the data. ELPD was calculated with the brms package in R (Bürkner, 2017) using leave-one-out cross validation. Table 6 shows the ELPD differences (and standard error of the differences) between the best fitting model and the others. The best fitting model was the ex-Gaussian model and the other two models performed measurably worse when taking into account that the ELPD differences were > 2 standard errors of their respective differences.

Table 6

ELPD differences for Model Comparison

Model	ELPD – difference	SE
Ex-Gaussian	0	0
Shifted-log normal	-271.60	92.80
Gaussian	-4772.30	118.80

Note. SE = standard error of the difference. A difference greater than 2 × SE reflects a notable difference between a given model and the best fitting model.

Taken together, both the posterior-predictive checks and ELPD-differences demonstrate that the ex-Gaussian model best fit response time data from inducer locations in Experiment 1. To maintain consistency, ex-Gaussian models were subsequently applied to all other RT analyses reported in this study.

Appendix C: Error Rate Analyses

In this section all parameter estimates are in units of the log-odds of the error rate. For more interpretable error rate estimates see Table 2 in the main text. Full-ROPE percentages are not reported as I had no a-priori understanding of a range of error rate differences that would be considered a null effect. In addition, floor effects make it difficult to measure a meaningful difference (i.e., significant in the Bayesian sense) in error rate. The *pd* of the log-odds of error rate for the compatibility effect, PC effect, and LSPC effect are still reported as indices of effect existence.

Experiment 1

Inducer Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 2.18, $HDI_{95\%}$ = [1.70, 2.68], $pd_{positive}$ > .999. There was an effect of PC (MI – MC), μ = -.70, $HDI_{95\%}$ = [-1.15, -.27], $pd_{negative}$ > .999. There was no location-specific PC effect ($MI_{\text{compatibility effect}}$ – $MC_{\text{compatibility effect}}$), μ = .57, $HDI_{95\%}$ = [-.30, 1.40], $pd_{positive}$ = .916.

Diagnostic Proximal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.67, $HDI_{95\%}$ = [1.05, 2.31], $pd_{positive}$ > .999. There was a possible effect of near PC (near MI – near MC), μ = -.37, $HDI_{95\%}$ = [-.88, .12], $pd_{negative}$ = .925. There was no location-specific PCeffect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = .02, $HDI_{95\%}$ = [-.96, 1.01], $pd_{positive}$ = .502.

Diagnostic Distal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.56, $HDI_{95\%}$ = [.93, 2.22], $pd_{positive}$ > .999. There was no effect of near PC (near MI – near MC), μ = -.41,

$HDI_{95\%}$ = [-.95, .15], $pd_{negative}$ = .936. There was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -.35, $HDI_{95\%}$ = [-1.33, .62], $pd_{positive}$ = .762.

Experiment 2

Inducer Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 2.09, $HDI_{95\%}$ = [1.58, 2.64], $pd_{positive}$ > .999. There was a possible effect of PC (MI – MC), μ = -.37, $HDI_{95\%}$ = [-.80, .03], $pd_{negative}$ = .967. There was no location-specific PC effect ($MI_{compatibility\ effect}$ – $MC_{compatibility\ effect}$), μ = .36, $HDI_{95\%}$ = [-.45, 1.24], $pd_{positive}$ = .811.

Diagnostic Proximal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.44, $HDI_{95\%}$ = [.86, 2.08], $pd_{positive}$ > .999. There was no effect of near PC (near MI – near MC), μ = -.32, $HDI_{95\%}$ = [-.79, .04], $pd_{negative}$ = .929. There was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -.53, $HDI_{95\%}$ = [-1.34, .31], $pd_{negative}$ = .895.

Diagnostic Distal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.62, $HDI_{95\%}$ = [1.15, 2.11], $pd_{positive}$ > .999. There was no effect of near PC (near MI – near MC), μ = -.25, $HDI_{95\%}$ = [-.65, .13], $pd_{negative}$ = .905. There was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -.04, $HDI_{95\%}$ = [-.72, .66], $pd_{negative}$ = .551.

Experiment 3

Inducer Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.58, $HDI_{95\%}$ = [1.22, 1.92], $pd_{positive}$ > .999. There was a possible effect of PC (MI – MC), μ = -.30, $HDI_{95\%}$ =

[-.62, .01], $pd_{negative}$ = .974. There was no location-specific PC effect ($MI_{compatibility\ effect}$ – $MC_{compatibility\ effect}$), μ = .10, $HDI_{95\%}$ = [-.50, .68], $pd_{positive}$ = .614.

Diagnostic Proximal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.60, $HDI_{95\%}$ = [1.05, 2.13], $pd_{positive}$ > .999. There was no effect of near PC (near MI – near MC), μ = -.17, $HDI_{95\%}$ = [-.50, .21], $pd_{negative}$ = .831. There was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = -.39, $HDI_{95\%}$ = [-1.08, .32], $pd_{negative}$ = .860.

Diagnostic Distal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.80, $HDI_{95\%}$ = [1.34, 2.31], $pd_{positive}$ > .999. There was no effect of near PC (near MI – near MC), μ = -.34, $HDI_{95\%}$ = [-.87, .16], $pd_{negative}$ = .909. There was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = .72, $HDI_{95\%}$ = [-.24, 1.71], $pd_{positive}$ = .666.

Experiment 4a

Inducer Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 2.02, $HDI_{95\%}$ = [1.53, 2.51], $pd_{positive}$ > .999. There was an effect of PC (MI – MC), μ = -.57, $HDI_{95\%}$ = [-1.02, -.18], $pd_{negative}$ = .997. There was no location-specific PC effect ($MI_{compatibility\ effect}$ – $MC_{compatibility\ effect}$), μ = -.38, $HDI_{95\%}$ = [-1.14, .38], $pd_{negative}$ = .838.

Diagnostic Proximal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.87, $HDI_{95\%}$ = [1.26, 2.46], $pd_{positive}$ > .999. There was a likely effect of near PC (near MI – near MC), μ = -.58, $HDI_{95\%}$ = [-1.17, -.05], $pd_{negative}$ = .983. There was no location-specific PC effect (near $MI_{compatibility\ effect}$ – near $MC_{compatibility\ effect}$), μ = .03, $HDI_{95\%}$ = [-.98, 1.05], $pd_{positive}$ = .520.

Diagnostic Distal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.36, $HDI_{95\%}$ = [.73, 2.01], $pd_{positive}$ > .999. There was a possible effect of near PC (near MI – near MC), μ = -.49, $HDI_{95\%}$ = [-1.07, .09], $pd_{negative}$ = .959. There was no location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = .31, $HDI_{95\%}$ = [-.69, 1.36], $pd_{positive}$ = .717.

Experiment 4b

Inducer Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 2.24, $HDI_{95\%}$ = [1.78, 2.69], $pd_{positive}$ > .999. There was an effect of PC (MI – MC), μ = -.52, $HDI_{95\%}$ = [-.95, -.12], $pd_{negative}$ = .998. There was no location-specific PC effect ($MI_{\text{compatibility effect}}$ – $MC_{\text{compatibility effect}}$), μ = .01, $HDI_{95\%}$ = [-.73, .78], $pd_{negative}$ = .518.

Diagnostic Proximal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 1.78, $HDI_{95\%}$ = [1.13, 2.57], $pd_{positive}$ > .999. There was a possible effect of near PC (near MI – near MC), μ = -.45, $HDI_{95\%}$ = [-.97, .07], $pd_{negative}$ = .966. There was no location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = -.59, $HDI_{95\%}$ = [-1.49, .30], $pd_{negative}$ = .909.

Diagnostic Distal Locations

A flanker compatibility effect (Incongruent – Congruent) was observed, μ = 2.08, $HDI_{95\%}$ = [1.38, 2.79], $pd_{positive}$ > .999. There was no effect of near PC (near MI – near MC), μ = -.27, $HDI_{95\%}$ = [-.81, .30], $pd_{negative}$ = .836. There was no location-specific PC effect (near $MI_{\text{compatibility effect}}$ – near $MC_{\text{compatibility effect}}$), μ = .33, $HDI_{95\%}$ = [-.65, 1.32], $pd_{positive}$ = .752.

Appendix D: Flanker Stimuli Frequencies

Table 7

Combined Frequencies of Flanker Stimuli for Inducer Locations

	Distractor							
	MC				MI			
Target	^^^^	vvvv	<<<<	>>>>	^^^^	vvvv	<<<<	>>>>
^	21	1	1	1	3	7	7	7
v	1	21	1	1	7	3	7	7
<	1	1	21	1	7	7	3	7
>	1	1	1	21	7	7	7	3

Note. These are the combined frequencies of both inducer locations (both bottom left or both top right) for MC and MI conditions.

Table 8

Frequencies of Flanker Stimuli for Each Diagnostic Location

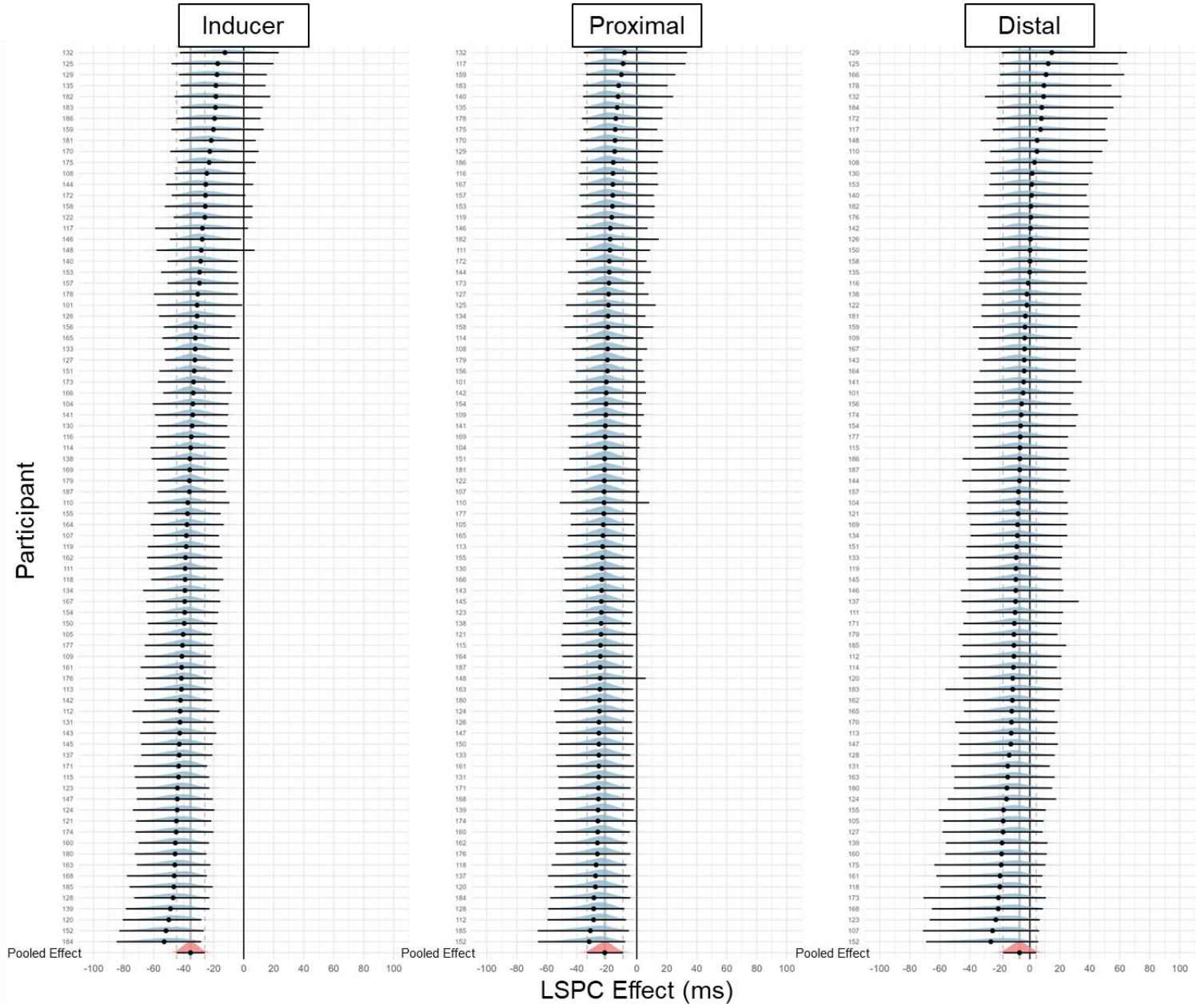
	Distractor			
Target	^^^^	vvvv	<<<<	>>>>
^	6	2	2	2
v	2	6	2	2
<	2	2	6	2
>	2	2	2	6

Note. These are the frequencies for each of the four diagnostic locations (i.e., near MC proximal, near MC distal, near MI proximal, and near MI distal).

Appendix C: Forest Plots of LSPC Effects

Figure 16

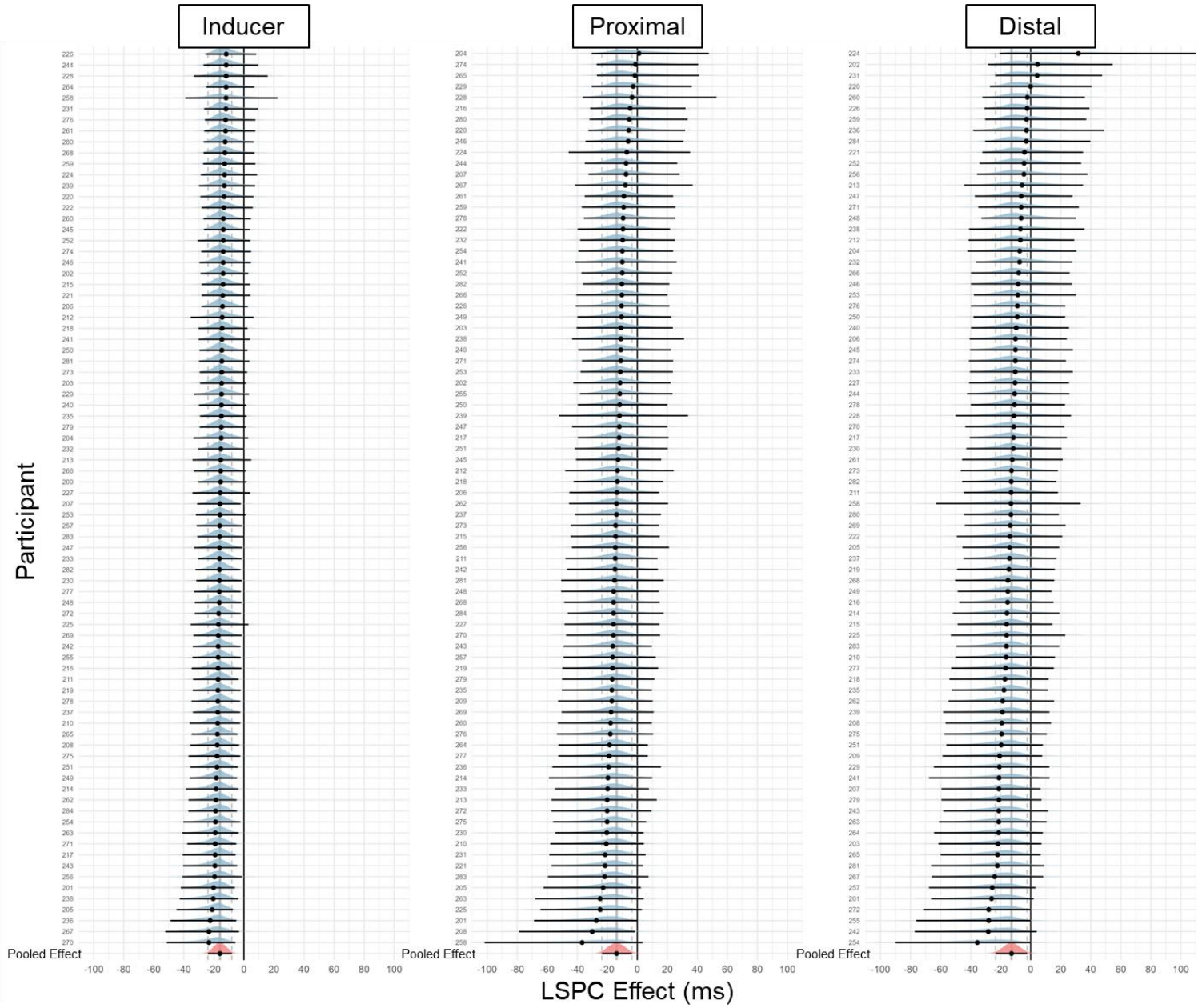
Forest Plot of LSPC Effects for Experiment 1



Note. Depicted for each participant from Experiment 1 is a posterior distribution with a mean (solid black dot) and 95% credible interval (black lines around the mean) of their LSPC effect for a given location type. The pooled LSPC effects from the entire sample (n = 82) are shown in the last row in red.

Figure 17

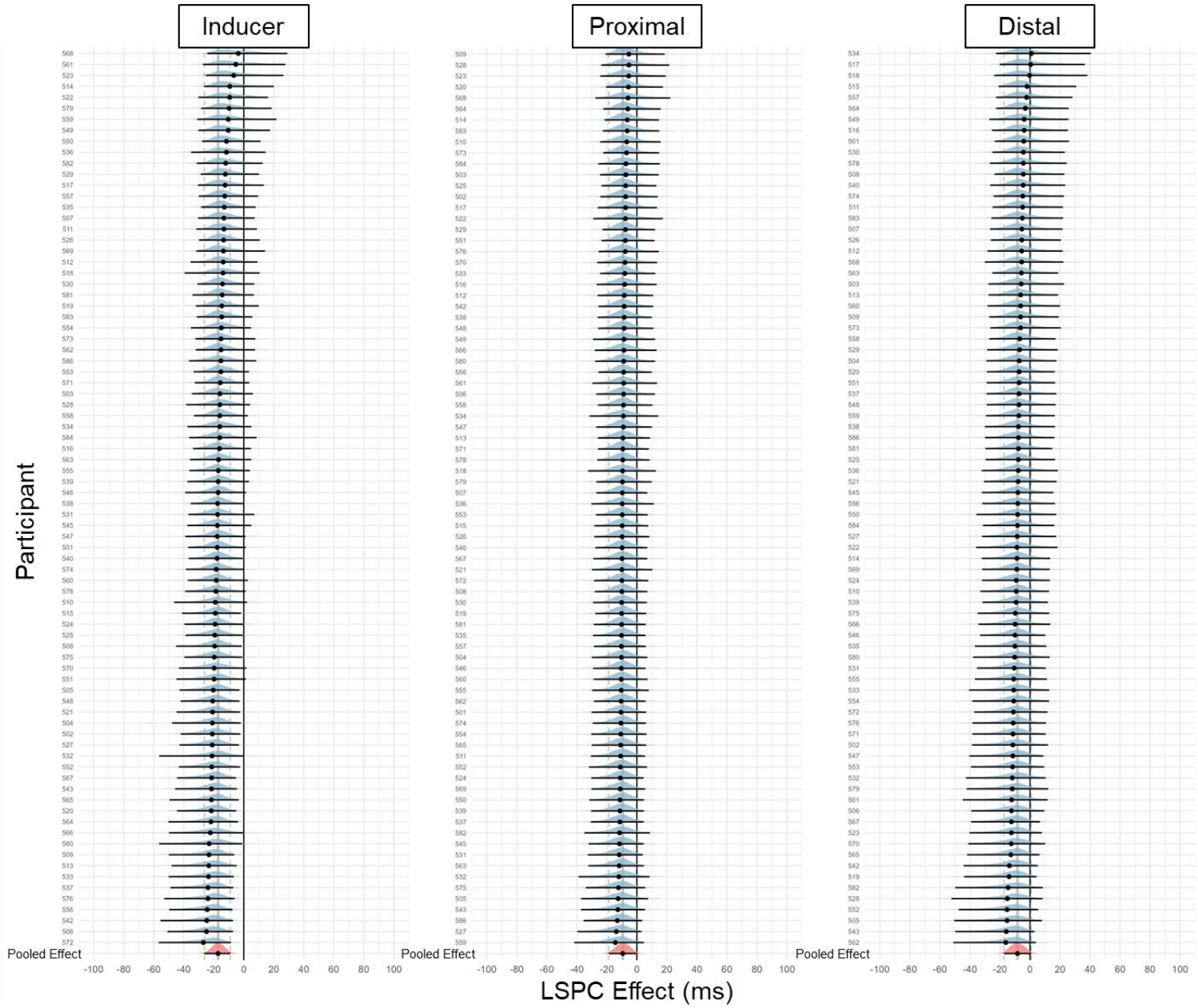
Forest Plot of LSPC Effects for Experiment 2



Note. Depicted for each participant from Experiment 2 is a posterior distribution with a mean (solid black dot) and 95% credible interval (black lines around the mean) of their LSPC effect for a given location type. The pooled LSPC effects from the entire sample (n = 82) are shown in the last row in red.

Figure 18

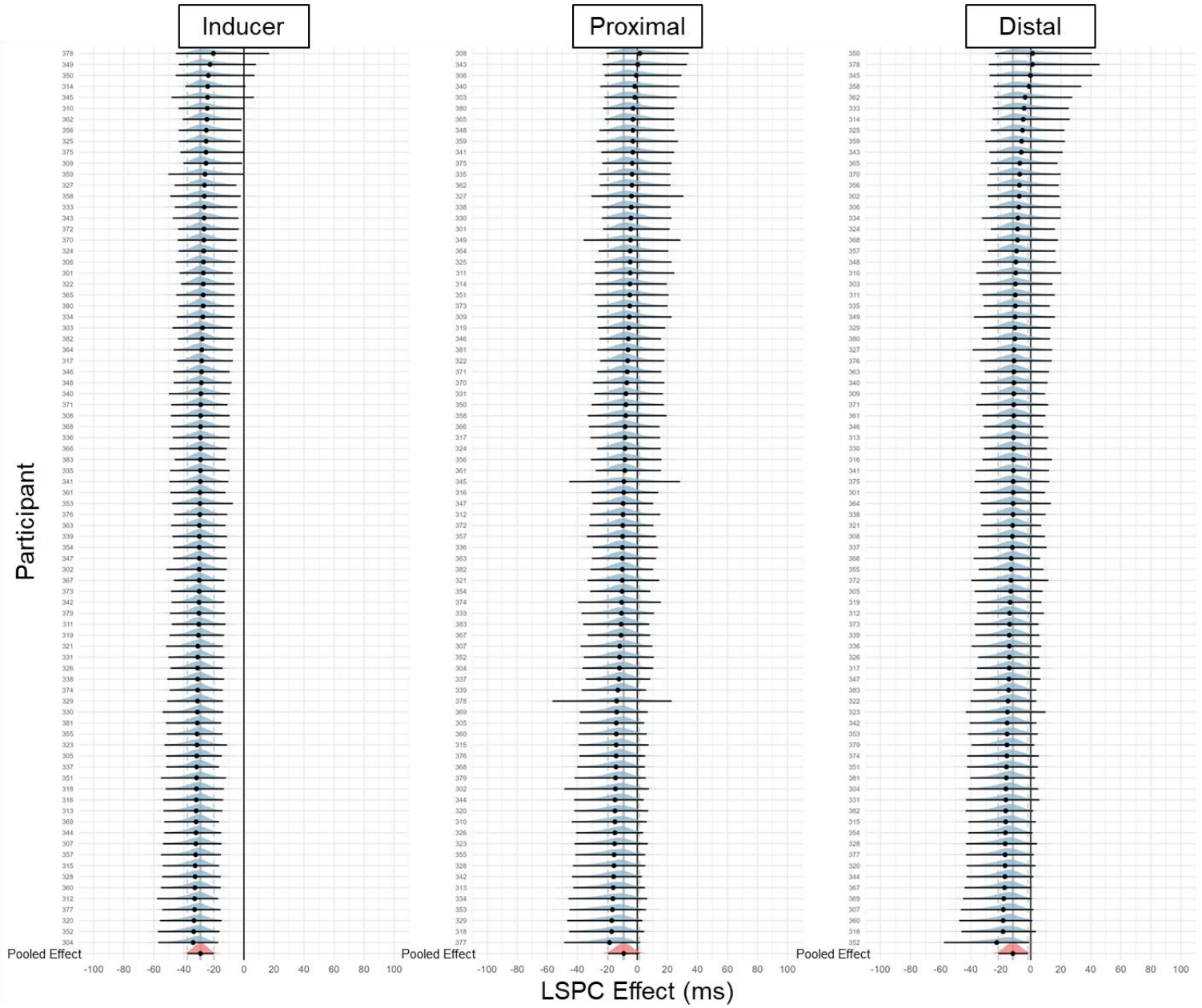
Forest Plot of LSPC Effects for Experiment 3



Note. Depicted for each participant from Experiment 3 is a posterior distribution with a mean (solid black dot) and 95% credible interval (black lines around the mean) of their LSPC effect for a given location type. The pooled LSPC effects from the entire sample (n = 82) are shown in the last row in red.

Figure 19

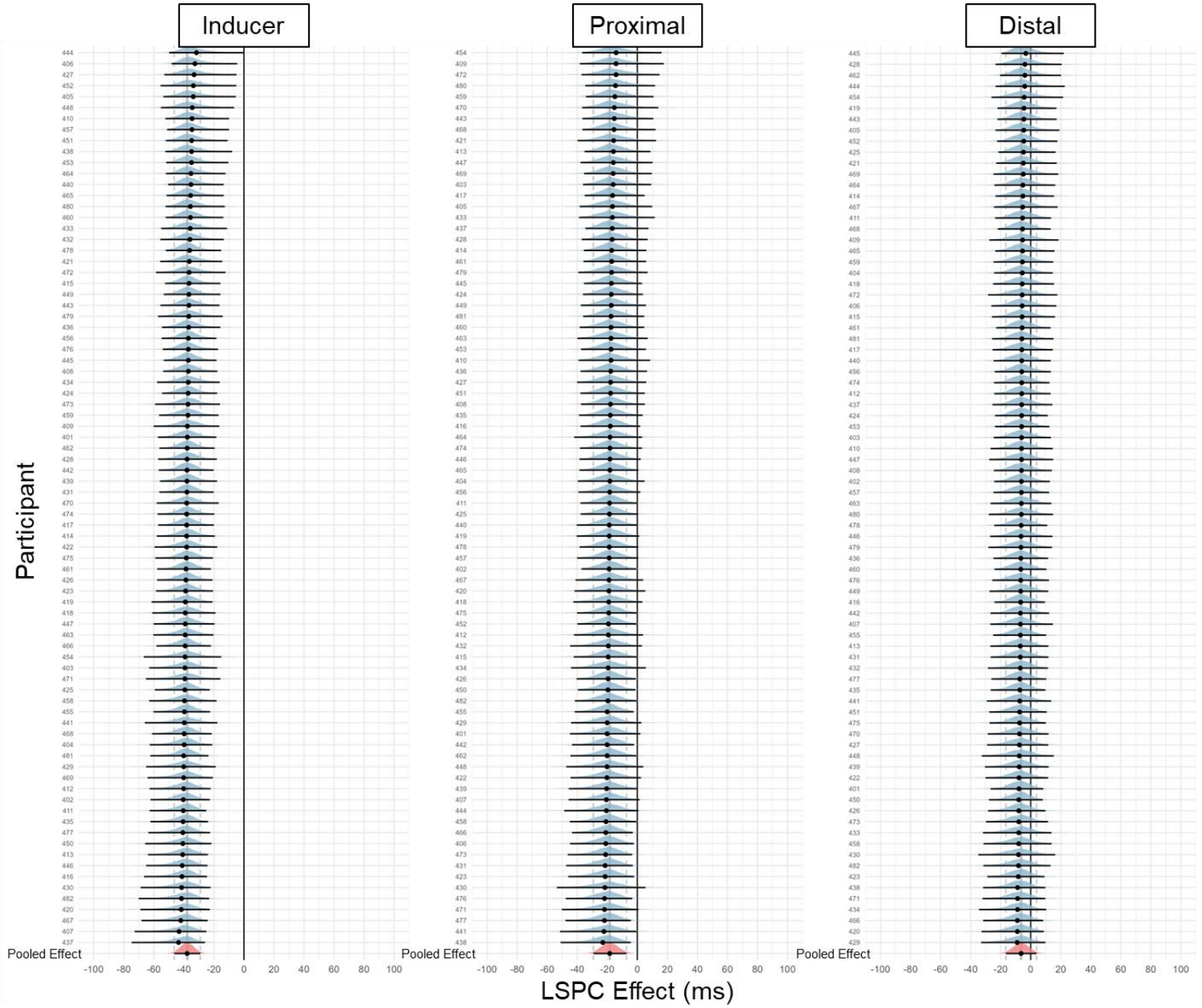
Forest Plot of LSPC Effects for Experiment 4a



Note. Depicted for each participant from Experiment 4a is a posterior distribution with a mean (solid black dot) and 95% credible interval (black lines around the mean) of their LSPC effect for a given location type. The pooled LSPC effects from the entire sample (n = 82) are shown in the last row in red.

Figure 20

Forest Plot of LSPC Effects for Experiment 4b



Note. Depicted for each participant from Experiment 4b is a posterior distribution with a mean (solid black dot) and 95% credible interval (black lines around the mean) of their LSPC effect for a given location type. The pooled LSPC effects from the entire sample (n = 82) are shown in the last row in red.