Reconsidering the role of movement in perceiving action-scaled affordances

Brett R. Fajen *, Gabriel Diaz, Christopher Cramer

Department of Cognitive Science, Rensselaer Polytechnic Institute, United States

ARTICLE INFO

Available online xxxx

PsycINFO classification:
2320
2330

Keywords:
Visual perception
Motor processes
Sports

ABSTRACT

Many locomotor tasks require actors to choose among different categories of action, such as when deciding whether to cross the street in front of an approaching vehicle or wait until it passes. In such cases, the actor’s locomotor capabilities partly determine which actions are possible, and therefore must be taken into account. The present study was designed to re-evaluate the claim that people do not know their locomotor capabilities until they begin moving because they rely entirely on information that is picked up “on the fly” (Oudejans, Michaels, Bakker, & Dolné, 1996). Three experiments were conducted in which participants judged while stationary or moving whether it was within their capabilities to catch a fly ball or pass through a shrinking gap. The main finding was that judgments were equally accurate regardless of whether participants were stationary or allowed to move for a brief period. We conclude that stationary and moving actors know their locomotor capabilities equally well, and that actors do not rely entirely on information that is picked up on the fly.

© 2010 Elsevier B.V. All rights reserved.

1. Introduction

In competitive sports, athletes are often put in situations in which they need to know the limits of their action capabilities. Consider, for example, the situation encountered by a centerfielder in baseball when a weak fly ball is hit toward short centerfield. Suppose that the fielder is too slow to reach...
the landing location on time. If the fielder runs at full speed toward the landing location, and arrives a moment too late, then she may need to make a difficult play to keep the ball from skipping by. If the ball is uncatchable, the smarter play is to slow down and let the ball bounce far enough in front of the fielder that it can be easily caught after it hits the ground.

Even in routine daily behavior, people often find themselves in situations in which they must know the movement capabilities of their bodies or the systems that they control. For example, pedestrians must know their locomotor capabilities when deciding whether to cross the street in front of an approaching car or let it pass (Oudejans, Michaels, van Dort, & Frissen, 1996; Plumert, Kearney, & Cremer, 2007; te Velde, van der Kamp, Barela, & Savelsbergh, 2005), and when deciding whether to pass through a moving gap formed by two other pedestrians crossing their future path. Similarly, drivers must know their vehicle’s acceleration capabilities when deciding whether to overtake a slowly moving lead vehicle, and deceleration capabilities when deciding whether to slow down or swerve to avoid a rapidly decelerating lead vehicle (see Fajen, 2005c; Fajen, 2007b).

These examples underscore the importance of perceiving the environment in relation to one’s movement capabilities. It is essential that actors be able to see what actions are possible within the limits of their capabilities. In other words, actors must be able to perceive action-scaled affordances (Fajen, Riley, & Turvey, 2009; Gibson, 1986; Turvey, 1992; Warren, 1988); that is, possibilities for action that depend on the fit between the environment and one’s action capabilities. To what should we attribute this ability to take one’s locomotor capabilities into account when perceiving the environment? One hypothesis is that actors must (in some sense) know their locomotor capabilities. More specifically, actors must know how their movements relate to changes in the optic array, as well as their capabilities to bring about desired changes in the optic array. The second hypothesis is that actors do not know their locomotor capabilities at all, but rather rely on information that is available “on the fly” as they move that tells them about their locomotor capabilities and specifies what is and is not possible.

One of the few studies on this problem was Oudejans, Michaels, Bakker, and Dolné (1996), in which the authors presented a compelling argument bolstered by data that supports a rather strong version of the on-the-fly hypothesis – that people do not know their locomotor capabilities at all, but rather rely entirely on information that is picked up on the fly. In other words, people can distinguish between actions that are within and beyond their movement capabilities, but they do so without actually knowing their movement capabilities.

1.1. The Oudejans et al. (1996) study

Their experiment focused on the task of perceiving the catchableness of a fly ball. Whether or not a ball is catchable depends on the distance to the landing location, the flight time, and the fielder’s running capabilities. So it would seem that fielders must know their running capabilities to perceive catchableness, an interpretation that would be consistent with the first of the two aforementioned hypotheses. As Oudejans et al. (1996) showed, however, catchableness could also be perceived based on visual information without knowledge of running capabilities. The proposed source of visual information is the optical variable originally identified by Chapman (1968), who pointed out that the second derivative of the tangent of the ball’s elevation angle (\(\alpha\)) provides information about the sufficiency of the fielder’s current running speed (see also McLeod, Reed, & Dienes, 2006; Michaels & Oudejans, 1992). For a fielder running forward to catch a ball, if \(d^2(\tan \alpha)/dt^2 = 0\), then the fielder’s current running speed is sufficient and she will arrive at the landing location at the same time as the ball. If \(d^2(\tan \alpha)/dt^2 > 0\), then the fielder’s running speed is excessive and she will overshoot the landing location unless she decelerates. If \(d^2(\tan \alpha)/dt^2 < 0\), then the fielder’s running speed is insufficient and she will undershoot the landing location unless she accelerates.

In addition to providing information for regulating running speed, \(d^2(\tan \alpha)/dt^2\) also tells the fielder about the catchableness of the ball under some circumstances. If the fielder is running in such a way that \(d^2(\tan \alpha)/dt^2 = 0\), then her current running speed is sufficient. As long as current running speed can be maintained, \(d^2(\tan \alpha)/dt^2 = 0\) specifies that the ball is catchable. Similarly, if the fielder is running forward at her maximum running speed and \(d^2(\tan \alpha)/dt^2 < 0\), then her maximum speed is insufficient, and so the ball is uncatchable. The important point is that fielders do not necessarily need to know their running capabilities to perceive catchableness because visual information specifies

whether the ball is catchable or uncatchable. Thus, fielders can distinguish between actions that are within and beyond their capabilities, without actually knowing their capabilities, as predicted by the on-the-fly hypothesis.

To test this hypothesis, Oudejans et al. (1996) had stationary and moving fielders make judgments of catchableness. \( \frac{d^2a}{dt^2} \) provides information about catchableness, but only when the fielder is moving. When the fielder is stationary, \( \frac{d^2a}{dt^2} \) does not provide any information about catchableness. Therefore, ratios were calculated by comparing judgments to actual catches. The ratio provided a measure of the degree to which participants overestimated or underestimated their ability to catch fly balls. When participants overestimated their locomotor capabilities (i.e., when they tended to judge uncatchable balls as catchable), the ratio was greater than 1.0. Conversely, when they underestimated their locomotor capabilities (i.e., when they tended to judge catchable balls as uncatchable), the ratio was less than 1.0. Before comparing ratios in the Stand and Move conditions, the authors calculated the absolute difference between each ratio and 1.0, as this provides a measure of absolute error regardless of whether the error was due to overestimation or underestimation. The main finding was that judgments were more accurate (i.e., absolute error was smaller) in the Move condition compared to the Stand condition. This is consistent with the authors' predictions and with the on-the-fly hypothesis, and led them to conclude that “Rather than having stored knowledge about action capabilities, actors determine their capabilities anew each time they perform an action” (p. 887). Despite the compelling evidence, there are reasons to reconsider this conclusion.

1.2. Reconsidering the role of movement: the accuracy of judgments

Although Oudejans et al. (1996) based their conclusions on the analysis of absolute error, ratios were not symmetrically distributed around 1.0. There was an overall tendency in both conditions to judge uncatchable balls as catchable, resulting in ratios that were significantly greater than 1.0 in both the Stand (\( M = 1.34, SE = .08, t(11) = 4.47, p < .01 \)) and Move (\( M = 1.18, SE = .05, t(11) = 3.51, p < .01 \)) conditions. At first glance, it appears that participants tended to overestimate their locomotor capabilities, as if they thought they were faster than they actually were. However, this could also reflect the fact that people do not always catch balls that are initially catchable. Occasionally, fielders may get a bad jump on the ball, begin moving slower than they actually can, or miss the ball at the last minute because they did not arrive on time but because they did not get their hand in the right position. Because ratios were calculated by comparing judgments to actual catches, occasionally missing catchable balls would artificially inflate the ratios. Thus, what appears to be a bias to overestimate one’s locomotor capabilities may actually reflect a tendency to base one’s judgments on what could have happened in a best-case scenario rather than on what most likely would have happened.

1 Strictly speaking, using optical acceleration to perceive catchableness does require one to know when one is running at maximum speed and whether or not maximum speed can be maintained. However, we view this as a weak sense of knowing one’s locomotor capabilities. The hypothesis that actors know their locomotor capabilities requires a stronger sense of knowing one’s locomotor capabilities — that is, knowing how fast one can move regardless of how fast one is currently moving.

2 The only exception is when the fielder is stationary and \( \frac{d^2a}{dt^2} = 0 \), in which case the ball is catchable. However, this only occurs when the fielder happens to be standing at the landing location.
Although ratios were significantly greater than 1.0 in both conditions, they also tended to be smaller in the Move condition compared to the Stand condition by an average of 0.16 (SE = 0.086). This difference was only marginally significant (t(11) = 1.94, p = .079), but it suggests that participants were more likely to judge balls as uncatchable in the Move condition. One might attribute this difference to differences in the availability of information about catchableness when moving versus when standing, which would be consistent with the on-the-fly hypothesis. However, it is not clear why allowing participants to move should make them more likely to judge balls as uncatchable, rather than simply make them more accurate.

We propose the following alternative explanation for the lower ratios in the Move condition. During the Move block, participants started running when the ball was launched, knowing that they would decelerate to a stop 1 s later when vision was occluded, and then return to the starting location only to repeat the sequence again. Over trials, they may have lost motivation to run quickly, and therefore covered less distance in the first 1 s than they could have. As a result, balls that were catchable had participants run at full speed may have been judged as uncatchable. Because the ratios were computed by comparing catchableness judgments to actual catches, a tendency to judge catchable balls as uncatchable would result in lower ratios. Importantly, this would only affect judgments in the Move condition. Thus, the lower ratios in the Move condition could be attributed to a bias that was introduced by blocking trials in which participants made judgments and trials in which they attempted to catch the ball.

To summarize, we offered two possible explanations for why ratios may have differed from the “correct” value of 1.0: (1) participants may have based their judgments on what they could have caught under the best-case scenario rather than on what they most likely would have caught, and (2) participants may have covered less distance during the first 1 s of trials in the Move block compared to trials in the Catch block. The first factor would have the effect of inflating ratios in both conditions, and the second factor would have the effect of deflating ratios but only in the Move condition. When these two factors are combined, one would expect ratios to be greater than 1.0 in both conditions, but by a smaller amount in the Move condition. In other words, one would expect ratios to be closer to 1.0 in the Move condition, which is exactly what is reported in Oudejans et al. (1996). Thus, the difference between the Stand and Move conditions in absolute error could have resulted from the two aforementioned biases, both of which are unrelated to whether participants were stationary or moving.

We tested this alternative explanation in Experiment 1 by replicating Oudejans et al. (1996) with two modifications. First, we computed ratios in a way that adjusted for the possibility that participants may not always catch balls that are catchable. This eliminated the first of the two possible sources of bias listed above. Second, rather than presenting judge trials and action trials (i.e., trials in which participants attempted to catch the ball) in separate blocks, judge and action trials were randomly interspersed within a single block. Because participants did not know in advance whether the next trial was a judge trial or an action trial, they should run at the same speed and cover the same distance during the first 1 s of both trial types. Thus, the potential problem of participants running at less than full speed on judge trials is avoided. If the difference in absolute error between the Stand and Move conditions was due to the combined effects of two biases described above, then judgments in the two conditions should be equally accurate when the sources of these biases are eliminated; that is, when the data are appropriately analyzed and when judge trials and action trials are randomly interspersed.

1.3. Reconsidering the role of movement: the consistency of judgments

Oudejans et al. (1996) also compared judgments in the Stand and Move conditions using a second dependent measure other than judge-to-action ratio. The measure in question corresponds to the consistency with which participants responded “yes” on easy trials and “no” on difficult trials, where difficulty is a function of the flight time and distance to the landing location. The consistency of judgments was greater in the Move condition compared to the Stand condition. The difference did not reach statistical significance, and so the authors did not draw any conclusions about the necessity of movement based on this analysis. However, in the interest of being thorough, it is worth...
considering why judgments tended to be more consistent in the Move condition. The explanation that accords with Oudejans et al.’s (1996) on-the-fly hypothesis is that judgments are more consistent in the Move condition because actors only know their locomotor capabilities when they are moving. However, there was another, subtle but potentially important difference between the Stand and Move conditions that may explain the difference in consistency. If a fielder is running to catch a fly ball, vision is suddenly occluded, and the fielder is asked to indicate whether the ball was catchable, one would expect her judgment to be based on what she perceived immediately before vision was occluded. This was the situation that participants encountered in the Move condition. Participants ran for 1 s as if to catch the ball, vision was occluded, and then participants judged whether they could have caught the ball had vision not been occluded. Thus, judgments in the Move condition were most likely based on what participants perceived immediately before vision was occluded – that is, after the ball moved along its trajectory for a full 1 s.

In the Stand condition, participants were instructed to judge whether they could have caught the ball had they started moving when the ball was released. Because participants did not actually move, they could not base their judgments on what they perceived immediately before vision was occluded in the same way that they could in the Move condition. So how did participants make their judgments in the Stand condition? One possibility is that their judgments were based on what they saw immediately after the ball was released – that is, before the instruction not to run affected what a fielder would normally do. Alternatively, participants may have imagined what they would have seen had they been allowed to move.

Either way, the Stand and Move conditions were not equivalent in the way that Oudejans et al. (1996) assumed. If participants imagined what they would have seen had they been allowed to move, the poorer performance in the Stand condition could be attributed to the need to rely on imagery rather than perception. On the other hand, if they based their judgments on what they saw immediately after the ball was released, then judgments in the Stand condition were based on conditions that existed 1 s earlier than the conditions that existed when participants made their judgments in the Move condition. This could also explain why judgments in the Stand condition were less consistent. As the ball moves closer to the landing location, the difference between catchable and uncatchable balls should become more salient, and hence easier to discriminate. Consider, for example, what would happen if vision in the Move condition was occluded just 100 ms before the ball reached the height of the fielder’s glove. By that point, the difference between catchable and uncatchable balls would be even more obvious. Another way to think about this is to consider that the task required participants to make a judgment about the event of “running to catch a fly ball.” In the Move condition, this event was allowed to unfold in the normal way for 1 s before vision was occluded. In the Stand condition, the event of running to catch a fly ball was terminated at the moment that participants did not run when they would normally do – that is, the event was terminated shortly after the ball was released. Thus, the poorer performance in the Stand condition could also be attributed to the difference in the amount of time that the event of running to catch a fly ball was allowed to unfold.

This alternative explanation was tested in Experiment 1 by adding a new third condition, which we refer to as the Delayed-Move condition, to control for the aforementioned difference between the Stand and Move conditions. In the Delayed-Move condition, participants were not allowed to start moving until a go signal (a whistle) was presented 1 s after the ball was released. At the same time that the go signal was presented, vision of the ball was occluded. The participants’ task was to judge whether the ball was catchable had they started moving when the go signal was presented (i.e., 1 s after the ball was released). Because the go signal was played at the same time that the ball disappeared, participants had to base their judgments on information that was picked up before movement was initiated. In other words, judgment trials in the Delayed-Move condition were like those in the Stand condition in that participants were standing still when they judged catchability. But because they had to wait for the go signal, those judgments were based on the conditions that existed 1 s after launch, as in the Move condition. If the conclusions of Oudejans et al. (1996) are correct and stationary participants are unable to judge catchability, then judgments in the Delayed-Move condition should be similar to judgments in the Stand condition and less consistent than judgments in the Move condition. On the other hand, if participants in Oudejans et al. performed better in the Move condition...
than in the Stand condition because the event was allowed to unfold for one additional second, then judgments in the Delayed-Move condition should be more consistent than judgments in the Stand condition and similar to judgments in the Move condition.

2. Experiment 1: perceiving the catchability of a fly ball

Experiment 1 was designed to re-evaluate the claim that stationary fielders cannot judge catchability, and hence that people do not know their action capabilities. In particular, we replicated Oudejans et al. (1996) in an ambulatory virtual environment with several modifications to test the alternative explanation of their findings that was described above. The full design of Experiment 1 is summarized in Fig. 1. (Readers are encouraged to refer back to Fig. 1 as needed because it also provides illustrations of the task associated with each condition.) Participants completed four blocks of trials in two 1.5 h-long sessions. In the Move Block, participants either attempted to catch the ball (action trials) or judged whether the ball was catchable (judge trials). Participants were instructed to start moving so as to catch the ball as soon as the ball was projected into the air. On 50% of the trials,

<table>
<thead>
<tr>
<th>Block</th>
<th>Trial type (# of trials)</th>
<th>Task</th>
<th>Viewing time (s)</th>
<th>Go signal time (s)</th>
<th>Oudejans equivalent</th>
<th>Remaining flight time (s)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Move</td>
<td>Judge (64)</td>
<td></td>
<td>1.0</td>
<td>0</td>
<td>Move</td>
<td>FT\textsubscript{init} - 1.0</td>
</tr>
<tr>
<td></td>
<td>Action (64)</td>
<td></td>
<td>∞</td>
<td>0</td>
<td>Catch</td>
<td></td>
</tr>
<tr>
<td>Stand</td>
<td>Judge (64)</td>
<td></td>
<td>1.0</td>
<td>0</td>
<td>Stand</td>
<td>FT\textsubscript{init}</td>
</tr>
<tr>
<td>Delayed-Mixed</td>
<td>Judge (64)</td>
<td></td>
<td>1.0</td>
<td>1.0</td>
<td>None</td>
<td>FT\textsubscript{init} - 1.0</td>
</tr>
<tr>
<td></td>
<td>Action (64)</td>
<td></td>
<td>∞</td>
<td>1.0</td>
<td>None</td>
<td></td>
</tr>
<tr>
<td>Delayed-Judge</td>
<td>Judge (64)</td>
<td></td>
<td>1.0</td>
<td>1.0</td>
<td>None</td>
<td>FT\textsubscript{init} - 1.0</td>
</tr>
</tbody>
</table>

Fig. 1. Design of Experiment 1 indicating type and number of trials, task, viewing time, go signal time, equivalent block from Oudejans et al. (1996), and remaining flight time for the Move, Stand, Delayed-Mixed, and Delayed-Judge blocks. Viewing time refers to the amount of time that the ball remained visible after launch. A viewing time of ∞ indicates that ball remained visible throughout entire trial. Go signal time refers to the time after launch at which the go signal (i.e., the whistle telling participants that they could start moving) was presented. Remaining flight time refers to the amount of flight time remaining at the latest point at which catchability judgments could be made. FT\textsubscript{init} stands for initial flight time. Shading indicates how judge and action blocks were paired when judge-to-action ratios were computed.

the ball disappeared 1 s after it was launched and participants were asked to judge whether or not they could have caught the ball had it not disappeared. On the remaining 50% of trials, the ball remained visible and participants were required to try to catch the ball. Judge trials in the Move Block are similar to the Move condition in Oudejans et al. (1996) and action trials are similar to the Catch condition. The main difference is that judge and action trials were randomly interspersed, rather than blocked. This allowed us to test the alternative explanation for the lower judge-to-action ratios in the Move condition. If, in fact, participants in Oudejans et al. tended to judge balls as catchable less often in the Move condition because they ran at less than full speed during the first 1 s of Move trials, then the difference between the Stand and Move conditions should be eliminated when Move trials and Catch trials are randomly interspersed.

In the Stand Block, the ball always disappeared after 1 s. Participants were instructed to stand at the home location and judge whether or not they could have caught the ball had they started moving when it was launched. The Stand Block is similar to the Stand condition in Experiment 2 of Oudejans et al. (1996). Based on the results of Oudejans et al., it was expected that judgments in the Stand Block would be slightly less consistent than judgments in the Move Block.

The Delayed-Mixed Block was similar to the Move Block in that the ball disappeared after 1 s on 50% of the trials and remained visible on the remaining trials. As in the Move Block, participants made catchability judgments when the ball disappeared and attempted to catch the ball when it remained visible. Unlike the Move Block, however, participants were not allowed to start moving until a go signal (a whistle) was played 1 s after the ball was released. If participants do not know their locomotor capabilities until they start moving, then judgments in the Delayed-Mixed Block should be just as inconsistent as judgments in the Stand Block, and both conditions should be less consistent than judgments in the Move Block. If, on the other hand, judgments in the Move condition in Oudejans et al. were more consistent because the event was allowed to unfold for one additional second, then judgments in the Delayed-Mixed Block should be more consistent than judgments in the Stand Block and equally consistent with judgments in the Move block.

The final block, the Delayed-Judge Block was similar to the Stand Block in that the ball always disappeared after 1 s. As in the Delayed-Mixed Block, a whistle was played at the same time that the ball disappeared. Participants were told to stand at the home location and judge whether they could have caught the ball had they started moving when the whistle was played.

The Move and Stand Blocks were completed in one session, and the Delayed-Mixed and Delayed-Judge Blocks were completed in the other session. The order of sessions was counterbalanced across participants.

2.1. Method

2.1.1. Participants

Sixteen students participated in the experiment. All participants reported that they had normal or corrected-to-normal vision and no visual or motor impairments. All participants also reported that they had at least some experience playing ball sports.

2.1.2. Equipment

The experiment was conducted in a 6.5 m × 9 m virtual environment laboratory. Participants wore an nVis nVisor SX stereoscopic, head-mounted display. The HMD weighed 1000 g, the resolution was 1280 pixels × 1024 pixels per eye, and the diagonal field-of-view was 60°. Participants’ head and right hand position and orientation were tracked using an Intersense IS-900 motion tracking system. Data from the head and hand tracker were used to update the position and orientation of the simulated viewpoint and 3D model of the right hand. The cables from the HMD and tracking system were bundled together, and held by the experimenter, who walked alongside the participant as he or she moved to ensure that movement was not restricted by the cables. The virtual environment was created using Sense 8 World Tool Kit software running on a Dell Workstation 650 with a Wildcat 7110 dual-head graphics card. The 3D model of the right hand with fingers in the open position was created using 3DStudioMax.

2.1.3. Simulated environment and procedure

The simulated environment consisted of a flat, grass-textured ground plane and a solid blue sky (see Fig. 2A). Nine large vertical posts (8 m high \times 0.1 m radius) were positioned along a 180° arc at a distance of 15 m from the home location. The posts provided a stable visual reference which, unlike the ground plane, was visible even when participants were looking up to track the ball. At the beginning of each trial, participants stood at the home location, marked by a yellow translucent box (0.5 m wide \times 0.5 m long \times 2.0 m high). To ensure that participants were always at the same location at the beginning of each trial, the trial would not begin unless the participant was standing inside the box.

The simulated environment and procedure for the Move Block will be described first, followed by the Stand, Delayed-Mixed, and Delayed-Judge Blocks.

**Move Block.** Participants held a remote mouse in their left hand. Each trial was initiated by pressing and holding the left button on the remote mouse. In the Move Block, the ball (a solid yellow sphere with a radius of 7 cm) appeared on the ground plane directly ahead of the participant at an initial distance that varied randomly between 8 and 12 m (see Fig. 2B). Initial distance was randomized to eliminate the presence of simple cues that participants could use to judge catchableness. If the ball was always launched from the same location, then the number of unique trajectories (16) would be small because there were only four landing locations crossed with four flight times. With enough practice, participants could learn to recognize the trajectories and base their judgments on memory. By
randomizing initial distance, the same combination of landing location and flight time could result from a peaked trajectory or a flat trajectory (i.e., a pop fly or a line drive). Thus, participants could not base their judgments on memory because each trajectory was unique. After a brief foreperiod lasting between 0.5 and 1.5 s, the ball was projected into the air and followed a parabolic flight path to the landing location. There were four flight times (1.75, 2.0, 2.25, and 2.5 s) and four landing locations (2.0, 3.0, 4.0, and 5.0 m from the home position). The trajectory of the ball was determined by a combination of the ball’s initial position, landing location, flight time, and simulated gravity (which was set to 9.8 m/s²). There was no simulated drag.

In the Move Block, participants waited until the ball was launched before starting to move. A go signal (a whistle) was played at the same time that the ball was launched. The whistle was presented by the computer so that the timing relative to the release of the ball could be precisely controlled. If the participant moved more than 25 cm from the initial location before the ball was launched, then the trial was automatically terminated. Fifty percent of the trials were action trials and the remaining 50% were judge trials. Action and judge trials were randomly interleaved. On action trials, the ball remained visible and participants attempted to move into position and reach out to catch the ball. If the center of the ball passed within 25 cm of the center of the hand, then the catch was considered successful and the virtual ball stuck to the virtual hand. A distance larger than the radius of the ball (7 cm) was used to simulate catching with a baseball glove. If the ball did not pass within range, then it continued moving along its predetermined flight path until it reached the ground, at which point a “thump” noise was played. On judge trials, the ball disappeared 1 s after it was launched and participants made a yes/no judgment about whether they could have caught the ball by pressing the left mouse button for Yes and the right mouse button for No. The response (“Yes” or “No”) was presented on the HMD screen for 1 s. Participants were told that if they accidentally entered the wrong response, they should inform the experimenter, who would correct the response. After each trial, participants returned to the home location and pressed the left mouse button to begin the next trial. There were four repetitions per condition in the Move Block for a total of 128 trials (2 trial types × 4 flight times × 4 landing locations × 4 repetitions). Half of the trials (two repetitions per condition) were completed before the Stand Block and the other half were completed after the Stand Block.

Stand Block. The Stand Block was similar to the Move Block with the following exceptions: (1) the ball always disappeared 1 s after it was launched and (2) participants were instructed to stand at the home location and judge whether they could have caught the ball if they had started moving when it was released. To ensure that participants stayed at the home location, the program was designed to automatically terminate the trial if participants moved outside of the home box. There were four repetitions per condition for a total of 64 trials (4 flight times × 4 landing locations × 4 repetitions) in the Stand Block.

Delayed-Mixed Block. The Delayed-Mixed Block was similar to the Move Block in that 50% of the trials were judge trials and 50% were action trials. The only differences between the Delayed-Mixed Block and the Move Block were that: (1) the go signal (whistle) telling participants to start moving was played 1 s after the ball was launched (i.e., at the same time that the ball disappeared on judge trials) and (2) the range of ball trajectories was shifted toward the home location by 1.5 m. That is, initial ball distance varied between 6.5 and 10.5 m, and the four landing locations were 0.5, 1.5, 2.5, and 3.5 m from the home location. Closer initial distances and landing locations were used in the Delayed-Mixed Block (and the Delayed-Judge Block; see below) because participants had less time to move into position to catch the ball. During pilot testing, we found that the overall % of catches in the Move and Delayed-Mixed Blocks would be roughly equal (~50%) if the trajectories were shifted 1.5 m closer to the home location. Because both the initial distances and landing locations were shifted forward by 1.5 m, the shapes of the trajectories were the same as they were in the Move and Stand Blocks. Note that shifting the range of ball trajectories closer to the home location does not make it easier for participants to distinguish between catchable and uncatchable balls. In fact, for any given flight time, it is actually easier to perceive that a ball is uncatchable as the landing location moves farther away. There were four repetitions per condition in the Delayed-Mixed Block for a total of 128 trials (2 trial types × 4 flight times × 4 landing locations × 4 repetitions). Half of the trials were completed before the Delayed-Judge Block and the other half were completed after the Delayed-Judge Block.

Delayed-Judge Block. The Delayed-Judge Block was similar to the Delayed-Mixed Block with the following exceptions: (1) the ball always disappeared 1 s after it was launched and (2) participants were instructed to stand at the home location and judge whether they could have caught the ball if they started moving when the whistle was played. There were four repetitions per condition for a total of 64 trials (4 flight times \( \times \) 4 landing locations \( \times \) 4 repetitions) in the Delayed-Judge Block.

Practice. Participants completed two short practice blocks at the beginning of the first session and one practice block at the beginning of the second session. The practice blocks were similar to the experiment blocks except that all trials were action trials (i.e., there were no judgment trials). The practice blocks were necessary because participants need time to adapt to the visuomotor rearrangements experienced in virtual environments (Welch, 2002) and gain confidence moving around while wearing a head-mounted display. Through extensive pilot testing, we found that two practice blocks were sufficient for most inexperienced VE users to adapt well enough to perform the ball catching task. Only one practice block was needed at the beginning of the second session. It should be noted that participants in Oudejans et al. (1996) did not practice catching balls before starting the experiment, and completed both judgment blocks before completing the catch block. The possible implications of this difference between Oudejans et al. and the present study will be considered in Experiment 2.

2.1.4. Data analysis

Participants occasionally failed to catch balls that were obviously catchable. For example, if a participant overran a ball, then the trial was recorded as a miss even though the ball was obviously catchable. Because the accuracy of catchableness judgments was measured by comparing judgments to actual catches, such trials could unfairly decrease the accuracy of judgments. To avoid this problem, ball and hand trajectories were analyzed on each catch trial. If the participant missed because the hand overshot the ball, then the result was changed from a miss to a catch. This occurred on 4.8% of action trials.

2.2. Results and discussion

2.2.1. Overall performance on action trials

The percentage of balls that were caught (hereafter, % caught) varied widely across participants [53.5% \( (SD = 13.4\%) \) for the Move Block and 43.8% \( (SD = 10.1\%) \) for the Delayed-Mixed Block], reflecting individual differences in catching skill. Fig. 3 shows the mean % caught broken down by landing location and flight time for action trials in the Move and Delayed-Mixed Blocks. As expected, the % caught decreased with distance to the landing location and increased with flight time.

2.2.2. Consistency of catchableness judgments

Following Oudejans et al. (1996), we considered two different ways in which movement could affect the quality of catchableness judgments. For the first analysis, the % of balls that were judged as catchable (hereafter, % catchable) for each participant and each block was plotted as a function of the locomotor speed required to reach the landing location on time. Sample data for one participant on one block are shown in Fig. 4. These data were then fit by a logistic function using the following equation: 
\[
y = \frac{1}{1 + e^{-k(c - x)}}
\]
where \( k \) and \( c \) are parameters corresponding to the slope of the curve and the critical value at which 50% of the balls were judged as catchable, respectively. Following Oudejans et al., we computed the correlation coefficient of the fitted curve, and then used a Fisher Z transformation to normalize the distribution of correlation coefficients. The resulting Fisher Z values provide a measure of the consistency of judgments. When % catchable consistently decreased with required speed (as in Fig. 4), the Fisher Z value was high. When % catchable changed less consistently with required speed (e.g., when % catchable fluctuated with required speed), the Fisher Z value was low.

3 We found similar results when using required acceleration rather than required speed (as did Oudejans et al.).
4 For three of the 64 total blocks (i.e., 16 participants \( \times \) 4 blocks/participant), the fits were nearly perfect, resulting in Fisher Z values that exceeded the mean by more than three standard deviations. These values were adjusted to equal the maximum value of the other three conditions. The values that were adjusted came from three different participants and three different blocks.

A repeated measures ANOVA revealed a significant effect of block on the Fisher Z values of the correlation coefficients ($F(3, 45) = 3.54, p < .05, \eta^2 = .19$; see Fig. 5A). The mean Fisher Z value was higher in the Move Block than in the Stand Block, although contrast effects revealed that this difference did not reach significance ($p = .13$). This result replicates Oudejans et al. (1996), who also found that Fisher Z values were higher in the Move condition than in the Stand condition, but not significantly so. The important new finding was that Fisher Z values in the Delayed-Mixed and Delayed-Judge Blocks were significantly higher than in the Stand Block. Together, the results indicate that judgments were equally consistent regardless of whether participants were stationary or moving, contrary to what one would expect if actors do not know their locomotor capabilities except when they are moving.

In the analysis above, which was based on the same analysis in Oudejans et al. (1996), % catchable was plotted as a function of the speed required to reach the landing location in time to catch the ball. The underlying assumption is that the difficulty of each condition can be approximated by required running speed; that is, as required speed increases, a point will be reached at which participants can no longer reach the landing location in time. However, required running speed is not a perfect predictor of the difficulty of each condition because participants do not move at a constant speed. Oudejans et al. were aware of this issue, and addressed it by re-running the same analysis using required acceleration. However, because participants do not maintain a constant rate of acceleration, required acceleration is not a perfect predictor of difficulty either. Typically, locomotor speed increases from

Fig. 3. Percentage of balls that were caught as a function of initial distance to landing location and flight time for the Move Block (A) and the Delayed-Mixed Block (B).

zero to some roughly constant maximum speed. Moreover, the rate of acceleration and the maximum running speed differs across participants. Therefore, we re-ran this analysis using an alternative, more principled method for estimating the difficulty of each condition. The first step was to use the data from action trials to estimate each individual participants’ maximum locomotor capabilities. Specifically, for each time step between zero and 2.5 s (in 0.02 s increments), the data from action trials in the Move and Delayed-Mixed blocks were analyzed to find the farthest distance travelled at each point in time. Because participants caught balls with their hand, we added 50 cm to the head position data when maximum distance travelled was determined. Fig. 6A shows data from a sample participant for both the Move (solid curve) and Delayed-Mixed (dashed curve) blocks. The second step was to calculate, for each combination of flight time and initial distance, the difference between the amount of time that the participant needed to reach the landing location (based on the estimate from the previous step) and the flight time of the ball, which we refer to as time-to-spare. Using the data in Fig. 6A, for the condition in which flight time was 2.0 s and the landing location was 4.0 m, time-to-spare is equal to 0.27 s [2.0 s (the flight time) minus 1.73 s (the time needed to travel 4.0 m)]. Time-to-spare provides an estimate of difficulty of the condition, measured in terms of the amount of time that the participant had to spare (when the difference was positive) or fell short of the landing location (when the difference was negative). The third step was to plot % catchable as a function of time-to-spare (see Fig. 6B). Finally, we fit the data using a sigmoid function and calculated the correlation coefficient and corresponding Fisher Z value to estimate the goodness-of-fit.

The overall trend across blocks was similar to the analysis based on required speed (see Fig. 5B), but the effect of block was not statistically significant ($F(3,45) < 1$, $\eta^2 = .05$). Thus, this analysis revealed no further evidence to support the hypothesis that catchableness is perceived more consistently when participants are moving.

### 2.2.3. Judge-to-action ratio

The second analysis provides a measure of the degree to which participants overestimated or underestimated their locomotor capabilities, if at all. Following Oudejans et al. (1996), we calculated (for each participant and each block) the critical value of required running speed at which 50% of the judgments were yes, and the critical value of required locomotor speed at which 50% of the balls were caught. The ratio of the former to the latter was then calculated.
We found that the ratios were significantly greater than 1.0 in all four conditions ($M = 1.25$, $SE = .07$, $t(15) = 3.74$, $p < .01$ for the Move condition; $M = 1.15$, $SE = .06$, $t(15) = 2.59$, $p < .05$ for the Stand condition; $M = 1.48$, $SE = .09$, $t(15) = 5.23$, $p < .01$ for the Delayed-Mixed condition; $M = 1.47$, $SE = .09$, $t(15) = 5.49$, $p < .01$ for the Delayed-Judge condition). There was also a significant main effect of condition on judge-to-action ratios, $F(3,45) = 12.26$, $p < .01$, $\eta^2 = .45$. Post-hoc tests (using the Bonferroni adjustment) revealed that ratios in the two Delayed conditions were significantly greater than ratios in the Move and Stand conditions, but that there was no significant difference between ratios in the Move condition compared to the Stand condition as they were in Oudejans et al.

Fig. 5. Fisher Z values for each block indicating the goodness-of-fit of the best-fitting curve for % catchable versus required speed (A) and % catchable versus time-to-spare (B).
First, as in Oudejans et al. (1996), judge-to-action ratios were consistently greater than 1.0 regardless of whether judgments were made while standing or moving. In the Introduction, we pointed out that ratios greater than 1.0 could also reflect the fact that participants do not always catch balls that are initially catchable because they sometimes get a bad jump, begin moving slower than they can, or simply miss the ball at the last second. To address this problem, we adjusted the % caught from any condition in which the participant caught at least one of the four balls to 100% catchable, and then ratios were consistently higher in the Delayed-Mixed and Delayed-Judge conditions compared to the Move and Stand conditions.

First, as in Oudejans et al. (1996), judge-to-action ratios were consistently greater than 1.0 regardless of whether judgments were made while standing or moving. In the Introduction, we pointed out that ratios greater than 1.0 could also reflect the fact that participants do not always catch balls that are initially catchable because they sometimes get a bad jump, begin moving slower than they can, or simply miss the ball at the last second. To address this problem, we adjusted the % caught from any condition in which the participant caught at least one of the four balls to 100% catchable, and then

Fig. 6. (A) Estimate of a representative participant's maximum locomotor capabilities, measured in terms of the farthest distance travelled at each point in time during action trials in the Move (solid curve) and Delayed-Mixed (dashed curve) blocks. Circles correspond to conditions in which the ball was catchable on at least one of the four trials (closed circles) or uncatchable on all four trials (open circles). Circles within the solid and dashed rectangles correspond to trials in the Move and Delayed-Mixed block, respectively. (B) Data from the same participant showing % catchable as a function of time-to-spare. Each "x" corresponds to one of the 16 different conditions.

recalculated the critical value of required speed and judge-to-action ratios. The justification for this adjustment is that if a participant was able to catch the ball once out of the four repetitions per condition, then the ball was catchable and should be judged as such. Using the adjusted data, the mean ratio is very close to 1.0 in both the Move ($M = 1.06$) and Stand ($M = .98$) blocks (see Fig. 7A), and $t$-tests with a test value of 1.0 were not significant ($t(15) = 1.27, p = .22$ and $t(15) = .49, p = .63$ for the Move and Stand conditions, respectively). The fact that judgments so closely matched the adjusted catch data suggests that participants neither overestimated nor underestimated their locomotor capabilities, but they were optimistic in that their judgments were based on what could have happened in a best-case scenario rather than on what mostly likely would have happened. This is also a likely explanation for why judge-to-action ratios were significantly greater than 1.0 in Oudejans et al. (1996).

Second, regardless of whether one looks at the adjusted or unadjusted data, judge-to-action ratios in our study were not significantly closer to 1.0 in the Move condition as they were in Oudejans et al.
(1996), who reported mean ratios of 1.34 (SE = .08) and 1.18 (SE = .05) in the Stand and Move conditions, respectively. In Section 1, we pointed out that the difference could also be attributed to participants running at less than full speed in the Move condition because they knew that vision would be occluded before they could catch the ball anyway. In our experiment, this problem was avoided by randomly interleaving judge and action trials so that participant would not know which trial type was next, thereby ensuring that the difference in distance traveled during the first 1 s of judge trials and action trials in the Move block (M = .003 m, SE = .008 m) was negligible (t(15) = .39, p = .70). The fact that judge-to-action ratios were close to 1.0 in both the Move and Stand conditions is consistent with our alternative explanation.

The accuracy of judgments can also be measured in terms of absolute error, which is calculated by taking the absolute difference between each ratio and 1.0. Recall that this is the measure of accuracy for which Oudejans et al. (1996) found a significant difference between the Stand and Move conditions. As expected, no differences between the Stand (M = 0.15, SE = .02) and Move (M = 0.12, SE = .04) conditions were found when accuracy was measured in terms of absolute error (t(15) = .76, p = .48).5 Taken together, these analyses of judge-to-action ratios suggest that the difference between the Stand and Move conditions reported in Oudejans et al. may not reflect an inability of stationary actors to take their movement capabilities into account after all. This issue is further explored in Experiment 2 by blocking judge trials, and analyzing movement during the first 1 s.

Third, for the Delayed-Mixed and Delayed-Judge Blocks, the judge-to-action ratios were significantly higher than 1.0 (t(15) = 3.32, p < .01 and t(15) = 3.66, p < .01 for the Delayed-Mixed and Delayed-Judge Blocks, respectively). The apparent overestimation of locomotor capabilities cannot be due to the fact that participants were stationary in the Delayed-Mixed and Delayed-Judge Blocks. If participants overestimate their locomotor capabilities when they are not moving, then the judge-to-action ratio would also be greater than 1.0 in the Stand Block, which it was not. An analysis of movement initiation time on action trials in the Delayed-Mixed Block suggests an alternative explanation. Because the go signal was always presented 1 s after launch in the Delayed-Mixed Block, one might assume that participants would perfectly anticipate the go signal and begin moving almost immediately. In fact, the mean time after the go signal to initiate movement on action trials in the Delayed-Mixed Block was 233 ms, which was shorter than in the Move Block (M = 342 ms) but still longer than one might expect given the predictable go signal. Why did participants wait so long to begin moving in the Delayed-Mixed Block? Because judgment trials were randomly interspersed among action trials, and because the ball disappeared at the same time as the go signal, participants did not have to move at all on 50% of the trials in the Delayed-Mixed Block. (By comparison, participants moved on both judgment and action trials in the Move Block.) Consequently, participants may have hesitated before starting to move to make sure that the trial was not a judgment trial (in which case it was not necessary to move), resulting in fewer catches. If participants failed to account for such hesitation when judging catchability, and instead reported what they could have caught had they perfectly anticipated the go signal, this would explain why judge-to-action ratios were significantly greater than 1.0 in the Delayed-Mixed and Delayed-Judge Blocks. Further evidence in support of this interpretation comes from a marginally significant positive correlation (p = .07) between judge-to-action ratio and movement onset time in the Delayed-Mixed Block, revealing that participants who hesitated longer also tended to have higher judge-to-action ratios (see Fig. 7B). Thus, the elevated ratios in the Delayed-Mixed and Delayed-Judge blocks does not appear to be due to the fact that participants were stationary, but rather that they failed to account for hesitating when judging catchability. These issues are further explored in Experiment 2, in which judgment trials and action trials were performed in separate blocks.

To summarize, two main results from Experiment 1 cast doubt on the claim that actors need to move to know their locomotor capabilities. First, we replicated the (non-significant) difference reported by Oudejans et al. (1996) between the Stand and Move conditions in the consistency of judgments. One might attribute this result to the inability of stationary actors to know their locomotor

---

5 The values reported in the text are based on the adjusted data. No significant differences were found when the unadjusted data were used either [M = 0.20 (SE = .04) and M = 0.25 (SE = .06) in the Stand and Move conditions, respectively: (t(15) = .90, p = .38).]
capabilities. However, in a new third condition in which participants were stationary but other differences between the Stand and Move conditions were controlled for, we found that the consistency of judgments was not significantly different from that in the Move condition. Second, we failed to replicate the result that judge-to-action ratios were lower and closer to 1.0 in the Move condition, finding instead that ratios were close to 1.0 in both the Stand and Move conditions. The difference in results between the two studies suggests that stationary actors do know their locomotor capabilities, and that the lower ratios in the Move condition in Oudejans et al. can be attributed to the blocking of judge and action trials, which may have led participants to run at less than full speed during the first 1 s of trials in the Move block. This explanation is further tested in Experiment 2.

3. Experiment 2: perceiving passability through a closing gap

Experiment 2 was designed with three aims. The first aim was to further test our explanation of the lower judge-to-action ratios in the Move condition compared to the Stand condition reported by Oudejans et al. (1996). If our explanation is correct, then our failure to replicate this finding in Experiment 1 was due to the interleaving of judge trials and action trials. When judge and action trials are blocked as in Oudejans et al., then judge-to-action ratios should be lower and closer to 1.0 in the Move condition compared to the Stand condition. Further, analysis of participant movement should reveal that participants cover less distance in the first 1 s on judge trials in the Move condition compared to action trials.

The second aim was to further test our explanation of the unexpectedly high judge-to-action ratios in the Delayed conditions in Experiment 1 – i.e., that participants caught fewer balls in the Delayed condition because they hesitated before moving to determine whether the next trial was a judge trial or a catch trial. If this explanation is correct, then the judge-to-action ratio for the Delayed condition should be lower and close to 1.0 when judge and action trials are blocked.

The third aim was to rule out the possibility that participants in Experiment 1 were able to make equally consistent judgments while stationary (i.e., in the Delayed-Mixed and Delayed-Judge blocks) only because they received practice beforehand. Practice was necessary in that experiment because the task was a difficult perceptual-motor skill performed in a virtual environment. Most people require a period of adaptation when first entering a virtual environment before normal perceptual and motor performance can be regained. This is especially important for a difficult perceptual-motor skill like catching a fly ball, which requires one to learn the relevant visual information to detect and how to couple that information to changes in running speed and direction. In Experiment 2, we used a simpler, more routine task. The new task required participants to perceive the passability of a closing gap – that is, to judge whether they could pass through a gap between a pair of converging obstacles. Just as the catchability of a fly ball depends on both the trajectory of the ball and the fielder's running capabilities, the passability of a closing gap depends on the distance to the point of convergence, the time-to-closure of the gap, and the actor's locomotor capabilities. In this sense, the shrinking gap task is analogous to the fly ball task. However, because passing through a shrinking gap is a more routine task that most people can easily perform even in a virtual environment, participants need not practice the task before the experiment begins. Practice on an unrelated task that allows participants to familiarize themselves with moving through a virtual environment should be sufficient. Thus, by using the shrinking gap task, we can rule out the possibility that participants in Experiment 1 made consistent judgments while stationary only because they received practice beforehand.

3.1. Method

3.1.1. Participants

Twelve students participated in the experiment. All participants reported that they had normal or corrected-to-normal vision and no visual or motor impairments.

3.1.2. Simulated environment and procedure

Participants completed three judgment blocks (Stand, Move, and Delayed-Move), followed by two action blocks (No Delay, Delay). The order of the three judgment blocks and the two action blocks was...
counterbalanced, with the constraint that all judgment blocks were completed first. As in the methods section of Experiment 1, the Move Block will be described first, followed by the other blocks. The full design of Experiment 2 is summarized in Fig. 8, and a screenshot and schematic of the setup from Experiment 2 appear in Fig. 9.

![Figure 8: Design of Experiment 2 indicating type and number of trials, task, viewing time, go signal time, and remaining time-to-closure for the Move, Stand, Delayed Move, No Delay Action, and Delayed Action blocks. Viewing time refers to the amount of time that the cylinders remained visible after starting to move. A viewing time of $\infty$ indicates that cylinders remained visible throughout entire trial. Go signal time refers to the time after cylinder movement onset at which the go signal (i.e., the whistle telling participants that they could start moving) was presented. Remaining time-to-closure refers to the time-to-closure at the latest point at which passability judgments could be made. TTC$_{init}$ stands for initial time-to-closure. Shading indicates how judge and action blocks were paired when judge-to-action ratios were computed.](image)

Move Block. Each trial was initiated by standing in the home position and pressing and holding the left button on the remote mouse. Two yellow cylinders (2 m high \times 0.05 m radius) appeared in front and to the side of the participant, with one cylinder on the left and the other on the right. The initial positions varied randomly on each trial between 5 and 6 m in depth and between 2.5 and 3.5 m to the side, but the cylinders were always positioned symmetrically about the midline. After a brief foreperiod, the cylinders began moving at a constant speed toward their respective final positions. When each cylinder reached its final position, the size of the gap between the cylinders was equal to the participant’s shoulder width, which was measured prior to the beginning of the experiment. The cylinders moved both inward and toward the home position, as shown in Fig. 9B. The distance in depth from the home position to the final cylinder position was manipulated as an independent variable with four levels (2.25, 3.0, 3.75, 4.5 m). Cylinder speed was determined by the initial time-to-closure, which was defined as the amount of time it took for the cylinders to reach their final positions. Initial time-to-closure was also manipulated as an independent variable (2.25, 2.5, 2.75, and 3.0 s).

In the Move Block, the go signal was presented at the same time that the cylinders started moving. Participants were instructed that they could walk as quickly as they wanted, but that they could not run. On each trial, the cylinders disappeared 1 s after they started moving, and participants made a yes/no judgment about whether they could have safely passed through the gap without running and without rotating their shoulders. Participants entered their responses by pressing one of two buttons on the remote mouse. There were four repetitions per condition for a total of 64 trials in the Move block (4 initial TTCs \times 4 initial distances to the intersection point \times 4 repetitions).

Fig. 9. (A) Screenshot illustrating virtual environment used in Experiment 2. (B) Schematic diagram of setup used in Experiment 2.

Stand Block. The Stand Block was similar to the Move Block with the exception that participants were instructed to stand at the home location and judge whether they could have passed through the gap if they started moving immediately. The initial TTC and distance values were the same as those in the Move Block, and there were 64 trials.

Delayed-Move Block. The Delayed-Move Block was similar to the Move Block with the following exceptions: (1) the go signal was presented 1 s after the cylinders started moving (i.e., at the same time that the cylinders disappeared) and (2) the range of final positions was shifted toward the home location by 1.5 m such that the distance in depth from the home position to the final cylinder position was 0.75, 1.5, 2.25, or 3.0 m.

No Delay Action Block. The same initial TTC and initial distance values that were used in the Move and Stand blocks were used in the No Delay Action block. The go signal was presented simultaneously with the onset of cylinder movement. The only difference was that the cylinders remained visible and participants were instructed to try to pass through the gap if they could. They were instructed that they could walk as quickly as they wanted, but that they could not run and they could not rotate their shoulders to squeeze through the gap. The trial ended in one of three ways: the participant successfully passed through the gap before it closed, the participant collided with one of the cylinders, or the gap closed before the participant passed through the gap. A collision with a cylinder was counted when the distance between the participant’s cyclopean eye and the edge of one of the cylinders was less than one-half of the participant’s shoulder width.

Delay Action Block. The Delay Action block was similar to the No Delay Action block with the following exceptions. First, the go signal was presented 1 s after the cylinders started to move. Second, the range of initial distances was decreased by 1.5 m, as in the Delayed-Move block.

Practice. Prior to starting the experiment, participants completed a short practice block designed to familiarize them with moving in the virtual environment. The task that participants performed in the practice block was the fly ball catching task used in Experiment 1. Participants did not make catch-ability judgments during the practice session, but rather tried to catch the fly ball. The conditions were relatively easy such that participants could reach the landing location in time to catch the ball without moving quickly.

3.2. Results and discussion

3.2.1. Overall performance

As in Experiment 1, the percentage of successful trials (hereafter, % success) varied widely across participants [57.9% (SD = 15.5%) for the No Delay Action Block and 50.9% (SD = 10.6%) for the Delay Action Block]. Fig. 10 shows the mean % success broken down by distance to intersection point and time-to-closure for both action blocks.

3.2.2. Consistency of judgments

As in Experiment 1, we measured the consistency of judgments by fitting a sigmoid function to the % of “yes” responses plotted as a function of the difficulty of each condition, and computing the Fisher Z value corresponding to the goodness-of-fit. As in Experiment 1, the difficulty of each condition was measured in terms of the speed required to safely pass through the gap, and the time-to-spare. For the required speed analysis (see Fig. 11A), the effect of block on Fisher Z values was significant ($F(2,22) = 4.17, p < .05, \eta^2 = .28$). Contrast effects revealed that the difference between the Move and Stand conditions, which did not reach significance in Experiment 1, was significant in Experiment 2 ($p < .05$). The difference between the Delayed-Move and Stand conditions was also significant ($p < .05$), replicating this result from Experiment 1 and providing additional counterevidence against the hypothesis that actors only know their locomotor capabilities when they are moving. The trend was very similar for the time-to-spare analysis (see Fig. 11B), but the effect of block did not reach significance ($F(2,22) = 2.57, p = .10, \eta^2 = .19$).

3.2.3. Judge-to-action ratio

As in Experiment 1, we used the ratio of the critical value of required speed for judgment trials to action trials as a measure of the degree to which participants overestimated or underestimated their
locomotor capabilities. For the Move and Stand conditions, the critical values were scaled to the critical value in the No Delay Action block. For the Delayed-Move condition, the critical value was scaled to the critical value in the Delay Action block. Again, the data from the action blocks were adjusted such that the % successful trials was changed to 100% if any of the four trials within that condition was successful.

The mean ratios were very close to 1.0 in both the Stand ($M = 0.99$) and Delayed Move ($M = 1.01$) conditions (see Fig. 12A). $t$-tests confirmed that the ratio did not differ significantly from 1.0 in either condition ($t(11) = 0.14$, $p = .89$ and $t(11) = .13$, $p = .90$ in the Stand and Delayed Move conditions, respectively). The mean ratio was smaller in the Move condition ($M = .91$), although it did not differ significantly from 1.0 ($t(11) = 2.01$, $p = .07$). Nor did the main effect of block reach significance ($F(2,22) = 1.86$, $p = .25$).

Although the difference between the Move and other two conditions did not reach significance, the direction of the effect is consistent with our explanation of the smaller ratios in the Move condition reported by Oudejans et al. (1996). To further investigate, we compared the mean distance covered in during the first 1 s (at 250 ms intervals) of judge trials in the Move condition with the same distance for action trials in the No Delay action condition. As expected, participants covered less distance on judge trials compared to action trials, with significant differences emerging at 0.75 s ($t(11) = 2.76$, $p = .016$).
At the level of individual participants, we found a significant negative correlation ($r = -0.61$, $p < 0.05$) between the difference in distance that participants covered in the first 1 s of trials in the Move and No Delay block, and the judge-to-action ratio (see Fig. 12B). In other words, participants who covered less distance on trials in the Move block relative to trials in the No Delay block tended to have judge-to-action ratios lower than 1.0; those who covered the same distance on both blocks tended to have judge-to-action ratios close to 1.0. Interestingly, one participant covered more distance on trials in the Move block compared to No Delay block. This participant's judge-to-action ratio was 1.22, which was the highest judge-to-action ratio by a sizeable margin. With this participant excluded from the analysis, the $t$-test on the difference between judge-to-action ratios in the Move condition and 1.0 reached significance ($t(10) = 3.18$, $p < 0.05$).

When accuracy was measured in terms of the absolute difference between each unadjusted ratio and 1.0 [i.e., the same dependent measure used by Oudejans et al. (1996)], a significant difference

Fig. 11. Fisher Z values for each judgment block indicating the goodness-of-fit of the best-fitting curve for % yes versus required speed (A) and % yes versus time-to-spare (B).
between the Stand and Move conditions was found ($t(11) = 2.23, p < .05$). As in Oudejans et al., the absolute difference from 1.0 was greater in the Stand condition ($M = 0.193; SE = 0.046$) than it was in the Move condition ($M = 0.101; SE = 0.035$). Both values are slightly lower than the corresponding values in Oudejans et al. ($M = .37$ and $SE = 0.06$ in the Stand condition and $M = .20$ and $SE = 0.04$ in the Move condition). However, in both studies, errors in the Move condition are significantly smaller by a factor of slightly less than 2. Thus, when the design was the same and when the data were analyzed in the same way, the basic findings of Oudejans et al. were replicated.

Taken together, the results of Experiments 1 and 2 strongly favor a reinterpretation of the difference in judge-to-action ratios in the Move and Stand conditions reported by Oudejans et al. When judge and action trials were blocked in Experiment 2, participants tended to cover less distance in the first 1 s of judge trials compared to corresponding action trials, resulting in judge-to-action ratios that were lower and closer to 1.0. When this problem was avoided by randomly interleaving judge and action trials in Experiment 1, judge-to-action ratios were close to 1.0 in both the Move and Stand

---

**Fig. 12.** (A) Mean judge-to-action ratios for each block in Experiment 2. (B) Scatterplot showing judge-to-action ratio as a function of the difference in mean distance travelled in the first 1 s of trials in the Move and No Delay Action Blocks. Each point corresponds to an individual participant.
conditions. Thus, we conclude that the effect reported by Oudejans et al. should be attributed to differences in behavior on judge and action trials that resulted from blocking by trial type, rather than to differences in the availability of information while moving. In other words, their effect should not be interpreted as evidence that stationary actors do not know their locomotor capabilities.

The second aim of Experiment 2 was to further test our explanation of the high judge-to-action ratios in the Delayed-Move condition in Experiment 1, which we attributed to participants hesitating on action trials in the Delayed-Mixed block. As expected, judge-to-action ratios in the Delay condition were very close to 1.0 when judge trials were blocked in Experiment 2 (see Fig. 12A).

Third, the results of Experiment 2 allow us to rule out the possibility that consistent judgments while stationary depend on having recently performed the task. Because judge trials and action trials were interspersed in Experiment 1, participants judged catchableness after having recently caught fly balls. By comparison, participants in Oudejans et al. (1996) completed all judgment trials before performing any action trials. In Experiment 2, we used a similar design, with participants completing three judgment blocks (Stand, Move, and Delayed-Move) prior to performing actions trials (i.e., trials in which they actually attempted to pass through the gap). Thus, the high consistency of judgments by stationary actors cannot be attributed to having recently performed the task.

4. Experiment 3: perceiving passability through a closing gap (interleaved trials)

In Experiment 2, judge-to-action ratios were lower in the Move condition than they were in the Stand condition, as in Oudejans et al. We attributed this difference to the blocking of judge and action trials, which resulted in participants traveling less distance during the first 1 s of judge trials compared to action trials. Converging evidence supporting this interpretation comes from Experiment 1, in which judge and action trials were randomly interleaved rather than blocked, and judge-to-action ratios in both the Move and Stand conditions were not significantly different from 1.0. However, such comparisons across experiments are complicated by the fact that different tasks were used in Experiments 1 and 2. Therefore, we ran a third experiment in which judge trials in the Move condition and action trials were randomly interleaved while subjects performed the shrinking gap task. If our interpretation of Experiments 1 and 2 is correct, then unlike Experiment 2 in which judge-to-action ratios were below 1.0, ratios should not significantly differ from 1.0.

4.1. Method

4.1.1. Participants

Twelve students, none of whom were subjects in Experiments 1 or 2, participated in Experiment 3. All participants reported that they had normal or corrected-to-normal vision and no visual or motor impairments.

4.1.2. Simulated environment and procedure

The simulated environment and task were identical to those used in the Move Block and No Delay Action Block in Experiment 2. The only difference was that trials were randomly interleaved as they were in Experiment 1 rather than blocked as they were in Experiment 2. The experiment was broken down into two identical blocks between which subjects took a short break. Both blocks consisted of 64 trials (4 initial TTCs × 4 initial distances to the intersection point × 2 repetitions × 2 trial types) for a total of 128 trials per session. To ensure that subjects were comfortable moving in the virtual environment before starting the experiment, they completed the same fly ball practice block that was used prior to Experiment 2.

4.2. Results and discussion

4.2.1. Distance traveled during the first 1 s

Recall that the purpose of randomly interleaving judge trials and action trials was to eliminate the difference in distance travelled during the first 1 s as a cause of the lower judge-to-action ratios in the
Move condition. To ensure that randomly interleaving trials had the desired effect, we compared the distance that subjects covered during the first 1 s of judge trials and action trials. As in Experiment 1 when trials were also randomly interleaved, and unlike Experiment 2 when trials were blocked, the difference in distance covered during the first 1 s of judge trials and action trials ($M = .0006$ m, SE = .006 m) was insignificant ($t(11) = .11, p = .92$).

4.2.2. Judge-to-action ratio

As in the first two experiments, we adjusted the data from action trials by changing the % of successful trials to 100% for any condition in which the subject successfully passed through the gap in at least one of the four trials. We then fit a sigmoid function to the % of “yes” responses on judgment trials (or the % of successful trials on action trials) plotted as a function of the required speed for each condition. The point at which the sigmoid curve crossed 50% was used as the critical value. We then calculated the judge-to-action ratios by dividing the critical value for judgment trials by the critical value for action trials.

As predicted, judge-to-action ratios were not significantly different from 1.0 ($M = 1.02$, SE = .04, $t(11) = .54, p = .60$). This contrasts with Experiment 2, in which judge-to-action ratios were less than 1.0 ($M = .91$). A one-tailed, independent samples $t$-test confirmed that the difference in judge-to-action ratios in Experiments 2 and 3 was statistically significant ($t(22) = 1.88, p < .05$). Because the only difference between the two experiments was that trials were blocked in Experiment 2 and randomly interleaved in Experiment 3, we can eliminate the nature of the task as an explanation for the lower ratios in the Move condition of Experiment 2. We conclude that the difference between the Move and Stand conditions observed in Experiment 2 of the present study and in Oudejans et al. was due to the difference in distance traveled during the first 1 s of judge trials in the Move condition and action trials.

5. General discussion

The aim of this study was to reconsider the claim that actors determine their locomotor capabilities anew each time they perform an action, and that moving is a prerequisite to knowing one’s locomotor capabilities. The main piece of evidence that Oudejans et al. (1996) presented to support these claims was that participants made more accurate judgments when they were allowed to move for 1 s compared to when they were stationary. We replicated this finding in Experiment 2, but argue that it ought to be reinterpreted in light of the other results presented in this study.

The apparent difference in accuracy between the Stand and Move conditions was primarily due to the fact that participants judged uncatchable balls as catchable in both conditions, but less so in the Move condition. There are two aspects of this finding that require explanation: (1) the overall tendency to judge uncatchable balls as catchable in both conditions, and (2) the fact that this tendency was weaker in the Move condition. The first of the two findings can be attributed to the difference between what one is capable of doing and what actually happens on any given trial. An actor may fail to catch a fly ball that he or she is capable of catching for a variety of reasons, such as getting a bad jump, running at less than maximum speed, or failing to put the hand in the right position as the ball passes within reach. If participants sometimes fail to catch balls that are catchable, then what appears to be a bias to overestimate one’s capabilities may actually reflect a preference to base one’s response on what would happen in the best-case scenario. Because our experimental design allowed for multiple repetitions of the same condition, we were able to better estimate what each participant was capable of catching or passing through by identifying conditions in which at least one out of four trials were successful. When this adjusted estimate of each participant’s actual capabilities was used, judge-to-action ratios in Experiments 1 and 3 were not significantly different from 1.0 in either the Stand or Move conditions. Thus, what appeared to be a general tendency to overestimate catchableness is more likely a consequence of the difference between what is maximally possible under the best-case scenario and what actually happens on any given trial. When the data are analyzed in a way that takes this into account, it is clear that participants neither overestimated nor underestimated their locomotor capabilities.
The second component of this finding concerns the difference between the Move and Stand conditions in terms of the degree of apparent overestimation. The alternative explanation that was offered in the Introduction was that participants ran at less than full speed during the first 1 s of trials in the Move block because they knew that the trial would terminate after 1 s. When this problem was circumvented in Experiments 1 and 3 by randomly interleaving judge and action trials during the Move block, judge-to-action ratios were not significantly different from 1.0. To rule out the possibility that some other difference between our study and Oudejans et al. led to the difference in findings, judge trials and action trials were blocked in Experiment 2, as in Oudejans et al. (1996). Under these conditions, judge-to-action ratios were lower and closer to 1.0 in the Move condition compared to the Stand condition. Furthermore, analyses of participant movement during the first 1 s confirmed that participants covered less distance on judgment trials in the Move block compared to action trials in the corresponding (No Delay) action block. Lastly, analyses of individual participants revealed a consistent relation between judge-to-action ratios and the difference in distance covered during the first 1 s of judge trials in the Move block and action trials. That is, participants who covered less distance on judge trials compared to action trials had judge-to-action ratios lower than 1.0. Participants who covered the same distance had judge-to-action ratios close to 1.0. Taken together, our findings suggest that stationary actors can make accurate judgments, and that the difference observed by Oudejans et al. was a consequence of different behavior in their Move and Catch blocks, rather than an inability of stationary actors to know their locomotor capabilities.

Oudejans et al. (1996) also analyzed a second dependent variable that reflected the consistency of judgments. They found that the consistency of judgments was higher when judgments were made while moving. Because the difference did not reach significance, they did not draw any conclusions based on this finding. However, we also considered this measure in the interest of thoroughness. In the Introduction, we suggested that the difference between the Move and Stand conditions in the consistency of judgments could also be attributed to the fact that the event of running to catch a fly ball was allowed to unfold in the normal way for one additional second in the Move condition. When we controlled for this difference by instructing participants to remain at the home location and judge catchableness or passability if they started moving after 1 s, judgments were equally consistent with those in the Move condition, and significantly more consistent than those in the Stand condition. Thus, our findings do not support the hypothesis that actors only know their locomotor capabilities when they are moving, and suggest that differences in consistency between the Move and Stand conditions were due to a confound rather than the manipulation of movement.

5.1. Methodological differences between Oudejans et al. (1996) and the present study

One difference between Experiment 1 of the present study and Oudejans et al. (1996) is that there was less variability in flight time and landing location in the present study compared in Oudejans et al., in which variability was necessarily introduced by the use of a pitching machine. The skeptical reader might wonder whether participants in our Experiment 1 learned a simple heuristic for discriminating between catchable and uncatchable balls based on the optical trajectory of the ball. The counterargument is that because initial ball position was randomized on each trial, the shape of the trajectory on each repetition of the same condition varied widely. Thus, in Experiment 1, for any given combination of landing location and flight time, the ball could have followed a “pop fly” trajectory or a “line drive” trajectory depending on the randomly selected initial position. This made it impossible to reliably discriminate between catchable and uncatchable balls by using any sort of simple heuristic. The same applies to Experiment 2, in which the initial position of the cylinders was randomized.

A second difference between Oudejans et al. (1996) and the present study is that our study was conducted in a virtual environment. One might wonder if there are any differences between real and virtual environments that might explain our findings. Several studies have reported that size and distance are underestimated in virtual environments (Knapp & Loomis, 2004; Thompson et al., 2004). However, the tasks used in those studies involved verbal judgment and open-loop visually directed actions (e.g., blind walking, throwing). Such perceptual biases may not influence continuously controlled visually guided actions, such as catching. Indeed, we found no evidence that participants consistently overran fly balls, as one might expect if the compression of visual space affected behavior.
On judgment trials, a compression of visual space could bias participants to judge uncatchable balls as
catchable, or unpassable gaps as passable. However, there was no evidence in either experiment that
participants tended to overestimate their locomotor capabilities as one would expect if visual space
was compressed. Judge-to-action ratios were consistently close to 1.0 in the Move and Stand blocks
of both experiments. Although there was a small bias to overestimate locomotor capabilities in the
Delayed-Mixed and Delayed-Judge Blocks of Experiment 1, an alternative explanation for this bias
was suggested in Section 2.2 of Experiment 1. The results of Experiment 2 provide strong support
for this alternative explanation. The absence of an overall overestimation bias is consistent with recent
research suggesting that the compression of visual space is nearly eliminated with several minutes of
experience in the virtual environment (Mohler, Creem-Regeh, & Thompson, 2006; Waller &
Richardson, 2008).

Another potential difference between real and virtual environments is that participants are unli-
ely to move as quickly in virtual environments due to greater postural instability or fear of colliding
with objects in the room. Although participants moved cautiously at the very beginning of the first
session, we found that most participants moved naturally by the end of the practice blocks. More
importantly, the fact that judge-to-action ratios were so close to 1.0 suggests that participants were
able to successfully take into account the degree to which their locomotor capabilities were altered
in the virtual environment. This is not surprising, given the variety of factors (e.g., poor traction, fati-
gue, and carrying a heavy load) that can alter one’s locomotor capabilities and to which one must
adapt in the real world. Thus, the ability to adapt to the virtual environments that was exhibited by
participants in the present study reflects a well-developed ability to adapt to changes in the real
world.

A third potential concern is that participants may have had more difficulty picking up optical infor-
mation while moving through the virtual environment due to small movements of the HMD on the
participant’s head. In principle, this could have resulted in less accurate judgments in the Move Block
(i.e., the one condition in which participants moved on judgment trials) compared to the other blocks.
However, the magnitude of the difference between the Move and Stand Blocks was similar to the mag-
nitude of the difference between the Move and Stand blocks in Oudejans et al. (1996). Furthermore,
this factor cannot explain why judgments were significantly more consistent in the Delayed-Mixed
and Delayed-Judge Blocks compared to the Stand Blocks, which is one of the critical findings of this
experiment.

To summarize, there is no reason to believe that catchableness judgments in the Move, Delayed-
Mixed, and Delayed-Judge Blocks were equally consistent simply because the experiment was con-
ducted in a virtual environment. It is also worth pointing out that there were some clear advantages
to running this experiment in the virtual environment, such as having more precise control over the
ball trajectories.

5.2. Related research on road crossing behavior

In reevaluating the claim that movement is needed to perceive action-scaled affordances, our focus
was primarily on the results of Oudejans et al.’s (1996) study on perceiving catchableness. However, it
is worth mentioning that Oudejans and colleagues published a companion study (Oudejans, Michaels,
Bakker, et al., 1996) on road crossing behavior in which the authors drew the same conclusions. This
was a naturalistic study in which pedestrians were videotaped as they crossed an actual intersection.
Analyses focused on crossing behavior as a function of the amount of time remaining before the next
vehicle arrives at the crosswalk. The main finding was that participants left a smaller temporal gap
when crossing was initiated from a walk compared to when crossing was initiated from a standing
position. The difference was greater than the difference in the amount of time that it takes for standing
and moving pedestrians to cross the road. So the results cannot be attributed to simply accounting for
the additional time it takes to accelerate from a stationary position. Instead, the findings were attrib-
uted to being more conservative when crossing from a standing position, presumably because station-
ary actors do not know their movement capabilities as well. However, there are other possible
explanations for the finding. For example, it is possible participants were reluctant to stop once they
were moving because of the energetic costs associated with stopping and restarting. This could have

Please cite this article in press as: Fajen, B. R., et al. Reconsidering the role of movement in perceiving action-
biased participants to be more risky. Thus, although there was no confound similar to Oudejans et al.'s fly ball study, the results do not provide conclusive support for the claim that pedestrians can more accurately perceive crossability when moving.

5.3. Theoretical implications

One of the challenges of the theory of affordances is to explain how people perceive affordances without appealing to knowledge of body dimensions and action capabilities stored in memory. For certain affordances that depend on body dimensions, such as the passability of an aperture, the problem is solved by the availability of body-scaled information in the optic array (Mark, 1987; Warren, 1984; Warren & Whang, 1987). In these cases, knowledge of body dimensions is superfluous because the optic array contains eyeheight-scaled information about the relevant dimensions of the environment. Because other body dimensions (e.g., shoulder width, leg length) are generally in fixed proportion to eyeheight, relevant dimensions of the environment (e.g., aperture width and riser height) are automatically scaled in intrinsic units corresponding to the relevant dimensions of the body. By simply picking up such information, actors can directly perceive whether a gap is passable or a surface is climbable.

Oudejans et al. (1996) sought an analogous solution to explain how one could perceive action-scaled affordances, such as catchableness, without appealing to stored knowledge of movement capabilities. We concur that action-scaled affordance perception does not involve a memory-based representation of movement capabilities that exists independent of perception, as this would imply that the required movement (e.g., required running speed) is perceived in extrinsic units unrelated to the actor’s movement capabilities. However, the solution proposed by Oudejans et al., which relies on information in optic flow, can only explain how actors perceive catchableness under two conditions: (1) when the fielder has reached a sustainable speed that is sufficient to arrive at the landing location before the ball, and (2) when the fielder has reached her maximum speed and that speed is not sufficient. In the present study, we found that actors can perceive catchableness with equal accuracy regardless of whether they were moving or stationary. This should not come as a surprise, as there are a variety of other kinds of action-scaled affordances for which the use of on-the-fly information is not even a possibility. For example, ballistic actions such as jumping and throwing are by definition completed before information resulting from the movement is available. Affordances for ballistic actions are nonetheless accurately perceived, as demonstrated by Pepping and Li (2000), who found that the maximum height to which a person can reach by jumping is perceived by stationary observers within 1–2 cm. The accuracy with which such affordances are perceived provides additional evidence that on-the-fly information is not necessary.

Even for affordances related to continuously controlled actions, such as those used in the current study, it is not enough to know whether an action is within or beyond one’s limits only some of the time. Navigation through complex, dynamic environments requires actors to choose modes of action (e.g., to go this way or that way, to go now or wait until later, to run or walk) in ways that take their locomotor capabilities into account. Oftentimes, such choices must be made before movement is initiated. Even after an action is selected, actors must continue to make on-line adjustments to ensure that the task can be performed without having to exceed their locomotor capabilities. Such behavior has been demonstrated in the context of a visually guided braking task by showing that actors time the initiation of braking and continually adjust deceleration to ensure that the deceleration needed to stop never exceeds the maximum possible deceleration (Fajen, 2005a; Fajen, 2005c). Thus, actors must know their locomotor capabilities both when choosing among different modes of action and during the on-line perceptual guidance of action (Fajen, 2005b; Fajen, 2007a).

What does it mean for actors to know their locomotor capabilities? We believe that knowing locomotor capabilities is best captured, not in terms of knowledge stored in memory, but by the concept of calibration. Through experience moving through the world, actors learn how their biomechanical activity relates to the patterns of optic flow that accompany movement through the world. Every time a movement is made, information is revealed that is useful, not only for on-line control, but also for maintaining perceptual-motor calibration. When locomotor capabilities change, the mapping from biomechanical activity to optic flow changes, and actors must re-learn how their movements influence...
optical flow; that is, they must recalibrate. Numerous studies have demonstrated that locomotion is quickly recalibrated when participants walk while being exposed to altered patterns of global optic flow (Anstis, 1995; Bruggeman, Zosh, & Warren, 2007; Durgin & Pelah, 1999; Durgin et al., 2005; Mohler et al., 2007; Mulavara et al., 2005; Proffitt, Stefanucci, Banton, & Epstein, 2003; Rieser, Pick, Ashmead, & Garing, 1995). If actors can learn how their movements influence global optic flow, then they may also be able to learn their capability to bring about a desired change in a particular optical variable (e.g., a zero optical acceleration of a fly ball) by moving. Thus, a properly calibrated actor (1) knows how to move to bring about changes in the relevant information variables that correspond to successful outcomes, and (2) can perceive affordances by knowing whether or not the desired changes can be brought about. Our current research is focused on the problem of how actors learn the relation between their movements and changes in the relevant informational variables.

To summarize, the present study provides both theoretical and empirical reasons to doubt the conclusion that actors need to move to distinguish between actions that are within and beyond their locomotor capabilities. The findings are also inconsistent with the claims that actors rediscover their locomotor capabilities anew each time they perform an action, and that information in optic flow is sufficient to explain how actors perceive action-scaled affordances. We agree with Oudejans et al. (1996) that actors do not rely on knowledge of locomotor capabilities stored in memory, and propose instead that calibration allows actors to learn how to bring about desired patterns of optic flow.

Acknowledgements

This research was supported by a grant from the National Science Foundation (BCS 0545141). The authors thank Jeremy Kauffman, Cameron Fischer, and Sean Sullivan for programming the virtual environments.

References


