

Carving Up Space at Imaginary Joints: Can People Mentally Impose Arbitrary Spatial Category Boundaries?

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Empirical attempts to understand connections between abstract cognition and sensori-motor processes have pointed toward an embodied view of cognition, where cognitive activity is strongly tied to sensori-motor activity. Here the authors test the ability of the cognitive system to impose structure on the world using a well-established phenomenon in spatial cognition—biases near spatial category boundaries. Results from 5 experiments suggest that participants were unable to mentally impose a spatial category boundary without perceptual support, even when explicitly instructed to do so. The authors conclude by considering the implications of these findings for abstraction within other domains of cognition.

Keywords: embodied cognition, spatial recall biases, categorization

Human beings lead rich mental lives that often seem far removed from the sensori-motor world. For instance, scholars have made fundamental contributions to knowledge by imagining impossible events (e.g., traveling at the speed of light; Einstein, 1987) and by contemplating abstract concepts like truth, morality, and respect (Uzgalis, 2005). Although these examples clearly reflect the abstract nature of thought, recent evidence suggests that humans' rich mental lives are more grounded in the sensori-motor world than appears at first blush. For instance, Richardson, Spivey, Barsalou, and McRae (2003) demonstrated implicit spatial orientations for words like *respect* and *admire*. In this study, participants listened to sentences like "The athlete admired the coach" across study trials. At test, participants were faster to verify pictures depicting key elements of these sentences (e.g., an athlete and a coach) when the pictures were aligned consistently with the spatial implication of the verb (i.e., with the coach above the athlete). Results like these suggest that even abstract terms carry some implicit sensory information.

Empirical attempts to address the sensori-motor foundations of abstract cognition have pointed toward an embodied view of cognition, where cognitive activity is intricately and obligatorily connected to sensori-motor activity (e.g., Barsalou, 1999; Gibson, 1966; Glenberg, 1997; Thelen & Smith, 1994). But is abstract cognition always tied to the sensori-motor world? Can people, for instance, mentally impose abstract concepts onto the world in

completely arbitrary ways, that is, unsupported by sensori-motor activity? As is evident from the discussion above, seemingly abstract and arbitrary concepts may be linked to sensori-motor systems in subtle ways (see also Barsalou & Wiemer-Hastings, 2005; Spivey, Richardson, & Fitneva, 2004). This makes it challenging to address the mental imposition question by studying such concepts.

A different approach is to examine mental abilities in tasks that are more closely tied to the sensori-motor system, with the goal being to control and eliminate the reliance on sensori-motor cues. Nautical navigation may provide such a case: Here heading must be maintained without any visible landmarks for guidance (e.g., Hutchins, 1995; Newcombe & Huttenlocher, 2000). In one well-studied population, Puluwat seamen have been reported to mentally impose virtual islands to aid in their navigation (Gladwin, 1970; Hutchins, 1995). These virtual islands are not grounded within the visual array—indeed, their purpose is to provide cues when no others are available. As such, the initial location of the virtual island can be considered arbitrary with regard to the current sensory array (though, of course, it is not arbitrary with regard to the seaman's goal—to get home!). Closer inspection reveals, however, that this abstract ability resides in real-time sensori-motor updating. The type of spatial coding used in the seaman example is referred to as *dead reckoning* and relies on information from movement of the self, rather than on visible cues in the environment (Newcombe & Huttenlocher, 2000). Although dead reckoning is abstracted from visual sensory information, it relies heavily on other sensori-motor cues to determine heading and to update on the basis of extent and direction of self-motion.

This example reveals, once again, the challenges of investigating whether people can mentally impose arbitrary structure onto the world: The sensori-motor system is richly structured, making it difficult to determine the extent to which cognition is abstracted away from sensori-motor experience. Clearly, then, one must have exquisite control over the multiple sources of potential sensori-motor influence to examine mental imposition abilities. Note that this challenge actually highlights a central point of the embodied

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cognition view: Cognitive activity that is grounded in the sensori-motor world can be quite complex because sensori-motor experience is so richly structured.

The goal of this study was to use a well-studied phenomenon in spatial cognition—biases near spatial category boundaries—to examine whether people can mentally impose arbitrary divisions on the world. We selected spatial category boundaries because there is a recognized measure of category boundary use that has been established within a rich empirical literature. Moreover, we have exquisite control over the contexts in which people use category boundaries. Thus, spatial category boundaries can provide a simple test case in which to examine how closely abstract, arbitrary concepts must be tied to the sensori-motor structure of the world.

Spatial Category Boundaries

Use of spatial category boundaries has typically been assessed in spatial recall tasks in which participants see a target location, there is a short delay (e.g., 5–20 s), and then participants reproduce the target location. When targets are presented to the left or right of a category boundary, responses are biased away from the boundary over delay with an associated delay-dependent increase in response variability (e.g., Spencer & Hund, 2002). Moreover, there is a reduction in response variability very near or aligned with the boundary. This pattern of performance is pervasive across a range of situations (e.g., Huttenlocher, Hedges, & Duncan, 1991; Spencer & Hund, 2002) and is an accepted index of spatial categorization.

Most empirical studies of spatial recall have used category boundaries that are aligned with perceptual features of the task space, like edges or symmetry axes. For instance, studies have shown that adults' recall responses are biased away from the visible edges of *L*- and *V*-shaped frames as well as their symmetry axes (Engebretson & Huttenlocher, 1996; Schiano & Tversky, 1992, Experiments 1 and 2; Tversky & Schiano, 1989). Similarly, older children's and adults' responses are biased away from the edges and midline symmetry axis of a small rectangular piece of paper (Huttenlocher, Newcombe, & Sandberg, 1994; Spencer, Simmering, & Schutte, 2006). Huttenlocher and colleagues have demonstrated that adults' recall responses are biased away from the vertical and horizontal symmetry axes of the paper or screen on which a circle is presented (Huttenlocher, Hedges, Corrigan, & Crawford, 2004; Huttenlocher et al., 1991; Sandberg, Huttenlocher, & Newcombe, 1996). Spencer and colleagues have shown that adults' responses are biased away from the midline symmetry axis of a large table (Spencer & Hund, 2002). Finally, in a similar task, Plumert and Hund (2001) found that adults' memory for object locations is biased away from the vertical and horizontal symmetry axes of a large square box.

Adults' use of edges in this collection of studies clearly reflects cognitive activity grounded in the perceptual array because edges are visible. But what about the use of symmetry axes? Are adults mentally imposing category boundaries in such cases? After all, symmetry axes are not visible per se. Evidence from the perceptual literature suggests that adults' use of symmetry axes is grounded in the same perceptual processes used to perceive visible edges. Wenderoth and van der Zwan (1991) used orientation illusions to show that symmetry axes are perceived like weak visible lines.

Moreover, the accuracy with which people perceive symmetry axes scales with the salience of visible cues (Beh, Wenderoth, & Purcell, 1971; Li & Westheimer, 1997). For instance, increasing the salience of a symmetry axis by adding perceptual cues that specify symmetry properties (e.g., placing two dots on an axis) increases the strength of category boundary effects in perceptual tasks such as location discrimination (Simmering, Spencer, & Schöner, 2006; for related effects, see Diedrichsen, Werner, Schmidt, & Trommershauser, 2004; Li & Westheimer, 1997; Werner & Diedrichsen, 2002; Werner & Schmidt, 2000). Finally, data suggest that symmetry axes have a special developmental status in that they are accurately perceived across the lifespan. Studies have shown that 4-month-old infants are capable of perceiving vertical symmetry (Bornstein, Krinsky, & Benasich, 1986; Quinn, 2000) and adults are capable of perceiving vertical, horizontal, and oblique symmetry axes (Beh et al., 1971; Hartley, 1982; Palmer & Hemenway, 1978; Wenderoth, 1997).

Studies with adults also show that vertical symmetry axes are perceived more robustly than horizontal axes, which are in turn stronger than diagonal axes (Palmer & Hemenway, 1978). This is likely the reason that adults only occasionally show bias away from diagonal symmetry axes (e.g., Schiano & Tversky, 1992; Tversky & Schiano, 1989). Thus, although there is clearly a sense of mental imposition in people's use of symmetry axes in spatial recall tasks, such results do not provide the strong evidence of arbitrary mental imposition one would need to support claims that cognition is abstracted away from the sensori-motor world.¹ Indeed, use of symmetry axes bears some similarity to the case of the Puluwat seaman discussed above: In both cases, people are mentally imposing some structure on the world, but in both cases, this structure is grounded in sensori-motor activity.

Overview of the Present Study

Because spatial category biases are pervasive and clearly defined, they provide a unique opportunity to probe the link between abstract cognition and the sensori-motor world. In particular, in the present study, we asked whether participants could mentally impose an arbitrary spatial category boundary in otherwise empty space, that is, in a region of space devoid of both visible edges and symmetry axes. Furthermore, we used a task setting where proprioceptive cues fail to support spatial subdivision, thereby eliminating this contribution to the spatial cognitive system.

The details of our task and setting stem from two recent experiments using our spaceship task (Spencer & Hund, 2002). In the first experiment, we demonstrated that participants will use a

¹ There is some evidence in the literature that suggests biases away from category boundaries are not as universal as described here. For instance, Nelson and Chaiklin (1980) and Laeng, Peters, and McCabe (1998) reported that reproductions of dot locations tended to be displaced toward the visible edge of a circle or oval rather than away from the edge. Close inspection of these data reveal, however, that, in both cases, biases appear to move away from the center of the circle toward an intermediate radius approximately 70% of the radial distance between the center of the circle-oval and its edge, with accurate performance near the visible edge. This is consistent with findings from Huttenlocher et al. (1991). Similarly, recent studies from our lab showed accurate memory performance for targets near highly salient boundaries, as is provided by the visible edge of the circle (Lipinski, Spencer, & Samuelson, 2005; Spencer & Simmering, 2002).

perceptually specified virtual axis placed in otherwise empty space as a category boundary when the axis is visible but will use a midline symmetry axis otherwise (Simmering & Spencer, 2007; Spencer & Simmering, 2002). Participants were asked to remember the location of a spaceship on a large homogeneous table, shown schematically in Figure 1. In one condition, the spaceships were presented without nearby perceptual support (see Figure 1A). Responses to both targets in this case were biased clockwise, away from the midline symmetry axis. In another condition (usually in a separate session), two reference dots were presented, specifying a virtual line that could be used as a category boundary between the targets (parallel to the midline symmetry axis; see Figure 1B). Now responses to the left target flipped and were biased counterclockwise or away from the new boundary specified by the reference dots. Additionally, errors to both targets were smaller and less variable in this condition—the presence of the category boundary helped stabilize spatial memory. Thus, in this experiment, participants were able to use a spatial category boundary specified by relatively sparse perceptual cues when such cues were visible.

Results from a second recent experiment demonstrate that the lateral axis shown in Figure 1 is not supported by the proprioceptive system in our task setting. We asked participants to respond to visually specified targets similar to those shown in Figure 1, at the same delays using the same type of response (moving a mouse cursor) but in complete darkness (Peterson, Darling, Simmering, &

Spencer, 2003; Simmering, Peterson, Darling, & Spencer, 2007). Thus, in this task, the only stable reference frames participants could use to subdivide space were provided by the proprioceptive system, that is, participants' perception of where they were in space. Under these conditions, participants showed some evidence of subdividing space based on perception of body midline. We also found an influence of a hand-centered reference frame. Critically, however, there was no evidence that people could use proprioceptive cues to subdivide space around the lateral axis shown in Figure 1B, even when we aligned this axis with the shoulder.

The present study built on these recent findings, examining whether people can mentally impose the virtual axis in Figure 1B when it is not visually specified and use this axis as a spatial category boundary. In particular, we compared participants' performance when the reference dots were present with performance when the dots were absent. If participants can mentally impose this boundary, then responses to the left and right targets should be biased away from the lateral axis over delays on all trials, with relatively low variability. If, however, participants cannot mentally impose the axis when the dots are not visible, then responses should be biased away from the lateral axis when the dots are present with relatively low variability and away from midline (i.e., clockwise) with higher variability when the dots are not visible. Note that responses to the left target provide the most critical test of mental imposition given that the absence of mental imposition should lead to a directional shift in responses across conditions (vs. the change in magnitude of clockwise bias for the right target). This highlights an important aspect of our experimental design: We can detect both the presence and absence of mental imposition without predicting a null effect.

An important component of the question raised in this study is whether people can impose an arbitrary boundary. In what sense is the boundary in Figure 1B arbitrary? We consider the boundary in Figure 1B arbitrary because the virtual axis is not supported by any perceptual cues when the reference dots are not visible. Thus, the precise location of the boundary is arbitrary with respect to the current sensory array. In some ways, this is similar to the example of the Puluwat navigators imposing islands that are out of view on the basis of their memory of the islands' layout. However, our paradigm differs in three important ways from the Puluwat example. First, we are probing the use of spatial category boundaries to subdivide space and are not probing the use of mentally imposed reference points. Second, evidence suggests that the Puluwat seamen use self-motion cues to specify imagined locations, whereas our recent evidence suggests people do not use proprioceptive cues to specify the location of the lateral category boundary in our task. Third, we are probing a type of mental imposition that could only be formed over a relatively short period of time—the course of the session—rather than over years of navigational experience.

That said, there is the potential for learning-related effects in the present study. Specifically, when participants repeatedly respond to the same locations over the course of a session, long-term memory for these locations builds up, which can result in more accurate responding (i.e., lower mean error and variability; see, e.g., Hund & Spencer, 2003; Schutte, Spencer, & Schöner, 2003; Spencer & Hund, 2002; Spencer, Smith, & Thelen, 2001). Although such effects could potentially reduce the magnitude of spatial category biases (see Hund & Spencer, 2003, for a particularly dramatic example of this), we included repeated trials to the

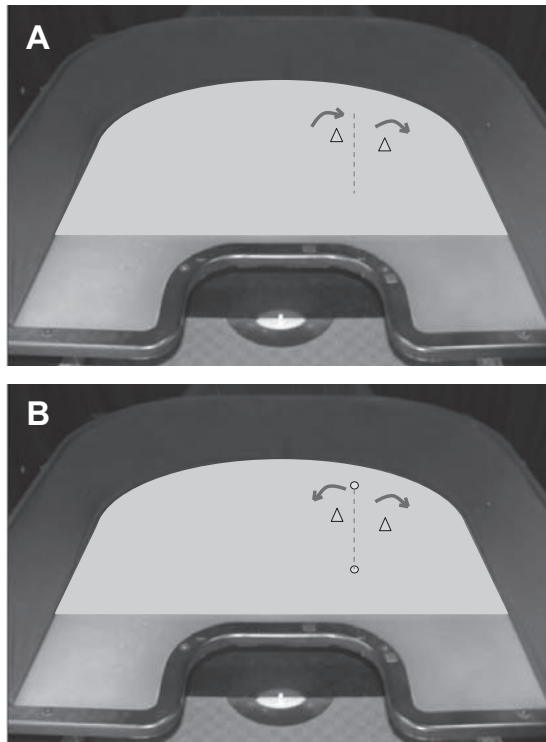


Figure 1. Apparatus and targets used by Spencer and Simmering (2002). Dotted lines are added here to indicate the boundary between targets and were not visible in the task; arrows indicate direction of memory bias. A: With no boundary defined, both targets were biased away from the midline symmetry axis of the table. B: When the boundary was defined by two dots, responses to the left target flipped direction of bias.

same target locations with the same reference cues to maximize the likelihood that participants could mentally impose a category boundary. Such repetition should help participants detect the advantage of using the boundary (i.e., lower variability) and, ultimately, impose it.

We conclude by noting that because we are probing a clearly defined pattern of bias—delay-dependent bias away from boundaries with an associated increase in variability over delays and lower response variability very near the boundary—we can ask a more subtle question than simply whether people can or cannot impose a boundary in our task: If people are able to mentally impose a boundary, can they do this with the same precision seen with perception? That is, can people achieve the same benefits from the cognitive system as they achieve from perception?

Experiment 1

This experiment examined whether people can mentally impose the virtual axis in Figure 1B when it is not visually specified and use this axis as a spatial category boundary. Participants completed six blocks of trials with alternating display conditions within a single session. During reference condition (Ref) blocks, we presented visible cues (two dots) that specified the virtual axis in Figure 1B. We expected responses in these blocks to show categorical biases away from the virtual axis, with relatively low variability given the stabilizing influence of the category boundary (Spencer & Simmering, 2002). The critical question was whether participants would show the same response pattern during no-reference condition (NoRef) blocks when we removed the visible cues. If participants can mentally impose this arbitrary category boundary, then they should show the same response pattern during NoRef blocks—biases away from the category boundary with low variability. If, however, participants cannot impose an arbitrary category boundary, then responses should be biased away from the midline symmetry axis—the nearest perceptually specified reference frame—with higher variability. Responses to the left target should show particularly dramatic evidence of the absence of an arbitrary mentally imposed boundary. If participants cannot impose the boundary, then response biases to the left target should switch directions across the two display conditions: counterclockwise in the Ref condition and clockwise in the NoRef condition.

Note that participants received feedback at the end of each trial in this task: The target was reilluminated after each response so participants could compare their recall response with the actual target location. In addition, participants received points based on their accuracy. Given that our previous work has shown a reduction in response bias and variability near category boundaries (Spencer & Simmering, 2002), we expected this feedback would encourage participants to mentally impose an arbitrary boundary—they would see that their performance was more accurate when the visible cues were present and would attempt to compensate by imagining the virtual axis even in the NoRef condition.

Method

Participants. Twelve adults (7 women, 5 men; mean age = 19 years 7.98 months, $SD = 1.12$ years) participated in this experiment. Participants were recruited through an introductory psychology course and received research exposure credit for their partic-

ipation. All participants were right-handed and had normal or corrected-to-normal vision.

Apparatus and stimuli. Participants sat within an arc cut out from the side of a large (0.921×1.194 m) table (see Figure 1). The table had a homogeneous gray surface (background luminance = 0.0 cd/m²), and three edges of the table were covered with a curved border to occlude the corners from participants' view. This eliminated the table's diagonal symmetry axes. Sessions were conducted in a dimly lit room with black curtains covering the walls and ceiling, which eliminated external reference cues. Stimuli were projected onto the surface of the table from below using a Sony VPL-PX LCD projector.

Two types of images were used: blue equilateral triangle spaceships (13-mm base) and yellow reference dots (8-mm diameter). Stimuli were presented in a 15 cm deep \times 22.5 cm wide area centered 15 cm to the right of the midline of the tabletop (see Figure 1). Targets were presented at two locations on test trials: -10° and 25° relative to the virtual axis at a distance of 15 cm from the start location (the location of the front reference dot; see Figure 1). These targets match the targets used in Spencer and Simmering (2002). Targets were presented with one of two display conditions. In the NoRef condition, no perceptual information marked the lateral axis (see Figure 1A). In the two-dot reference (Ref) condition, two yellow reference dots were added along the axis, 30 cm apart (see Figure 1B).

Participants placed their heads in a chin rest to ensure consistent viewing angle and distance throughout the sessions. Participants responded with a wireless mouse located on a black mouse pad in the bottom right corner of the table, out of their field of view. A low-light video camera was positioned behind the table, facing the participant, at a distance of approximately 2.2 m. This camera was connected to a monitor for the experimenter to view outside of the testing area to ensure that participants were complying with the task instructions. Because of the curtains and dim lighting in the room, most participants could not see the camera.

Procedure. Participants were seated at the table and positioned at the chin rest. The experimenter explained the spaceship game to participants and played a demonstration trial to explain the task. Demonstration trials were repeated as necessary until the participant understood the task, although most participants needed only one. Once participants understood the task, a warm-up game familiarized them with the movement of the mouse. For this game, a grid of white dots appeared on the tabletop. Every 4 s, participants heard a tone and a spaceship appeared on a randomly selected dot. Participants moved a set of crosshairs with the mouse to follow the spaceship. The warm-up game lasted 2 min.

Next, the practice trials began. Each trial began when the computer stated, "Beginning search for enemy spaceships." At the same time, the reference dots appeared (when applicable) and remained on until the trial ended and the feedback screen appeared (see below). After a random pretrial delay, a target spaceship appeared for 2 s and then disappeared. To prevent fixation on the location during the delay, we instructed participants to look up from the table after the spaceship disappeared until they heard "ready, set, go." The delay between when the spaceship disappeared and the go signal was 0 or 10 s and varied randomly across trials. When participants heard the start of the "ready, set, go" sequence, they looked down at the table and a set of crosshairs appeared at the start location. On go, participants moved the

crosshairs to the remembered spaceship location and clicked the left mouse button. Then, the spaceship reappeared for 1.5 s, followed by a feedback screen that was displayed for 2 s.

Four types of feedback information were displayed after each trial: (a) initiation time relative to the go signal, (b) the sum of an initiation time score and an accuracy score, (c) the total accumulated points, and (d) a flight rank and number of stars based on the total number of points. To receive the maximum initiation time score, participants had to begin moving within 40 ms of the go signal. If participants moved more than 120 ms before or after the go signal, the computer delivered a warning message and the participant received an initiation time score of 0. Note that participants were told to focus on initiation time relative to the go signal rather than movement speed. This ensured that the delay periods remained comparable across trials and participants. In addition to the initiation time score, participants received a 5-point score based on the distance between the actual target location and the location of the crosshairs when the participant clicked the mouse button. Accuracy scores ranged from 5 points for responses 0–1 cm from the target to 0 points for responses 3 cm or greater from the target. Stars were awarded every 80 points, and the flight rank increased every two stars.

Participants completed 12 practice trials with no reference dots visible. Targets for the practice trials appeared on the left side of the table, so participants would not be preexposed to the target locations used on the test trials. After practice, the experimenter answered any remaining questions about the task. Then, participants completed three blocks of test trials in each display condition. The display condition alternated across blocks, starting with a block of Ref trials to give participants experience with the perceptually supported boundary at the beginning of the session. Participants completed eight trials to each target at each of two delays (0 and 10 s) in each display condition in each of three blocks, for a total of 64 trials per block and 192 trials overall. Breaks were offered between blocks, though few participants took them. All participants took a mandatory break halfway through the session. Sessions lasted approximately 90 min. Participants were debriefed at the end of the session.

Method of analysis. For each trial, we recorded the *x*- and *y*-coordinates of the mouse crosshairs when the participant pressed the left mouse button. Then, we computed the angular separation between the vector from the start location to the response and the vector from the start location to the actual target location (in degrees). These directional errors were computed such that positive errors were clockwise and negative errors were counterclockwise. Directional errors were checked for outliers by removing trials on which errors were greater than $\pm 50^\circ$. Three outlier trials (0.13% of all trials) were removed. Mean directional errors were computed by averaging across trials to the same target, delay, and display condition in each block. Directional error variability was computed as the corresponding standard deviation.

Results

Mean directional error. Figure 2A shows mean directional errors to each target at each delay across display conditions and blocks. As this figure shows, responses at the 10-s delay were dramatically affected by the display condition. Errors to the left target (-10°) were counterclockwise in the Ref condition but

clockwise in the NoRef condition. Errors to the right target (25°) generally showed the same sawtooth pattern across conditions, although the direction of error was always clockwise. Of note, these influences of the display were only present at the 10-s delay; performance at no delay was accurate.

We analyzed these data using a four-way analysis of variance (ANOVA), with target (-10° , 25°), delay (0, 10 s), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed significant main effects of target, $F(1, 11) = 17.65$, $p < .01$, and delay, $F(1, 11) = 6.65$, $p < .05$, which were subsumed by a significant Target \times Delay interaction, $F(1, 11) = 21.78$, $p < .01$. This interaction replicates the basic finding of delay-dependent spatial bias away from category boundaries (Spencer & Hund, 2002). Tests of simple effects showed no target effects at 0 s but a significant target effect at 10 s, $F(1, 11) = 20.00$, $p < .01$. At the 10-s delay, mean errors to the 25° target were large and clockwise ($M = 4.90^\circ$), whereas mean errors to the -10° target were small and counterclockwise ($M = -0.35^\circ$). The relatively small mean errors to -10° result from averaging together clockwise (i.e., positive) and counterclockwise (i.e., negative) errors across display conditions.

The ANOVA also revealed a significant Delay \times Display Condition interaction, $F(1, 11) = 5.38$, $p < .05$. Tests of simple effects revealed a significant display condition effect at the 10-s delay, $F(1, 11) = 5.00$, $p < .05$, but not at the 0-s delay. At the 10-s delay, mean errors were lower in the Ref condition ($M = 0.69^\circ$) than in the NoRef condition ($M = 3.85^\circ$). This difference reflects the sawtooth pattern evident for both targets in Figure 2A. These relatively low errors in the Ref condition result from averaging across opposite directions of drift for the two targets.

Lastly, because the direction of responses to the -10° target at the 10-s delay was central to testing regardless of whether participants could mentally impose an arbitrary category boundary, we conducted planned comparisons to see whether errors to this target were significantly different from zero error in the Ref and NoRef conditions. The *t* tests revealed that the -10° target was significantly biased counterclockwise (i.e., away from the boundary) in the Ref condition, $t(11) = -5.47$, $p < .001$. Thus, when the reference dots were present, participants showed a robust spatial category bias to this target. This was not the case, however, when the reference dots were removed. In this case, directional errors did not differ significantly from zero error. It appears, therefore, that participants were not able to mentally impose the category boundary in this condition. Note that the clockwise bias (i.e., away from midline) in the NoRef condition evident in Figure 2A did not differ significantly from zero error. This failure to reach significance was likely due to increased variability in the NoRef condition (see below).

Directional error variability. Figure 2B shows the mean standard deviation of directional error (directional error variability) for responses to each target at each delay across display conditions and blocks. As this figure shows, variability was higher at the 10-s delay than at the 0-s delay. Variability was also higher in the NoRef condition than in the Ref condition, giving variability the same sawtooth pattern across display conditions seen in the analysis of mean directional error. Finally, there was a tendency for variability to decrease across blocks.

We analyzed these data using a four-way ANOVA, with target (-10° , 25°), delay (0, 10 s), display condition (Ref, NoRef), and

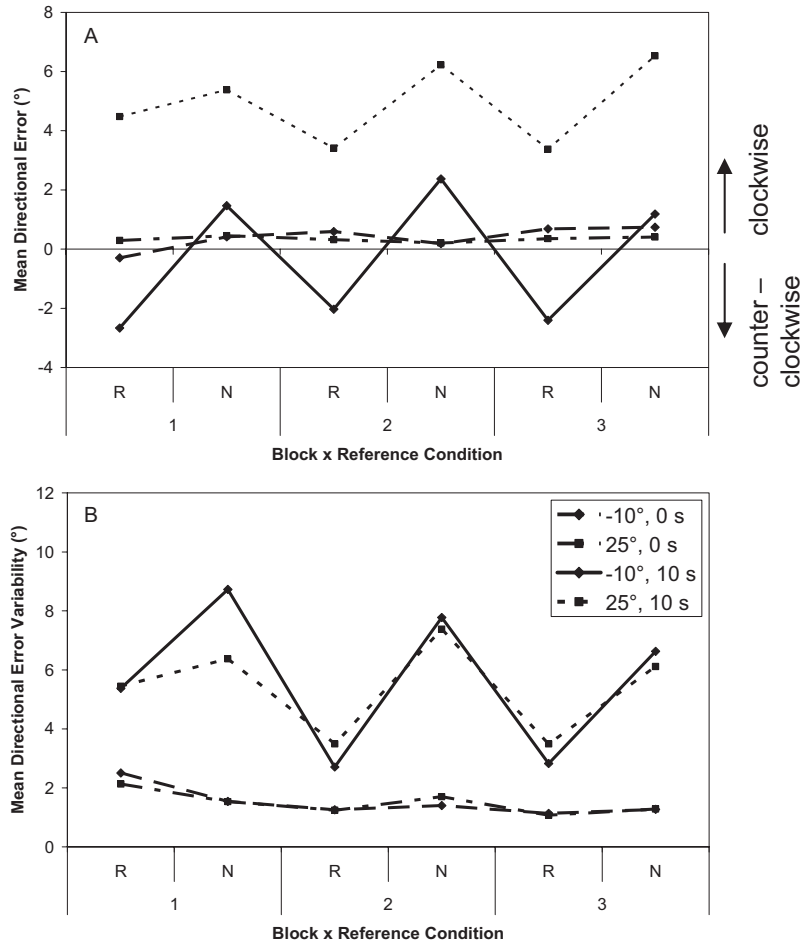


Figure 2. Mean directional error (A) and variability (B) across display conditions and blocks for Experiment 1. Positive values indicate clockwise directional errors; negative values indicate counterclockwise directional errors. R = reference condition; N = no-reference condition.

block (1–3) as within-subjects variables. This analysis revealed significant main effects delay, $F(1, 11) = 118.83, p < .001$, and display condition, $F(1, 11) = 35.54, p < .001$, which were subsumed by a significant Delay \times Display Condition interaction, $F(1, 11) = 45.61, p < .001$. Tests of simple effects revealed a significant effect of display condition at the 10-s delay, $F(1, 11) = 49.88, p < .001$, but no effect at the 0-s delay. As can be seen in Figure 2B, this interaction was driven by higher variability in the NoRef condition ($M = 7.17^\circ$) than in the Ref condition ($M = 3.89^\circ$) at the 10-s delay. This replicates our previous findings showing that the presence of reference dots has a stabilizing effect on spatial memory (Spencer & Simmering, 2002). Of note, however, participants were not able to maintain this low variability during NoRef blocks, again suggesting that they were not able to mentally impose the category boundary during these trials.

The overall ANOVA also revealed a significant Display Condition \times Block interaction, $F(2, 10) = 5.21, p < .05$. Tests of simple effects showed a significant block effect for the Ref condition only, $F(2, 22) = 7.99, p < .01$. The t tests showed that variability decreased significantly from Block 1 ($M = 3.86^\circ$) to Block 2 ($M = 2.17^\circ$), $t(11) = -2.80, p < .05$, and from Block 1 to

Block 3 ($M = 2.13^\circ$), $t(11) = -2.97, p < .05$. Blocks 2 and 3 did not differ significantly. This reduction in variability after Block 1 likely reflects an improvement in performance as participants repeatedly responded to the same two targets in the presence of cues that specified a category boundary.

Discussion

Results from the Ref condition replicated findings from Spencer and Simmering (2002). Responses to both targets were accurate at no delay and drifted away from the boundary established by the two reference dots as the delay increased. Moreover, performance was less variable in the Ref condition, showing the stabilizing influence of perceptual structure in the task space. In the NoRef condition, by contrast, biases to the left target flipped direction and drifted clockwise, that is, toward the location of the boundary and away from the midline of the table. In addition, variability increased in the NoRef condition. Thus, participants did not appear to mentally impose the boundary when it was not perceptually specified by dots in the task space. This is despite the fact that

performance was generally more accurate in the Ref condition, with consistently lower variability and smaller mean bias.

There may be two possible reasons for participants' failure to impose the boundary. First, because we used a blocked design with 32 trials per reference block, participants may have simply failed to notice that performance was more accurate in the Ref condition. It might be difficult to accumulate the results of feedback across such a large number of trials; therefore, participants may not have tried to impose the category boundary when it was not visibly specified. Second, even if participants were trying to impose the boundary, it might be too difficult for them to consistently mentally impose an arbitrary category boundary across 32 NoRef trials in the absence of perceptual support. Thus, rather than showing that participants cannot impose an arbitrary category boundary, these results might demonstrate that this ability depends on the details of the task. In the next experiment, we tested this possibility by changing the reference display from trial to trial.

Experiment 2

Results from Experiment 1 suggest that participants did not mentally impose a lateral category boundary when it was not visually specified. This failure to impose the boundary may simply indicate that the conditions used in Experiment 1 were very challenging. In particular, because trials were blocked by display condition, participants had to accumulate the results of feedback across 32 consecutive Ref trials and then systematically impose a category boundary across 32 consecutive NoRef trials. If participants had difficulty with either aspect of the task, then randomly interspersing Ref and NoRef trials should lead to evidence of mental imposition. This change in the design should help participants notice the adaptive benefits of using the category boundary because they can compare feedback across the two display conditions on a trial-by-trial basis. Moreover, this change in the design should make the mental imposition task easier. Indeed, on some trials, participants will only have to carry over the reference display seen on the previous trial and impose it on the current trial.

Method

Participants. Thirteen adults (9 women, 4 men; mean age = 26 years 7.33 months; $SD = 10.68$ years) participated in this experiment. Participants were recruited through an introductory psychology course and received research exposure credit for their participation. All participants were right-handed and had normal or corrected-to-normal vision.

Apparatus, stimuli, and procedure. All details of the apparatus, stimuli, and procedure were identical to those in Experiment 1 with one exception. Instead of blocking trials by display condition, we randomly intermixed these trials within each of the three blocks, with 64 trials per block (maintaining the total number of trials from Experiment 1).

Method of analysis. The method of analysis was identical to Experiment 1. Nine outlier trials (0.31% of all trials) were excluded. An additional three trials (0.10% of all trials) had no response because of an equipment problem and were excluded. Note that although trials with different display conditions were randomly intermixed within blocks, we still grouped trials by display condition within each of three blocks for analysis to facilitate comparison with Experiment 1.

Results

Mean directional error. Figure 3A shows mean directional errors to each target at each delay across display conditions and blocks. As in Experiment 1, responses at the 10-s delay showed a sawtooth pattern across display conditions, despite the fact that we randomly intermixed Ref and NoRef trials within each block. Errors to the -10° target were clockwise in the NoRef condition but counterclockwise in the Ref condition, and errors to the 25° target decreased in magnitude in the Ref condition. Of note, these influences of the reference were only present at the 10-s delay; performance at no delay was generally accurate.

We analyzed these data using a four-way ANOVA, with target (-10° , 25°), delay (0, 10 s), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed significant main effects of target, $F(1, 12) = 19.86$, $p < .01$, and delay, $F(1, 12) = 20.03$, $p < .01$, which were subsumed by a significant Target \times Delay interaction, $F(1, 12) = 12.70$, $p < .01$. Tests of simple effects showed significant target effects at both 0 s, $F(1, 12) = 10.54$, $p < .01$, and at 10 s, $F(1, 12) = 16.37$, $p < .01$. At the 0-s delay, mean errors were slightly clockwise to the 25° target ($M = 0.27^\circ$) and were slightly counterclockwise to the -10° target ($M = -0.08^\circ$). At the 10-s delay, errors to 25° were large and clockwise ($M = 3.13^\circ$) but still small and counterclockwise to -10° ($M = -0.11^\circ$). These effects were comparable with performance in Experiment 1 with the exception of the small but systematic biases at 0 s.

The overall ANOVA also revealed a significant main effect of display condition, $F(1, 12) = 18.14$, $p < .01$, along with significant two-way interactions of Delay \times Display Condition, $F(1, 12) = 19.67$, $p < .01$, and Delay \times Block, $F(2, 11) = 4.03$, $p < .05$. These effects were subsumed by a significant Delay \times Display Condition \times Block interaction, $F(2, 11) = 5.76$, $p < .05$. Tests of simple effects revealed significant Display Condition \times Block interactions at both the 0-s delay, $F(2, 24) = 4.31$, $p < .05$, and at the 10-s delay, $F(2, 24) = 5.16$, $p < .05$. Additional tests of simple effects examining the 0-s delay data revealed a significant display condition main effect only in Block 3, $F(1, 12) = 7.21$, $p < .05$. As can be seen in Figure 3A, this small effect was driven by larger errors in the NoRef condition ($M = 0.46^\circ$) than in the Ref condition ($M = 0.02^\circ$).

Tests of simple effects examining the 10-s delay data revealed a significant display condition effect in Block 1, $F(1, 12) = 40.73$, $p < .001$, a trend toward a significant display condition effect in Block 2, $F(1, 12) = 4.38$, $p = .058$, and a significant display condition effect in Block 3, $F(1, 12) = 10.15$, $p < .01$. In each of these cases, directional errors were larger in the NoRef condition than in the Ref condition, reflecting the sawtooth pattern evident for both targets in Figure 3A. These results replicate the central findings from Experiment 1 and suggest that participants were unable to mentally impose the category boundary. It is not clear why there was a reduction in the display condition effect in Block 2. This may reflect a type of practice effect (see below).

Lastly, to probe whether the direction of errors to the -10° target at the 10-s delay changed across display conditions, we conducted planned comparisons to see whether these errors were significantly different from zero. As in Experiment 1, t tests revealed that the -10° target was significantly biased counterclockwise (i.e., away from the boundary) in the Ref condition, $t(12) = -5.06$, $p < .001$.

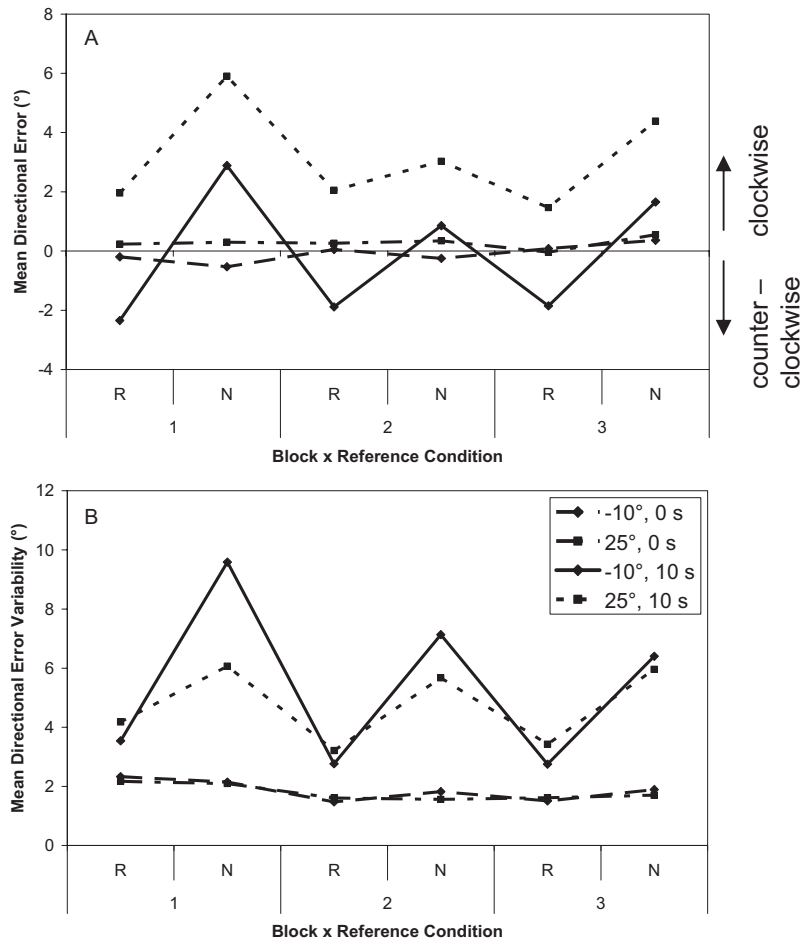


Figure 3. Mean directional error (A) and variability (B) across display conditions and blocks for Experiment 2. Positive values indicate clockwise directional errors; negative values indicate counterclockwise directional errors. R = reference condition; N = no-reference condition.

This was not the case in the NoRef condition, where responses were biased clockwise (i.e., away from midline). Note, however, that these clockwise biases did not differ significantly from zero error. Once again, it is likely this is due to the increased variability in the NoRef condition.

Directional error variability. Figure 3B shows mean directional error variability for responses to each target at each delay across display conditions and blocks. As in Experiment 1, variability at the 10-s delay generally showed a sawtooth pattern with higher variability in the NoRef condition, and variability at the 0-s delay was generally low. Variability also decreased slightly over blocks.

To analyze these effects, we conducted a four-way ANOVA, with target (-10° , 25°), delay (0, 10 s), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed significant main effects of delay, $F(1, 12) = 112.42$, $p < .001$, and display condition, $F(1, 12) = 50.77$, $p < .001$, as well as significant two-way interactions of Delay \times Display Condition, $F(1, 12) = 5.04$, $p < .05$, and Target \times Display Condition, $F(1, 12) = 18.25$, $p < .01$. These effects were all subsumed by a significant Target \times Delay \times Display Condition interaction, $F(1, 12) = 7.38$, $p < .05$.

Tests of simple effects revealed no significant effects at the 0-s delay. At the 10-s delay, there was a significant main effect of display condition, $F(1, 12) = 58.08$, $p < .001$. This replicates a key finding from Experiment 1, with significantly greater variability in the NoRef condition. In addition, there was a significant Display Condition \times Target interaction at the 10-s delay, $F(1, 12) = 12.17$, $p < .01$. Additional tests of simple effects showed no significant differences in variability across targets in the Ref condition, likely because of the stabilizing influence of the category boundary. By contrast, there was a significant target main effect in the NoRef condition, $F(1, 12) = 9.89$, $p < .01$. As can be seen in Figure 3B, variability was higher to the -10° target ($M = 7.70^\circ$) than to the 25° target ($M = 5.90^\circ$) in this condition. This larger variability may reflect the large shift in mean errors to the -10° target across Ref and NoRef trials. Because responses to this target shift so dramatically across conditions, it is less likely that responses will overlap from trial to trial, and response overlap is one key contributor to the reduction in error over blocks (see Spencer & Hund, 2002).

In addition to the three-way interaction, the overall ANOVA revealed a significant main effect of block, $F(2, 11) = 9.95$, $p < .01$. The t tests showed that variability decreased significantly from

Block 1 ($M = 4.01^\circ$) to Block 2 ($M = 3.15^\circ$), $t(12) = -4.59$, $p < .01$, and from Block 1 to Block 3 ($M = 3.16^\circ$), $t(12) = -2.69$, $p < .05$. Blocks 2 and 3 did not differ significantly. As in Experiment 1, this reduction in variability after Block 1 likely reflects improvement from repeatedly responding to the same two targets.

Discussion

As in Experiment 1, analyses of both mean directional errors and variability showed significant effects of display condition. In the Ref condition, both targets drifted away from the lateral category boundary over delay with relatively low variability. In the NoRef condition, on the other hand, the left target flipped direction to drift toward the lateral boundary, or away from the midline of the table, with high variability. These results suggest that, as in Experiment 1, participants were not able to mentally impose the lateral category boundary.

Further inspection of the data suggests, however, that the specific order of trials may have influenced performance. Specifically, consider trials to the -10° target at the 10-s delay in the NoRef condition. These trials can be considered the critical trials because this is where we have consistently seen the most dramatic effect—reversal in the direction of error relative to the Ref condition. The likelihood of imposing the boundary on a critical trial may depend on how recently, and for how many trials, the reference cues were available. If the immediately previous trial was in the NoRef condition, imposing the boundary should be harder than if the previous trial was in the Ref condition. Moreover, repeated trials in the Ref condition preceding a critical trial should increase the likelihood of imposing the category boundary because repetition should help construct a strong memory of the boundary.

To explore this possibility, we classified critical trials in Experiment 2 according to the number of preceding trials in the Ref condition (0, 1, or 2+). Because each participant received trials in random order, the number of trials in each of these categories varied across participants. This prevented us from analyzing the results statistically, but the mean errors in these categories suggest that higher numbers of previous trials in the Ref condition led to lower directional errors (0 previous mean = 2.91° , 1 previous mean = 1.37° , 2+ previous mean = 0.03°). Although this is not a complete reversal of the direction of error, the reduction of error with increasing numbers of previous Ref trials suggests that participants may be imposing the boundary during some of the critical trials following 2+ trials in the Ref condition.

Although these data are suggestive, it is also possible to explain these results on the basis of build-up of long-term memory over trials. Spencer and Hund (2002) reported that participants con-

struct a long-term memory of each target over trials that can stabilize memory in some cases (or introduce bias in others). Critically, the 2+ repetitions analyzed above include some trials during which participants moved to the same target (-10°) for the second or third time in a row. In these cases, participants would likely have a relatively accurate long-term memory of the -10° location, which could explain the mean error near zero. Note that the stabilizing influence of long-term memory might also explain why mean directional errors were generally smaller across conditions in Block 2 and why there was a general reduction in variability over blocks here and in Experiment 1.

Experiment 3

If participants were imposing an arbitrary category boundary on some—but not all—trials in the previous two experiments, it is possible that we failed to detect this because of the nature of our experimental designs. Thus, in the present experiment, we specifically manipulated the number of Ref trials preceding a critical trial—trials to the -10° target at the 10-s delay in the NoRef condition—and analyzed these trials separately to look for evidence of mental imposition. Moreover, we manipulated whether the trials preceding a critical trial were to the same or a different target to control for the influence of long-term memory effects.

The central elements of the design are shown in Table 1. We created four sets of trials based on which target and display condition preceded each critical trial: same target, same display condition (-10° NoRef); same target, different display condition (-10° Ref); different target, same display condition (25° NoRef); different target, different display condition (25° Ref). By using these four sets, we could separate out effects on performance to the left target because of just-prior repetitions of the same target, the same reference, both, or neither. Of note, because all critical trials were to the -10° target in the NoRef condition, any systematic differences in error can be attributed to experience with the five preceding trials in each set.

Method

Participants. Twelve adults (7 women, 5 men; mean age = 25 years 4.08 months; $SD = 5.01$ years) participated in the study. Participants were recruited either through an introductory psychology course and given research exposure credit or from the community with fliers and were paid \$7 an hour for their participation. All participants were right-handed and had normal or corrected-to-normal vision.

Table 1
Trial Sets for Experiments 3 and 4

	Set 1		Set 2		Set 3		Set 4	
	Condition	Target	Condition	Target	Condition	Target	Condition	Target
Trials 1–5	NoRef	-10°	Ref	-10°	NoRef	25°	Ref	25°
Trial 6	NoRef	-10°	NoRef	-10°	NoRef	-10°	NoRef	-10°

Note. Each six-trial set included five trials to the same target-condition combination and ended with a critical trial to the -10° target in the no-reference (NoRef) condition. Ref = reference.

Apparatus, stimuli, and procedure. All details of the apparatus, stimuli, and procedure were identical to those in Experiment 1 with two exceptions. First, we used only the 10-s delay because errors at the 0-s delay were similar across targets and conditions in Experiments 1 and 2. Indeed, it is possible that participants' consistently accurate responses at 0 s obscured the utility of using the lateral boundary; that is, the inclusion of these trials made it difficult to notice the improved performance in the Ref condition. Second, trials were grouped into six-trial sets (see Table 1). Each set consisted of five trials to one target in one display condition, followed by a critical trial to the -10° target in the NoRef condition. To equate the number of trials to each target and display condition to the numbers used in Experiments 1 and 2, we distributed additional filler trials between sets across the session. These filler trials were inserted with the constraint that they did not match the target or display condition of the subsequent six-trial set. The six-trial sets and filler trials were then arranged in random order such that each set appeared twice in each of the three testing blocks. Four different trial orders were generated, and participants were randomly assigned to one of these orders.

Method of analysis. The method of analysis was identical to Experiment 2 with one addition: Critical trials were analyzed separately. Mean directional errors on critical trials were computed by averaging across the two trials in the same set (see Table 1) in each block. Seven outlier trials (0.30% of all trials) were excluded. One of these trials was a critical trial and thus was excluded from these analyses as well.

Results

Mean directional error. Figure 4A shows mean constant directional errors to each target across display conditions and blocks. This figure shows the same sawtooth pattern as in Experiments 1 and 2; however, biases to the -10° target in the NoRef condition were very close to zero error in Blocks 2 and 3. There is also some evidence of a general reduction in error over blocks.

To analyze these effects, we conducted a three-way ANOVA, with target (-10° , 25°), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed a significant main effect of target, $F(1, 11) = 13.51$, $p < .01$, reflecting the lower mean error to the -10° target ($M = 0.20^\circ$) than to the 25° target ($M = 3.50^\circ$). Additionally, there was a significant main effect of display condition, $F(1, 11) = 29.19$, $p < .001$. As in Experiments 1 and 2, this effect reflects the sawtooth pattern in Figure 4A with lower errors in the Ref condition ($M = 0.20^\circ$) than in the NoRef condition ($M = 3.49^\circ$). Note that, as before, the low mean errors to the -10° target and in the Ref condition reflect the averaging of clockwise and counterclockwise errors.

The overall ANOVA also revealed a significant main effect of block, $F(2, 10) = 16.59$, $p < .01$. The t tests showed that directional error decreased significantly from Block 1 ($M = 2.60^\circ$) to Block 2 ($M = 1.01^\circ$), $t(11) = -4.61$, $p < .01$, and from Block 1 to Block 3 ($M = 1.45^\circ$), $t(11) = -2.39$, $p < .05$. Blocks 2 and 3 did not differ significantly. This reduction in bias is similar to the reduction in the reference effect seen in Block 2 of Experiment 2. It is likely that we found a more dramatic effect of block in the present study due to the inclusion of the six-trial sets. Recall that these sets included five repetitions to the same target and condi-

tion. Thus, participants had ample opportunity to use feedback to hone in on the exact target location during these repetitions.

Lastly, we conducted planned comparisons to see whether errors to the -10° target were significantly different from zero error in each of the display conditions. As in Experiments 1 and 2, t tests revealed that the -10° target was significantly biased counterclockwise (i.e., away from the lateral axis) in the Ref condition, $t(11) = -2.69$, $p < .05$. Again, the clockwise bias (i.e., away from midline) in the NoRef condition was not significant.

Directional error variability. Figure 4B shows mean directional error variability for responses to each target across blocks and display conditions. As this figure shows, variability was generally higher in the NoRef condition, creating the same sawtooth pattern seen in Experiments 1 and 2. We conducted a three-way ANOVA, with target (-10° , 25°), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed significant main effects of target, $F(1, 11) = 6.65$, $p < .05$, and display condition, $F(1, 11) = 10.14$, $p < .01$, which were subsumed by a significant Target \times Display Condition interaction, $F(1, 11) = 19.92$, $p < .01$. Tests of simple effects showed a significant display condition effect to both targets: -10° , $F(1, 11) = 67.39$, $p < .001$; 25° , $F(1, 11) = 22.43$, $p < .01$. Thus, the sawtooth pattern across conditions was statistically robust as in the previous experiments. Additional tests of simple effects showed a significant target effect in the NoRef condition, $F(1, 11) = 20.39$, $p < .01$, but no effect in the Ref condition. As in Experiment 2, this target effect was driven by higher variability to the -10° target ($M = 7.57^\circ$) than to the 25° target ($M = 5.94^\circ$).

Critical trials. Figure 4C shows mean directional error on critical trials, grouped across blocks by the target and reference that were presented on Trials 1–5 in each set. For instance, performance on the critical trials for set 1 in Table 1 is captured by the *Prev -10°* line and the PN (i.e., previous condition = NoRef) blocks in Figure 4C. Because all of the critical trials were to the -10° target in the NoRef condition, we would expect counterclockwise errors if participants were able to impose the category boundary and clockwise errors otherwise. As can be seen in the figure, responses were generally biased clockwise with one exception: Responses in Block 3 showed a counterclockwise bias for Set 4 (critical trials following five Ref trials to the 25° target).

We analyzed these data using a three-way ANOVA, with previous target (-10° , 25°), previous display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed no significant effects. We also conducted planned comparisons to see whether directional errors on the critical trials differed significantly from zero error for any of the four sets. None of these tests yielded significant findings. In summary, then, performance on critical trials was not significantly influenced by the target or display condition presented on the five preceding trials, suggesting that participants were not mentally imposing the boundary.

Discussion

The goal of this experiment was to test whether repeated trials in the Ref condition would support the imposition of an arbitrary category boundary. The overall analyses replicated key findings from Experiments 1 and 2. Moreover, analysis of critical trials showed no effect of the just-previous target and condition. Thus, results from three experiments paint a consistent picture: Partici-

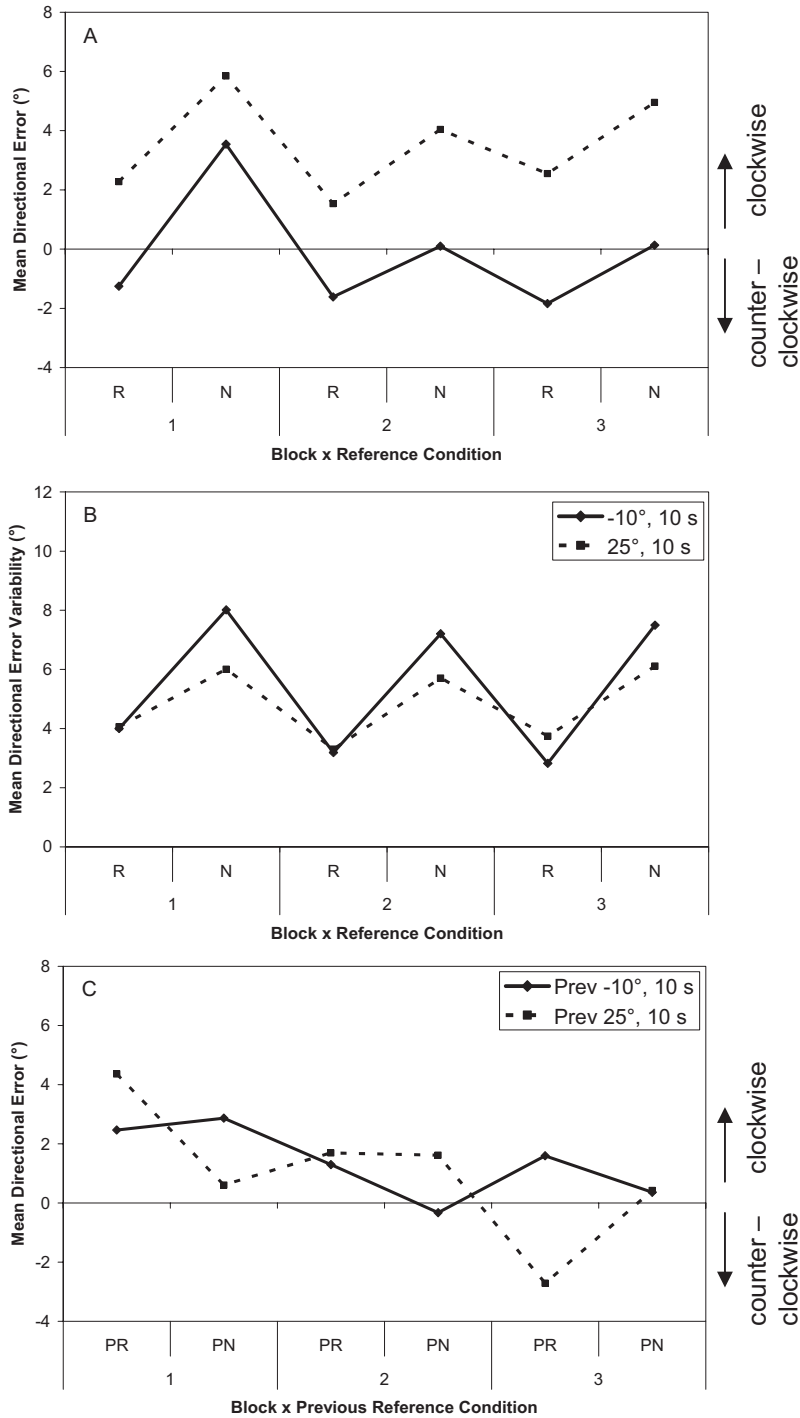


Figure 4. Mean directional error (A) and variability (B) across display conditions and blocks for all trials in Experiment 3; mean directional error on critical trials across previous (Prev) display conditions and blocks (C). Note that all critical trials are to the -10° target in the no-display condition—separate lines indicate the previous target set. Positive values indicate clockwise directional errors; negative values indicate counterclockwise directional errors. Labels *PR* and *PN* indicate critical trials on which the previous sets of trials were in the reference (R) and no-reference (N) conditions, respectively.

pants are not mentally imposing an arbitrary category boundary in this task.

One possible reason for participants' failure to impose the boundary across these experiments is that it is not required by the task—people are still able to remember the spaceship locations when the dots are not present (albeit not as accurately). We included feedback on all trials so that participants could discover that their performance was better in the Ref condition (as evidenced by lower errors and variability). However, this may not have been a strong enough incentive for participants to try to impose the boundary in the absence of perceptual support. If this is the case, then participants may be capable of mentally imposing an arbitrary category boundary but might only do so under more intentional circumstances.

The proposal that intentional or explicitly conceptual processes might impact performance in our task is consistent with results from Tversky and Schiano (1989). These researchers reported that participants' responses within an *L*-shaped frame were biased toward the diagonal (i.e., away from the edges) of the frame when participants were told the figure was a graph but showed no significant biases when the figure was described as a map. In a follow-up study, Schiano and Tversky (1992) found that participants' responses were biased away from the diagonal symmetry axis of the *L*-shaped frame when they reproduced the lines immediately but replicated the bias toward the diagonal when participants were instructed to "compare [the target line] to the (imaginary) 45° or diagonal line" (p. 16). Although these experiments probed responses around an axis of symmetry grounded in the visible frame, the modulation of category bias based on the interpretive context suggests that participants might impose an arbitrary category boundary in our task if given appropriate instructions. We examined this possibility in the next experiment.

Experiment 4

Results from Experiments 1–3 suggest that adults were not able to mentally impose an arbitrary category boundary without perceptual support. One possible explanation for this failure is that participants were simply not trying to impose the boundary. Although there were clear advantages to using the boundary in the previous experiments—most notably, lower mean error to the right target and lower overall variability—it is possible that participants failed to detect such advantages despite a rich array of feedback. The current experiment tested this possibility by asking participants to explicitly try to impose the boundary when it was not visible. Note that we chose not to give participants an interpretive context as in Tversky and Schiano (1989) because the sparse perceptual support in our task space did not lend itself easily to such things. Moreover, we reasoned that explicit instructions should produce the strongest effects.

Method

Participants. Twelve adults (9 women, 3 men; mean age = 18 years 8.12 months; $SD = 0.34$ years) participated. Participants were recruited through an introductory psychology course and received research exposure credit for their participation. All participants were right-handed and had normal or corrected-to-normal vision.

Apparatus, stimuli, and procedure. All details of the apparatus, stimuli, and procedure were identical to those in Experiment 3 with three exceptions. First, half of the practice trials were in the Ref condition, so participants would have some experience with the reference dots when the experimenter explained the instructions. Second, after the practice trials, the experimenter instructed participants to "act as if the yellow dots are there on every trial, even when they aren't visible." Participants were reminded of the instructions before each test block. Note that we left the instructions somewhat ambiguous (rather than asking them to imagine a line or boundary) because we did not want to influence how people used the dots when they were available. In particular, ad hoc discussions with participants from Experiments 1–3 suggested that some people attempted to use the virtual line connecting the dots, whereas other people tried to focus on either the lower dot or the upper dot. To capture such idiosyncratic tendencies, we included a questionnaire at the end of the session to ask participants whether they felt they were able to mentally impose the dots and, if so, how useful they found the imposed dots compared with the visible dots.

Method of analysis. The method of analysis was identical to Experiment 3. Three outlier trials (0.13% of all trials) were excluded; one of these trials was a critical trial.

Results

Mean directional error. Figure 5A shows mean directional errors to each target across blocks and display conditions. This figure shows the same sawtooth pattern evident in Experiments 1–3 but only for the -10° target. Specifically, the direction of error for the -10° target switched with condition, with clockwise biases in the NoRef condition and counterclockwise biases in the Ref condition. By contrast, directional error to the 25° target was initially more accurate in the Ref condition, but by Block 2, performance in the Ref and NoRef conditions was comparable.

To analyze these effects, we conducted a three-way ANOVA, with target (-10° , 25°), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed significant main effects of target, $F(1, 11) = 12.30$, $p < .01$, and display condition, $F(1, 11) = 18.13$, $p < .01$, which were subsumed by a significant Target \times Display Condition interaction, $F(1, 11) = 5.99$, $p < .05$. Tests of simple effects showed a significant display condition effect for the -10° target only, $F(1, 11) = 13.97$, $p < .01$. This effect was driven by clockwise errors in the NoRef condition ($M = 0.84^\circ$) and counterclockwise errors in the Ref condition ($M = -1.77^\circ$). This effect replicates the same sawtooth pattern seen to the -10° target in the previous three experiments.

The omnibus ANOVA also revealed a significant main effect of block, $F(2, 10) = 4.65$, $p < .01$, which was subsumed by a significant Target \times Block interaction, $F(2, 10) = 6.24$, $p < .05$. Tests of simple effects revealed that this interaction was driven by changes in error over blocks for the 25° target, $F(2, 22) = 8.70$, $p < .01$, with no block effect for the -10° target. Follow-up *t* tests showed that errors to the 25° target decreased significantly from Block 1 ($M = 3.53^\circ$) to Block 2 ($M = 1.67^\circ$), $t(11) = -4.21$, $p < .001$, and from Block 1 to Block 3 ($M = 2.23^\circ$), $t(11) = -2.36$, $p < .05$. Blocks 2 and 3 did not differ significantly. This reduction in error over blocks is consistent with evidence of improved performance over trials in Experiments 2 and 3. It is interesting to note, however, that the reduction in error over blocks was more dramatic

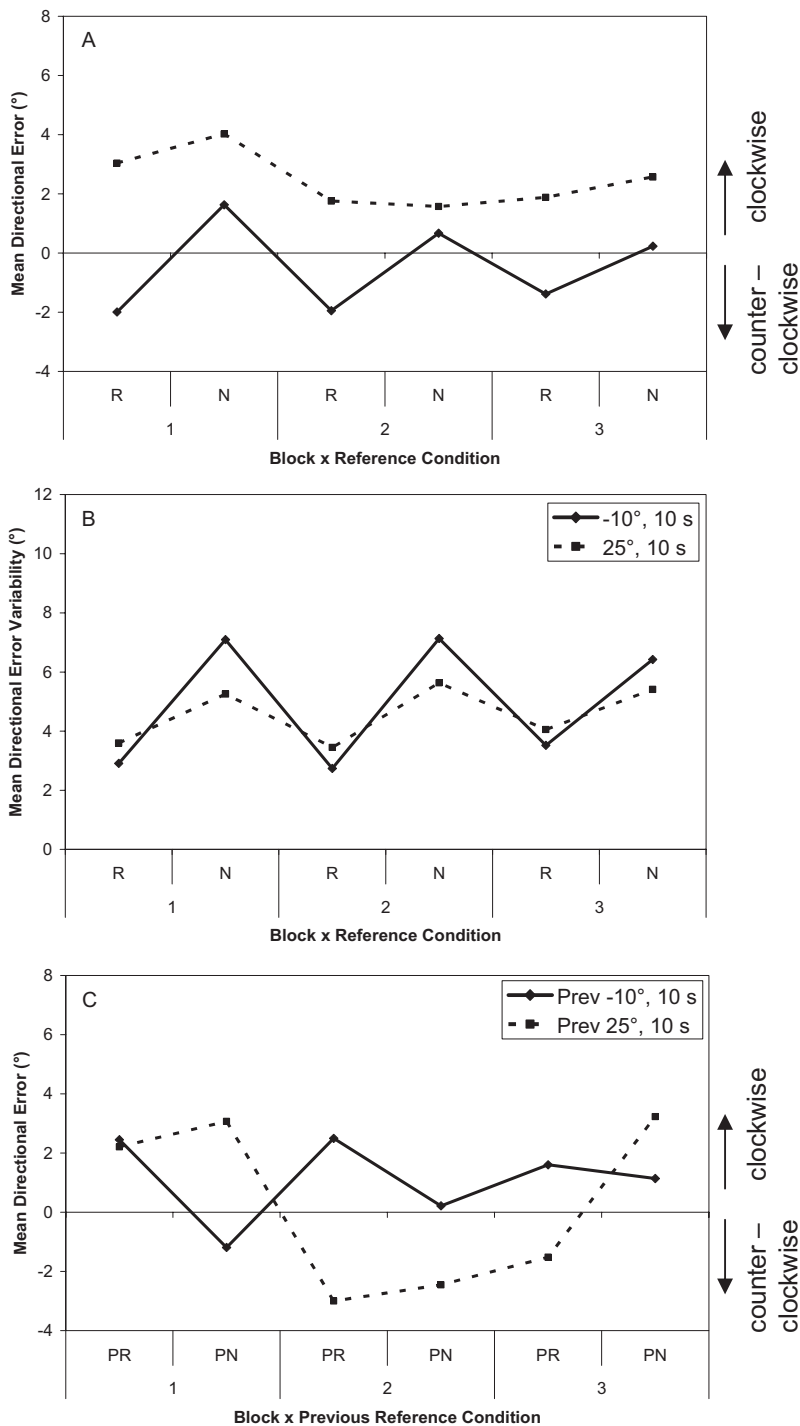


Figure 5. Mean directional error (A) and variability (B) across display conditions and blocks for all trials in Experiment 4; mean directional error on critical trials across previous (Prev) display conditions and blocks (C). Positive values indicate clockwise directional errors; negative values indicate counterclockwise directional errors. Labels PR and PN indicate critical trials on which the previous sets of trials were in the reference (R) and no-reference (N) conditions, respectively.

here, particularly in the NoRef condition where errors in Blocks 2 and 3 were comparable with errors in the Ref condition. This suggests participants may have engaged in a weak form of mental imposition but only on trials to the right target.

Lastly, we conducted planned comparisons to see whether errors to the -10° target were significantly different from zero in each of the display conditions. The t tests revealed that the -10° target was significantly biased counterclockwise (i.e., away from the bound-

ary) in the Ref condition, $t(11) = -6.57, p < .001$. However, as in the previous experiments, the clockwise bias (i.e., away from midline) in the NoRef condition was not significant. Given that we generally saw a small but consistent clockwise bias to the -10° target in the NoRef condition across four experiments, we probed whether aggregating the data across experiments might help overcome the large variability in this condition. This was indeed the case: Aggregate t tests across all four experiments revealed that the -10° target was significantly biased counterclockwise in the Ref condition, $t(48) = -8.99, p < .001$, and significantly biased clockwise in the NoRef condition, $t(48) = 2.53, p < .05$.

Directional error variability. Figure 5B shows mean directional error variability for responses to each target across blocks and display conditions. As this figure shows, variability was generally higher in the NoRef condition, creating the same sawtooth pattern as in Experiments 1–3. We conducted a three-way ANOVA, with target ($-10^\circ, 25^\circ$), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed a significant main effect of display condition, $F(1, 11) = 26.80, p < .01$, which was subsumed by a significant Target \times Display Condition interaction, $F(1, 11) = 20.69, p < .01$. Tests of simple effects showed a significant display condition effect to both targets: $-10^\circ, F(1, 11) = 30.68, p < .001$; $25^\circ, F(1, 11) = 14.59, p < .01$. Thus, the sawtooth pattern across conditions was statistically robust. Additional tests of simple effects showed a significant target effect in the NoRef condition, $F(1, 11) = 23.95, p < .001$, but no effect in the Ref condition. As in Experiments 2 and 3, this target effect was driven by higher variability to the -10° target ($M = 6.88^\circ$) than to the 25° target ($M = 5.43^\circ$).

Critical trials. Figure 5C shows mean directional error on critical trials, grouped across blocks by the target and reference that were presented on Trials 1–5 in each set (see Experiment 3 and Table 1). Again, because all of the critical trials were to the -10° target in the NoRef condition, we would expect counterclockwise errors if participants are imposing the category boundary and clockwise errors otherwise. As this figure shows, errors for critical trials following trials to the -10° target (solid line in Figure 5C; Sets 1 and 2 in Table 1) were consistently biased clockwise with one exception near zero error (see PN in Block 1). Thus, as in Experiment 3, there was little evidence of mental imposition for these sets of critical trials. By contrast, results for critical trials following trials to the 25° target (Sets 3 and 4 in Table 1) were mixed: Three means were biased in a clockwise direction, but three were biased in a counterclockwise direction.

Although these three counterclockwise cases are suggestive of mental imposition, these findings were not statistically robust. We analyzed these data using a three-way ANOVA, with previous target ($-10^\circ, 25^\circ$), previous display condition (Ref, NoRef), and block (1–3) as within-subjects variables. As in Experiment 3, this analysis revealed no significant effects. We also conducted t tests to see whether errors on the critical trials were significantly different from zero error for any of the four sets. One t test reached significance: critical trials following the -10° target and the Ref condition, $t(11) = 2.53, p < .05$. Note that we expected this to be the most supportive condition for mental imposition. Nevertheless, errors were significantly biased clockwise—away from midline—in this condition. Thus, the most robust finding from analyses of critical trials suggests that people were not mentally imposing the boundary.

Questionnaire responses. Although our statistical analyses yielded only weak evidence that participants were able to mentally impose the boundary on the critical trials, it is possible that this simply reflected a lack of compliance with our instructions. Participants' responses on the postexperiment questionnaire suggested otherwise. The questionnaire asked participants to describe how they used the reference dots as well as whether they felt they were able to mentally impose the dots when they were not visible. The first question read, "Did you find the presence of the dots made it easier to remember the spaceship locations (i.e., you did better when they were visible)?" to which all participants responded yes. Moreover, on a scale of 1 (*least*) to 9 (*most*), participants rated the dots as relatively helpful, with an average rating of 7.67. These responses verify that participants were sensitive to their performance difference across display conditions.

The next question asked, "Did you feel you were able to use the dots on trials where they weren't visible?" Ten out of the 12 participants responded yes, and all of these participants also said the dots "helped on trials where they weren't visible." This suggests that, although our results provide only weak evidence of mental imposition, most participants felt they were able to use a mentally imposed boundary to some extent. Finally, on a scale of 1 to 9, participants rated the imagined dots as somewhat helpful, with an average rating of 5.60. Of note, 9 of the 10 participants who said they could use the mentally imposed dots gave a lower rating on helpfulness relative to the visible dots. Thus, although most participants felt they were able to impose the dots in some way, most also felt that these imagined dots were less useful than the visible ones.

We also asked participants how they used the reference dots (both when visible and when not visible) and if they had any specific strategies. We posed these questions to see whether participants used explicitly different strategies on Ref and NoRef trials, which would lead to little or no evidence of mental imposition. Participants' responses suggested this was not the case—most reported that their strategy on the NoRef trials was to try to remember or imagine the dots from the Ref trials. Indeed, a number of participants noticed that the response crosshairs always appeared at the location of the bottom dot and tried to use this as an aid in imagining the reference dots.

Discussion

Results from this experiment are generally consistent with the previous three experiments. Directional errors to the -10° target replicated the findings of Experiments 1–3, showing the same sawtooth pattern for trials with support versus without visual support of the boundary. Variability for both targets also showed the same sawtooth pattern seen in the previous experiments. Moreover, analyses of critical trials showed no robust evidence of mental imposition. Rather, the one significant finding from these analyses was in the opposite direction (i.e., clockwise or away from midline). Of note, this occurred on trials that we expected to yield the strongest evidence in favor of mental imposition: the critical trials that followed five trials in a row to the -10° target with the reference visible. Considered together, these results suggest that people cannot impose an arbitrary category boundary even when asked explicitly to do so.

Two findings qualify this conclusion. First, although not statistically robust, participants showed three cases of counterclockwise errors on critical trials—two more than we observed in Experiment 3. We investigated whether these effects were weak because of the performance of 1 or 2 participants with large clockwise errors. This was not the case: The majority of participants (8 out of 12) showed primarily clockwise errors that were larger than 1° in magnitude (54.5% of trials) or responses within $\pm 1^\circ$ of the target (13.0% of trials) on critical trials. Four participants did show some evidence of mental imposition on critical trials, but even these participants showed a substantial percentage of clockwise (29.2%) and accurate (9.4%) responses. Thus, at best, the counterclockwise errors on the critical trials in the overall analysis point toward a weak ability to mentally impose an arbitrary boundary. At worst, these errors simply reflect the stochastic influences on spatial memory performance in our sparse task space that contained little perceptual structure.

The second effect that qualifies our conclusion that participants cannot mentally impose an arbitrary boundary was the comparable directional error to the 25° target across display conditions. Although this effect is consistent with mental imposition, it could also result from long-term memory processes. In particular, participants might have developed a relatively accurate memory of the right target by Block 2. Because the 25° target is far from the midline axis, this axis should exert a relatively weaker influence on this target during NoRef trials (see Spencer & Hund, 2002, for evidence of such metric effects). It is possible that a stronger long-term memory was able to counteract the weak bias away from midline on the NoRef trials, leading to smaller clockwise errors in this experiment. This is consistent with the reduction in error in Block 2 to the 25° target in Experiment 2. Thus, although results from the 25° target suggest a weak form of mental imposition, these data are inconclusive. In summary, even though we asked participants to “act as if the yellow dots are there on every trial, even when they aren’t visible,” and participants felt they were able to impose the dots in some way, we found only weak evidence of mental imposition abilities.

Experiment 5a

The previous experiments probed whether participants could mentally impose a previously available boundary that was specified by the virtual axis between two dots. Results suggest that, at best, people have only a weak ability to impose such a spatial category boundary. It is possible, however, that the use of a virtual axis in our previous studies made the task too difficult because participants had to first impose the dots and then recreate the virtual axis between them. Perhaps people would be better able to mentally impose a visible boundary between the targets. To explore this possibility, we used the same design as in Experiment 4 but presented a solid line connecting the two reference dots, as shown in Figure 6 (right side only). Moreover, use of a solid line as the boundary allowed us to set up a clearer interpretive context, explicitly labeling it as a boundary in the instructions. Given that Tversky and Schiano (1989) found an effect of their interpretive context but we found little or no effect of our explicit instructions in Experiment 4, we wanted to test whether a more elaborate context would help participants impose the boundary.

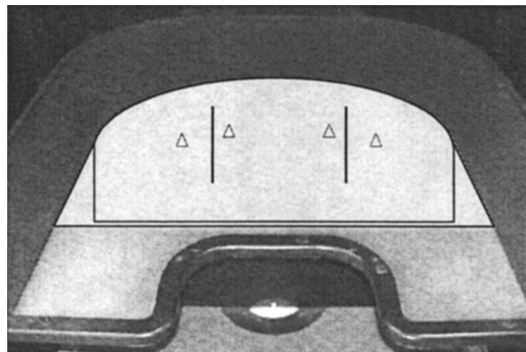


Figure 6. Display condition used for Experiment 5a on the right side of the table and for Experiment 5b on both sides of the table.

Although use of a solid line as a boundary may help participants mentally impose the boundary when it is no longer visible, there is one possible drawback to this modification of our basic design: Previous work from our lab shows that more salient reference cues lead to reductions in mean directional error (Lipinski et al., 2005; Spencer & Simmering, 2002). Thus, we expected that the presence of the line would reduce counterclockwise errors on Ref trials. This might make it more difficult to assess mental imposition of the boundary because we would be comparing rather small counterclockwise errors in the Ref condition with relatively small clockwise errors on NoRef trials.

Method

Participants. Twelve adults (10 women, 2 men; mean age = 22 years 11.33 months; $SD = 3.71$ years) participated in this experiment. One additional participant was excluded from analyses because of incomplete data. Participants were recruited from the community with fliers and paid \$8 an hour for their participation. All participants were right-handed and had normal or corrected-to-normal vision.

Apparatus, stimuli, and procedure. All details of the apparatus, stimuli, and procedure were identical to those in Experiment 4 with three exceptions. First, on Ref trials, we presented a solid green line connecting the two reference dots. Second, we changed the instructions to introduce an interpretive context. In particular, we told participants,

On some trials, a green line will appear. It’s part of the game too—it’s a boundary that can help identify where the spaceships are located. Unfortunately, it won’t be there on every trial because the radar on your spaceship is on the fritz. One of the challenges of this game is to try to find the spaceships even when the line isn’t visible. To help with this, try to imagine the boundary line on trial when it isn’t visible.

These instructions were given before the practice trials, and participants were reminded of the instructions before each test block. Lastly, we modified the questionnaire at the end of the session to correspond to the changes in the reference display (e.g., questions asked about the boundary instead of the dots).

Method of analysis. The method of analysis was identical to Experiment 3. One outlier trial (0.04% of all trials) was excluded.

Results

Mean directional error. Figure 7A shows mean directional errors to each target across blocks and display conditions. As this figure shows, the pattern of errors to the -10° target differed from previous experiments. Instead of showing alternation between clockwise and counterclockwise bias as before, performance to this target was generally accurate. This is consistent with previous studies from our laboratory showing smaller categorical biases in the presence of salient reference cues (Lipinski et al., 2005; Spencer & Simmering, 2002). The one exception was in the NoRef condition in Block 1, which showed a large clockwise error as in previous experiments. For the 25° target, on the other hand, we found the same sawtooth pattern as in previous experiments, with lower errors in the Ref condition.

To analyze these effects, we conducted a three-way ANOVA, with target (-10° , 25°), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed a significant main effect of target, $F(1, 11) = 9.60$, $p < .05$, which was subsumed by a significant Target \times Display Condition interaction, $F(1, 11) = 12.88$, $p < .01$. Tests of simple effects showed a significant display condition effect for the 25° target only, $F(1, 11) = 9.78$, $p < .05$. As in previous experiments, this effect was driven by larger errors in the NoRef condition ($M = 3.88^\circ$) than in the Ref condition ($M = 1.43^\circ$).

The omnibus ANOVA also revealed a significant main effect of block, $F(2, 10) = 4.36$, $p < .05$, which was subsumed by a significant Target \times Block interaction, $F(2, 10) = 5.31$, $p < .05$. Tests of simple effects revealed that this interaction was driven by changes in error over blocks for the -10° target, $F(2, 22) = 10.67$, $p < .01$, with no block effect for the 25° target. Follow-up t tests showed that errors to the -10° target decreased significantly from Block 1 ($M = 2.33^\circ$) to Block 2 ($M = 1.33^\circ$), $t(11) = 2.80$, $p < .05$, and from Block 1 to Block 3 ($M = 1.22^\circ$), $t(11) = 2.51$, $p < .05$. Blocks 2 and 3 did not differ significantly. This reflects the large reduction in error to the -10° target after Block 1, which was most dramatic in the NoRef condition (see Figure 7A).

Lastly, we conducted planned comparisons to see whether errors to the -10° target were significantly different from zero in each of the display conditions. The t tests revealed that the -10° target was not significantly biased in either the Ref or NoRef condition. This stands in contrast to the previous experiments, where the -10° target was biased counterclockwise in the Ref condition. Once again, this is consistent with results showing that salient reference cues help stabilize memory for locations (Lipinski et al., 2005; Spencer & Simmering, 2002).

Directional error variability. Figure 7B shows mean directional error variability for responses to each target across blocks and display conditions. As this figure shows, variability was generally higher in the NoRef condition, creating the same sawtooth pattern seen in Experiments 1–4. We conducted a three-way ANOVA, with target (-10° , 25°), display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed a significant main effect of display condition, $F(1, 11) = 122.31$, $p < .001$, which was subsumed by a significant Target \times Display Condition interaction, $F(1, 11) = 16.68$, $p < .01$. As in previous experiments, tests of simple effects showed a significant display condition effect to both targets: -10° , $F(1, 11) = 120.12$, $p < .001$; 25° , $F(1, 11) = 52.09$, $p < .001$. Thus, the sawtooth

pattern across conditions was statistically robust. Additional tests of simple effects showed a significant target effect in the Ref condition only, $F(1, 11) = 35.30$, $p < .001$. This target effect was driven by lower variability to the -10° target ($M = 2.05^\circ$) than to the 25° target ($M = 3.26^\circ$) when the visible boundary was present.

Critical trials. Figure 7C shows mean directional error on critical trials, grouped across blocks by the target and reference that were presented on Trials 1–5 in each set (see Experiment 3 and Table 1). Again, because all of the critical trials were to the -10° target in the NoRef condition, we would expect counterclockwise errors if participants are imposing the category boundary and clockwise errors otherwise. Note, however, that this pattern must be qualified by the dramatically smaller errors in the Ref condition in the present experiment (see above). That is, because the visible line helped stabilize memory for the -10° target, we would expect either counterclockwise errors or accurate performance if participants are mentally imposing the boundary.

As can be seen in Figure 7C, results for critical trials were mixed. There were four cases with clearly clockwise errors, four cases near zero error, and three cases of counterclockwise errors. We analyzed these data in a three-way ANOVA, with previous target (-10° , 25°), previous display condition (Ref, NoRef), and block (1–3) as within-subjects variables. This analysis revealed a significant Previous Target \times Previous Display Condition \times Block interaction, $F(2, 10) = 5.96$, $p < .05$. Tests of simple effects showed that this effect was driven by a significant Previous Target \times Previous Display Condition interaction in Block 3 only, $F(1, 11) = 7.87$, $p < .05$. No effects reached significance in Blocks 1 and 2. Further tests of simple effects on Block 3 revealed a trend toward a significant previous target effect for critical trials following the Ref condition, $F(1, 11) = 4.47$, $p = .058$. This effect was driven by clockwise errors when -10° was the previous target ($M = 1.64^\circ$) but counterclockwise errors when 25° was the previous target ($M = -2.29^\circ$). Follow-up t tests showed that although these errors differed from each other, neither differed significantly from zero error. It is not clear why responses showed this particular pattern, though the clockwise errors when -10° was the previous target is consistent with the significant t test in the same direction in Experiment 4. Finally, we conducted planned comparison t tests to see whether errors on the critical trials were significantly different from zero error for any of the four sets. None of these analyses reached significance.

Questionnaire responses. As in Experiment 4, we asked participants to describe how they used the reference line as well as whether they felt they were able to mentally impose the line when it was not visible. All participants confirmed that “the presence of the line made it easier to remember the spaceship locations,” rating the line as relatively helpful, with an average rating of 7.00. These responses verify that participants were sensitive to their performance difference across display conditions.

Seven of the 12 participants reported that they “were able to use the line on trials where it wasn’t visible,” 6 of whom also said the line “helped on trials where it wasn’t visible.” Of note, fewer people felt they were able to mentally impose the boundary than in Experiment 4 (10 participants), even though we increased both the salience of the boundary and the interpretive context. The 6 participants who responded that the imposed boundary was helpful rated the imagined line as somewhat helpful, with an average rating of 4.33. Four of the 6 participants who said they could use

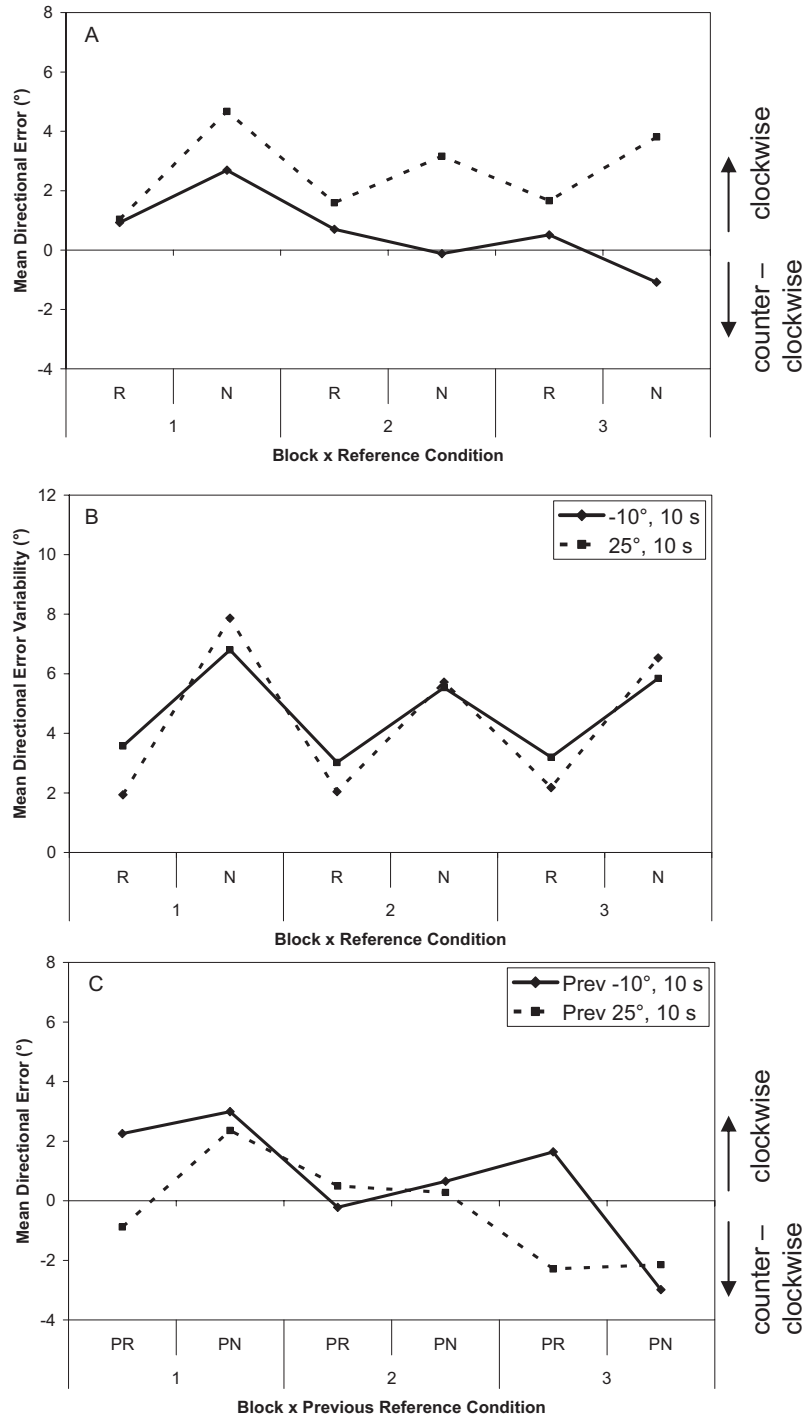


Figure 7. Mean directional error (A) and variability (B) across display conditions and blocks for all trials in Experiment 5; mean directional error on critical trials across previous (Prev) display conditions and blocks (C). Positive values indicate clockwise directional errors; negative values indicate counterclockwise directional errors. Labels PR and PN indicate critical trials on which the previous sets of trials were in the reference (R) and no-reference (N) conditions, respectively.

the mentally imposed boundary gave a lower rating on the helpfulness of this boundary relative to the visible boundary.

As in Experiment 4, we also asked participants how they used the reference line (both when visible and when not visible) and

whether they had any specific strategies. Participants generally said they tried to estimate the distance from the line to the spaceship, although a few also mentioned measuring how far up the line the spaceship was located. When the line was not visible, most

participants reported that they tried to imagine or remember its location. Some participants also reported that they used the location of the crosshairs (which appeared just before the “go” signal) as a cue to the line’s location.

Discussion

Overall, results from this study were mixed. On one hand, performance overlapped considerably with the previous experiments: The sawtooth pattern was present in the mean directional errors to the 25° target as well as in the variability for both targets. These results suggest that participants did not mentally impose the boundary. However, mean directional errors to the -10° target were less consistent. In Block 1, errors generally followed the sawtooth pattern, but performance in the Ref condition was now relatively accurate rather than being biased counterclockwise. Beyond Block 1, mean performance for the -10° target was relatively accurate on both Ref and NoRef trials. Moreover, results from the critical trials were mixed.

The response pattern on Ref trials was consistent with previous results from our lab showing that more salient reference cues help stabilize memory for locations (Lipinski et al., 2005; Spencer & Simmering, 2002). That is, in the presence of the reference line, mean error and error variability were lower. For instance, variability to the -10° target in the Ref condition was significantly lower than variability to the 25° target.

But what led to the accurate performance on NoRef trials? One possibility is that participants were mentally imposing the line but in a weak or variable fashion (which would account for the high variability in this condition). However, the large clockwise errors to the -10° target in Block 1 in the NoRef condition suggest a different explanation: Participants may have built up a very accurate long-term memory of the left target during Ref trials that, by Block 2, yielded low mean errors on NoRef trials as well. We directly tested this possibility in the next experiment by reducing participants’ experience with each individual location while keeping the total number of trials the same as in the present experiment.

Experiment 5b

Mean directional errors to the -10° target in Experiment 5a were consistent with a weak form of mental imposition but could also be explained by the build-up of a relatively accurate long-term memory to that target. The present experiment sought to differentiate these possible explanations by reducing the strength of the long-term memory for each individual target location. To do this, we presented half of the trials at the target locations used in the previous experiments and half of the trials at mirror image locations around a lateral axis 15 cm to the left of the midline symmetry axis (see Figure 6). This allowed us to reduce participants’ experience with each individual location but maintain the same number of total trials. Moreover, by including side (i.e., left side of the table, right side of the table) as a variable in our analyses, we effectively maintained the statistical power needed to detect differences in performance across Ref and NoRef conditions. We also note that shifting the targets to the left side of the table provides an initial probe into whether our results generalize to other regions of the task space.

If participants’ accurate long-term memory of the -10° target built up on Ref trials in the previous experiment influenced performance on NoRef trials in Blocks 2 and 3, then we would expect that the sawtooth pattern to the -10° target evident in Block 1 of the previous study would survive into Blocks 2 and 3 here. By contrast, if participants’ accurate performance to the -10° target on NoRef trials in the previous experiment was indicative of a weak form of mental imposition, we would expect to see similarly accurate performance to this target in the present experiment.

Method

Participants. Eighteen adults (11 women, 7 men; mean age = 20 years 4.93 months; $SD = 4.00$ years) participated. Note that we increased the number of participants relative to previous experiments because we expected variability to increase here because of the decreased strength of long-term memory effects. One additional participant was excluded from analyses because of a large number of responses (~8% of trials) to the incorrect target (e.g., we presented the target at -10° and the participant moved near 25°). Participants were recruited either through an introductory psychology course and received research exposure credit for their participation or they were recruited from the community with fliers and were paid \$8 an hour for their participation. All participants were right-handed and had normal or corrected-to-normal vision.

Apparatus, stimuli, and procedure. All details of the apparatus, stimuli, and procedure were identical to those in Experiment 5a with one exception: Participants were randomly assigned to one of two “side” orders. For half of the participants, practice trials were presented at -25° and 10° around the left boundary, and the first set of 32 test trials was presented at -10° and 25° around the right boundary (the same as in Experiments 1–5a). Then, for the second set of 32 test trials, targets were presented at -25° and 10° around the left boundary; the side continued to alternate for the remaining sets of test trials. For the other half of participants, practice trials were presented around the right boundary, and test trials began around the left boundary, with sides alternating across the remaining trial sets (see Figure 6). Participants were reminded before each trial set which boundary would be presented. Each of the three blocks used in analyses, then, consisted of 32 trials around each boundary, for a total of 64 trials (as in Experiments 1–5a).

Method of analysis. The method of analysis was identical to Experiment 3 with two exceptions. First, responses to targets around the left boundary were coded in a mirror-image fashion relative to Experiments 1–5a: Positive errors were counterclockwise (i.e., away from midline), and negative errors were clockwise (i.e., toward midline). These responses could then be analyzed together with data around the right boundary. Note that for ease of comparison with the previous experiments, we use the same terminology as before, referring to the targets as -10° and 25° and errors as clockwise and counterclockwise (even though the raw data around the left boundary were reversed).

The second analysis difference was needed because participants in the present experiment occasionally moved to the wrong target locations. Therefore, in addition to excluding outlier trials, we also excluded trials on which participants responded to the incorrect target. For trials in the Ref condition, we excluded nine responses that were on the wrong side of the visible boundary. For trials in

the NoRef condition, we computed the mean and standard deviation of the x -coordinates of each participant's responses separately for the two targets. We then removed 6 trials that were greater than 3.5 standard deviations in the direction of the incorrect target (i.e., in the direction of 25° when -10° was the target). Note that participants made these types of errors more frequently than in the previous experiments (15 trials in the current experiment compared with, for instance, 5 trials in Experiment 5a). With the addition of this criterion, a total of 23 trials (0.67% of all trials) were excluded: 7 were outlier trials, 2 had no response because of computer error, and 15 were responses to the incorrect target.

Results

Mean directional error. Figure 8A shows mean directional errors to each target across blocks and display conditions. As this figure shows, errors showed the same sawtooth pattern seen in Experiments 1–4, although this pattern was more pronounced for the -10° target. Moreover, responses to the -10° target in the Ref condition were relatively accurate as in Experiment 5a. To analyze these effects, we conducted a four-way ANOVA, with target (-10°, 25°), display condition (Ref, NoRef), block (1–3), and side (right, left) as within-subjects variables. This analysis revealed a significant display condition main effect, $F(1, 17) = 7.06, p < .05$, which was subsumed by a Target \times Display Condition \times Side interaction, $F(1, 17) = 5.89, p < .05$.

Tests of simple effects for responses to targets on the right side of the table showed a significant display condition main effect, $F(1, 17) = 6.65, p < .05$, which was subsumed by a significant Target \times Display condition interaction, $F(1, 17) = 5.26, p < .05$. Additional tests of simple effects showed a significant display condition effect for the -10° target only, $F(1, 17) = 7.63, p < .05$. As in Experiments 1–4, errors to the -10° target in the Ref condition were slightly counterclockwise ($M = -0.26^\circ$), whereas errors in the NoRef condition were clockwise ($M = 3.55^\circ$). Tests of simple effects for responses to targets on the left side of the table showed a significant display condition main effect $F(1, 17) = 4.15, p < .05$. This effect was driven by slightly counterclockwise errors in the Ref condition ($M = -0.33^\circ$) and clockwise errors in the NoRef condition ($M = 1.54^\circ$). Thus, the sawtooth pattern was robust for responses to both targets on the left side of the table. Considered together, these results replicate the central findings from Experiments 1–4.

Lastly, we conducted planned comparisons to see whether errors to the -10° target were significantly different from zero in each of the display conditions. Like the aggregate t tests for Experiments 1–4, t tests revealed that the -10° target was significantly biased clockwise in the NoRef condition, $t(17) = 2.42, p < .05$, again suggesting that participants were not imposing the boundary. As in Experiment 5a, the counterclockwise bias in the Ref condition was not significant, supporting our previous findings that a more salient reference axis reduces directional error for nearby targets.

Directional error variability. Figure 8B shows mean directional error variability for responses to each target across blocks and display conditions. As this figure shows, variability was generally higher in the NoRef condition, creating the same sawtooth pattern as in Experiment 5a. We conducted a four-way ANOVA, with target (-10°, 25°), display condition (Ref, NoRef), block (1–3), and side (right, left) as within-subjects variables. This

analysis revealed a significant main effect of display condition, $F(1, 17) = 79.99, p < .001$, and a significant Target \times Display Condition interaction, $F(1, 17) = 25.61, p < .001$. These effects were subsumed by a significant Target \times Display Condition \times Block interaction, $F(2, 16) = 5.82, p < .05$.

As in previous experiments, tests of simple effects showed a significant display condition effect for both targets: -10°, $F(1, 17) = 139.40, p < .001$; 25°, $F(1, 17) = 16.28, p < .01$. Thus, the sawtooth pattern across conditions was statistically robust. Additional tests of simple effects showed a significant target effect in the Ref condition only, $F(1, 11) = 39.85, p < .001$. This was driven by lower variability to the -10° target ($M = 2.30^\circ$) than to the 25° target ($M = 4.28^\circ$). As in Experiment 5a, this is consistent with our previous findings that increasing the salience of a reference axis has a stabilizing effect for targets near the axis (Lipinski et al., 2005; Spencer & Simmering, 2002). In addition, there was a trend toward a significant block effect for the 25° target, $F(2, 34) = 2.70, p = .081$. This was driven by a slight increase in variability over blocks (Block 1: $M = 5.00^\circ$; Block 2: $M = 5.21^\circ$; Block 3: $M = 5.91^\circ$).

Critical trials. Figure 8C shows mean directional error on critical trials for each block by the target and reference that were presented on Trials 1–5 in each set (see Experiment 3 and Table 1). Again, because all of the critical trials were to the -10° target in the NoRef condition, we would expect counterclockwise errors if participants are imposing the category boundary and clockwise errors otherwise. As this figure shows, errors for critical trials were all biased clockwise or near zero error. Errors for critical trials following the 25° target (dashed line) were particularly large, suggesting that long-term memory might play a role on critical trials.

We analyzed these data in a three-way ANOVA, with previous target (-10°, 25°), previous display condition (Ref, NoRef), and block (1–3) as within-subjects variables. As in Experiments 3 and 4, no effects reached significance. Additionally, we conducted planned comparison t tests to see whether errors on the critical trials were significantly different from zero error for any of the four sets. Errors were significantly biased clockwise on critical trials following the 25° target in the NoRef condition (Set 3 in Table 1), $t(17) = 2.82, p < .0125$, and following the 25° target in the Ref condition (Set 4 in Table 1), $t(17) = 2.45, p < .05$. This provides strong evidence that participants were not mentally imposing the boundary on critical trials.

Questionnaire responses. As in Experiments 4 and 5a, we asked participants to describe how they used the boundary as well as whether they felt they were able to mentally impose the line when it was not visible. All participants confirmed that “the presence of the line made it easier to remember the spaceship locations,” rating the line as relatively helpful, with an average rating of 7.39. These responses verify that participants were sensitive to their performance difference across display conditions.

Eight of the 18 participants reported that they “were able to use the line on trials where it wasn’t visible,” 7 of whom also said the line “helped on trials where it wasn’t visible.” As in Experiment 5a, fewer people felt they were able to mentally impose the boundary than in Experiment 4, even with the increased salience and richer interpretive context. The 7 participants who responded that the imposed boundary was helpful rated the imagined line as somewhat helpful, with an average rating of 4.29. All of the participants who said they could use the mentally imposed bound-

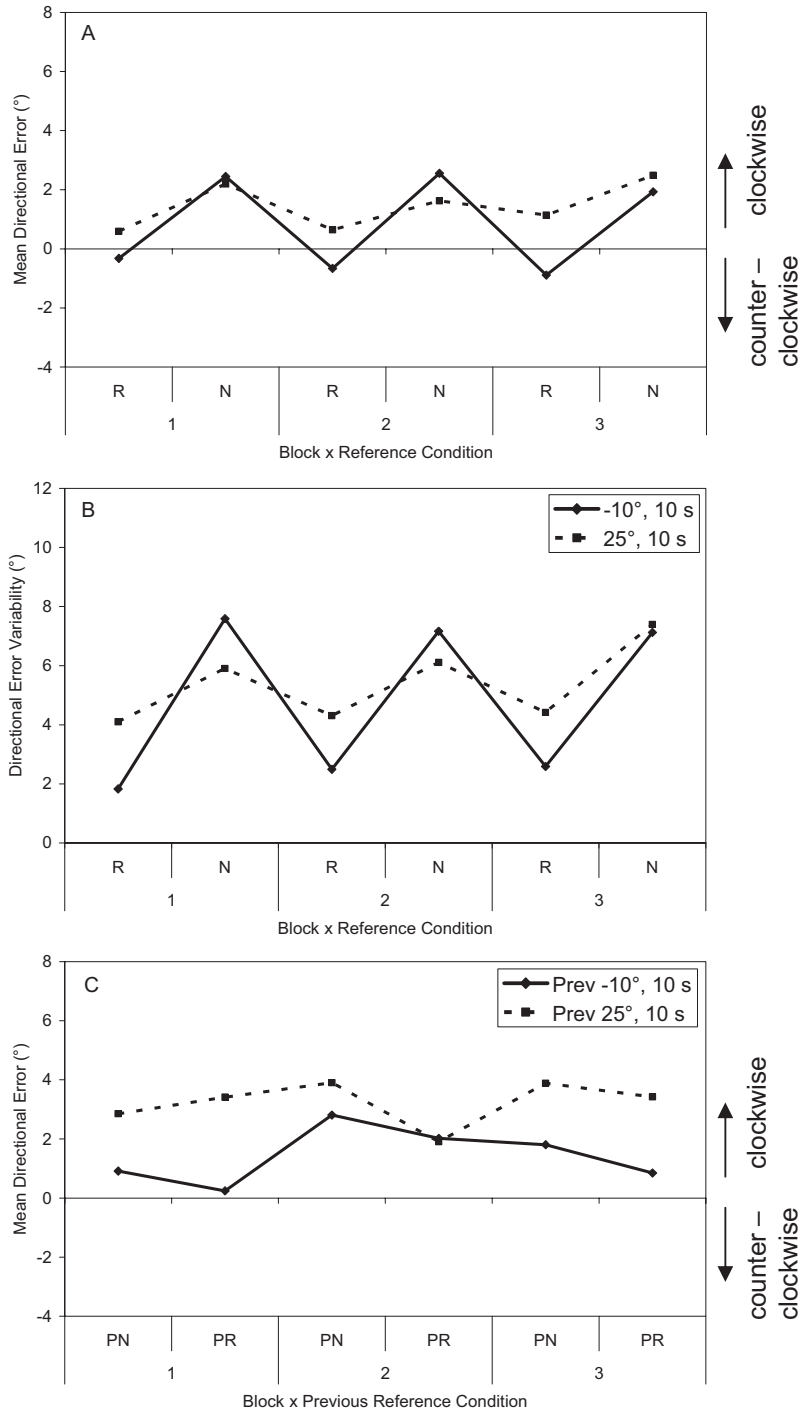


Figure 8. Mean directional error (A) and variability (B) across display conditions and blocks, collapsed across axes, for all trials in Experiment 5a; mean directional error on critical trials across previous (Prev) display conditions and blocks, collapsed across axes (C). Positive values indicate clockwise directional errors; negative values indicate counterclockwise directional errors. Labels *PR* and *PN* indicate critical trials on which the previous sets of trials were in the reference (R) and no-reference (N) conditions, respectively.

ary gave a lower rating on the helpfulness of this boundary relative to the visible boundary.

We also asked participants how they used the reference line (both when visible and when not visible) and if they had any

specific strategies. Again, participants' responses generally reported estimating the distance from the line to the spaceship and how far up the line the spaceship was located. When the line was not visible, most participants reported simply trying to remember

or imagine the line, although a few mentioned using other landmarks such as the appearance of the crosshairs as a cue for where the line should be. Overall, then, questionnaire responses were consistent with Experiments 4 and 5a. This is important because it suggests that participants' ability to use the line as a boundary, and their subjective feelings of being able to impose the boundary, were not adversely affected by half of the trials being presented around the left axis. Instead, participants' reports were consistent with previous experiments, where all trials were around the right axis.

Discussion

Results from this experiment suggest that the lower mean directional errors to the -10° target in Experiment 5a were strongly influenced by the build-up of a relatively accurate long-term memory on the Ref trials. In the current experiment, we moved half of the trials to the mirror image locations around a left axis, thereby reducing the build-up of long-term memory without reducing the total number of trials that participants completed. This restored the sawtooth pattern from Experiments 1–4 for the -10° target in mean error and maintained this pattern in response variability. Moreover, this sawtooth pattern generalized to the left region of the task space. In addition, analyses of critical trials showed significant clockwise errors when the previous target was at 25° . This was the opposite of what would be expected if participants were imposing the boundary.

It is important to emphasize that we found evidence against the mental imposition view in the present experiment even though we presented a single, salient reference line on Ref trials; we gave participants explicit instructions to impose the boundary on NoRef trials; we added an interpretive context to the game to highlight the boundary as a relevant cue; and we included sets of trials in which the reference line was visible for five trials in a row and participants had to impose the boundary across a single critical trial. Considered together, then, data from the five experiments in the present study suggest that people have, at best, a very weak ability to mentally impose an arbitrary spatial category boundary.

General Discussion

The goal of this study was to use a well-studied phenomenon—spatial category biases—to examine whether people can mentally impose arbitrary divisions on the world. Using our spaceship task, we varied the perceptual support for a lateral category boundary in otherwise empty space. Experiment 1 presented blocks of trials with the reference dots present, followed by blocks with the dots absent. Experiment 2 randomly intermixed trials with and without the reference dots. Results from these experiments suggested that participants were not imposing the boundary—instead, performance depended on the available perceptual structure. However, further inspection of the data from Experiment 2 indicated that participants may be capable of imposing the boundary under more supportive conditions. Experiment 3 provided more support by presenting ordered sets of five consecutive trials in the Ref condition before a critical trial in the NoRef condition. Experiment 4 added specific instructions asking participants to use the boundary even when it was not visibly specified. In these experiments, both the overall analyses and the specific analyses of critical trials

showed that participants still failed to impose the boundary. Experiment 5 tested whether changing the reference display from two dots to a line—in conjunction with explicit instructions that provided a richer interpretive context—would help participants impose the boundary. Although results from Experiment 5a indicated that participants might have a weak ability to mentally impose the line, results from Experiment 5b indicated that this apparent ability was linked to accurate long-term memories built-up over trials in the presence of the salient boundary. In particular, when we directly reduced participants' experience with the individual target locations in the task space, we once again found that participants were unable to mentally impose the category boundary even on critical trials. Taken together, results suggest that participants require perceptual support to divide space into categories and show systematic biases away from an arbitrary category boundary. Similarly, the reduction in variability that accompanies the use of category boundaries also relies on perceptual support.

These findings provide an important addition to the literature on spatial category boundaries. Previous research has shown that people can use perceptual structure like edges and symmetry axes to define spatial category boundaries. To our knowledge, the present study is the first to demonstrate that people are unable to impose a previously available boundary when perceptual support is no longer provided. These results qualify previous evidence that people can impose a boundary in alignment with a symmetry axis (Engelbreton & Huttenlocher, 1996; Huttenlocher et al., 2004, 1991; Plumert & Hund, 2001; Sandberg et al., 1996; Schiano & Tversky, 1992; Spencer & Hund, 2002; Spencer et al., 2006; Tversky & Schiano, 1989). In particular, our results—combined with evidence that the visual system treats symmetry axes in a manner similar to visible edges (Beh et al., 1971; Li & Westheimer, 1997; Wenderoth & van der Zwan, 1991)—suggest that in all of these cases, participants are grounding spatial category boundaries in the perceived structure of the task space.

Consider, for example, findings by Tversky and Schiano (Schiano & Tversky, 1992; Tversky & Schiano, 1989) that participants show different patterns of bias depending on task instructions. Specifically, they found bias away from the edges of an *L*-frame when the display was described as a graph but not when it was described as a map (Tversky & Schiano, 1989, Experiments 5 and 6). In a series of follow-up experiments, participants showed bias away from the diagonal symmetry axis when the displays were called figures instead of graphs (Schiano & Tversky, 1992, Experiments 1 and 2). However, when participants were explicitly instructed to use the diagonal as a reference line, the original pattern of bias away from the edges reemerged, regardless of whether the displays were described as graphs (Schiano & Tversky, 1992, Experiments 3 and 4). The authors attributed these differences in effects across experiments to the use of “cognitive reference frames.” Although this may be construed as mentally imposing a boundary under some conditions, in all cases the boundaries in use are visibly defined (albeit tenuously in the case of diagonal symmetry; see Palmer & Hemenway, 1978). Thus, rather than showing mental imposition of a boundary, this study shows that people can flexibly attend to different visible cues as the situation demands. Note that this interpretation is consistent with Tversky and Schiano's (1989) emphasis on the importance of perceived structure in spatial categorization.

Results like these, showing differential use of visible cues, suggest that there is a selection process involved in using spatial category boundaries. This raises an alternative interpretation of our results: Perhaps participants failed to impose an arbitrary boundary in our study, not because they are incapable of imposing boundaries but because they actively selected the stronger, midline symmetry axis in the NoRef conditions rather than using a weaker imposed axis. Our questionnaire data suggest this is generally not the case. In response to questions asking participants to describe how they performed the task on NoRef trials, 69% of participants directly reported that they tried to imagine the reference cues (dots or lines). Indeed, only 1 participant (out of 42) reported using the midline symmetry axis and this was in Experiment 5b where midline was directly in between the left and right boundaries. None of the other participants mentioned any other cues or potential competing boundaries.

Note that we do not raise the questionnaire data here to imply that the midline symmetry axis was not serving as a boundary on the NoRef trials—on the contrary, it is precisely this bias that allowed us to directly evaluate mental imposition without predicting a null effect. Rather, participants' questionnaire responses suggest they were not strategically or intentionally selecting among competing boundaries (i.e., midline vs. an imposed boundary). That said, even if people were strategically selecting midline over an imposed boundary, in our view this would point toward a cognitive ability that might not be terribly useful. We intentionally limited the amount of visual structure available in our task by using a large, homogeneous surface in a dimly lit room with few distal cues. Under these conditions, the midline symmetry axis should provide only weak perceptual structure. If this weak visible cue is enough to override an imposed boundary, then mental imposition would be of limited use in most situations, which are more richly structured than our task space and therefore contain strong competing cues.

The suggestion that participants engaged in a weak form of mental imposition also raises the possibility that we failed to find evidence of mental imposition because spatial category boundaries are particularly difficult to mentally impose. That is, use of spatial category boundaries might be a special, particularly constrained phenomenon. Our theoretical account of spatial category biases is consistent with this view. According to our dynamic field theory, memory drift over delays results from inhibition associated with perception of reference axes in the task space (for details see, e.g., Simmering et al., 2006; Spencer, Simmering, Schutte, & Schöner, 2007). Critically, these inhibitory effects only arise when perceptual cues are stably present. When reference cues are transient, our model predicts either no effect or a weakly attractive influence (Spencer et al., 2007).

At a global level, the fact that inhibition-based effects arise in our model because of the influence of perception on spatial memory processes is consistent with numerous other repulsion effects in the literature that also arise from perceptually grounded processes. For example, Tipper and colleagues have shown that pointing trajectories deviate away from objects within view, suggesting an area of inhibition surrounding the visible objects (Howard, Lupiáñez, & Tipper, 1999; Tipper, Lortie, & Baylis, 1992). As another example, briefly presented stimuli appear to be repelled away from the focus of attention (e.g., Suzuki & Cavanagh, 1997). Although the perceptually grounded nature of spatial category

biases may have stacked the deck against observing mental imposition in the present study, we chose this domain partly because previous accounts of spatial category biases viewed boundary placement as flexible and imposable (e.g., Huttenlocher et al., 1991). Our data suggest this is not the case.

If spatial category boundaries are indeed a special case that may be difficult to impose, perhaps other forms of mental imposition are still possible. A second domain in which similar questions about mental imposition have arisen is in the domain of language. For decades, theorists have considered what impact language might have on thought. Perhaps the strongest position on the side of language is the Whorfian hypothesis, which states that thoughts are structured by language, controlling even one's perception of the world (Carroll, 1956). The area of spatial language lends itself particularly well to probes of this hypothesis and an examination of whether spatial perception and memory can be structured by language (for a discussion of the real-time coupling between spatial language and spatial memory, see Spencer, Lipinski, & Samuelson, in press).

Different languages divide up spatial relationships in different ways. One common example of this is the Korean distinction between tight- and loose-fitting relationships (*kkita* and *nehta*, respectively), which is not distinguished in English. Rather, English distinguishes *in* and *on* relations in a way not reflected in Korean. McDonough, Choi, and Mandler (2003) compared how preverbal infants and adults responded to visual scenes showing the English versus Korean contrasts. Their results showed that 9- to 14-month-old infants responded to both of these distinctions, whereas adults responded according to their native language, essentially ignoring the distinction unsupported by their language. The authors concluded that language allows adults to categorize these spatial relations, consistent with the Whorfian hypothesis. Although this might be a case of mentally imposing language-relevant structure on the world, these data could also be interpreted as a linguistically guided selection of perceptual information relevant to one's native language. By this view, adults can still perceive the differences in the displays not contrasted in their own language, but they fail to use distinctions that are not behaviorally relevant on the basis of their past experiences. Thus, rather than language imposing arbitrary structure on the world, linguistic systems are constructed to capitalize on the rich structure available in the perceptual array. That is, both the *kkita-nehta* or *in-on* contrasts are perceptually supported, but adults' history with their language has led them to focus their attention on only one distinction, essentially disregarding the other distinction (for a discussion of these issues, see Choi & McDonough, 2007).

Another area that may provide insight into the interaction between language and spatial abilities is American Sign Language (ASL). When using ASL, the signer uses the physical space around him or her to convey meaning and as structure for discourse. For example, presenting identical motions in front of different parts of the body may indicate entirely different meanings (e.g., signs for *summer*, *ugly*, and *dry*; Petitto & Bellugi, 1988). For other signs, direction of movement in space may specify morphological characteristics (e.g., *give regularly* vs. *give over time*; Petitto & Bellugi, 1988). Signers may also indicate their familiarity with or preference for an object or person by presenting a sign closer to their own body (Petitto & Bellugi, 1988). Lastly, an obvious use of

space in ASL is to communicate about spatial relations between objects (e.g., *in front of* vs. *behind*; Emmorey, 2001).

Another example, particularly relevant to our question here, is the association of nominals with spatial loci. Specifically, a signer may establish a subject of discussion at a particular location in space in the beginning of a conversation. Then, when referring to that subject later in conversation, the signer may just point to that location (e.g., Emmorey, Corina, & Bellugi, 1995; Petitto & Bellugi, 1988). Both parties involved in the conversation must maintain this reference system in order to communicate effectively. Could this be an example of language imposing structure on space? This is an intriguing possibility. Unfortunately, no studies have examined how precisely reference frames are maintained in such situations and whether such frames are grounded in the current sensori-motor array. For instance, the body of the signer provides a stable reference frame because signs are presented in front of one's body. It is conceivable that reference frames and reference points used in conversation are arbitrary with respect to the perceptual structure of the space. It is also possible that the signer links a reference frame to stably perceived anchor points in the signing space. Given that people fluent in ASL spend years learning this form of communication (as is true of any language learning), examining the issue of mental imposition with this population could probe the upper range of mental imposition abilities given that ASL speakers might have developed expertise in this area. That said, any probe of arbitrary mental imposition of spatial category boundaries with ASL speakers would have to control for the many sources of perceptual structure examined here.

This reemphasizes the lengths to which we had to go to probe the use of arbitrary spatial category boundaries, bringing us back to the bigger picture raised by the embodied perspective—cognition is situated in the body and must use the body to act in the world (Barsalou, 1999; Gibson, 1966; Glenberg, 1997; Thelen & Smith, 1994). Because the world provides such rich structure, the cognitive system may have no functional need to impose arbitrary structure. Consider the task we used here. To ensure that we could isolate effects of imposing a boundary, we had to precisely control the environment, reducing the visible cues in the room and on the table, preventing participants from fixating the remembered locations, and ruling out the possible use of proprioceptive cues. Although these manipulations provided a strong test of our empirical question, they result in an extremely artificial situation. Even the example of Puluwat navigation through the ocean with limited visual cues is less extreme—the Puluwat can use their own movement, dead-reckoning cues, or apparent motion of the stars to guide their navigation. Given the artificiality required to test the mental imposition view, perhaps our question should not be whether the mind can impose arbitrary divisions on the world. Rather, we might benefit more from considering how the mind uses what is provided by the richly structured world.

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