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Orientation Specificity and Spatial Updating of Memories for Layouts

David Waller, Daniel R. Montello, Anthony E. Richardson, and Mary Hegarty
University of California, Santa Barbara

This article examines the degree to which knowledge about the body's orientation affects transformations in spatial memory and whether memories are accessed with a preferred orientation. Participants learned large paths from a single viewpoint and were later asked to make judgments of relative directions from imagined positions on the path. Experiments 1 and 2 contribute to the emerging consensus that memories for large layouts are orientation specific, suggesting that prior findings to the contrary may not have fully accounted for latencies. Experiments 2 and 3 show that knowledge of one's orientation can create a preferred direction in spatial memory that is different from the learned orientation. Results further suggest that spatial updating may not be as automatic as previously thought.

Much of the current interest in spatial cognition involves characterizing the qualities of memorial representations of space and attempting to understand the ways by which external events and internal processes can transform them. One quality of spatial representations that has received a great deal of recent attention concerns the *orientation specificity* of spatial memory for large spaces and spatial layouts (Christou & Bülthoff, 1999; Diwadkar & McNamara, 1997; Féry & Magnac, 2000; Mou & McNamara, 2002; Presson, Delange, & Hazelrigg, 1987, 1989; Presson & Hazelrigg, 1984; Richardson, Montello, & Hegarty, 1999; Roskos-Ewoldsen, McNamara, Shelton, & Carr, 1998; Shelton & McNamara, 1997, 2001a; Sholl & Nolin, 1997; Simons & Wang, 1998). Spatial memory is said to be orientation specific when memorial representations are coded (and hence accessed) in a preferred direction. For example, some investigators have suggested that spatial memory of layouts consists primarily of stored egocentric views (Diwadkar & McNamara, 1997; Shelton & McNamara, 1997). If this is true, it would imply that the orientation in which spatial stimuli are viewed during learning is preferred in memory and serves to organize other (nonviewed) spatial relationships. Orientation-specific representations are contrasted with orientation-free representations that are coded in a way that allows access equally easily from any orientation (Evans & Pezdek, 1980; Presson et al., 1989). Orientation-free representations may result, for example, if spatial relationships are stored in a nonegocentric (e.g., allocentric) frame of reference.

One potentially vexing problem for investigators who attempt to characterize the orientation specificity of spatial memory is that memorial representations can change as a result of experience. Perhaps the most common and fundamental experience that transforms spatial representations of navigable environments is the act of moving through them. Another rich area of current research in spatial cognition has examined the phenomenon of spatial updating—people's ability to keep track of changing egocentric spatial relationships as they move through an environment (Amorim, Glasauer, Corpinot, & Berthoz, 1997; Farrell & Thomson, 1998; Klatzky, Loomis, Beall, Chance, & Golledge, 1998; Loomis, Klatzky, Philbeck, & Golledge, 1998; May & Klatzky, 2000; Presson & Montello, 1994; Rieser, Guth, & Hill, 1986; Sholl, 1989). Much of this work suggests that representations in spatial working memory are naturally and easily transformed as a result of moving through an environment, even when one moves through it without vision.

Bringing the orientation specificity literature together with the spatial updating literature raises an interesting question: If updating affects memorial representations of space, can it change the orientation specificity of these representations? For example, Sholl and Bartels (2002) have suggested that moving through an environment without vision may facilitate imagining multiple views of it (see also Sholl & Nolin, 1997). Having these multiple, *virtual* views may enable one's representation to become more flexible—perhaps orientation free (Presson et al., 1989; but see Shelton & McNamara, 1997, 2001a; Simons & Wang, 1998). In the present article, we address this question in two parts. First, we add to the growing consensus in the literature by providing evidence that spatial representations of room-sized layouts are represented in an orientation-specific manner. In so doing, we show that prior conclusions to the contrary may have resulted from insufficiently accounting for latencies. Having established that memories for room-sized layouts are orientation specific, the second aim of this article is to relate the orientation specificity of spatial memory to processes involved with spatial updating during locomotion. We show that the act of updating one's orientation is able to facilitate (or interfere with) the mental transformations performed on spatial memories. Although such facilitation can act to produce orientation-free performance, it does not appear to alter the orientation specificity of memory representations. We begin with a brief summary of prior research examining the orientation specificity of human spatial memory.

David Waller, Anthony E. Richardson, and Mary Hegarty, Department of Psychology, University of California, Santa Barbara; Daniel R. Montello, Department of Geography, University of California, Santa Barbara.

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Correspondence concerning this article should be addressed to David Waller, who is now at the Department of Psychology, Miami University, Oxford, Ohio 45056-1601. E-mail: wallerda@muohio.edu

Prior Research on the Orientation Specificity of Memory for Large Layouts

In a series of influential studies, Presson and his colleagues (Presson et al., 1987, 1989; Presson & Hazelrigg, 1984) showed evidence for orientation-free representations for memories of large spatial layouts. They did this by asking participants to study various four-point paths from a single location, and then to make judgments of relative directions from viewing perspectives that either had the same orientation as the viewpoint during learning or views that were 180° different from the orientation in which the path was learned.[1] For example, after studying the path illustrated in Figure 1 from the vantage point shown, people were asked two kinds of questions. In *aligned* questions, people were asked to point to one location as if they were facing the same direction as they were during learning (e.g., point to Location 3 as if standing at Location 1, facing toward Location 2). In *misaligned* questions, participants were asked to point to a location as if they were facing in the opposite direction as they were during learning (e.g., point to Location 2 as if standing at Location 3, facing Location 4). When people learned a layout from its representation on a small map, they were significantly more accurate on aligned questions than on misaligned questions. However, if the learned space was large enough (e.g., 3.6 × 3.6 m), people showed no alignment effect—they were able to make their judgments as accurately from misaligned orientations as they could from aligned ones.

Presson et al. (1989) regarded the fact that participants answered questions involving novel orientations with no more error than those involving previously viewed ones as evidence for an orientation-free memorial representation of large spaces. They explained the difference between the presence of alignment effects with small stimuli and the absence of alignment effects with large stimuli by suggesting that two distinct cognitive systems code spatial information depending on whether the remembered space affords navigation. In general, large spaces that afford navigation surround the viewer and thus make the viewer a part of the environment. Presson et al. suggested that for this reason, these spaces tend to be coded by means of a reference system that includes the viewer as an object in the environment—not as its central organizing feature. This way of coding large environments more easily captures interobject relationships such as distances (e.g., A and B are 10 m apart) and directions (e.g., A is due north of B) that are independent of the viewer's orientation. If spatial relationships are coded independently of a particular orientation, then no orientation is preferred in memory and no alignment effects will arise. On the other hand, when people acquire spatial information from a small nonnavigable object such as a map, the viewer himself is not a part of the learned environment. Presson et al. suggested that in this case, spatial information contained in a map tends to be coded in relationship to the viewer (e.g., A is directly in front of me). When spatial relationships are coded with respect to a particular orientation—such as the viewer's orientation during learning—then alignment effects can arise.

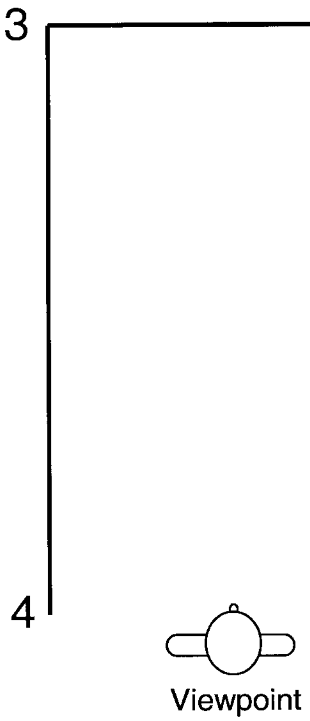

Figure 1. Schematic diagram of a typical path used in the present experiments. Participants learned a layout of four locations labeled 1–4 from the fixed viewpoint shown and were later asked to make judgments of relative directions based on various orientations on the path.

The finding that people do not exhibit an alignment effect after they have learned a large spatial layout has been difficult to replicate. For example, Roskos-Ewoldsen et al. (1998, Experiment 1) using very similar methods as Presson et al. (1989), found that both large and small stimuli produced alignment effects. Alignment effects also seem to appear when large paths are learned by walking them while blindfolded (Presson et al., 1987) or through verbal descriptions (Bachmann & Perrig, 1988; Bosco, Filomena, Sardone, Scalisi, & Longoni, 1996; Perrig & Kintsch, 1985; Wilson, Tlauka, & Wildbur, 1999). In addition, several experiments outside of the orientation-specificity literature are consistent with the idea that the orientations of some learned views of large spatial layouts are easier to recall than others (Easton & Sholl, 1995; Hintzman, O'Dell, & Arndt, 1981; May, 1996; Presson & Montello, 1994; Rieser, 1989). In two experiments, Sholl and Nolin (1997, Experiments 1 and 2) repeated Presson et al.'s (1989) procedures (with seemingly very minor alterations) and found that their participants exhibited an alignment effect after having studied large spatial arrays. Sholl and Nolin were eventually able to replicate Presson et al.'s results, but only after carefully controlling the viewing angle from which participants studied the paths and by testing people on the path (after trying to disorient participants by wheeling them in a wheelchair). When testing occurred in a remote site, the alignment effect returned. Results like this strongly suggest that the absence of an alignment effect in Presson et al.'s

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[1] Providing participants with only one view of a test space is one way to control the number of experienced views and to thus make inferences about what is stored in memory. It is important to note that, in general, several studies that are routinely—and erroneously—used as evidence for or against orientation specificity did not control the views of the test space that the participants had (e.g., Hintzman, O'Dell, & Arndt, 1981; May, 1996; Rieser, 1989).

original studies depended on very specific aspects of his experimental situation.

It has been suggested that Presson et al.'s (1989) finding of a lack of an alignment effect as a result of learning large layouts may have been due in part to participants keeping track of their body's orientation during testing (Roskos-Ewoldsen et al., 1998; Sholl & Nolin, 1997). The testing procedures used by Presson et al. involved circuitously wheeling or walking participants onto the learned path to the location and orientation they were asked to imagine. It has been shown that when people are able to remain oriented with the learned space during transport and are then tested at the to-be-imagined location, the task of imagining a nonviewed orientation is facilitated by knowledge of one's current orientation (Rieser et al., 1986). This direct knowledge of one's orientation with respect to his or her immediate surrounds is often called *sensorimotor awareness* of orientation. The degree to which this facilitation affects the alignment effect is not known, and parts of Experiments 1 and 2 were designed to address this. Discussion of the role of the body's orientation during testing will lead us to consider the processes involved in spatial updating. How these processes affect the orientation specificity of spatial representations is the subject of Experiments 2 and 3.

Experiment 1

Given the controversy that surrounds the question of whether memories for large-scale environments are orientation specific, Experiment 1 was designed simply as an attempt to replicate Presson et al.'s (1989) original finding of a lack of an alignment effect after learning large 4-point paths. We were heavily influenced by the work of Sholl and Nolin (1997) who deemed additional factors (in conjunction with large-scale stimuli) are necessary to find no alignment effect. These additional factors include maintaining a relatively low viewing angle during learning and testing on the path. Participants learned several paths from a single viewpoint and, in one condition (*wheel*), were tested after being disoriented by circuitously wheeling them to the testing location. This condition most closely replicates the procedures used by Presson et al., and we were especially interested whether an alignment effect arises in it. If people perform equally well on questions involving orientations that they have not viewed as those that they have (i.e., if there is not an alignment effect), we will conclude that people are able to access memories from unseen orientations as easily as from previously viewed ones. This will replicate Presson et al.'s findings and provide evidence for orientation-free coding of spatial information in memory.

On the other hand, if we do find an alignment effect in the wheel condition, we will conclude that there is a preferred orientation in memory and hence that spatial representations of large layouts are orientation specific. In this case, we will also be interested in the degree to which knowledge of one's body's orientation influences this alignment effect. Two other conditions in the experiment allowed us to examine this issue. In one condition (*stay*), people answered questions without moving from the learning location, maintaining the same orientation as they had during learning. We assume that in the stay condition, people generally maintain a high degree of sensorimotor awareness of their orientation, whereas in the wheel condition, people will be disoriented with respect to their immediate surroundings. Thus, contrasting performance in the wheel and stay conditions will enable us to examine whether sensorimotor awareness of orientation acts in a primarily facilitative or interfering capacity to influence judgments of relative directions. For example, Rieser (1989) has suggested that sensorimotor awareness of orientation facilitates judgments of directions. If this is the case, we would expect performance in aligned trials in the stay condition (in which a person's awareness of his or her orientation corresponds to the orientation he or she is asked to imagine) to be faster and more accurate than aligned trials in the wheel condition (in which people are disoriented and cannot be aided by their sensorimotor awareness of orientation). Thus, the difference between performance on aligned trials in the stay versus the wheel conditions will serve as a measure of facilitation. Alternatively, as suggested by Presson & Montello (1994), sensorimotor awareness of orientation may act primarily to interfere with judgments of directions when imagined and actual orientations are not the same. If this is true, then we would expect misaligned trials in the stay condition (in which one's sensorimotor awareness of orientation is contrary to what he or she must imagine) to be slower or less accurate than misaligned trials in the wheel condition. We will thus use performance differences between misaligned trials in the stay versus the wheel conditions as a measure of interference.

Finally, to provide motivation for our treatment of updating in these experiments, we included a third condition called *direct walk.* This condition differed from the stay and wheel conditions only during the interval between learning and testing. After learning each spatial arrangement, participants in the direct walk condition walked (while being blindfolded and guided) directly to the location and orientation that they would be asked to imagine. This allowed them to update their mental representation to match the orientation that they would be asked to imagine. Based on our experience and on previous literature (Loomis et al., 1998; Rieser et al., 1986), we expected both aligned and misaligned questions in this condition to be answered relatively accurately. The direct walk condition thus provided a baseline for the effectiveness of sensorimotor updating in answering questions that involve the imagination of unseen views. Such a condition should allow us to examine the degree to which sensorimotor updating is able to affect spatial representations and to eliminate the alignment effect.

Method

Participants. Twenty-four students (12 men and 12 women) participated in the experiment in order to satisfy a requirement in their introductory psychology course.

Materials. Nine of the 4-point paths used by Presson et al. (1989) were constructed from hinged slats of wood. These wooden slats were 3.0 cm wide, 0.8 cm high, and varied from 0.71 to 3.96 m in length. The path segments illustrated by Presson et al. were proportionately scaled to fit in the 6 m × 6 m testing space. The largest segment (from Presson et al.'s path number four) was 3.96 m long. The numerals 1 through 4 were printed on a 10.80-cm diameter cardboard circle and were placed at each corner of each path. Participants viewed each path from a wheelchair stationed at the center of the base of the path. From this position, each path could be seen in its entirety without turning the head.

During the experiment, participants wore a Virtual Research V8 head-mounted display (HMD) on which was mounted an Intersense IS-300 inertial tracker. Participants wore the HMD over their eyes throughout the experiment, except when viewing the stimulus arrays. The HMD served as a blindfold during the retention period of each trial, and was used to present the questions during testing. The HMD provided monoscopic images at a resolution of 640 × 480 and a 48° horizontal field of view. The tracker was used to record participants' head direction during testing and had a latency of less than 5 ms, an accuracy of 3° RMS, and a resolution of 0.02°. Participants carried a button in their dominant hand which, when pressed, either advanced the display to the next stimulus screen or recorded the participants' current facing direction. Presentation of the stimuli and the collection of reaction times and pointing estimations were controlled through a scripting facility in the Python programming language (Version 2.0, van Rossum, 2000) that had been supplemented with a utility module written by Andrew Beall specifically for virtual environment applications.

Procedure. Each participant learned nine paths in the same random order. The first three paths were practice trials—once for each type of testing condition (stay, wheel, and direct walk). Practice trials were given in the order that they were to appear in the rest of the experiment. To ensure they understood their task, participants were given error-corrective feedback only during the practice trials and only if they turned to point in the direction opposite to the imagined target. Throughout all of the experiments reported in this article, such feedback was given on approximately 8% of the practice trials.

All trials began with a 30-s learning phase, followed by a 30-s retention interval, followed by a testing phase. The learning phase began when participants lifted their blindfold (i.e., the HMD). They studied the path for 30 s, and then replaced the blindfold over their eyes. The retention phase followed. The stay, wheel, and direct walk conditions differed only in what occurred in the retention phase. During this time, participants were either wheeled circuitously to a location on the path (wheel condition), walked directly to the path (direct walk condition), or asked to remain in place (stay condition) for the next 30 s. Circuitous wheeling involved pushing the participant at a normal walking pace along a randomly curved trajectory. When testing occurred on the path (during the wheel and direct walk conditions), participants were taken to the location and orientation that they would be asked to imagine. Thus, in the wheel and direct walk conditions, participants' physical orientation and location during testing was, for both aligned and misaligned trials, always the same as the orientation and location they were asked to imagine during testing. In the stay condition, participants' physical orientation and location during testing was identical to that during learning (making physical and imagined orientation consistent for aligned trials and inconsistent for misaligned trials).

Participants were required to stand for the testing phase of each trial. For each path, testing consisted of one aligned and one misaligned question. These two questions were separated by 30 s of either wheeling, direct walking, or waiting (staying), depending on the condition. The order of the aligned and misaligned trials, as well as the target locations (in front or behind), were counterbalanced for each participant within each of the three testing conditions.

Test questions were administered through the HMD. For each question, the computer displayed two stimuli (see Figure 2). The first stimulus presented text indicating the sighting location and an orientation (e.g., At 3, facing 4). When the participant imagined that he or she was oriented at this location, he or she pressed a button that triggered the presentation of the second stimulus. The time between the onset of the first stimulus and this button press was recorded and called *orientation time.* The second stimulus presented the target location (e.g., point to 1) and was shown immediately after the participant's first button press. The participant was instructed to turn his or her head in the direction of the target as if he or she was at the sighting location. Participants were advised that some targets would be behind them and that pointing to them may require turning their body. When participants had turned and were confident that they were facing in the correct direction, they pressed the button. The time between the onset of the second stimulus and the participant's having turned more than 10° was recorded and called *reaction time.* The time between participants' first 10° of rotation turn and their final button press was recorded and called *movement time.* Participants were instructed to try to determine the proper direction before they began moving. They were advised to respond quickly, but not at the expense of accuracy. Participants were also advised that successful task performance required their remembering the direction (clockwise or counterclockwise) of the consecutive numbers on the path. They were told that if they forgot this direction, that they should report this to the experimenter after the trial was over.

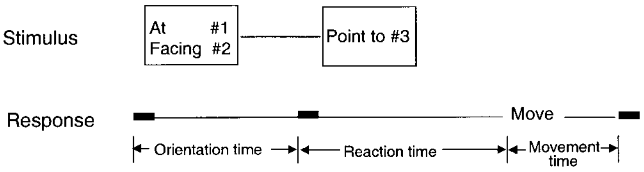

Figure 2. Partitioning of latencies into three types. Time proceeds from left to right, and the small black rectangles on the response line represent participants' button presses. Orientation time was measured as the time from the onset of the first stimulus to the first button press. Reaction time was measured as the time from the onset of the second stimulus to the time when participants turned their heads more than 10° to point to the target. Movement time is the time from participants' first 10° of rotation to their third button press, indicating the direction to the target.

Nonpractice trials were presented to participants blocked by testing condition, into three groups of two paths. While the order of path configurations learned was the same for all participants, the order of testing conditions was counterbalanced (for both men and women), ensuring that across participants, each path appeared equally often in each testing condition. Other variables were counterbalanced within each condition, including whether the correct answer involved a clockwise or a counterclockwise rotation, whether the target was in front or behind the imagined location, and whether the path contained all right angles. In addition, correct values for turning responses in the aligned trials $M = 5.94°$ clockwise, $SD = 99.58$ did not differ substantially from the correct values for misaligned trials $M = 22.49°$ counterclockwise, $SD = 9.99$. Two paths were learned in each condition, and participants' errors and reaction times were averaged over these two replications. The experiment represents a 2 (Alignment: aligned or misaligned) × 3 (Test: stay, wheel, or direct walk) within-subjects design.

Results

Participants occasionally reported forgetting the clockwise or counterclockwise ordering of the numbers on the path during the retention interval. Data from three paths among three participants reporting this were excluded from analyses, as were the data from the three practice trials. Orientation and reaction times from three responses (less than 1%) were eliminated because of malfunctioning equipment. For each participant, absolute pointing error (the unsigned difference between the estimated and correct turning direction), orientation time, reaction time, and movement time were averaged over the two replications of each combination of testing condition and alignment. In general, movement time did not relate significantly to any of the factors of interest and was not included in subsequent analysis. The effect of gender was examined in all of the statistical analyses conducted in this article. When the effect of gender was significant, or when it interacted with other factors, it was always because men performed slightly more quickly or slightly more accurately than women. However, gender effects were relatively small and were not consistent or systematic across the experiments. Because these effects are not the focus of our investigations (and because all experiments were gender balanced), all analyses reported in this article collapse over gender.

Across all measures, there was an effect of alignment in the stay and wheel conditions. In the stay condition, participants were 35.95° less accurate in their responses to questions, took 1.12 s more to orient, and took 4.41 s more to respond to questions that involved imagining an orientation that was misaligned from the one they had learned than they did to respond to questions about an aligned orientation. An attenuated effect of alignment appeared in the wheel condition. After being circuitously wheeled to each testing site, participants were 10.16° less accurate, took 1.92 s more to orient, and took 2.39 s more to react to misaligned questions than to aligned ones. In the direct walk condition, performance was relatively fast and accurate for both aligned and misaligned questions. Table 1 presents means and standard deviations of these variables in each of these conditions.

Statistical analysis confirmed these observations. For all of the inferential tests reported in this article, individual differences in overall response latencies were accounted for by normalizing each participant's times. Orientation times and reaction times for each participant were thus converted to z scores based on each participant's distribution of times. We refer to these as *normalized* times. Differences between conditions were tested in a 3 (test type) × 2 (alignment) multivariate analysis of variance (MANOVA) that used normalized orientation time, normalized reaction time, and absolute pointing error as dependent variables. The MANOVA revealed a significant effect of alignment $F(3, 21) = 31.55$, $p < .01$, indicating that people were faster and more accurate on aligned trials than on misaligned ones. There was also a significant effect of test type, $F(6, 18) = 6.59$, $p < .01$, confirming that overall performance in the direct walk condition was generally superior to that in other conditions. These effects were qualified by a significant interaction between alignment and test type $F(6, 18) = 5.93$, $p < .01$. Much of this interaction was due to the lack of an alignment effect in the direct walk condition. Yet interaction contrasts comparing only the alignment effect in the stay condition with that in the wheel condition were generally significant: reaction time, $t(23) = 2.84$, $p < .01$; error, $t(23) = 2.52$, $p = .02$; orientation time, $t(23) = 0.31$, $p = .756$ indicating that the alignment effect in wheel was significantly attenuated from that of stay. Tests of simple main effects of alignment showed a significant effect of alignment in the stay condition $F(3, 21) = 37.33$, $p < .01$ and in the wheel condition $F(3, 21) = 5.95$, $p < .01$, but not in the direct walk condition $F(3, 21) = 1.36$, $p = .28$.

To represent these analyses graphically, we created a composite variable called *difficulty,* which represented a combination of participants' error and latency data. To form this variable, mean reaction times and orientation times were first converted to z scores for each participant, relative to each participant's distribution of times. These scores, as well as absolute errors were then converted to z scores based on their distribution across all participants. These three scores were then averaged to form *difficulty,* a composite measure of speed and accuracy in performing the experimental task. Values for this variable ranged from −1.41 (*good performance*) to 1.77 (*poor performance*). These scores are illustrated in Figure 3 for each alignment and testing type.

As we mentioned in the introduction, an attenuation of the alignment effect in the wheel condition may result from either a facilitative effect of sensorimotor awareness (i.e., aligned trials are easier in the stay condition than in the wheel condition) or an interference effect of sensorimotor awareness (i.e., misaligned trials are more difficult in the stay condition than in the wheel condition). To test these hypotheses numerically, two new variables were created. *Facilitation* was computed as the difference between *difficulty* in the wheel aligned and stay aligned condition, and *interference* was computed as the difference between *difficulty* in stay misaligned condition and wheel misaligned condition. Across all participants, *facilitation* ($M = 0.32$, $SD = 0.78$) was nearly identical to *interference* ($M = 0.33$, $SD = 0.76$). The magnitudes of both effects were marginally significantly different

Table 1
Means and Standard Deviations for Orientation Times (s), Reaction Times (s), and Absolute Errors (degrees) on Aligned and Misaligned Trials for the Experimental Groups in Experiments 1, 2, and 3

	Orientation time		Reaction time		Absolute error	
Experiment	A	M	A	M	A	M
1 (*N* = 24)						
Condition						
Stay	3.88 (1.61)	5.00 (2.04)	2.55 (1.76)	6.96 (5.26)	18.12 (15.85)	54.07 (36.22)
Wheel	4.59 (2.10)	6.51 (3.71)	3.16 (2.17)	5.55 (5.41)	25.98 (20.06)	36.14 (28.35)
Direct walk	2.60 (1.65)	3.64 (2.37)	2.62 (2.41)	2.50 (1.38)	22.11 (11.62)	23.70 (16.26)
2 (*N* = 28)						
Condition						
Stay	4.50 (1.98)	6.05 (3.94)	2.34 (1.24)	4.30 (2.90)	16.34 (10.80)	55.43 (46.46)
Wheel	5.82 (2.76)	6.24 (4.11)	2.39 (1.33)	3.81 (2.44)	30.94 (25.63)	45.22 (33.17)
Deceptive wheel	5.73 (4.43)	5.95 (3.34)	3.01 (2.15)	3.26 (1.97)	32.36 (31.73)	47.70 (35.91)
Rotate	5.84 (3.34)	6.46 (4.85)	3.83 (2.27)	3.19 (1.79)	49.63 (40.01)	46.05 (41.83)
3 (*N* = 24)						
Condition						
Stay	3.94 (1.37)	5.96 (2.67)	2.13 (1.31)	5.23 (3.84)	18.33 (15.48)	24.82 (17.52)
Rotate-ignore	4.63 (1.96)	6.94 (3.39)	2.63 (1.84)	5.38 (4.12)	13.09 (8.730)	25.76 (17.30)
Rotate-update	6.43 (3.13)	5.26 (1.27)	5.98 (3.51)	3.25 (1.47)	34.30 (30.32)	16.65 (07.99)

Note. Standard deviations are in parentheses. A = aligned; M = misaligned.

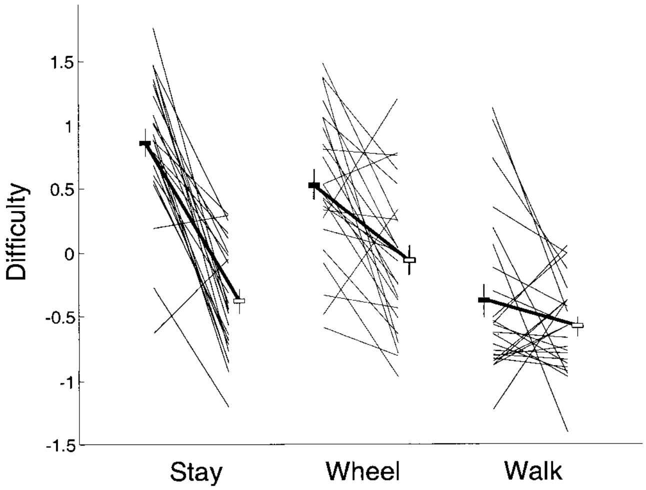

Figure 3. *Difficulty* for aligned (open bars) and misaligned (closed bars) trials in the three testing conditions of Experiment 1. Each participant's aligned and misaligned trials are connected by a line. Means and standard errors are displayed at the side of participants' individual data. *Difficulty* was computed as the mean *z* score of participants' (individually normalized) orientation time, reaction time, and absolute pointing error.

from zero: *facilitation,* $t(23) = 1.99$, $p = .06$; *interference,* $t(23) = 2.08$, $p = .05$, and they were not significantly different from each other.

Discussion

Experiment 1 has shown that when judgments of relative directions are based solely on memory and not on one's current body orientation (i.e., in the wheel condition), alignment effects occur after learning room-sized spatial layouts. In general, this effect on errors is rather modest—misaligned trials result in about 10° more error in pointing than do aligned trials, which correspond to an effect size (Cohen's *d*) of 0.42. The alignment effect on total time to respond is slightly larger—approximately 4 s—and corresponds to a mean effect size of 0.79. Because the effect in errors is not particularly large, it seems likely that previous experiments that reported no effects under these conditions did not have sufficient power to detect it. It is also worth noting that if we had analyzed only errors, we would not have concluded that there was a significant alignment effect in the wheel condition. By combining error and latency data into one multivariate analysis, we have been able to detect this effect more reliably. The finding of significant alignment effects in the wheel condition demonstrates that spatial memories for these layouts were accessed with a preferred orientation and thus strongly suggests that these memories were orientation dependent. These conclusions are contrary to those of several previous influential studies (Presson et al., 1989; Presson & Hazelrigg, 1984; Sholl & Nolin, 1997), and much more in accord with the growing body of literature showing that alignment effects are robust across a variety of learning conditions (Mou & McNamara, 2002; Roskos-Ewoldsen et al., 1998; Shelton & McNamara, 2001a).

Although significant, the alignment effect in the wheel condition was significantly smaller than that in the stay condition. This attenuation appeared to be due equally to both an elimination of the facilitative effect of sensorimotor awareness in aligned trials and its interfering effect in misaligned trials. These results complement the work of May (1996) who concluded similarly that sensorimotor awareness of orientation has both a facilitative and interfering effect on cognitive judgments of relative directions.

For most participants, knowledge of one's body orientation clearly facilitated directional judgments in the direct walk condition. In general, participants' exceptionally good performance in the misaligned trials of the direct walk condition suggests that in this condition, the orientation of participants' mental representation of the layout that was easiest to retrieve at the time of testing was not the one they learned; rather, it was the one that their body was in during testing. These conclusions support the hypothesis that the preferred orientation of memorial representations of spatial layouts (as measured by ease of retrieval) is not fixed to the learned orientation, but can be modified by proprioceptive experience. It is possible that the transformations that occur as a result of this proprioceptive experience actually alter the preferred orientation of the representation, rendering the learned view obsolete and ineffectual. Experiments 2 and 3 examine in more detail the psychological status of the learned view after updating.

Finally, it is interesting to note that there appears to be large individual differences in the degree to which updating processes affect spatial representations in memory. For example, Figure 3 shows that several participants maintained a very large alignment effect in the direct walk condition. This suggests either that these participants did not update their representation as a result of walking, or that the view they learned had more impact on their judgments than did the information available from updating. We will return to this issue in Experiment 3, where we suggest that some of these individual differences derive from people's interpretation of the task at hand.

Experiment 2

The results of Experiment 1 were similar to those of Sholl and Nolin (1997, Experiments 1 and 2) and Roskos-Ewoldsen et al. (1998). In failing to replicate Presson et al.'s (1989) finding of no alignment effect in a wheel condition, both of these groups suggested that Presson et al.'s results were due to his participants' ability to update their position and orientation at a sensorimotor level during transport to the test site. Although, unlike Presson et al., we found a significant alignment effect in our wheel condition, our results may garner a similar reaction: If updating in the wheel condition is a viable strategy for solving our task, then we may have underestimated the magnitude of the alignment effect. Although previous research (Sholl, 1989) and another experiment in our lab suggested that 30 s of circuitous wheeling immediately after learning was sufficient for disorienting people, we cannot be certain that people's ability to track their orientation did not play a role in unduly reducing our estimation of the magnitude of the alignment effect.[2]

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[2] Another experiment in our lab measured people's ability to update after being blindfolded and wheeled circuitously for 15 to 40 s. Although people were able to maintain their orientation at better than chance level, performance was exceptionally error prone (mean absolute pointing error = 65°) and did not correlate with the magnitude of their alignment effect in learning spatial layouts, $r(36) = .07$.

In Experiment 2, we examined this issue by adding a condition, after learning, called *deceptive wheel* in which participants were circuitously wheeled to the opposite orientation from the one they were asked to imagine. On these trials, participants' physical orientation during testing was always contrary to the orientation they were asked to imagine. If participants were able to keep track of their physical orientation during wheeling, then we assume that they would be aware that their physical orientation during testing conflicted with the to-be-imagined orientation. Presumably, this conflict would be present on all trials, would take time to resolve, and would lead to less accurate task performance. Thus, if participants are able to update in the deceptive wheel condition, we would expect increased latencies and errors in both aligned and misaligned deceptive wheel trials relative to those in the wheel condition of Experiment 1. Moreover, if people are able to update their position and orientation accurately during transport, performance in the deceptive wheel condition should be similar to that for misaligned trials in the stay condition (in which people are also aware of the difference between their physical and imagined orientations). More generally, the deceptive wheel condition will allow us to examine whether sensorimotor updating was responsible for a possible underestimation of the alignment effect in Experiment 1.

Another condition in Experiment 2 was designed to determine the psychological status of the learned view after sensorimotor updating to another orientation has occurred. The new condition, called *rotate*, was identical to the stay conditions in Experiment 1 with the exception that people were required to turn 180° away from the path before answering both questions. The rotate condition thus placed participants' bodily orientation in direct opposition to the orientation they had learned. If the preferred orientation of participants' spatial representation is influenced primarily by the learned orientation, we would expect an alignment effect in the same direction as that in the stay condition. Indeed, the degree to which the (signed) magnitude of the alignment effect in rotate approaches that of the stay condition offers a measure of the strength of the learned view to govern the preferred orientation. On the other hand, if by rotating in place the preferred orientation of people's representation is altered to their bodies' orientation, we would expect a reversal of the alignment effect seen in the stay condition. In this case, misaligned trials—those requiring people to imagine an orientation different from what they learned—would be consistent with the participants' body orientation and would be facilitated. Similarly, aligned trials in the rotate condition would be now contrary to participants' body orientation and would be more difficult. The degree to which the rotate condition yields a *reverse-alignment effect* will inform us about the degree to which the preferred orientation after updating is the current body's orientation—not the learned one.

Our analysis of Experiment 2 will also address one additional issue. Recently, it has been claimed that the alignment effect literature has, in general, insufficiently recognized the importance of individual differences by averaging together performance patterns that are vastly different (Rossano, Warren, & Kenan, 1995). Figure 3, for example, shows rather large differences between participants in the wheel condition such that a few participants actually show improved performance on misaligned trials relative to aligned ones. Rossano et al. have claimed that a significant subset of people do not exhibit an alignment effect after map learning. However, their conclusion was reached solely from an analysis of errors. Perhaps the group of participants that Rossono et al. deemed to be free from alignment effects were merely more ready to trade speed for accuracy. We were interested in determining whether we could identify a similar set of people who showed no alignment effect after learning a room-sized spatial layout. If such a group can be identified, we would then be curious whether they show an alignment effect in latencies.

Method

Participants. Twenty-eight students (14 men and 14 women) participated in the experiment in order to satisfy a requirement in their introductory psychology course.

Materials. Materials for Experiment 2 were the same as those reported for Experiment 1. However, because of the added condition, two additional paths from Presson et al. (1989) were adapted for use in the experiment.

Procedure. Experiment 2 generally followed the same procedures as Experiment 1, with the exception of having four testing conditions (stay, wheel, deceptive wheel, and rotate) instead of three. The experimenter's description of the experiment to the participants did not suggest that there would be a difference between wheel and deceptive wheel trials. Thus, from the participant's point of view, there were only three kinds of testing situations. Three practice trials (one wheel, one stay, and one rotate) were given in the order that they would appear in the experiment.

Practice trials were followed by eight experimental trials, in four blocks of two. Each block presented two paths which were tested in one of the four conditions. Four possible orderings of these conditions, based on a Latin square, were presented to participants, counterbalanced for each gender. Wheel and stay conditions were identical to those in Experiment 1. The deceptive wheel condition was the same as the wheel condition with the exception that participants were also wheeled to another orientation and position than they would be asked to imagine. Participants were always wheeled circuitously to the center of the room and placed facing the opposite direction than they were asked to imagine. The rotate condition was identical to the stay condition with the exception that immediately after learning the path, participants were required to turn 180° and answer both questions with their backs to the path.

Results

Data from one path of one participant was excluded from analyses because on this path the participant reported forgetting the ordering of the locations. Orientation and reaction times from three responses (less than 0.7%) were eliminated because of malfunctioning equipment. The first three practice trials were also not analyzed. For each participant, absolute pointing error, normalized orientation time, and normalized reaction time were averaged over the two replications of each combination of testing condition and alignment.

The effects of alignment and testing type were similar to those that we found in Experiment 1, with the exception of a smaller overall effect of testing type. Across all measures, there was an effect of alignment in the stay, wheel, and deceptive wheel conditions. This effect was greatest in the stay condition, was greatly attenuated in the wheel, and further attenuated slightly in the deceptive wheel condition. In the rotate condition, reaction times and absolute errors showed a reverse effect of alignment (misaligned trials were more accurate and faster), but this effect was generally very small. In general, both aligned and misaligned trials in the rotate condition were relatively slow and inaccurate. Table 1 presents means and standard deviations of these variables in each of these conditions.

These effects were tested statistically in a 4 (test type) × 2 (alignment) MANOVA that used normalized orientation time, normalized reaction time, and absolute pointing error as dependent variables. This analysis revealed a significant overall effect of alignment $F(3, 25) = 10.52$, $p < .01$, indicating that participants were slower and less accurate on misaligned trials than on aligned ones. There was not a significant effect of test type $F(9, 19) = 1.88$, $p = .12$, however test type did interact significantly with alignment $F(9, 19) = 4.26$, $p < .01$. Much of this interaction was due to the relative lack of an alignment effect in the rotate condition coupled with a large alignment effect in the stay condition. Planned contrasts that compared the alignment effect in the wheel with that in the deceptive wheel condition were generally not significant: error, $t(27) = 0.10$, $p = .92$; orientation time, $t(27) = 0.58$, $p = .57$; reaction time $t(27) = 2.82$, $p < .01$. The significant interaction effect on reaction times indicated a larger alignment effect in wheel than in deceptive wheel.

Tests of simple main effects of alignment revealed a significant effect of alignment in the stay condition $F(3, 25) = 25.83$, $p < .01$, and wheel condition $F(3, 25) = 8.17$, $p < .01$. In the deceptive wheel condition, the effect of alignment did not attain statistical significance $F(3, 25) = 1.39$, $p = .27$. However, a univariate test specifically examining the effect of alignment on errors in the deceptive wheel condition approached significance $t(27) = 2.01$, $p = .05$. The simple main effect of alignment in the rotate condition was not significant $F(3, 25) = 1.40$, $p = .27$, nor were any univariate tests of alignment on the three dependent variables.

As in the previous experiment, we computed the composite variable, *difficulty,* which is presented in Figure 4. From this variable, we computed the facilitative effect of body orientation as the difference between aligned trials in the wheel condition and in the stay condition. Similarly, the interfering effect of body orientation was calculated as the difference between misaligned trials in the stay condition and in the wheel condition. In general, the facilitative effect of sensorimotor awareness of orientation: wheel ($M = 0.38$, $SD = 0.91$); deceptive wheel ($M = 0.39$, $SD = 0.96$) was greater than the interfering effect wheel ($M = 0.16$, $SD = 0.78$); deceptive wheel ($M = 0.33$, $SD = 0.87$), but these differences did not attain statistical significance: wheel $t(27) = 1.05$, $p = .30$; deceptive wheel $t(27) = 0.24$, $p = .81$.

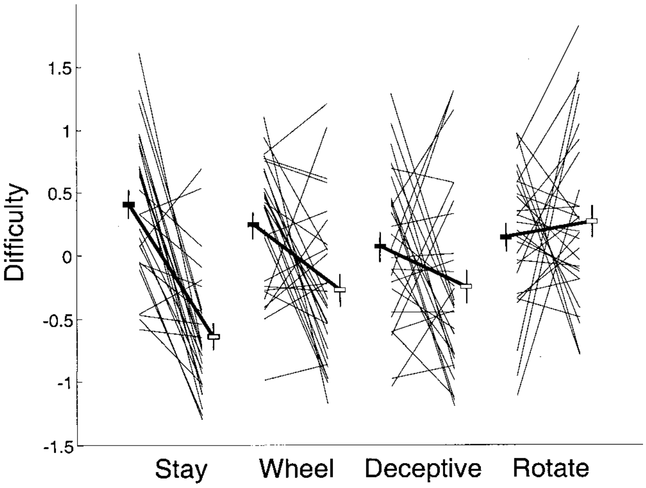

Figure 4. *Difficulty* (computed as the mean z score of participants' [individually normalized] orientation time, reaction time, and absolute pointing error). For aligned (open bars) and misaligned (closed bars) trials in the four testing conditions of Experiment 2. Means and standard errors are shown next to the individual data.

To examine whether groups of people who show little alignment effect in errors also show little alignment effect in latencies, data from the wheel conditions of Experiments 1 and 2 were combined. All participants were classified as either showing an alignment effect in errors or not, based on the criteria established by Rossano et al. (1995). Based on these criteria, 26 out of 52 participants were classified as showing no alignment effect in errors on wheel trials. Errors and latencies (summed over orientation and reaction times) for these participants in comparison with the participants who did show an alignment effect are illustrated in Figure 5. The figure shows that, in general, the no effect group exhibited a pronounced alignment effect in latencies. This effect was similar in magnitude to the alignment effect in latencies for the other participants. These observations were tested in a 2 (Group: no effect, effect) × 2 (Alignment: aligned or misaligned) MANOVA that used orientation time and reaction times as dependent variables. The only statistically significant factor from this analysis was alignment $F(2, 49) = 8.85$, $p < .01$. Tests of simple main effects showed that the no effect group showed a significant alignment effect in latencies $F(2, 24) = 4.44$, $p = .02$. Four participants in the no effect group were also classified according to Rossano et al.'s (1995) criteria as showing no alignment effect for response times (orientation time plus reaction time) in the wheel condition. No other participants were classified as showing no effect in latencies.

Because the magnitude of response times is similar between the groups of participants who either did or did not show an alignment effect in errors, it appears unlikely that participants who showed no alignment effect in errors did so because they traded speed for accuracy. This was confirmed by computing, for each participant, the correlation between absolute pointing error and total response time (orientation time plus reaction time) across all nonpractice trials in the experiment. A negative value for this correlation indicates the presence of a speed–accuracy tradeoff. Across all participants, this correlation ranged from $-.52$ to $.90$ ($M = .19$, $SD = .31$). It was significantly positive for 6 of the participants (about 12%), but was not significantly negative for any of them. Among the 19 participants (about 37%) for whom the correlation between speed and accuracy was negative, 10 were classified as not having shown an alignment effect in errors and 9 were classified as showing an alignment effect in errors.

Discussion

Experiment 2 replicated our previous finding that a significant alignment effect occurs for large spatial layouts in the wheel condition. This indicates that memories of these layouts had a preferred orientation—the orientation in which they were learned. Moreover, participants' abilities to update their orientations during passive transport did not lead us to underestimate this alignment effect. The fact that the deceptive wheel condition resulted in similar, or even slightly superior performance, as in the wheel condition, should put to rest the question of whether sensorimotor

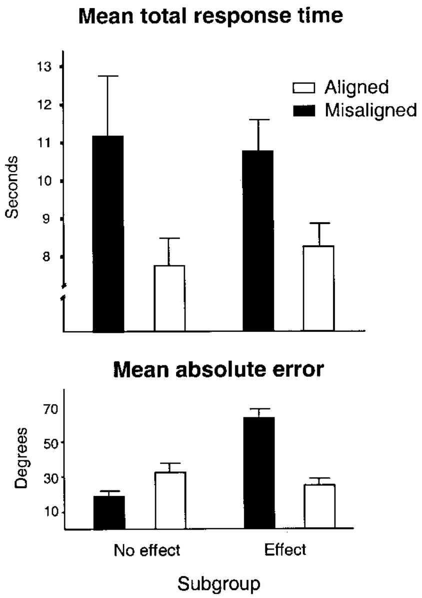

Figure 5. Mean overall latency (sum of orientation time and reaction time) and mean absolute pointing error combined from Experiments 1 and 2 for participants who did or did not show an alignment effect in errors. Error bars represent one standard error.

updating accounts for the magnitude of the alignment effect in these experiments. Participants were clearly not able to update accurately in the deceptive wheel condition. If they had, they would have made errors more comparable to those in the misaligned trials of the stay condition.

Our use of multiple dependent measures was important in showing that nearly every participant exhibited an alignment effect. Those participants who did not show an alignment effect in errors still showed a significant effect in latencies. This is an important finding because it has been claimed that the alignment effect literature has tended to ignore individual differences and has unduly generalized the phenomenon of alignment effects as applying to all people (Rossano et al., 1995). Our results do not support the conclusion that a significant group of people are immune to alignment effects; rather, they support the idea that nearly everyone shows an alignment effect in latencies but that, for some people, additional time does not help them imagine nonviewed perspectives as accurately as those that were previously experienced. Despite these differences, we found little evidence for a speed–accuracy tradeoff, either across or within participants. The group of people who showed little or no alignment effect in errors did not require more time than the others to deliberate. Nor for any participant were increased errors significantly associated with faster solution times. Although our data clearly show group differences in the presence of an alignment effect when measured by errors, at this point we can only speculate about the differences that underlie these groups. Likely factors include individual differences in visuospatial ability, conscientiousness, or motivation.

The results of the rotate condition were interesting and unexpected. We anticipated that one of two data patterns would emerge. After participants had rotated, we expected that their mental representation of the test space would have a preferred orientation corresponding to either the orientation they had during learning or to their current body orientation. If the preferred orientation corresponded with the learning orientation, we would have expected an alignment effect in the rotate condition that was similar to that in the stay condition. On the other hand, if the preferred orientation after rotation corresponded to the body's orientation, we would have expected a reverse alignment effect. Averaged over all participants, neither of these data patterns emerged. Although there was a slight reverse alignment effect in the rotate condition, it was extremely small (mean Cohen's $d = 0.08$) and was attained in conjunction with relatively high difficulty for both aligned and misaligned trials.

Inspection of the data from individual participants in the rotate condition is informative and suggests that the two hypothesized data patterns did exist among participants, and that analysis of mean data has concealed these differences. In the rotate condition, 15 of the 28 participants showed an alignment effect, with the rest showing a reverse effect. Figure 4 shows that many of the reverse effects were quite pronounced. The different data patterns among these participants suggest that some people, after rotating, relied primarily on the learned view of the layout, whereas others adopted their body orientation as the preferred orientation. Apparently, for the former group of participants, bodily rotation was not sufficient to overpower the memory of the layout they had seen. For these participants, physical movement per se was not sufficient to induce a new preferred orientation in memory for large layouts. It seems rather more likely that for these participants, physical movement in conjunction with a cognitive interpretation of what the movement means or implies is necessary for altering a preferred orientation in memory. Experiment 3 examines this issue in more detail.

Experiment 3

The results of the rotate condition in Experiment 2 suggest that after turning in place 180°, some people maintain a preferred orientation in their mental representation that is consistent with what they have seen. Others appear to adopt a new preferred orientation that corresponds with their facing direction after rotation. In Experiment 3, we attempt to bring these differences under experimental control by giving two groups of participants a different set of instructions about how to interpret their rotation.

Experiment 3 again employed a rotate condition and a stay condition. However, in the rotate condition, one group of participants was instructed to maintain the image of the array that they studied in their mind as they rotated. Implicitly, this group was asked to ignore their rotation. We call this the *ignore* condition. The other group was instructed to turn their backs to the path so that it was behind them after their rotation. Because they were implicitly asked to keep track of, and account for, their rotation, we call this the *update* condition. If the adoption of a preferred orientation depends not only on physical movement but on how this movement is interpreted, we would expect people who are instructed to ignore their rotation to show a similar alignment effect in rotate as in stay. Conversely, people who receive the update instructions should show a reversed alignment effect in rotate relative to the alignment effect in stay. Technically, this pattern of results can be interpreted as a 3-way interaction between instructional set (ignore or update), trial type (stay or rotate) and alignment. A hypothesized reverse-alignment effect in the update condition will also support the idea that, even after updating one's representation to a new preferred orientation, the representation remains orientation specific (but specific to another heading).

Method

Participants. Twenty-four students (12 men and 12 women) participated in the experiment in order to satisfy a requirement in their introductory psychology course.

Materials. Materials for Experiment 3 were the same as those reported for Experiment 1.

Procedure. Experiment 3 generally followed the same procedures as the previous experiments, however, there were only two testing conditions (stay and rotate) and an additional factor of instructional set was manipulated between participants. Participants were randomly assigned to receive one of two instructional sets. Three practice trials (one stay and two rotate) were given in the order that they would appear in the experiment. As with the previous experiments, participants were instructed during the practice trials that on stay trials, after learning each path, they were to (re)place the HMD on their head, stand up, and wait 30 s for a tone from the computer to signal their first question. Participants were given the same instructions for rotate trials, but, in addition, were told to turn 180° after standing up. Participants who received the ignore instructions were told that during rotate trials they should remember the image of the path that they had just learned and to pretend, as they rotated, that the path was rotating around with them, as if it were fixed to their bodies. They were asked to imagine that after rotating, the path was still in front of them. Participants who received the update instructions were simply told to turn so that the path was now behind them. Before testing in both conditions, participants were asked to point to each of the corners of the learned path. During rotate trials, if participants who received the ignore instructions did not point in front of themselves, they were turned around, and asked to relearn the path for 30 s. Similarly, if participants who received the update instructions did not point behind them to the corners of the path during rotate trials, they were asked to turn back and were reminded of the instructions. Practice trials were followed by six experimental trials, in two blocks of three. Each block presented three paths in either the stay condition or the rotate condition. The order of conditions was counterbalanced for each gender.

Results

Data from three paths of three participants were excluded from analyses because on these trials participants reported forgetting the ordering of the locations. Orientation and reaction times from five responses (less than 2% of all trials) were eliminated because of malfunctioning equipment.

Across all measures and participants, there was an effect of alignment in the stay condition, however the effect in errors was only 6.49°—considerably smaller than that of previous experiments. Participants who received the ignore instructional set showed an alignment effect of similar magnitude in the rotate as in the stay condition. On the other hand, participants who received the update instructions showed, on every measure, a reversed alignment effect in rotate trials.

These effects were tested in a 2 (instructional set) × 2 (test type) × 2 (alignment) MANOVA with all but the first factor represented within subjects. Dependent measures included in the analyses were normalized orientation time, normalized reaction time, and absolute pointing error. The important effect was a significant 3-way interaction between instructional set, test type, and alignment, which is illustrated in Figure 6. The figure shows that the instructions given to the participant had a differential effect on performance in the rotate condition. For participants receiving the ignore instructions, there was a significant main effect of alignment $F(3, 9) = 9.34$, $p < .01$. Although not apparent from Figure 6, this alignment effect interacted significantly with testing type $F(3, 9) = 8.29$, $p < .01$ primarily because of the relatively low errors on misaligned stay trials. Tests of simple main alignment effects for the ignore group showed a significant main effect in stay $F(3, 9) = 24.67$, $p < .01$. The simple main effect of alignment for the ignore group in the rotate condition did not attain significance $F(3, 9) = 2.56$, $p = .15$, although univariate tests of the alignment effect in this condition were generally close to significance: orientation time $t(11) = 1.68$, $p = .12$; reaction time $t(11) = 2.80$, $p = .02$; absolute error $t(11) = 2.04$, $p = .07$.

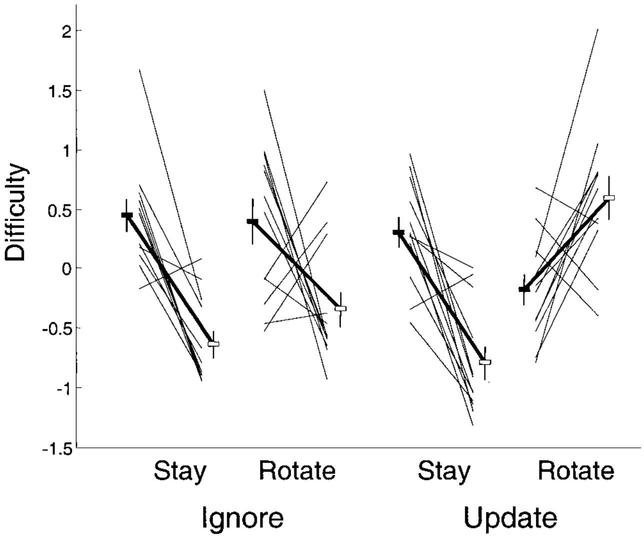

Figure 6. *Difficulty* (computed as the mean *z* score of participants' [individually normalized] orientation time, reaction time, and asbolute pointing error). For aligned (open bars) and misaligned (closed bars) trials in the two testing conditions and two instructional sets for Experiment 3. Means and standard errors are shown next to the individual data.

On the other hand, for participants receiving the update instructions, there was not a significant effect of alignment $F(3, 9) = 1.21$, $p = .36$ because of a significant crossover interaction between alignment and testing type $F(3, 9) = 12.60$, $p = .01$. Univariate tests showed a significant crossover interaction: orientation times $t(11) = 3.51$, $p < .01$; reaction times $t(11) = 5.83$, $p < .01$; and pointing error $t(11) = 2.73$, $p = .02$. Tests of simple main effects in the update group showed a significant alignment effect in the stay condition $F(3, 9) = 13.85$, $p < .01$ as well as a marginally significant reverse-alignment effect in the rotate condition $F(3, 9) = 3.62$, $p = .06$.

Figure 6 shows that the rotate condition was generally slightly more difficult for people receiving the update instructions than the ignore instructions. Collapsing over alignment, orientation times in the rotate condition took, on average, 0.12 more s for update participants than for ignore participants. Similarly, reaction times in rotate took 1.22 more s, and answers were 12.10° less accurate in the update condition than in the ignore condition. Relative difficulty between ignore and update instructions in the rotate condition was tested statistically in a 1-way MANOVA using orientation times, reaction times, and absolute errors for dependent variables and instructional set as the independent variable. The results showed that the rotate condition was significantly more difficult for update participants than for ignore participants $F(6, 17) = 3.06$, $p = .03$.

Figure 6 also suggests that the alignment effect in the rotate condition for ignore participants was of a very similar magnitude as the reverse-alignment effect for update participants in the rotate condition. A MANOVA confirmed that the absolute value of the alignment effect in the rotate condition was not significantly different between the two instructional sets $F(3, 20) = 1.54$, $p = .23$.

As with the previous experiments, there was no evidence for a speed–accuracy tradeoff within participants.

Discussion

In the rotate condition of Experiment 3, people were tested while standing in an orientation that was directly opposed to their orientation when learning the array. Whether their bodies' current testing orientation or its previous orientation during learning was preferred depended on the instructions that participants were given about how to interpret their rotation. People who were instructed to rotate so that the array remained behind them answered more quickly and accurately when judgments involved their current body orientation rather than the orientation during learning. This is good evidence that after rotating, participants' mental representation of the layout was still stored with a preferred orientation but that the preferred orientation had been altered on the basis of sensorimotor updating. This supports the speculation by Simons and Wang (1998) that spatial updating does not result in a viewer-independent representation—but rather a viewer-centered representation that corresponds to the person's current position.

On the other hand, participants who were instructed to imagine the path rotating with them answered more quickly and accurately when judgments involved their learning orientation. Ostensibly, this group was able to ignore their rotation because their pattern of alignment effects changed little between the stay and rotate conditions. The performance of this group is surprising because previous evidence has suggested that rotations of one's body are automatically updated and require mental effort to undo (Farrell & Robertson, 1998; Farrell & Thomson, 1998). Our results, on the contrary, suggest that there may have been little cost for participants to ignore their rotation. Indeed, overall performance in the rotate condition was significantly better for participants who were given instructions to ignore their rotation than for those who were not. One way to resolve the discrepancy between these findings is to hypothesize that mentally undoing a body rotation affects, primarily, latencies not errors. In the present experiment, we did not measure the time that participants required after their rotation in order to adjust to the viewing orientation implied by their instructions. It is possible that participants in the ignore condition required more time to reacquire the learned orientation after rotating than participants in the update condition required to adopt their current body orientation as the preferred direction of their mental representation. However, our results show that once ignore participants had adjusted the preferred direction of their representation to the view they had learned, accessing the representation was not more error prone and did not demand more time than if they had responded on the basis of an updated representation.

General Discussion

Experiments 1 and 2 have shown that when people are not oriented to their surroundings, judgments of the relative directions between objects are significantly easier when they are imagined from a previously viewed orientation than from an orientation that is directly opposite to a previously viewed orientation. Until recently, many investigators did not believe that such an alignment effect occurred as a result of learning a room-sized stimulus array (Presson et al., 1989; Presson & Hazelrigg, 1984; Sholl & Nolin, 1997). However, in the past few years, several articles have begun to cast doubt on this notion, showing that large arrays do in fact lead to alignment effects (Mou & McNamara, 2002; Roskos-Ewoldsen et al., 1998; Shelton & McNamara, 1997, 2001a). The present article adds support to the emerging consensus that room-sized spaces do in fact lead to alignment effects, but also perhaps offers some insight into why the original results were obtained. In addition to the possibility that participants in early experiments were not properly disoriented before making their judgments (for example, Presson et al., 1989, Experiment 1; Presson & Hazelrigg, 1984) our data suggest that some of the early results may have been obtained because the alignment effect is not always large when examined in terms of errors; it is often more robustly manifest in latencies. In Experiments 1 and 2, if we had collected and analyzed only errors, we would not have concluded that there was a significant alignment effect in the wheel condition. Indeed, half of all of the participants in Experiments 1 and 2 did not exhibit a strong alignment effect in errors, yet their alignment effect in latencies was highly significant. By combining error data with latencies, we were able to show that an alignment effect for large layouts is reliable and robust.

The present experiments have begun to clarify the degree to which mental representations and the transformational processes that act upon them are influenced after learning by sensorimotor awareness about the orientation of one's body. When moving through a learned environment, one's mental representation of the layout is easily and accurately updated to reflect the changes of heading assumed by one's body. In cases like this, we believe that two orientations compete for preferred status in spatial memory: the person's current orientation and the initial learned orientation. An important question is which orientation will be preferred in a given situation. Our evidence suggests that when people are oriented to their location and heading, their current orientation will be preferred as an organizer of their knowledge of the surrounds. In circumstances when people are at least somewhat disoriented, the preferred orientation becomes the viewpoint experienced during learning. For example, in our direct walk condition, most people updated their current orientations quite effectively. In this condition, questions that required people to imagine a viewpoint that was misaligned relative to the initially learned orientation were actually aligned relative to their current orientation after the walk. Performance in this condition was quite fast and accurate on misaligned trials, suggesting that the current orientation was preferred over the learned orientation. On the other hand, in wheel conditions, people were disoriented as to their current location and heading. Their current orientation thus had no impact on their judgments, and the learned orientation expressed itself, producing slower and less accurate pointing on misaligned than aligned questions.

Several investigators have shown a close association between body movement, mental imagery (Kosslyn, 1994; Schwartz, 1999; Schwartz & Black, 1999; Wexler, Kosslyn, & Berthoz, 1998), and spatial knowledge (Rieser et al., 1986; Shelton & McNamara, 2001b; Simons & Wang, 1998). One important question raised by this research, as well as ours, concerns the degree to which body movements affect long-term memory (as opposed to working memory). Although in our direct walk condition, the ability to imagine an unseen view was, in general, greatly facilitated by body movement, it is unclear whether this facilitation affected any long-term memorial representations of the stimuli. Another important, though yet unanswered question is whether the preferred orientation of a representation can be altered by means other than the processes associated with proprioceptive updating. For that matter, determining how the idiothetic elements of proprioceptive experience (vestibular, kinesthetic, efference copy) work separately or in conjunction to transform spatial memory is an important, though little researched, issue (but see Chance, Gaunet, Beall, & Loomis, 1998; Harris, Jenkin, & Zikovitz, 2000; Klatzky et al., 1998).

Our evidence suggests both that mental representations of space are view-dependent and that these representations are modified as a result of moving through the environment. In conjunction, these ideas imply that an updated representation is also view dependent—perhaps even dependent on a view that has not been seen. For example, in the update condition in Experiment 3, people who learned a spatial layout from one orientation and then turned to face the opposite direction for testing showed improved performance on questions about orientations that they had not seen relative to those they had. This pattern of data suggests that the act of rotating enabled the preferred orientation of participants' representation to be updated to match their body orientation. It is important to realize that these participants still showed an alignment effect after rotating. Thus, their representations were still orientation-specific, but had merely altered their preferred orientation to match that of their body.

These experiments raise an important question about the automaticity of spatial updating. In general, it is thought that updating one's mental representation as a result of moving in an environment is, if not automatic, at least obligatory (i.e., a necessary consequence of the action). Evidence for this comes from experiments in which participants show performance costs at tasks that require them to ignore the consequences of their movements (Farrell & Robertson, 1998; Farrell & Thomson, 1998; May & Klatzky, 2000) as well as from experiments that show the ease and accuracy with which people are able to update (Rieser, 1989; Rieser et al., 1986). Two of our results appear to contradict the notion that spatial updating is obligatory. First, several participants in Experiment 1 showed a very pronounced alignment effect both in the stay and the direct walk conditions. Thus, in the direct walk condition (in which responses could be greatly facilitated by updating) some participants responded as if they had not moved at all. Second, participants in the ignore condition of Experiment 3 made judgments about spatial layouts while facing in the opposite direction from that in which they learned the layouts. Again, nearly all of these participants responded as if they had not moved at all. In both of these cases, if updating was obligatory and impossible to ignore, we would have expected to see some performance differences between the motion and no motion conditions. These data suggest the possibility that updating mental representations on the basis of bodily rotation is not as automatic as previous research has shown, but rather requires a higher-level cognitive awareness of how the rotation affects the task at hand. Alternatively, it is also possible in these experiments that participants did not ignore their rotations, but that they were cognitively undone by recalling the original learning orientation. However, even if updating one's representation as a result of movement in the environment is automatic, our data clearly show that, at least in these environments, this updating can be undone (or ignored) with little subsequent cost to accuracy or speed.

The notion of a preferred direction in memories of space at all scales is gradually gaining acceptance. From this notion, it is not a great leap to believe that spatial memory consists of stored views that are mentally transformed when tasks demand it. In the present article, we have extended this idea by showing that the preferred orientation of spatial memories can be affected by events that occur after learning, specifically by proprioceptive experience. We suggest that as people travel through the environment—even without vision—the preferred orientation of their spatial memory can be updated to reflect their expectations of how they may act in the environment. Instead of working to create an orientation-free representation, such experience merely updates an orientation-specific representation and thus updates the alignment effect.

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