Journal of Experimental Psychology:
Learning, Memory, and Cognition
2015, Vol. 41, No. 1, 220–234

0278-7393/15/$12.00 http://dx.doi.org/10.1037/xlm0000032

Piloting and Path Integration Within and Across Boundaries

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Three experiments investigated whether navigation is less efficient across boundaries than within boundaries. In an immersive virtual environment, participants learned objects' locations in a large room or a small room. Participants then pointed to the objects' original locations after physically walking a circuitous path without vision. For participants who learned the objects in the large room, the testing position and the learning position were in the same room so that participants did not cross boundaries before testing; for participants who learned the objects in the small room, the testing position and the learning position were in 2 different rooms so that participants crossed boundaries before testing. Participants who learned the objects in the large room, during testing, either saw cues indicating the targets' locations (piloting group) or not (path integration group). Participants who learned the objects in the small room, during testing did not see any cues correctly indicating the targets' locations. The results showed that pointing accuracy was higher for those who learned the objects in the large room and in the piloting group than for those who learned the objects in the small room. However, this cross-boundary cost did not occur when we contrasted participants who learned objects in the large room and in the path integration group with participants who learned in a small room. These results suggested that navigation that relies on path integration only is not sensitive to boundary crossing, although navigation that relies on piloting is less efficient across boundaries than within boundaries.

Keywords: piloting, path integration, navigation, spatial memory, boundary

It is common for people to live in environments separated by boundaries. For example, the interior walls of a house separate the house into different rooms (e.g., living room, bedroom). The exterior walls of a house separate the house from the world outside the house. As boundaries separate environments and define individual spaces, studies of spatial cognition have found that boundaries play important roles in human spatial memory and navigation.

Boundaries are important in spatial memory and navigation within each individual environment. In spatial memory, boundaries define places within the boundaries (e.g., Burgess, 2008; Doeller & Burgess, 2008; Hartley, Trinkler, & Burgess, 2004; Mou & Zhou, 2013; O'Keefe & Burgess, 1996). Hartley et al. (2004) reported that human participants' searching behaviors in a rectangular room could be modeled using the distance between the goal positions and the four walls of the room. Kelly, Sjolund, and Sturz (2013) also reported that the global geometry of a boundary (i.e., a principal axis of a room) could determine a spatial reference direction that was used to specify spatial relations between objects in the boundary (see also Kelly & Avraamides, 2011; Shelton & McNamara, 2001). During navigation, people need to know their position and orientation in the environment. A large body of literature has indicated that human and nonhuman animals use global shapes of boundaries to reorient themselves after disorientation (Cheng, 1986; Hermer & Spelke, 1996; see Cheng & Newcombe, 2005, for a review). Kelly, McNamara, Bodenheimer, Carr, and Rieser (2008) reported that the global shape of a boundary is important not only for reorientation but also for orientation maintenance during continuous locomotion. Their results showed that performance in recognizing the origin of a path was comparable in trapezoidal, rectangular, and square rooms (one-fold, two-fold, and four-fold rotationally symmetric rooms, respectively), but it was worse in a circular room (∞-fold rotationally symmetric). These results suggest that the angular shape of the boundary may help to maintain spatial orientation.

Because boundaries separate the surrounding environment into individual local environments, human spatial representations of the surrounding environment seems fragmented and distorted (Brockmole & Wang, 2002; McNamara, 1986; Stevens & Coupe, 1978). Human spatial memory most likely involves a collective of *local maps*, and these various *local maps* may not be accurately interrelated (Han & Becker, 2014; McNamara, Sluzenski, & Rump, 2008; Meilinger & Vosgerau, 2010; Poucet, 1993). Just as contextual changes impair remembering in general (Tulving & Thomson, 1973), it has also been evidenced that boundary shifts impair remembering spatial relations across boundaries. Studies have found that the priming effect from a preceding trial on the following trial was greater when spatial information in two consecutive judgments was within a boundary than when spatial information in two consecutive judgments was across boundaries (e.g., Brock-

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Preparation of this article and the research reported in it were supported in part by a grant from the Natural Sciences and Engineering Research Council of Canada and the Qianjiang Scholarship, Hangzhou Normal University, China, to Weimin Mou. We are grateful to Keith Baldoz, Taylor Murphy, Lauren Poon, and Sarah Tan for collecting data.

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mole & Wang, 2002; McNamara, 1986). Studies have also shown that the judgment of spatial relations was less accurate when the spatial relations were across boundaries than when the spatial relations were within boundaries (McNamara, 1986; Montello & Pick, 1993; Stevens & Coupe, 1978). Kosslyn, Pick, and Fariello (1974) showed that both human adults and children judged a distance between two objects longer when the two objects were separated by an opaque barrier than when there was no barrier between these two objects. Wiener and Mallot (2003) reported the influence of regions or boundaries on route planning behaviors. However, there are few studies investigating whether navigation across boundaries is less efficient than navigation within a boundary.

Radvansky and Copeland (2006) found that the tracking of objects' locations was impaired across boundaries. In their study, the participants navigated in a virtual environment presented on a large display screen by pressing arrow keys on a keyboard. The results indicated that accuracy in keeping track of the objects in a room decreased when the participants walked outside of the room. Wang and Brockmole (2003a) also showed that the tracking of objects' locations was impaired when participants physically walked across boundaries. In one of their experiments (Experiment 2), the participants walked a path starting in a laboratory room in a building (i.e., a psychology building), passing outside the building, walking around the building, passing inside the building, and finally ending in the original laboratory room (see Figure 2 in Wang & Brockmole, 2003a). During the path, the participants were probed to point in the correct direction of another campus building, the student union. Immediately after the participants could point to the student union building, they were asked to point to the original laboratory room. The results showed that the majority of the participants could not point to the student union building until they were outside the psychology building. When the participants could point to the student union building, they usually could not point to the laboratory room. Similarly, Street and Wang (2010) reported that pointing to an office across floors was less accurate than pointing to an office on the same floor.

Wang and Brockmole (2003a) proposed that because spatial memories of different environments are separate (Brockmole & Wang, 2002) and spatial updating capacity is limited (Wang et al., 2006), navigation between two environments involves a "map switching" process. According to this proposal, when the participants walked inside the psychology building, they may have only used the *map* of the psychology building, which explains why they could not point to the student union building that was on the map of the campus. When they walked outside the psychology building, they may have switched to the *map* of the campus, thus allowing them to point to the student union building but not allowing them to point to the original laboratory room in the psychology building. Wang and Brockmole (2003b) furthermore demonstrated that *map switching* can be influence by instruction. Their results showed that people automatically updated the immediate environment but not a remote environment. However people could efficiently update a remote environment if they were instructed to do so. Hence, the cross-boundary cost might be a result of prioritization in the spatial updating system.

In the current study, we propose that in addition to the prioritization in the spatial updating system, as proposed by Wang and Brockmole (2003a, 2003b), the cross-boundary cost might also depend on the navigation mechanisms that people use. During navigation, people know where they are relative to previously visited but not immediately visible places using two types of mechanisms: piloting and path integration (dead reckoning, Gallistel, 1990; Gallistel & Matzel, 2013). Piloting is the process of determining the location of an invisible target by relying on the visible items (e.g., landmarks) and the representation of the spatial relations between the visible items and the invisible target (e.g., Foo, Warren, Duchon, & Tarr, 2005). The most basic step in piloting is to find the geocentric orientation in the environment, in which the visible items were located, because the spatial relations between the invisible target and the visible items are encoded in terms of the geocentric orientations (Gallistel & Matzel, 2013). Path integration is the process of determining the location of an invisible target (e.g., the origin of the path) on the traversed path by estimating the traversed distance and direction (e.g., Loomis, Klatzky, Golledge, & Philbeck, 1999; Mittelstaedt & Mittelstaedt, 1980; Philbeck, Klatzky, Behrmann, Loomis, & Goodridge, 2001). Foo et al. (2005) reported that participants used both mechanisms of navigation in an intermediate-sized environment. Piloting may be dominant when piloting and path integration conflict and indicate slightly different positions of the same targets. However, path integration became dominant when participants noticed that the piloting cues had been displaced.

In particular, we propose that navigation that relies on piloting is less efficient when people cross boundaries than when people remain within a boundary, whereas navigation that relies on path integration is as efficient when people cross boundaries as when people stay within a boundary. As reviewed above, local environments separated by boundaries are represented separately (e.g., Brockmole & Wang, 2002). When the visible landmarks and the invisible target are located in two separate environments, their spatial relations might not be directly represented or may be less accurately represented (McNamara, 1986; Montello & Pick, 1993; Stevens & Coupe, 1978; Wang & Brockmole, 2003a). Because piloting relies on the representations of the spatial relations between visible items (e.g., landmarks, features of a boundary) and the invisible target, piloting should be more accurate when the visible items and the invisible target are in the same environment than when they are in different environments. In contrast, path integration uses internal locomotion cues (e.g., vestibular and proprioceptive cues) and optical flows to determine walking directions and distances (Chance, Gaunet, Beall, & Loomis, 1998; Riecke, van Veen, & Bulthoff, 2002; Tcheang, Bulthoff, & Burgess, 2011). Hence, whether the visual landmarks and the invisible target are in the same environment or in different environments should not affect path integration. We refer to this hypothesis as the mechanism-dependent hypothesis.

To interpret the cross-boundary cost in previous studies (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a), the mechanism-dependent hypothesis speculates that participants in these studies primarily relied on piloting. In Radvansky and Copeland's (2006) study, the participants did not physically move. Hence, the path integration mechanism was the least involved, and participants relied primarily on piloting. Participants in Wang and Brockmole's (2003a) study may also have relied primarily on piloting because path integration is noisy and not very useful over large distances (e.g., Loomis et al., 1999), such as the distances used by Wang and Brockmole. When the participants in their study were inside the psychology building, they could not see visual landmarks outside. Hence, they could not point to the student union building. When they were outside the psychology building, they were able to point to the student union building because they saw landmarks that clearly indicated the orientation of the environment and landmarks that indicated the direction of the student union building.

In contrast to the mechanism-dependent hypothesis, there are two possible mechanism-independent hypotheses. The first hypothesis is that regardless of the navigation mechanism, navigation is impaired by boundary crossing. The other hypothesis is that regardless of the navigation mechanism, navigation is not impaired by boundary crossing. The latter hypothesis is implausible because the cross-boundary cost was reported in previous studies when visual cues were available (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a). Hence, the only meaningful alternative hypothesis in the current study is that regardless of the navigation mechanism, navigation is impaired by boundary crossing. We refer to this as the mechanism-independent hypothesis below. According to this hypothesis, when participants walk outside a boundary, regardless of the navigation mechanism, they lose track of the objects in the original boundary. Therefore, this mechanism-independent hypothesis also can explain the inferior navigation across boundaries in previous studies (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a). Similar to the mechanism-independent hypothesis, the “map switch and prioritization” hypothesis proposed by Wang and Brockmole (2003a, 2003b) did not predict that cross-boundary effects would differ when people use the two different navigation mechanisms.

The mechanism-dependent hypothesis and the mechanism-independent hypothesis both predict an inferior navigation across boundaries when people primarily use piloting. Hence, an inferior navigation across boundaries when people use piloting cannot distinguish between these two hypotheses. Consequently, the previous studies (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a) could not distinguish between these two hypotheses as participants in these studies might primarily use piloting. However, these two hypotheses have different predictions when people navigate only relying on path integration. The mechanism-independent hypothesis predicts that navigation across boundaries is less efficient than navigation within boundaries when people only use the path integration mechanism. In contrast, the mechanism-dependent hypothesis predicts that navigation across boundaries and navigation within boundaries is comparable when people only use the path integration mechanism. To distinguish these two hypotheses, we designed three experiments to compare navigation across boundaries and navigation within boundaries when participants only relied on path integration to navigate.

Any cross-boundary cost observed when people have piloting cues is not novel (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a) and cannot distinguish between the mechanism-independent hypothesis and the mechanism-dependent hypothesis. Hence, only Experiment 1 of the current study replicated the cross-boundary cost when participants used piloting cues in contrast to conditions in which the participants could only use path integration. Experiments 2 and 3, however, only included conditions in which the participants could only use path integration. It is important to note that the mechanism-dependent hypothesis predicts the null effect of cross-boundary when participants could only use path integration. Gallistel (2009) stated that although the conventional statistical analysis cannot support a null effect, Bayesian analysis can. To support the mechanism-dependent hypothesis, we used Bayesian analyses to support the null effect of cross-boundary when participants could only use path integration in the data analysis section of each experiment.

In all experiments, the participants viewed the boundaries via immersive virtual reality systems, so we could easily manipulate the size of the boundary and the availability of the piloting cues while keeping all other variables unchanged.

Experiment 1

The purpose of Experiment 1 was to demonstrate no cross-boundary cost when people could only rely on path integration. To replicate the cross-boundary cost reported in the previous studies (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a), Experiment 1 also included conditions in which participants could use piloting in navigation.

Participants learned the locations of five objects in one physical room (learning room in Figures 1A and 1B). They then walked a circuitous path to a second physical room (testing room in Figures 1A and 1B). At the end of the path, the participants pointed to the original locations of the objects. The participants never saw the physical rooms; instead, they saw virtual rooms. In the across-boundary conditions, the learning position and the testing position were in two small virtual rooms (Figure 1C), whereas in the within-boundary conditions, the learning and the testing positions were in the same large virtual room (see Figure 1D).

As discussed in the introduction, the important steps in piloting are to find the orientation of the environment and to see the landmarks in the environment. To facilitate piloting, the learning room, regardless of size, had a unique orientation cue formed by colored walls (see Figures 1C and 1D).

Half of the participants, regardless of whether they had learned objects in the large room (i.e. across-boundary) or in the small room (i.e. within-boundary), visually saw a room at test. In particular, those who had learned in the large room saw the same large room at test, whereas those who had learned in the small room saw the other small room. We expected that participants in the large room would perform better than those in the small rooms because both hypotheses predict such results. According to the mechanism-independent hypothesis, navigation is less efficient across boundaries than within boundaries regardless of the mechanisms used for navigation. Because participants in the large room did not cross boundaries, whereas participants in the small room crossed boundaries, participants in the large room would perform better than those in the small rooms. According to the mechanism-dependent hypothesis, navigation that relies on piloting is less efficient when participants cross boundaries. Participants in the same large room could use the colored walls in the room during testing to infer the locations of the targets because the colored walls not only provided the orientation of the environment but also functioned as a reference point to encode the location of the target. Hence, participants in this condition had valid piloting cues. However, the participants who were tested in a different small room could not use the visual cues in the testing phase to infer the locations of the target objects because the relationship between the learning room and the testing room could not be directly perceived. Hence participants in this

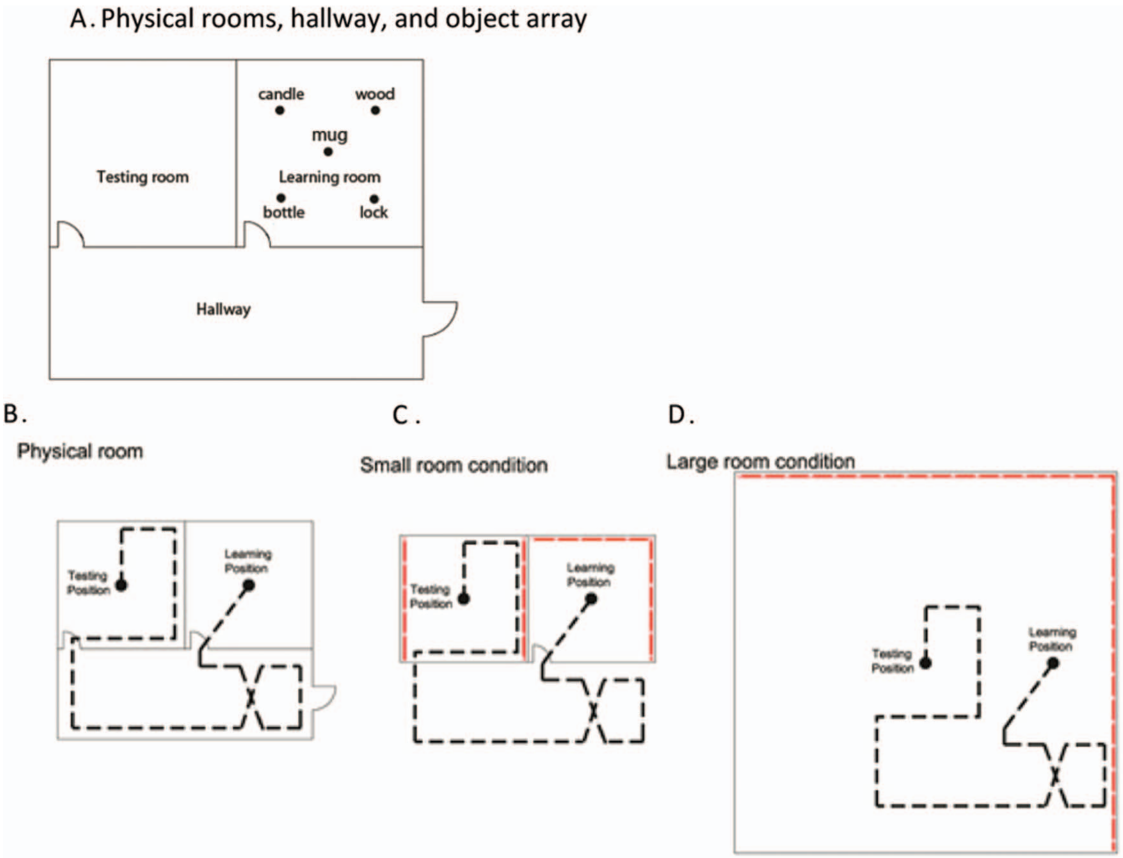


Figure 1. A. The two physical rooms, the physical hallway, and the object array in the learning room for Experiments 1 through 3. Each room has a door to the hallway. The participants never saw these physical environments. B. The walking path between the learning position and the testing position relative to the physical environment in Experiment 1. C. The virtual small learning room and the virtual small testing room as well as the walking path relative to the virtual rooms in Experiment 1. The virtual small rooms were the same size as the physical rooms and exactly overlapped the physical rooms. The dotted lines around the edges indicate the red colored walls in the virtual rooms. D. The large virtual room in both the learning and testing phases and the walking path relative to the virtual room in Experiment 1. The right wall of the virtual room overlapped with the right wall of the physical learning room. The dotted lines around the edges indicate the red colored walls in the virtual room. Note that the walking paths illustrated in panels B, C, and D were identical relative to the physical environment. See the online article for the color version of this figure.

condition did not have a valid piloting cue. As a result, participants who studied in the small room and saw the different room during test would perform worse than those who studied in the large room and saw the same large room during test.

The other half of the participants, regardless of whether they had learned objects in the large or in the small room, did not see a room at test. Instead, they only saw a floor of infinite size in an open field. Hence, these two groups of participants could only rely on path integration when they pointed to the locations of the objects. The comparison between these two conditions tested whether navigation that relies on path integration is impaired by boundary crossing; thus, the comparison could differentiate between the mechanism-independent hypothesis and the mechanism-dependent hypothesis. The mechanism-independent hypothesis predicted better performance in the within-boundary condition than in the across-boundary condition when navigation relied on path integration. In contrast, the mechanism-dependent hypothesis predicted comparable performances in these two conditions when navigation relied on path integration.

Method

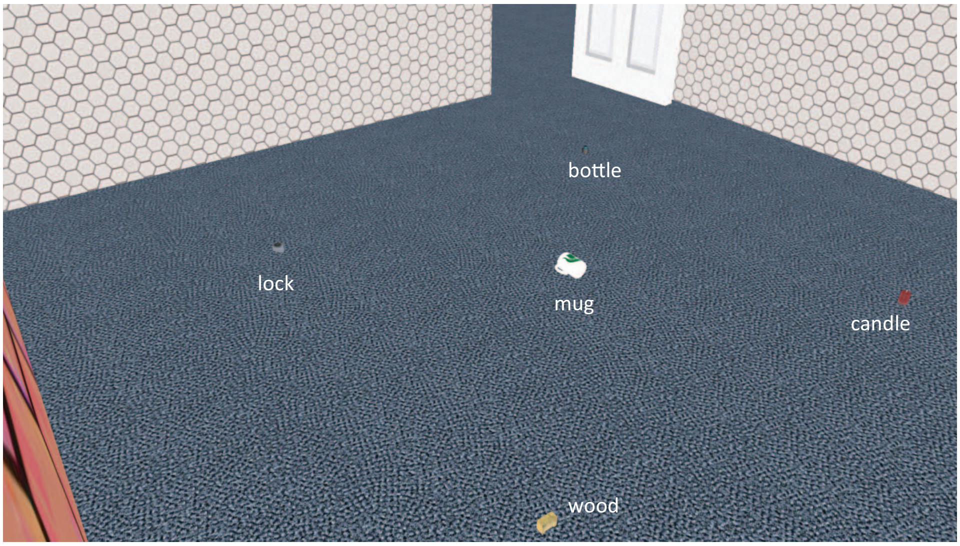
Participants. Eighty university students (40 men and 40 women) participated in this experiment as partial fulfillment of a requirement in an introductory psychology course.

Materials and design. The experiment was conducted in a laboratory of 11 m ×11 m. In the laboratory, there were two physical rooms (4.4 m × 4.4 m each) and a physical hallway (3.1 m × 8.8 m) outside of the rooms (see Figures 1A and 1B). The physical rooms are illustrated by the two squares in Figures 1A and 1B. The hallway is illustrated by the rectangle in Figures 1A and 1B. The participants in all experimental conditions stood at the same learning position, walking the same path consisting of 12 major turns, and had the same testing position in the physical environment. However none of the participants saw the physical environment.

The virtual room was displayed in stereo using an nVisor SX60 head-mounted display (HMD; NVIS, Inc.). The participants' head motions were tracked with an InterSense IS-900 motion tracking system (InterSense, Inc.) that was installed in each of the two physical rooms (Figures 1A and 1B) so that participants could see the virtual environments when they were in each of the physical rooms.

There were four conditions through a combination of two experimental factors: room size and visual cue. The two virtual small rooms (Figure 1C) each measured 4.4 m × 4.4 m and exactly overlapped the physical rooms (Figure 1B). The relations between the virtual small rooms and the physical rooms could be further specified by superimposing the walking path relative to the physical room (Figure 1B) and the walking path relative to the virtual small rooms (Figure 1C). One snapshot of the virtual small learning room is presented in Figure 2A. The virtual large room measured 13.2 m × 13.2 m (Figure 1D) and enclosed the physical rooms (Figure 1B), with the *right* wall of the virtual room meeting the *right* wall of one physical room (the right room in Figure 1A). The relations between the virtual large room and the physical rooms could be further specified by superimposing the walking path relative to the physical room (Figure 1B) and the walking path

A. small room condition



B. Large room condition

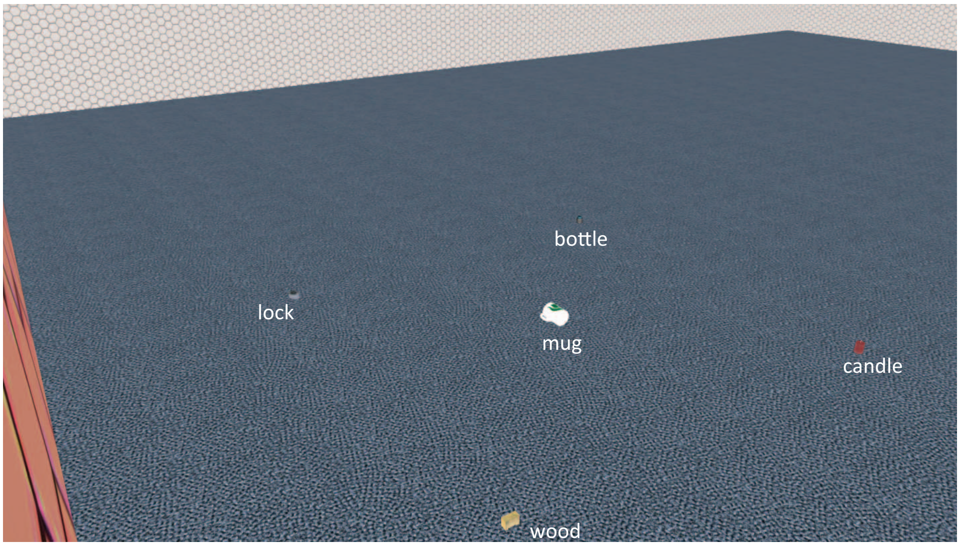


Figure 2. Snapshots of the virtual rooms in Experiment 1. The labels of the objects are added for readers only. A. The learning room in the small room condition. B. The learning room in the large room condition. See the online article for the color version of this figure.

relative to the virtual large room (Figure 1D). One snapshot of the virtual large learning room is presented in Figure 2B. The length of the large room was three times the length of the small room so that participants in the large room conditions stood at the center of the virtual room at test as participants in the small room condition. In the visual cue condition, the participants saw a room in the testing phase, whereas in the no-visual-cue condition, the participants only saw an open area at testing.

Twenty participants were randomly assigned to each of the four conditions with an equal number of males and females in each condition. The cues seen by the participants in different conditions during the learning phase and during the testing phase are summarized in Table 1. In particular, the participants in the Large Room × Visual Cue group saw the same virtual large room in the learning and the testing phases. The participants in the Small Room × Visual Cue group saw different virtual small rooms in the learning and testing phases. In both the small and the large rooms during the learning phase, two adjacent walls were painted with large red hexagons (illustrated by the dotted lines around the edges in the learning room of Figures 1C and 1D), and the other two walls were painted with small white hexagons to provide one unique orientation of the room. Hence, the walls could act as piloting cues during testing for the participants in the Large Room × Visual Cue group. In the small room during testing, two opposite walls were painted with large red hexagons (illustrated by the dotted lines around the edges in the testing room of Figure 1C), and the other two walls were painted with small white hexagons to indicate that it was a different room from the room where participants learned objects. The participants in the Large Room × No-Visual-Cue group saw the virtual large room in the learning phase but not in the testing phase. The participants in the Small Room × No-Visual-Cue group saw the virtual small room in the learning phase but not in the testing phase.

The participants in all conditions learned the locations of five virtual objects (mug, candle, wood, lock, and bottle) in the learning phase. One object (i.e., mug) was presented at the learning position, between participants' feet. The other four objects formed a square (see Figures 1A and 2). The center of the square was the learning location (i.e., the location of mug), and the edges of the square (e.g., from bottle to candle) were aligned with the walls of the room and measured 2 m in length. The physical versions of the five objects were placed in the same positions as their virtual versions so that the virtual version of the objects exactly overlapped the real objects. Note that the objects were located at the same locations in terms of the physical environment across different conditions.

At the beginning of the learning phase, the participants in all conditions were guided by the view of the virtual versions of the objects, walked toward and touched the physical versions so that they could calibrate the scale of the virtual environment with the physical environment. The right wall of the virtual room that the participants saw in the learning position, whether in the small or the large room, exactly met the right wall of the physical room (e.g., the right room in Figure 1A) in which the learning position was located. At the beginning of the learning phase, while the participants in all conditions saw the right wall of the virtual room, they walked toward and touched the right wall of the physical room so that they could again calibrate the scale of the virtual environment with the physical environment.

To help the participants in the small room groups know that they had walked out of the small room when they walked to the testing position, a virtual door was presented (see Figures 1C and 2A) and superimposed over the physical door in the small room where participants learned objects (see Figure 1B). The participants in the small room groups physically touched the physical door and the door frame when they walked outside the room. The participants in the large room groups did not touch the physical door or know of the existence of the physical door when they walked outside the physical learning room. Hence, they were not supposed to know that they walked outside a boundary. Furthermore, the participants in the small room groups were told that they would walk to a different room for testing, whereas the participants in the large room groups were told that they would walk to a different position in the same room for testing. In any group, participants were not told whether a room would be presented or not in the testing phase.

The participants pointed to the objects using a virtual stick that was controlled by an InterSense wand (InterSense, Inc.), the location and orientation of which was tracked. This approach allowed the participants who held the wand to indicate the locations of the virtual objects by pointing the virtual stick at the objects' locations on the virtual floor. The experience was analogous to dragging desktop icons using a mouse. The primary dependent variable was the absolute angular pointing error, which was measured by the angular difference between the direction from the testing position to the estimated location of a target and the direction from the testing position to the correct location of the target in the learning room. Signed angular error was also analyzed using circular statistics (Batschelet, 1981). No response latency was collected.

Procedure. The blindfolded participants were guided into the laboratory. The main door of the laboratory was closed so that participants could not use auditory cues outside the laboratory to determine their locations or orientation in the laboratory. The participants were disoriented in the hallway of the lab (Figure 1A) so that they could not use the entrance of the laboratory as a reference point. Next, the participants were led to the center of the physical learning room and oriented so they were facing one wall

Table 1
The Visual Cues Presented to Participants in Different Conditions During the Learning Phase and the Testing Phase in Experiment 1

Condition	Learning	Testing
Large Room × Visual Cue	Large room in Figure 1D	Large room in Figure 1D
Small Room × Visual Cue	Right room in Figure 1C	Left room in Figure 1C
Large Room × No Visual Cue	Large room in Figure 1D	Infinite floor only
Small Room × No Visual Cue	Right room in Figure 1C	Infinite floor only

(i.e., the top wall in Figure 1A). They then donned the HMD and removed the blindfold. The participants were required not to look at the physical room ("please do not peek at the physical environment"). The participants were instructed to look around the virtual room and to walk toward and physically touch the wall on their right. After they had touched the wall, they were led to the learning position again and instructed to learn five objects on the ground. At the beginning of the learning phase, they were instructed to find all of the virtual versions of the objects, walk toward the objects, and touch the physical version of each object. They were then led back to the learning position and learned the location of the objects. The participants studied the locations for 3 min. Then, all virtual versions of the objects disappeared. The participants were asked to put the objects back in their original positions using the InterSense wand while standing at the learning position. After the participants replaced each object, the same object was placed in the correct position as a means of feedback for the participants. Each participant engaged in 20 such trials, four trials for each of the five objects. After they finished these trials, the participants closed their eyes. The HMD was removed. Then the participants put on blindfolds. The participants were instructed to point to each object once with their fingers. They were then asked to turn 135° counterclockwise (facing the bottle, see Figure 2) and were instructed to point to each object again using their fingers. Last, they walked forward for 1.41 m, standing at where the bottle was located, and then were asked again to point to each object using their fingers.

Before walking outside the physical learning room, in the large room condition, the participants were told that they would be led to the test position in the same room and their memory would be tested at that test position. In the small room condition, the participants were told that they would be led to another room to perform the test. The participants who learned objects in the virtual small room touched the physical door and the physical door frame when they walked outside the room, whereas the participants who learned objects in the virtual large room did not touch anything. The participants were then led to the test position along the path (Figure 1B) and oriented along the last leg of the path. After the participants removed their blindfolds and donned the HMDs in the physical testing room, they were shown the virtual environments during the test. The participants in the visual cue conditions saw the virtual room in the testing position. After the participants said that they were ready to point to the objects, the room disappeared, and an open field was presented such that the walls in the small room did not block the participants' views when they replaced the objects. The participants in the no-visual-cue conditions only saw the open field in the testing position. They were asked to replace the objects in their original positions using the wand. No feedback was given during the testing phase. Each participant completed 20 testing trials, which consisted of four trials for each object.

Results and Discussion

Mean absolute angular pointing error as a function of room size and visual cue is plotted in Figure 3A. The absolute angular error was computed for each participant and each condition and then analyzed using an analysis of variance (ANOVA) with terms for room size and visual cue, both as between-subjects variables.

The main effect of room size was significant, $F(1, 76) = 5.80$, $MSE = 2640.00$, $p = .019$, $\eta_p^2 = .07$. The main effect of visual cue was not significant, $F(1, 76) = 0.04$, $MSE = 2640.00$, $p = .84$, $\eta_p^2 = .001$. The interaction between room size and visual cue was significant, $F(1, 76) = 7.23$, $MSE = 2640.00$, $p = .009$, $\eta_p^2 = .09$. The interaction occurred because angular error was smaller in the large room than in the small room when there was a visual cue, $t(76) = 3.60$, $p = .001$, whereas angular error was comparable in the large room and in the small room when there was no visual cue, $t(76) = 0.20$, $p = .84$. Furthermore, in the large room conditions, the participants who saw the testing room performed significantly better than those who did not see the testing room, $t(76) = 2.04$, $p = .04$. In the small room conditions, there was no significant difference between those who saw the testing room and those who did not, $t(76) = 1.76$, $p = .08$. The mean pointing error (66°) averaged across the two no-visual-cue groups was significantly smaller than 90° (chance level), $t(76) = 2.95$, $p = .004$.

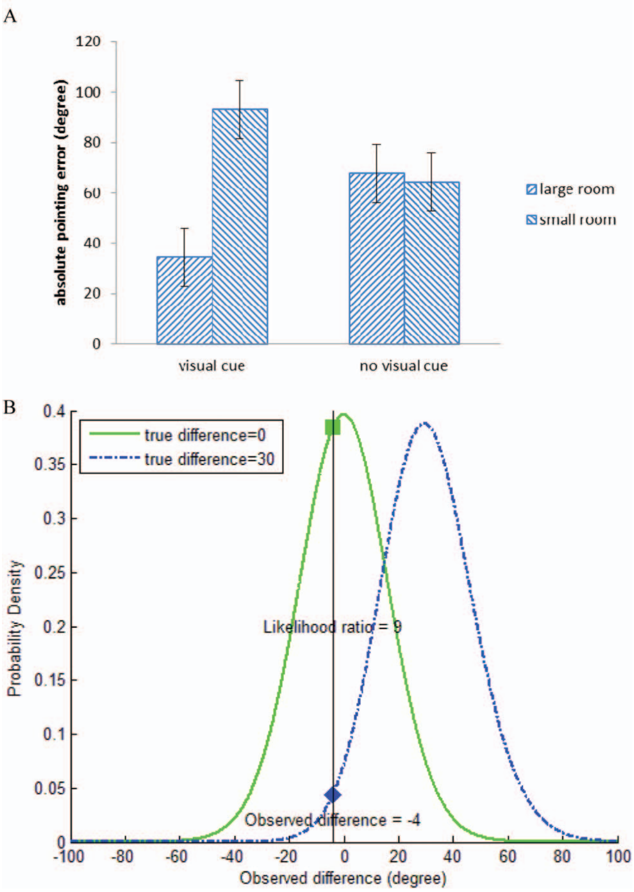


Figure 3. A. Mean absolute pointing error as a function of visual cue and room size in Experiment 1. The error bar is the standard error estimated in the analysis of variance. B. Likelihood (i.e., probability density) as a function of observed difference between the no-visual-cue groups under the null hypothesis that claims no boundary effect (true difference = 0) and under the alternative hypothesis that claims a boundary effect (true difference = 30). See the online article for the color version of this figure.

The circular mean of the signed angular pointing error and circular standard deviation across conditions are illustrated in Table 2. For each condition, the Rayleigh Z test was used to assess whether the signed angular error was a sample of a uniform

Table 2
Means (Circular Standard Deviation) of Signed Pointing Errors in All Experiments

Variable	Large room	Small room
Experiment 1		
Visual cue	5° (41°)	−112° (140°)[a]
No visual cue	29° (83°)	3° (76°)
Experiment 2	0° (88°)	2° (92°)
Experiment 3	−9° (75°)	16° (63°)

[a] Indicates a significant difference in the circular dispersion between the large and small rooms.

distribution, which indicates no clustered pointing direction across participants, or whether it was not a sample of a uniform distribution, which indicates a clustered pointing direction across participants. The confidence interval of the mean was also calculated for the conditions in which the signed angular error was clustered. The confidence interval of the mean estimation was used to test whether the clustered mean direction differed from 0°. If the mean direction did not differ from 0°, the participants might not have had a systematic bias in pointing.

The Rayleigh Z test showed that the angular error in the Small Room × Visual Cue group did not have a clustered direction ($Z = .052$, $p = .95$). In contrast, the angular error in the Small Room × No-Visual-Cue group had a clustered direction ($Z = 3.42$, $p = .03$), and the angular error in the group of Large Room × Visual Cue also had a clustered direction ($Z = 12.11$, $p < .001$). The confidence intervals in the latter two groups included 0°; therefore, the means (i.e., 5° and 3°) did not differ from 0°, indicating that the participants did not have systematic bias in pointing. The angular error in the Large Room × No-Visual-Cue group did not have a clustered direction ($Z = 2.41$, $p = .09$), although it might be due to the power issue.

The circular dispersion of the angular error between the different-sized rooms in each visual cue condition was also compared. The angular error was less variable in the large room than in the small room when there were visual cues (see Table 2), $F(19, 19) = 4.27$, $p = .001$ (Batschelet, 1981, p. 122). In contrast, the variance of angular error was comparable in the large room and in the small room when there were no visual cues (see Table 2), $F(19, 19) = 1.11$, $p = .41$ (Batschelet, 1981, p. 122). These results were consistent with the results in terms of the absolute error.

One concern is that the orientation cue in the Small Room × Visual Cue condition might be misleading. Participants in the Small Room × Visual Cue condition might be confused about which direction the room was oriented because the red walls in the room where they were tested were not the same as the walls in the room where they learned the objects.[1] Hence, the boundary effect, the smaller pointing error in the Large Room × Visual Cue condition than in the Small Room × Visual Cue condition might be due to the confusion in the small room due to confusing orientation cues. To remove this potential artifact, we also compared the absolute pointing errors in the Large Room × Visual Cue condition and in the Small Room × No-Visual-Cue condition, in which there were no misleading orientation cues. One-tailed t test indicated that the difference (30°) was significant, $t(76) = 1.84$, $p = .04$. Hence, the boundary effect due to piloting within boundaries and no piloting across boundaries (i.e., piloting vs. no piloting) is still evidenced and the true effect is about 30°, which would be used as the estimated boundary effect in the Bayesian analyses discussed in the next paragraph and in Experiments 2 and 3.

The null effect in the no visual groups is supported by the interaction between room size and visual cue. We also did Bayesian analyses to contrast the null hypothesis claiming no boundary effect and the alternative hypothesis claiming a boundary effect (Gallistel, 2009; Kelly et al., 2013; Rouder, Speckman, Sun, Morey, & Iverson, 2009). In particular, we examined whether the observed difference ($64° - 68° = -4°$) between the two no-visual-cue groups favored the null hypothesis (i.e., true difference = 0°) or the alternative hypothesis (i.e., true difference = 30°). One way of quantifying it is to contrast the likelihoods of observing the observed difference under these two hypotheses. The null hypothesis is favored if the likelihood ratio (null/alternative) is larger than 3 and strongly favored if the likelihood ratio (null/alternative) is larger than 10 (Rouder et al., 2009). Likelihood as a function of observed difference under the null hypothesis is a probability density function of a t distribution—$t = \frac{\sqrt{N}*\mathit{observed\ difference}}{\sqrt{(2*MSE)}}$, degree of freedom $df = 2*(N - 1)$, N is the subject number in each group—whereas likelihood as a function of observed difference under the alternative hypothesis is a probability density function of a noncentral t distribution—$t = \frac{\sqrt{N}*\mathit{observed\ difference}}{\sqrt{(2*MSE)}}$, $df = 2*(N - 1)$, noncentral parameter $\mathit{lambda} = \frac{\sqrt{N}*\mathit{true\ difference}}{\sqrt{(2*MSE)}}$. Both functions were illustrated in Figure 3B. When the observed difference was −4°, the likelihood under the null hypothesis was .38, whereas the likelihood under the alternative hypothesis was .04. The likelihood ratio (null/alternative) was 9, favoring the null hypothesis.

The results of Experiment 1 showed that participants who only relied on path integration (i.e., no-visual-cue groups) did not perform better, in the sense of smaller absolute error or less variable signed error, when they did not cross boundaries than when they did cross boundaries. Accordingly, these results supported the mechanism-dependent hypothesis and rejected the mechanism-independent hypothesis.

One may argue that the mechanism-independent hypothesis might still be valid if this hypothesis requires a premise that a boundary-cross cost occurs only when participants visually perceive whether they cross a boundary. According to this elaborated hypothesis, in Experiment 1, the visual information in the testing room for the visual cue groups affirmed that participants in the large room group were within the same room and the participants in the small room were in a different room. In contrast, the participants in the no-visual-cue conditions could not visually perceive whether they crossed a boundary in the testing position. Hence, the cross-boundary effect was observed in the visual cue groups but not in the no-visual-cue groups. Experiment 2 tested this possibility.

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[1] We are grateful to one anonymous reviewer for suggesting this concern.

Experiment 2

In Experiment 2, the participants saw the virtual testing room in the testing position. However, the visual cues of the testing room only indicated that the participants were in the same large room or in a different small room; the cues did not indicate an unambiguous orientation of the room or unambiguous landmarks (i.e., walls) so that the cues were not valid piloting cues. As illustrated in Figures 4B and 4C, in the learning room (whether large or small), two opposite walls were painted with large red hexagons (illustrated by the dotted lines around the edges in the learning room in Figures 4B and 4C), and the other two walls were painted with small white hexagons. Hence, the walls of the room provided two indistinguishable orientations of the room and indistinguishable walls. During the test, the participants who had learned objects in the large room saw the same room. In contrast, the participants who had learned objects in the small room saw a different room in which two adjacent walls were painted with large red hexagons (illustrated by the dotted line around the edges of the testing room in Figure 4B) and the other two walls were painted with small white hexagons. Hence, the participants in the large room visually saw that they were in the same room, whereas the participants in the small room visually saw that they were in a different room. Because the visual cues of the walls in the large room presented two indistinguishable but opposite orientations, these visual cues were not useful for piloting and therefore led to a mean absolute pointing error at the chance level of 90°. Hence, the elaborated mechanism-independent hypothesis, which stipulates that visual perception of cross-boundary in the testing position is critical to produce the cross-boundary effect, would predict a cross-boundary cost in Experiment 2. In contrast, the mechanism-dependent hypothesis would still predict no cross-boundary cost in Experiment 2.

The piloting groups in Experiment 1 (i.e., participants who saw a room with unambiguous orientations during learning and also saw rooms during testing) were not included in Experiment 2 because: (a) Experiment 2 was designed to remove the potential confounding factor that is only related to the path integration groups in Experiment 1; (b) cross-boundary costs in the piloting groups were reported in previous studies (e.g., Radvansky & Copeland, 2006; Wang & Brockmole, 2003a) and in Experiment 1 of the current study so it is not novel; (c) cross-boundary costs in the piloting groups could not differentiate between the mechanism-independent hypothesis and the mechanism-dependent hypothesis, which is indeed the primary goal of the current project.

Method

Participants. Forty university students (20 men and 20 women) participated in this experiment as partial fulfillment of a requirement for an introductory psychology course.

Materials, design, and procedure. The materials, design, and procedure used in Experiment 2 were identical to those used in Experiment 1, except for the followings. (a) In the learning room, regardless of size, two opposite walls were painted with large red hexagons, and the other two walls were painted with small white hexagons, whereas in the small testing room, two adjacent walls were painted with large red hexagons, and the other two walls were painted with small white hexagons (see Figures 4B and 4C). (b) All participants saw the testing rooms. Twenty participants were randomly assigned to each of the room conditions, with an equal number of males and females assigned to each condition.

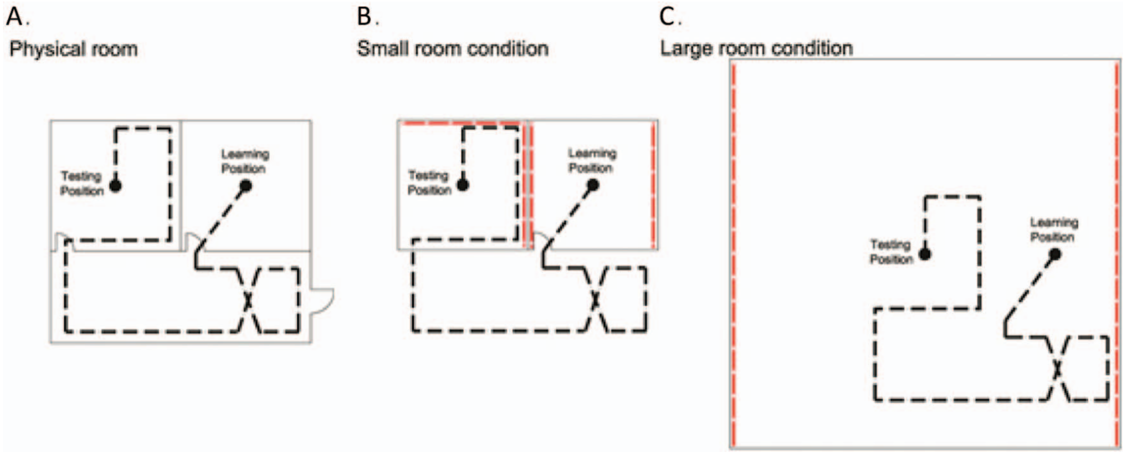

Figure 4. A. The walking path between the learning position and the testing position relative to the physical environment in Experiment 2. B. The virtual small learning room and the virtual small testing room as well as the walking path relative to the virtual rooms in Experiment 2. The virtual small rooms were the same size as the physical rooms and exactly overlapped the physical rooms. The dotted lines around the edges indicate the red colored walls in the virtual rooms. C. The large virtual room in both the learning and testing phases and the walking path relative to the virtual room in Experiment 2. The right wall of the virtual room overlapped the right wall of the physical learning room. The dotted lines around the edges indicate the red colored walls in the virtual room. Note that the walking paths that are illustrated in panels A, B, and C were identical relative to the physical environment. See the online article for the color version of this figure.

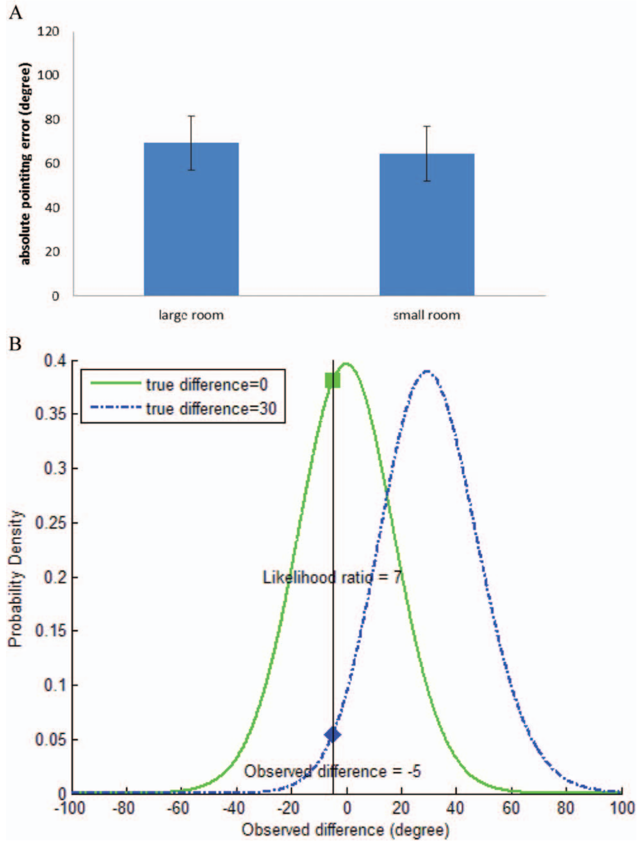

Figure 5. A. Mean absolute pointing error as a function of visual cue and room size in Experiment 2. The error bar is the standard error estimated in the analysis of variance. B. Likelihood (i.e., probability density) as a function of observed difference under the null hypothesis that claims no boundary effect (true difference = 0) and under the alternative hypothesis that claims a boundary effect (true difference = 30). See the online article for the color version of this figure.

Results and Discussion

The mean absolute angular pointing error as a function of room size is plotted in Figure 5A. The absolute angular error was computed for each participant and each condition and analyzed in an ANOVA with terms for room size, a between-subjects variable. The main effect of room size was not significant, $F(1, 38) = 0.08$, $MSE = 3080$, $p = .78$, $\eta_p^2 = .002$. Numerically, the angular error was even greater in the large room condition than in the small room condition (70° vs. 65°). The mean absolute pointing error (67°) averaged across the two room conditions was significantly less than 90° (chance level), $t(38) = 2.61$, $p = .013$.

The circular mean of the signed angular pointing error and the circular standard deviation across conditions are illustrated in Table 2. The Rayleigh Z test showed that the angular error in the small room group did not have a clustered direction ($Z = 1.53$, $p = .22$), and the angular error in the large room group did not have a clustered direction ($Z = 1.86$, $p = .16$). The variance of the angular error was comparable in the large room and in the small room (see Table 2), $F(19, 19) = 1.04$, $p = .47$ (Batschelet, 1981, p. 122).

Because the standard null hypothesis test cannot prove the null hypothesis claiming no boundary effect, we used Bayesian analyses to support the null hypothesis. As in Experiment 1, we examined whether the observed difference ($65° – 70° = -5°$) favored the null hypothesis (i.e., true difference = 0°) or the alternative hypothesis (i.e., true difference = 30°, which was estimated in Experiment 1). Likelihood (probability density) as a function of observed difference under the competing hypotheses is illustrated in Figure 5B. When the observed difference was −5°, the likelihood ratio (null/alternative) was 7, favoring the null hypothesis (Rouder et al., 2009).

We also contrasted the cross-boundary cost in the piloting groups (i.e., visual cue groups) in Experiment 1 with the cross-boundary effect in Experiment 2. The cross-boundary cost differed between these two experiments, qualified by a significant interaction between experiment (Experiment 1 vs. Experiment 2) and room (large vs. small), $F(1, 76) = 7.66$, $p = .007$, $\eta_p^2 = .092$.

In Experiment 2, like in the visual-cue groups in Experiment 1, the visual perception of the room in the testing position should ensure that the participants in the large room condition knew that they were in the same room and that the participants in the small room condition knew that they were in a different room. However, unlike in the visual-cue groups in Experiment 1, the visual perception of the room in the testing phase did not give the participants any unambiguous orientations or any unambiguous landmarks (i.e., walls) to support piloting. Hence, the results of Experiment 2 removed the possibility that the difference between the null cross-boundary cost when participants did not see the testing room and the cross-boundary cost when participants saw the testing room in Experiment 1 may have occurred because the participants in the visual cue groups could visually perceive whether they were in the same room, but the participants in the no-visual-cue groups could not.

The results of Experiments 1 (no-visual-cue conditions) and 2 indicate that navigation that relied on path integration was not impaired by boundary crossing. However, in both Experiments 1 and 2, a relatively complicated, circuitous path was used. Experiment 3 tested whether this conclusion could be extended to a simpler path.

Experiment 3

The purpose of Experiment 3 was to replicate the null cross-boundary effect for participants who navigated relying on path integration when participants walked a relatively simple path to test the generalizability of the null boundary-cross cost when participants only relied on path integration.

Method

Participants. Forty university students (20 men and 20 women) participated in this experiment as partial fulfillment of a requirement for an introductory psychology course.

Materials, design, and procedure. The materials, design, and procedure used in Experiment 3 were identical to those used in Experiment 2, except for the following. (a) A simple path with four major turns was used (Figure 6). (b) The rooms had four identical walls. Twenty participants were randomly assigned to each of the room conditions, with an equal number of males and females assigned to each condition.

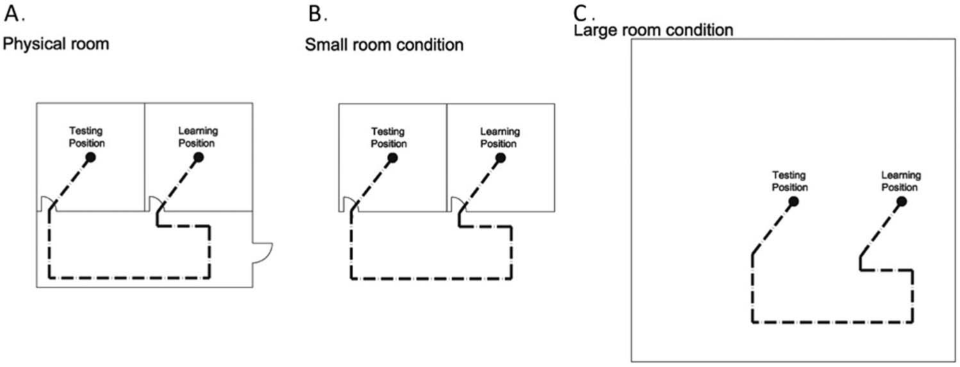

Figure 6. A. The walking path between the learning position and the testing position relative to the physical environment in Experiment 3. B. The virtual small learning room and the virtual small testing room as well as the walking path relative to the virtual rooms in Experiment 3. The virtual small rooms were the same size as the virtual rooms and exactly overlapped the physical rooms shown in panel A. Note that the participants did not see the small testing room; it is presented here for readers only. C. The large virtual room in both the learning and testing phases and the walking path relative to the virtual room in Experiment 3. The right wall of the virtual room overlapped the right wall of the physical learning room. Note that the walking paths that are illustrated in panels A, B, and C were identical relative to the physical environment.

Results and Discussion

The mean absolute angular pointing error as a function of room size is plotted in Figure 7A. The absolute angular error was computed for each participant and each condition and analyzed using an ANOVA with terms for room size, a between-subjects variable. The main effect of room size was not significant, $F(1, 38) = 1.32$, $MSE = 1132$, $p = .26$, $\eta_p^2 = .03$. Numerically, the absolute angular error was even greater in the large room condition than in the small room condition (48° vs. 36°). The mean absolute pointing error (42°) averaged across the two groups was significantly less than 90° (chance level), $t(38) = 9.07$, $p < .001$.

The circular mean of signed angular pointing error and the circular standard deviation across conditions are illustrated in Table 2. The Rayleigh Z test showed that the angular error in the small room condition had a clustered direction ($Z = 3.55$, $p = .02$), as did the angular error in the large room condition ($Z = 6.07$, $p = .002$). The confidence intervals in both groups included 0° indicating that the participants did not have a systematic bias in pointing. The angular error was as variable in large room as in the small room (see Table 2), $F(19, 19) = 1.28$, $p = .29$ (Batschelet, 1981, p. 122).

As in Experiments 1 and 2, we examined whether the observed difference ($36°– 48° = -12°$) favored the null hypothesis (i.e., true difference = 0°) or the alternative hypothesis (i.e., true difference = 30°). The likelihood (probability density) as a function of the observed difference under the competing hypotheses is illustrated in Figure 7B. When the observed difference was $-12°$, the likelihood ratio (null/alternative) was 127, strongly favoring the null hypothesis (Rouder et al., 2009).

We also contrasted the cross-boundary cost in the piloting groups (i.e., visual cue groups) in Experiment 1 with the cross-boundary effect in Experiment 3. The cross-boundary cost differed between these two experiments, qualified by a significant interaction between experiment (Experiment 1 vs. Experiment3) and room (large vs. small), $F(1, 76) = 15.18$, $p < .001$, $\eta_p^2 = .166$.

To qualify that the path in this experiment was indeed easier than that in Experiment 1, we compared the overall absolute pointing error in Experiment 3 with the error in the conditions of no visual cues in Experiment 1. The participants in Experiment 1 had a significantly larger pointing error (66° vs. 42°), $t(78) = 2.37$, $p = .02$, thus supporting the premise that path integration was more accurate in Experiment 3 than in Experiment 1.

In Experiment 3, the participants walked a simple path with four major turns. The results still indicated that pointing error was not affected by room size. Hence, the results of Experiment 3 indicated that navigation that only relied on path integration was not impaired by boundary crossing even when participants walked a path that was much less complicated than the path in Experiment 1.

General Discussion

The purpose of the current study was to investigate whether navigation across boundaries is less efficient than navigation within boundaries. The results of the experiments suggest that navigation that relies on path integration is not impaired by boundary crossing, whereas navigation that relies on piloting is impaired by boundary crossing.

In Experiment 1, the participants learned the locations of objects in a learning room with a unique orientation, walked a complicated path that was within a virtual room or across virtual rooms. The participants then pointed to the original locations of the objects. The results showed that when participants could not see the virtual testing room, those who had walked across the virtual rooms were not more accurate in pointing to the objects than those who had walked within the virtual room. When the participants visually saw the virtual testing room, those who had walked within the virtual room were more accurate in pointing to the objects than those who had walked across virtual rooms. In addition, the difference between the null cross-boundary cost when participants did not see the testing room and the cross-boundary cost when participants saw the testing room was not based on whether the participants

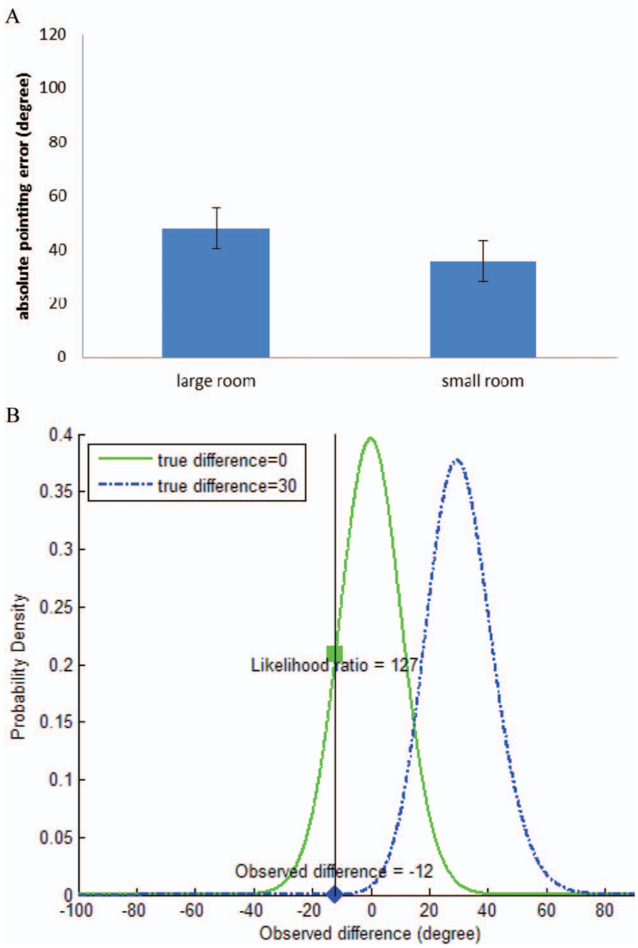

Figure 7. A. Mean absolute pointing error as a function of visual cue and room size in Experiment 3. The error bar is the standard error estimated in the analysis of variance. B. Likelihood (i.e., probability density) as a function of observed difference under the null hypothesis that claims no boundary effect (true difference = 0) and under the alternative hypothesis that claims a boundary effect (true difference = 30). See the online article for the color version of this figure.

could visually perceive whether they crossed a boundary in the testing position. In Experiment 2, the participants learned the locations of the objects in a virtual room with two-fold symmetry (Kelly et al., 2008), which provided two opposite orientations for piloting. The participants who had not crossed the virtual rooms saw the same room during the test, whereas the participants who had crossed virtual rooms saw a different virtual room during the test. Therefore, the participants saw that they had walked across rooms or within the same room. Nevertheless, the results showed that pointing accuracy was comparable for those who had walked across rooms and for those who had walked within the room. Hence the cross-boundary cost when participants saw the testing room in Experiment 1 might have occurred because participants in the large room condition could use the piloting mechanism by using the unambiguous colored walls, whereas participants in the small room condition did not have valid orientation cues or landmark cues that could be used in piloting (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a). In contrast, no cross-boundary cost when participants did not see the testing room in Experiment 1 might have occurred because participants had to rely on the path integration mechanism.

In Experiment 3, a simple path replaced the complicated path used in Experiment 1, and the participants did not see the testing room. The results again showed that pointing accuracy was comparable for those who had walked across rooms and for those who had walked within the room. Hence, all three experiments consistently showed that navigation that relied on path integration was not impaired by boundary crossing, thus supporting the mechanism-dependent hypothesis.

The finding that navigation that relied on path integration was not impaired by boundary crossing contrasts with findings in previous studies in which visual cues were available during navigation (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a). Wang and Brockmole (2003a) showed that participants could not point to the student union building until they walked outside the psychology building. Street and Wang (2010) reported that pointing to an office across floors was less accurate than pointing to an office on the same floor. These findings indicate that navigation is less efficient when people cross boundaries. We speculate that the results in previous studies might also be due to the participants' reliance on piloting for navigation. According to the mechanism-dependent hypothesis, piloting is less efficient when participants cross boundaries. It is well known that people can rely on visual landmarks or boundaries when locating targets (e.g., Doeller & Burgess, 2008; Foo et al., 2005). However, these visual cues may not be accessible after people cross a boundary (entering a different building or a different floor). Experiment 1 of the current study supports this speculation. In Experiment 1, when participants saw the testing room, those who had walked across rooms were not able to use the visual information in the new room to identify where the original room was, whereas those who had walked within the room could use the visual information in the same room to locate targets. The results showed that pointing was less accurate for participants who had walked across rooms than for participants who had walked within the room. Hence, the mechanism-dependent hypothesis can also explain the cross-boundary cost reported in previous studies (Radvansky & Copeland, 2006; Wang & Brockmole, 2003a).

The findings of the current study are consistent with the theoretical distinction between piloting and path integration in navigation. According to the theoretical models of navigation, piloting relies on the interobject spatial relations between landmarks that participants see at testing and the target that participants see at learning (e.g., Gallistel, 1990; Gallistel & Matzel, 2013). Contemporary models of spatial memory stipulate that spatial memory is fragmental and hierarchical (e.g., Brockmole & Wang, 2002; McNamara, 1986). Memory of spatial relations between two locations in different regions should be less accurate than that of two locations within the same region. Hence, the interobject spatial relations between landmarks that participants see at testing and the target that participants see at learning are less accurately encoded when participants cross a boundary than when participants stay within a boundary. As a result, piloting is less efficient across boundaries than within boundaries, which is confirmed by the results of Experiment 1.

By contrast, according to the theoretical models of navigation, path integration relies on participants' inertial cues (e.g., proprioceptive and vestibular cues) or optical flows to calculate the moving direction and speed. Unlike piloting, it does not rely on the spatial relations between visual landmarks (e.g., Gallistel, 1990; Gallistel, & Matzel, 2013). Accordingly, path integration should not be affected by the less useful visual landmarks due to boundary crossing. However, theoretical models of navigation cannot predict whether boundary crossing impairs path integration per se. It is possible that path integration itself is impaired by boundary crossing because people might disengage in tracking the locations of objects in the previous environment to engage in tracking the locations of objects in the current environment (Wang & Brockmole, 2003a). To our knowledge, the current study is the first to clearly demonstrate that boundary crossing does not impair path integration per se.

The mechanism-dependent hypothesis differs from the map switch and prioritization hypothesis proposed by Wang and Brockmole (2003a, 2003b). The map switch and prioritization hypothesis is not suitable to predict that the cross-boundary effect is modulated by different navigation mechanisms. However, the mechanism-dependent hypothesis is also not suitable to explain why the cross-boundary effect was modulated by instruction (Wang & Brockmole, 2003b). Future studies should investigate the interaction between instructions and navigation mechanisms when people navigate across boundaries. In addition, the current study used a relatively simple virtual environment. Whether the findings in the current study can be extended to more complicated physical environments warrants future research.

The current findings may also have important theoretical implications for how people develop spatial memory of large-scale environments. In a large-scale environment, people cannot see the locations of all objects from a single viewing position. Hence, spatial memory of large-scale environments may not be developed by visually perceiving interobject relations. However, people can locomote across the environment to view all of the objects' locations across the environments. Hence, people might represent interobject spatial relations in the large scale environment by path integration. Studies have found that people can develop survey knowledge of a large-scale environment by navigation (Ishikawa & Montello, 2006; Richardson, Montello, & Hegarty, 1999; Siegel & White, 1975; Thorndyke & Hayes-Roth, 1982). Ishikawa and Montello (2006) reported that some participants could point to invisible locations within one route quite accurately even after they had only navigated that route one time. It is reasonable to speculate that these participants developed survey knowledge using the path integration mechanism. Theorists in the field of spatial memory and navigation have proposed that path integration is important for developing a cognitive map (Foo et al., 2005; Gallistel, 1990, p. 106; Gallistel & Matzel, 2013; Jacobs & Schenk, 2003; McNaughton, Battaglia, Jensen, Moser, & Moser, 2006). For example, Jacobs and Schenk (2003) proposed that animals develop a sketch map of an individual position in the environment. In the sketch map, objects around the viewing position are represented in terms of a landmark. Animals also develop a bearing map of each viewing position based on path integration. Consequently, a cognitive map can be developed by integrating the bearing map and the sketch maps of each viewing position. Foo et al. (2005) reported that human participants may form a cognitive map through path integration when landmarks are not available, although the cognitive map formed in this way may not be accurate.

If we accept the idea that a cognitive map of a large-scale environment is formed primarily by path integration, we should appreciate the current finding that navigation that relies on path integration is not impaired by crossing boundaries. It is reasonable to claim that a large-scale environment usually involves multiple local spaces that are separated by boundaries. People may develop the ability to understand the spatial structure of the environment through path integration by coupling locomotion and the visual perception of the consequence of locomotion (e.g., Rieser, Hill, Taylor, & Bradfield, 1992; Tcheang et al., 2011). We speculate that this ability may initially be developed within an environment because the spatial structure of the path can be verified by visual perception within the environment. This ability can then be transferred to situations in which visual feedback is lacking (e.g., Rieser et al., 1992). In particular, this ability can be transferred to cross-boundary environments. Hence, the path integration mechanism can work across boundaries as well as within boundaries. This theoretical conjecture is consistent with the empirical findings of the current study.

The angular room did not facilitate spatial orientation in Experiment 2 of the current study, a result that is not consistent with the finding of Kelly et al. (2008). Although the participants saw the large room with two-fold rotational symmetry, the two-fold room did not facilitate spatial orientation, as evidenced by the finding that participants in the large room did not perform better than the participants in a different small testing room. However, Kelly et al. (2008) showed that participants could use a two-fold room to facilitate spatial orientation. It is important to note that in the current study, the participants were blindfolded and did not see the environment when they walked the path, whereas the participants in Kelly et al. (2008) viewed the room when they walked the path. Hence, the different findings between these two studies might suggest that the visual perception of an angular room (e.g., a two-fold rotational symmetric room) might remove the error of path integration when participants can see the room during path integration. Because the participants in the current study did not see the room while walking, they might have relied on either landmark or path integration at testing, depending on the reliability of each cue (Foo et al., 2005). In Experiment 2, because the room had two indistinguishable and opposite orientations that led to a mean absolute pointing error of 90° (i.e., chance level), the visual cue of the room was less reliable than the path integration system. Hence, the participants used the path integration system to point. In Experiment 1, however, because the large room had a unique orientation, the visual cue of the room was more reliable than the path integration system. Therefore, the participants used the piloting system to point.

Spiers, Hayman, Jovlekic, Marozzi, and Jeffery (2013) reported that place cells of rats could not disambiguate different compartments in an environment containing multiple visually identical compartment. This result suggests that rats' path integration system might not work across boundaries, which is not consistent with the finding in the current project. It is difficult to compare these two studies as Spiers et al. used rats and single-cell recording, whereas we used humans and behavioral measurement. One interesting explanation on the inconsistency might be verbal instruction in the current study. Participants in the small room conditions were explicitly instructed that they would walk to a different room to point to objects at the original locations. In contrast, the rats in Spiers et al. visually saw the identical compartment without the verbal instruction. Hence, the place cell might primarily be driven by the visual cues without cognitive control.

In summary, this project dissociated piloting and path integration mechanisms during navigation after the participants walked a path that was either across boundaries or within boundaries. The results indicated that navigation that relied on piloting was impaired by boundary crossing, but navigation that relied on path integration was not impaired by boundary crossing.

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Received September 17, 2013
Revision received April 29, 2014
Accepted April 30, 2014 ■