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ORIGINAL ARTICLE

What you see and what you are told: an action-specific effect that is unaffected by explicit feedback

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Abstract A critical question for theories of spatial vision concerns the nature of the inputs to perception. The action-specific account asserts that information related to action, specifically a perceiver's ability to perform the intended action, is one of these sources of information. This claim challenges assumptions about the mind in general and perception in particular, and not surprisingly, has been met with much resistance. Alternative explanations include that these effects are due to response bias, rather than genuine differences in perception. Using a paradigm in which ease to block a ball impacts estimated speed of the ball, participants were given explicit feedback about their perceptual judgements to test the response bias alternative. Despite the feedback, the action-specific effect still persisted, thus ruling out a response-bias interpretation. Coupled with other research ruling out additional alternative explanations, the current findings offer an important step towards the claim that a person's ability to act truly influences spatial perception.

Keywords Action-specific perception · Spatial perception · Response bias · Feedback

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Introduction

The action-specific account of perception asserts that factors such as energetic potential, body size, and other action-related factors bias perception of spatial layout such as distance, slant, size, and speed (Proffitt & Linkenauger, 2013; Witt, 2011, 2016c). For example, players who are batting better than other players during a softball game rate the ball as being larger (Gray, 2013; Witt & Proffitt, 2005). Similarly, golfers, archers, swimmers, and those trained in parkour see visual distortions as a function of their abilities and performance (Lee, Lee, Carello, & Turvey, 2012; Taylor, Witt, & Sugovic, 2011; Witt, Linkenauger, Bakdash, & Proffitt, 2008; Witt, Schuck, & Taylor, 2011). From the perspective of athletes, these findings are not surprising. Media coverage of various sports is filled with quotes from elite athletes claiming that the baseball looked as big as a grapefruit, the basketball hoop as big as a peach basket, and the tennis ball as if it were moving in slow motion. But from the perspective of vision science, these findings challenge several core assumptions about spatial perception.

Many theories of spatial perception do not consider action to be a source of information that can influence, or bias, what perceivers see. Action-specific effects challenge this assumption by showing that a person's ability to act influences perceptual judgments. Rather than modify current theories to account for the role of action, some researchers have questioned whether action-specific effects reflect genuine differences in perception (Durgin et al., 2009; Firestone & Scholl, 2016; Loomis, 2016; Woods, Philbeck, & Danoff, 2009; for review, see Philbeck & Witt, 2015). If a person's ability to act influences judgments, rather than perception itself, a person's ability to act should not be considered a source of information for spatial perception. As an internal state, perception cannot be measured directly, leaving the open possibility that action-specific effects are due to response biases or other influences on the judgments rather than on perception itself. Resolving this discrepancy is critical to understanding how spatial vision works because it speaks to whether prior assumptions can be retained or whether action should be considered as a source of information for spatial vision.

To address this question, we drew from the literature on crossmodal perception, which has had to wrangle with the same issue, namely whether information from one modality can genuinely influence perception within another modality, rather than merely influencing the judgments. Much like action-specific perception, crossmodal research has many skeptics that suggest the effects are nothing more than response biases (Choe, Welch, Gilford, & Joula, 1975; Grove, Ashton, Kawachi, & Sakurai, 2012; Lippert, Logothetis, & Kayser, 2007; Odgaard, Arieh, & Marks, 2003). Because of this controversy, many experimental techniques have been developed and employed by crossmodal researchers to eliminate response bias. One such technique is to provide feedback to participants regarding the accuracy of their perceptual judgments (Odgaard, Arieh, & Marks, 2009).

Why might feedback be an effective way to differentiate between perceptual and response-based effects? To answer this question, consider the various ways that action-specific effects could arise. These will be considered in the context of the Pong task which was used in the current experiments. In this task, participants use a joystick to control a virtual paddle in an attempt to block a moving ball (similar to the classic computer game Pong). The size of the paddle is manipulated from trial to trial. This impacts ease because the size of the paddle determines the likelihood of successfully blocking the ball. After each attempt, participants estimate the speed of the ball. The typical results are that the balls are estimated as moving faster when the paddle is smaller and less effective at blocking the ball than when the paddle is big and more effective at blocking the ball (Witt & Sugovic, 2010, 2012). This difference in estimated speed as a function of the ease to block the ball will be referred to as the paddle effect. Follow-up studies provide support for the claim that the paddle effect is an action-specific effect, and is not due other factors such as visual differences across paddle sizes. For example, in one experiment, participants watched as a computer controlled the paddle in one block of trials and then they controlled the paddle in the second block. They made speed judgments in both blocks, but these judgments were only influenced by paddle size in the block for which the participants acted and not in the block for which they watched the computer (Witt, Sugovic, & Taylor, 2012, Experiment 3). This result substantiates the idea that the paddle effect is an action-specific effect.

A key theoretical question concerns the reason for the difference in speed judgments across the paddle sizes. According to a simplistic response bias explanation, participants are able to decipher the purpose of the experiment and adjust their responses to comply with these demand characteristics. This explanation has been put forth as a way to explain other kinds of action-specific effects such as the effect of wearing a backpack on estimated hill slant (Durgin et al. 2009). In the Pong task, participants might infer that the purpose of the study is that when the paddle is small, they are supposed to estimate the ball as moving faster than when the paddle is big. Or, alternatively, participants might infer that the purpose of the study is that when they miss the ball, they are supposed to estimate the ball as moving faster than when they successfully block the ball. If participants alter their judgments based on either of these inferences about how they are supposed to respond, this would lead to the same pattern of results as has been obtained.

Assuming that this response bias explanation is correct, consider the impact of providing feedback regarding the accuracy of speed judgments. Such feedback would create the expectation that participants should respond as accurately as possible. This expectation would conflict with any inferences made about how participants are supposed to respond related to paddle size or blocking success. To the extent that explicit, salient feedback creates an expectation that overrides conflicting inferences, explicit feedback should successfully eliminate differences in estimated ball speed across paddle sizes due to response biases.

Response biases are not always so simplistic, however. One could imagine response biases that are less intentional. Indeed, Firestone and Scholl (2016) make this distinction explicit by considering response bias separate from judgment-based effects. They state, “whereas judgments of various visual qualities are often sincerely held even when they are subject to top-down influence…, other sorts of biases may reflect more active modulation of responses by participants—such that [response bias] is conceptually distinct from [effects due to judgments]” (p. 10). Effects due to judgments occur in situations for which participants respond based on their thoughts, beliefs, and/or conclusions about the properties of an object, rather than on how the object looks. For example, they state “subjects may have simply concluded that the target must have been farther away than it looked” (p. 9) and that the results suggest “that the original results reflected what subjects thought about the distance rather than how the distance truly looked” (p. 9).

How might explicit feedback affect action-specific effects that are due to judgments (as opposed to active modulation of responses)? If perceivers genuinely see the ball as moving the same speed regardless of paddle size, but instead of reporting on their perception make judgments based on thoughts, inferences, or conclusions, feedback should also serve to eliminate any differences in judgments across paddle sizes. Specifically, repeated feedback that "slow" responses were incorrect when playing with the big paddle would help participants reach the conclusion that the ball was not moving as slow as they might have inferred. At the same time, repeated feedback that "fast" responses were incorrect when playing with the small paddle would also have implications for inferences or conclusions made about ball speed. Thus, to the extent to which differences due to judgments are the result of using sources of information other than how the target looks, explicit feedback will provide another source of information from which these inferences could be drawn. The use of the information provided by explicit feedback would then eradicate the differences in estimated speed across paddle sizes.

A third explanation for the differences in estimated ball speed across paddle sizes is that perceivers genuinely see the ball as moving faster when they play with the small paddle compared with the big paddle. While it is clear that feedback could minimize differences due to response bias and judgment-based effects, it is also necessary to consider whether feedback should impact a genuine perceptual effect as well. Feedback has been shown to lead to better perceptual attunement (Biederman & Shiffrar, 1987), and may also facilitate reweighting of visual information (van der Kamp, Withagen, & de Witt, 2013). Despite the findings that feedback can affect perception, it is highly unlikely to have an impact on perception of ball speed in our studies for two reasons. First, many of the effects of feedback on perceptual learning require sleep between sessions to facilitate learning (Karni, Tanne, Rubenstein, Askenasy, 1994), and our studies took place during a 30-min session on a single day. Second, perceptual learning resulting from feedback requires extensive training. In one study, visual perceptual learning to perceive the offset direction of a Chevron required training with 400–800 trials per session and multiple sessions, whereas perceptual learning was not observed with only 160 trials per session (Aberg, Tartaglia, & Herzog, 2009). The Pong task involves less than 300 trials total. As such, there is not sufficient exposure to training for feedback to lead to perceptual learning. This does not mean that feedback could not have effects in the Pong task, given the right circumstances, but rather that feedback is highly unlikely to produce any effects due to perceptual learning in the present experiments.

Thus, we have clear predictions of the effects of feedback. If feedback eliminates the differences in estimated speed across paddle size, the evidence would favor a response bias or judgment-based account. In contrast, if feedback has no impact on differences in estimated ball speed, the evidence would be consistent with a perceptual explanation. It is also possible that both judgment-based processes and perceptual effects are involved, in which case feedback should reduce differences in estimated ball speed and the extent of this reduction can speak to the relative contribution of perceptual and judgment-based processes.

Experiment 1

Participants completed the Pong task in three phases. The first phase established a baseline effect. In the second phase, participants received explicit feedback about their speed judgments. It is possible that feedback could diminish the measured magnitude of an effect, even if the effect were genuinely perceptual. For example, in the case of the sound-flash illusion for which a single flash is perceived as two flashes when accompanied by two beeps, feedback reduced the measured magnitude of the illusion (Rosenthal et al. 2009). However, when feedback was removed, the measured illusion re-emerged. Many participants indicated that even though they responded that there was only one flash, they still had perceived two flashes. This indicates that the feedback created a response bias to respond that there was only one flash despite the perception of two flashes. If the presence of feedback diminishes the measure of an effect, it is important to examine the effect after feedback is removed. If the effect is genuinely perceptual but is not apparent when feedback is given, the effect should re-emerge once feedback is removed. Thus, in the third phase, feedback was withheld.

Method

Participants and design

Because the prediction of a perceptual account is that feedback should have little-to-no effect, it was important to conduct a power analysis to ensure that a non-significant interaction would not be due to lack of power. Across all published studies with this task, there are three reports of a significant interaction with paddle size and another factor (Witt & Sugovic, 2012; Witt et al. 2012). The mean effect size (η_p^2) for these interactions was 0.37. A power analysis reveals that 14 observers are needed to achieve at least 95% power. We stopped data collection on a specific date for which we could ensure that we had at least the desired number of participants. Twenty-four students (12 females) participated in the experiment for course credit. All gave informed consent, and the protocol was approved by Colorado State University's institutional review board. There were three test phases in the experiment (pre-feedback, feedback, and post-feedback), and all participants were exposed to every phase in that order.

Fig. 1 Illustration of the display at the beginning of a test trial. The paddle is delineated by its *top* and *bottom borders*

Stimuli and apparatus

Stimuli were presented on a computer screen (19″, resolution was 1024×768 pixels, refresh rate was 60 Hz) with a black background. The participant was seated approximately 55 cm from the screen with a joystick for responses and paddle control approximately 30 cm in front of the participant. The participant's head was not stabilized or restrained. A keyboard was directly in front of the monitor. The ball was a white circle 1 cm in diameter. The ball always moved from the left side of the screen to the right side of the screen at 1 of 6 speeds ranging from 26 to 67 cm/s. The program moved the ball in pixels/ms and so the speeds that are reported in cm/s are conversions based on the x and y displacement of the ball. While the speeds may appear irregular, in the program the step interval increases are consistent. The paddle was a white rectangle that was 0.86-cm wide and one of two heights (1.86, 9.28 cm). The paddle was on the right side of the screen overtop of a 0.86-cm wide and 18-cm tall white bar. This second bar covered the entire height of the display. As a result, the paddle height was visually specified by the distance between the two black lines that denoted the top and bottom of the paddle (see Fig. 1). This minimized the visual discrepancies between the two paddle conditions.

Procedure

After giving consent, participants were seated in front of the screen and completed two training phases. First, the participant was exposed to fast (74 cm/s) and slow (18 cm/s) anchor speeds three times each in a random order. Before each presentation, text on the screen stated "This is the slow speed" or "This is the fast speed". The ball moved left to right without any vertical displacement. Second, participants viewed the anchor speeds again, this time without text indicating their speed. After each presentation, participants identified whether the speed was fast or slow by pressing the corresponding button on the joystick. After each response was made, participants received feedback with green "correct" text or red "incorrect" text appearing on the screen. There were three trials each of the slow and fast speeds presented in random order.

Participants were instructed on how to perform the task, and were told that at some point, they would receive feedback on their responses. There were three test phases, pre-feedback, feedback, and post-feedback, completed in that order. At the start of each trial, the ball and paddle appeared on the display. The participant initiated the ball movement by pressing the trigger on the joystick. The ball moved left to right and also moved up and down, as if it was bouncing across the screen. The ball changed the vertical component of its movement when it reached the top and bottom of the screen and also at random other times. Participants could control the vertical placement of the paddle by moving the joystick back and forth. If they successfully blocked the ball, the ball stopped on the paddle, otherwise the ball continued past the paddle and past the edge of the screen. Then participants were prompted with the words "Fast or Slow?" Participants indicated whether the ball moved more like the slow anchor speed or more like the fast anchor speed that they had been trained on, by pressing the corresponding button on the joystick.

The only difference between the three test phases was what happened after participants made each speed judgment. During the pre-feedback and post-feedback phases, no feedback was given, and after each trial, there was a 1000 ms delay before the next trial began. During the feedback phase, feedback was given only after incorrect responses. Of the six test speeds, the three slowest speeds (26.2, 33.5 and 41.5 cm/s) were the speeds that were more like the slow anchor speed, and the three fastest speeds (50.0, 58.7, and 67.5 cm/s) were the speeds that were more like the fast speed. When participants incorrectly categorized a speed, such as saying one of the three slowest speeds was more like the fast speed, red text indicating "incorrect" appeared on the screen along with a buzzing sound (the buzz.wav file found in Windows). Participants only received feedback when they were incorrect, and received feedback each time they were incorrect during this phase.

Each test phase consisted of 4 blocks of 24 trials (2 paddle sizes × 6 speeds × 2 repetitions) for a total of 96 trials per phase. Order within block was randomized.

Results and discussion

As intended, participants' ball blocking performance was worse when using the small paddle (M = 54.8% balls

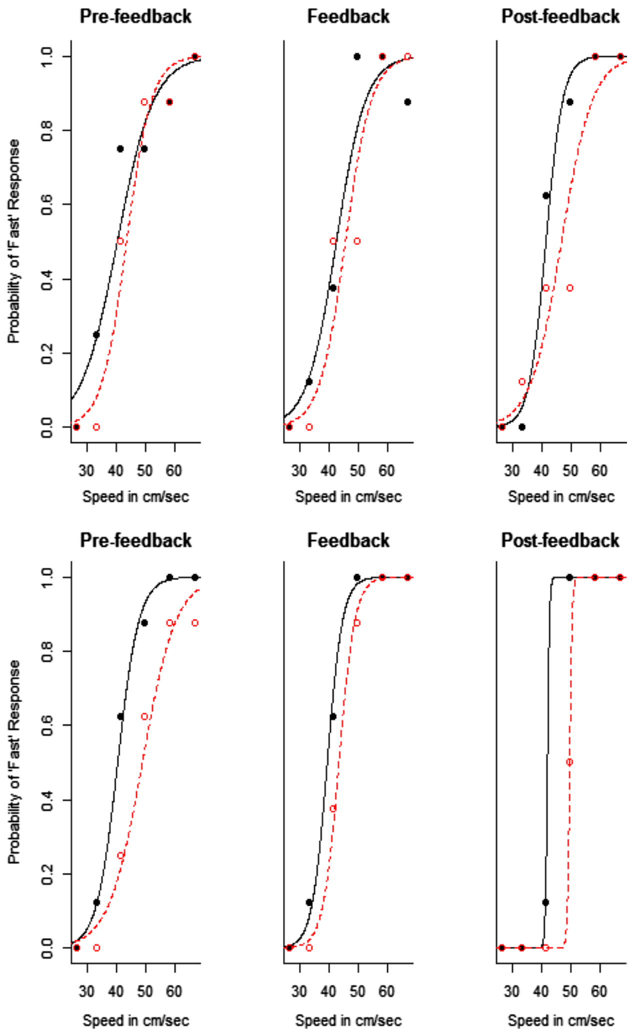

Fig. 2 Proportion of "fast" responses is plotted as a function of ball speed, paddle size, and phase of Experiment 1 for two representative participants. Each participant is plotted on a separate row. Points represent proportion of "fast" responses for the small paddle (*black closed circles*) and the big paddle (*red open circles*). *Lines* represent logistic regressions for the small paddle (*black solid line*) and the big paddle (*red dashed line*)

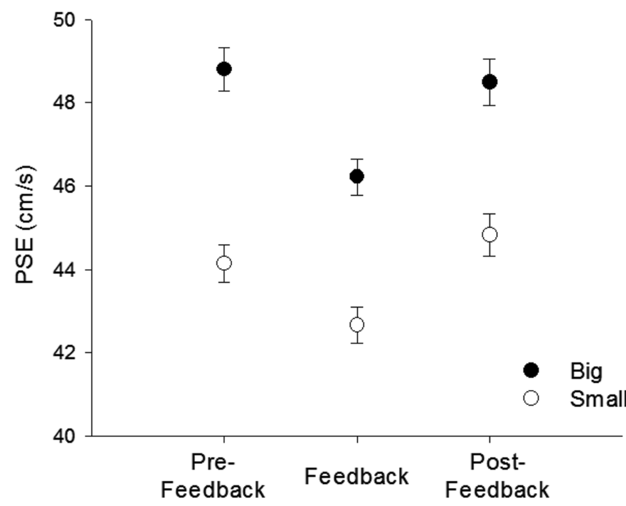

Fig. 3 PSEs plotted as a function of test phase and paddle sizes for Experiment 1. A lower PSE indicates that the ball was reported as moving faster. *Error bars* are 1 SEM calculated within-subject

successfully blocked, SD = 11.3%) compared to when they used the large paddle (M = 92.9%, SD = 7.7%). This confirms that the paddle size manipulation successfully affected performance.

In psychophysics methods like our speed bisection task, the data is analyzed by computing the point of subjective equality (PSE) from binary logistical regressions for each participant for each combination of phase and paddle size. The PSE is the calculated speed at which participants rated the ball as moving equally fast and slow. A higher PSE indicates judging the ball as slower. Figure 2 shows data from two representative participants. One participant had two PSEs that were more than 1.5 times greater than the interquartile range (for pre-feedback and post-feedback big paddle condition). Data from this participant was excluded from the analysis.

The PSEs were submitted to a repeated-measures ANOVA with paddle size (small and large) and phase (pre-feedback, feedback, and post-feedback) as within-subject factors. Paddle size significantly influenced PSEs, $F(1, 22) = 102.13$, $p < .001$, $\eta_p^2 = 0.82$. Participants rated the ball as moving faster (indicated by smaller PSEs) when they played with the small paddle than when they played with the big paddle (see Fig. 3). Phase significantly influenced PSEs, $F(2, 44) = 8.26$, $p = 0.001$, $\eta_p^2 = 0.27$. Within-subject contrasts showed that participants rated the ball as moving faster during the feedback phase (M = 44.94 cm/s, SE = 0.48) than both the pre-feedback phase (M = 46.28 cm/s, SE = 0.71) and the post-feedback phase (M = 47.05 cm/s, SE = 0.71), $ps = 0.002$, $\eta_p^2 s = 0.36$.

Critically, the interaction between paddle and phase was not significant, $F(2, 44) = 0.79$, $p = 0.46$, $\eta_p^2 = 0.04$. The lack of significant interaction suggests that paddle size had a similar effect in each phase of the experiment. This was further explored by conducting a paired-samples *t* test across paddle size for each phase separately. Paddle size significantly influenced PSEs in each phase, and the effect size was large for the pre-feedback phase, $t(22) = 6.78$, $p < .001$, $d_{rm} = 1.18$, the feedback phase, $t(22) = 6.77$, $p < 0.001$, $d_{rm} = 1.50$, and the post-feedback phase, $t(22) = 4.56$, $p < 0.001$, $d_{rm} = 0.93$. The non-significant interaction was further explored using Bayes factor. Paddle Effect scores were computed for each phase by subtracting the PSE with the small paddle from the PSE with the big paddle. A paired-samples *t* test indicated that the difference in paddle effect scores was not significant,

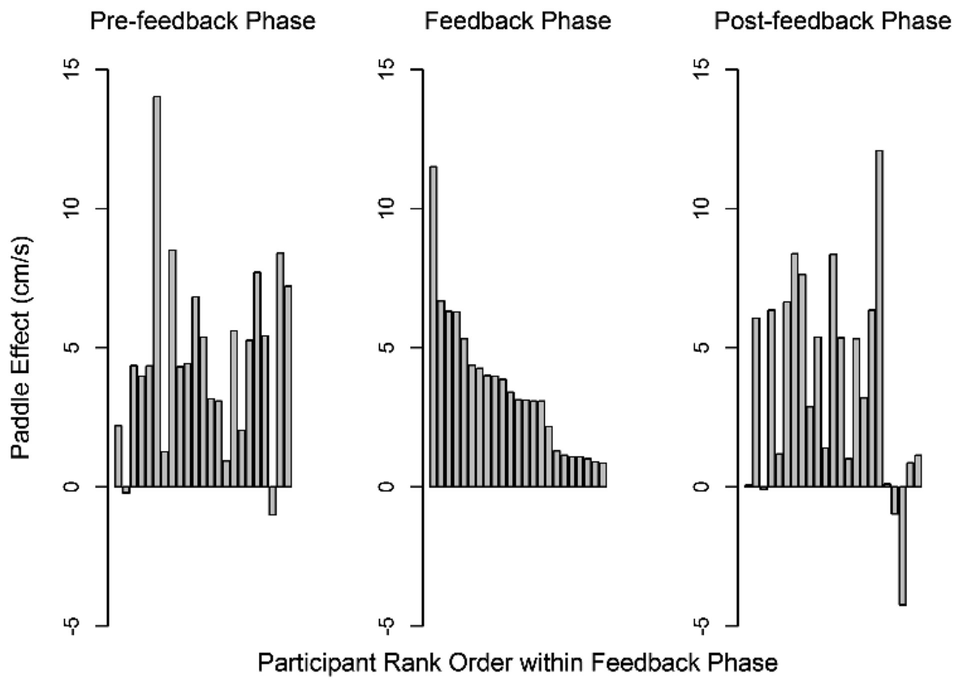

Fig. 4 Paddle effect (calculated as PSE with the big paddle minus PSE with the small paddle) for each participant for each test phase for Experiment 1. A larger paddle effect indicates that paddle size had a larger influence on PSEs. Participant rank order was calculated within the feedback phase

$t(22) = 1.16$, $p = 0.26$, $d_{rm} = 0.38$. The paddle effect scores for the pre-feedback and feedback phases were compared with a Bayesian t test (using the R package BayesFactor and a Cauchy prior). The resulting Bayes factor was 0.30. This indicates that the null hypothesis (that there was no difference in the paddle effect between the pre-feedback and feedback phases) was over three times more likely than the alternative hypothesis (that there was a significant difference in the paddle effect across phases). This lends support to the idea that feedback did not reduce the paddle effect.

In addition, all 23 participants (100%) showed a positive paddle effect (PSE with big paddle > PSE with small paddle) during the feedback phase (see Fig. 4). Most showed a positive paddle effect during the pre-feedback phase ($n = 21$, 91%) and during the post-feedback phase ($n = 20$, 87%). Such a robust effect across participants is not characteristic of an effect driven by response bias. For instance, in the classic Asch conformatory studies, the situation which gave rise to the biggest effects of conformity only showed biased responses in approximately one third of participants (Asch, 1955).

Feedback's influence on the paddle effect was not significant. Although this result is consistent with a perceptual explanation of the paddle effect, an obvious question is whether the feedback was sufficient to have had an influence if the paddle effect was due to response bias. The a priori power analysis revealed that we ran enough participants to have sufficient power. But perhaps the feedback itself was not particularly compelling. To determine if feedback had any effect, we computed proportion correct responses[1] for each participant for each phase and entered them into a repeated-measures ANOVA with phase as the within-subjects factor. Phase had a marginally significant effect on accuracy, $F(2, 44) = 3.10$, $p = 0.055$, $\eta_p^2 = 0.12$. Planned contrasts showed a significant difference in accuracy between the pre-feedback and feedback phases, $F(1, 22) = 7.14$, $p = 0.01$, $\eta_p^2 = 0.25$, but not between the feedback and post-feedback phases, $F(1, 22) < 1$ (pre-feedback: M 85.6%, SE 0.55%, calculated within-subjects; feedback: M = 87.5%, SE = 0.53%, post-feedback: M = 87.5%, SE = 0.65%). The difference between the pre- and post-feedback phases was not significant, $F(1, 22) = 3.07$, $p = 0.09$, $\eta_p^2 = 0.12$. This pattern suggests that feedback produced some effect on participants' responses, although the increased accuracy could be due to a practice effect rather than feedback.

Despite a statistically significant increase in accuracy with the inclusion of feedback, the magnitude of the increase was less than 3%. Again, this raises the issue of whether the feedback was sufficient to reduce the paddle effect if the paddle effect is indeed due to response bias. Feedback was only provided for one of the three phases of

[1] An alternative method would be to look at the just-noticeable differences (JNDs). However, JNDs cannot be computed for participants whose data showed quasi-complete separation, so proportion correct was considered instead.

the experiment. Thus, we conducted a second experiment for which feedback was provided throughout the experiment, and we questioned participants about the feedback at the end.

Experiment 2

Participants received explicit feedback on incorrect speed estimates throughout the entire experimental session to determine if increased exposure to feedback would eliminate or reduce the effect of paddle size on estimated ball speed.

Method

Participants and design

Seventeen participants (eight females) completed this experiment in exchange for course credit. All gave informed consent. A power analysis reveals that ten participants are needed to achieve at least 95% power to obtain the standard paddle effect. We stopped data collection on a specific date for which we could ensure that we had at least the desired number of participants. There were two paddle length conditions (small and large) and participants received feedback on the accuracy of their judgments throughout all experimental trials.

Stimuli and apparatus

The stimuli and apparatus were the same as in Experiment 1.

Procedure

The initial training phases were the same as in Experiment 1. The test trials were an expanded version of the feedback phase in Experiment 1. Participants received feedback on all incorrect responses throughout the entire experiment. The test phase consisted of 12 blocks of trials, with each block consisting of 24 trials (2 paddle sizes × 6 speeds × 2 repetitions). Order within block was randomized.

At the end of the experimental trials, participants were prompted with the following question: “If given $5 to keep, how much would you give back to the researchers to take away the buzzing noise?” This question was adapted from research that used this question to evaluate the intensity of an electrical shock and participant’s willingness to self-administer said shock in the absence of all other forms of stimulation (Wilson et al. 2014). This question was used to evaluate how motivating the feedback was for participants.

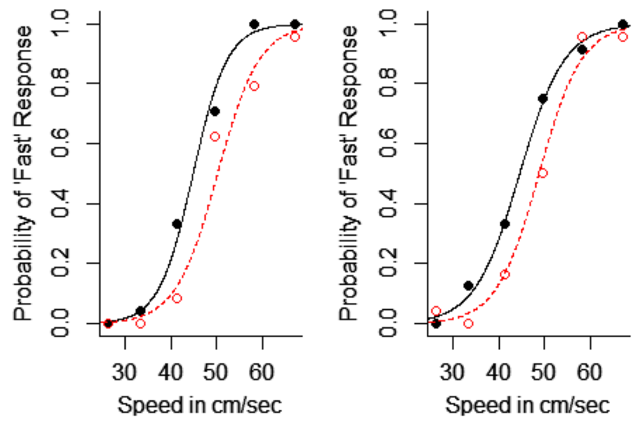

Fig. 5 Mean proportion of balls labeled as “fast” as a function of ball speed and paddle size for two representative participants in Experiment 2. Points represent proportion of “fast” responses for the small paddle (*black closed circles*) and the big paddle (*red open circles*). *Lines* represent logistic regressions for the small paddle (*black solid line*) and the big paddle (*red dashed line*)

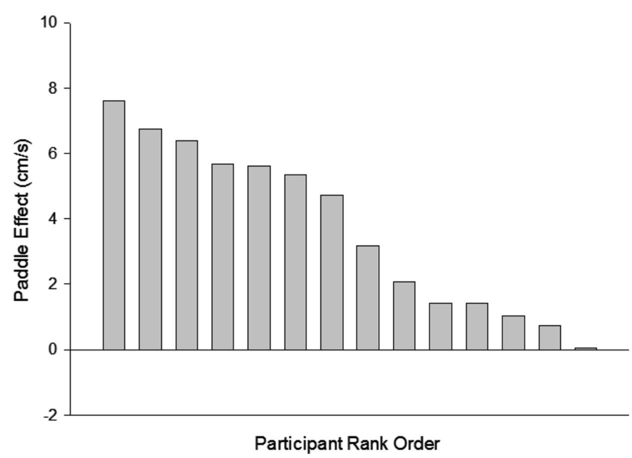

Fig. 6 Paddle effect for each participant in Experiment 2. The paddle effect was calculated as the PSE with the big paddle minus the PSE with the small paddle. A larger paddle effect indicates that paddle size had a larger influence on estimated ball speed. Positive values indicate an effect of paddle size in the direction theorized by the action-specific account

Participants verbally responded with any amount ranging from $0 to $5.

Results and discussion

PSEs were calculated for each paddle size for each participant. Three participants had at least one PSE that was at least 1.5 times beyond the interquartile range for these calculations. These participants were excluded, although, as in Experiment 1, the pattern of results did not change with their inclusion. Figure 5 shows mean responses across all speeds for two representative participants.

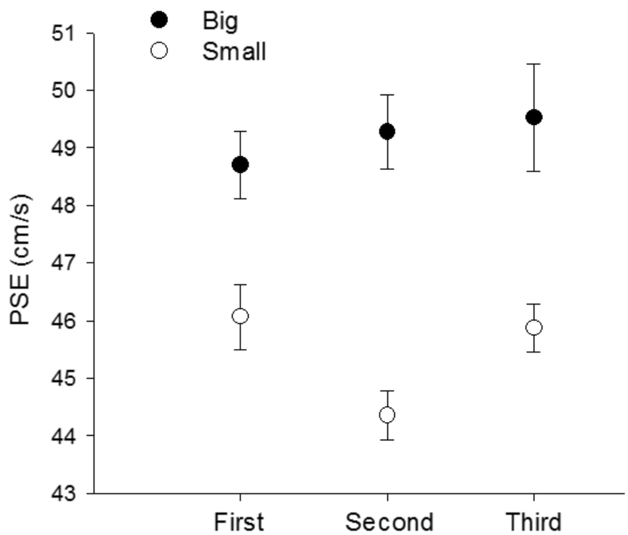

Fig. 7 Mean PSE as a function of paddle size and third of trials for Experiment 2. A lower PSE indicates reporting the ball as moving faster. *Error bars* are 1 SEM calculated within-subjects

These PSEs were then submitted to a paired-samples *t* test. The effect of paddle on PSE was significant, $t(13)=5.58$, $p<0.001$, $d_{rm}=1.49$. Participants estimated the ball as moving faster when the paddle was small, indicated by lower PSEs (M = 45.52 cm/s, SE = 0.26), than when the paddle was big (M = 48.99 cm/s, SE = 0.58). All 14 participants (100%) showed a positive paddle effect (see Fig. 6).

To determine if feedback had an effect over time, trials were split into thirds. PSEs were calculated for each third for each participant for each paddle size. Convergence was achieved on all calculations except in one case for which there was perfect separation. In this case, the estimate of the PSE is still a good approximation and was included in the analysis. A repeated measures ANOVA was conducted with paddle size and third as within subject factors. Paddle length had significant main effect on PSEs, $F(1, 13)=30.67$, $p<0.001$, $\eta_p^2=0.70$. There was no main effect for third, $F(2, 26)=1.63$, $p=0.23$, $\eta_p^2=0.11$. Critically, the interaction between paddle size and third was not significant, $F(2, 26)=1.77$, $p=0.19$, $\eta_p^2=.12$ (see Fig. 7). We further explored this null effect using Bayes factor, which was computed for the linear contrast (as a *t* test to compare the paddle effect for first third versus paddle effect for last third) to determine if the paddle effect was reduced after repeated exposure to feedback. The Bayes factor was 0.32, indicating that the null hypothesis (no difference in paddle effect) was three times more likely than the alternative (a significant difference in the paddle effect between the first and last thirds). The paddle effect endured despite persistent feedback throughout the experiment.

Across the trials, there was a small but significant boost in response accuracy. We computed mean accuracy within each block and submitted accuracy to a repeated-measures ANOVA with third as the within-subjects factor. Third significantly influenced accuracy, $F(2, 26)=7.34$, $p=0.003$, $\eta_p^2 = 0.36$. Participants were significantly less accurate in the first third (M = 85.9%, SE = 0.8%) than in the second third (M = 89.9%, SE = 0.7%), $F(1, 13)=22.62$, $p<0.001$, $\eta_p^2 = 0.64$. There was a marginally significant effect between the second third and the last third (M = 87.8%, SE = 1.3%), $p=0.055$, $\eta_p^2=0.26$, and a non-significant effect between the first and last thirds, $p=0.15$, $\eta_p^2=0.15$.

In addition, participants indicated a sensitivity to the feedback in their responses to how much money they would return out of \$5 to remove the annoying buzzing sound that accompanied incorrect responses (M = \$2.50, SD = \$1.74, 86% of participants were willing to give at least \$1 back). Anecdotally, the research assistants noted that several participants expressed outright frustration at the feedback including verbal grunts and even physically hitting the desk. Given that the feedback was effective at motivating a willingness to pay to eliminate the negative feedback, the finding that the paddle effect persisted despite the feedback is evidence for a perceptual explanation and evidence against a response bias explanation. Had participants ignored the feedback, it seems unlikely that they would have been willing to give any money back. It should be noted that participants who were willing to give back more money (and thus were likely highly motivated by the feedback) showed a similarly sized paddle effect as participants who were less willing to give back more money. We conducted a median split on amount of money that would be returned. For each participant in each group, we calculated paddle effect score by subtracting the PSE with the small paddle from the PSE with the big paddle. The paddle effect scores were not significantly different between the two groups, $t(12)=0.13$, $p>0.89$. For the group willing to give back \$3 or more, the paddle effect was significantly greater than 0, $t(5)=3.92$, $p=0.011$ (M = 3.37 cm/s, SD = 2.11 cm/s). For the group only willing to give back \$2 or less, the paddle effect was also significantly greater than 0, $t(7)=3.82$, $p=0.007$ (M = 3.54, SD = 2.62). Thus, even if some participants had ignored the feedback, the results still do not support a role for response bias in the paddle effect.

Combining experiments

An empirical question is to quantify the relative portions of the paddle effect that are due to perceptual processes versus judgment-based processes. Given the significant paddle effect in Experiment 2, there is evidence that at least some

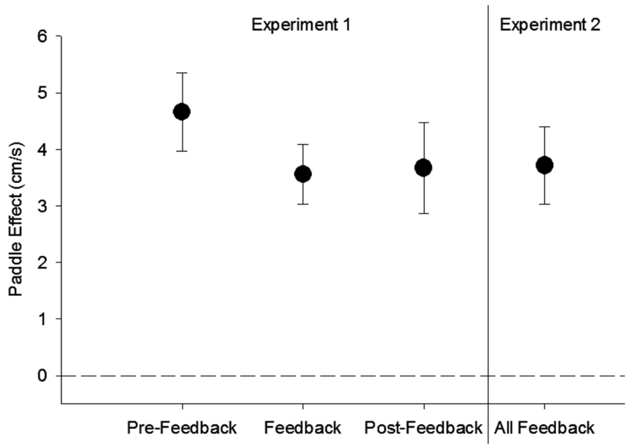

Fig. 8 Mean paddle effect is plotted as a function of Experiment and test phase. The paddle effect is the difference in the PSEs between the small and big paddles. A larger paddle effect indicates that paddle size had a larger influence on the PSEs. The dotted line is located at the place for which paddle size did not impact estimated speed. *Error bars* are 1 SEM calculated between-subjects

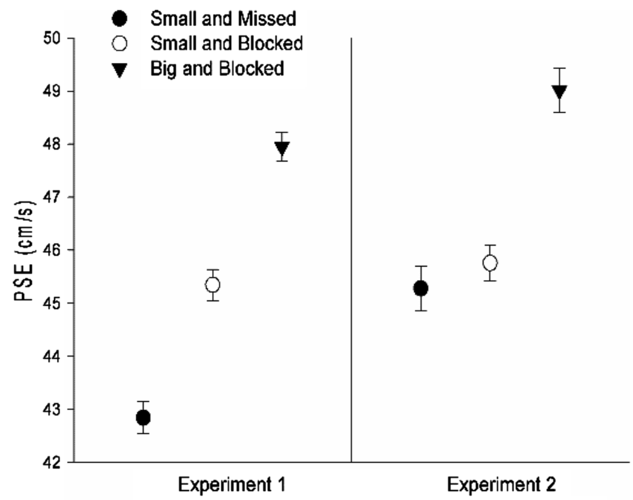

Fig. 9 Mean PSE is plotted as a function of paddle size and trial outcome (missed versus blocked). *Error bars* are 1 SEM calculated within-subjects for each experiment

portion of the paddle effect is perceptual. Because Experiment 2 had no control condition, we could not test whether the persistent feedback reduced the paddle effect, which would speak to some portion of the paddle effect being due to judgment-based processes. However, a comparison with the data collected in Experiment 1 allows for such comparison. Paddle effect scores were calculated as the PSE with the big paddle minus the PSE with the small paddle. The paddle effect scores across all trials for Experiment 2 were not significantly different than the paddle effect scores for the pre-feedback block in Experiment 1, $t(35)=1.18$, $p=0.25$, Cohen's $d=0.16$, Bayes factor $=0.45$ (see Fig. 8). This lack of difference suggests that the entire paddle effect is due to perceptual processes, and that judgment-based processes did not contribute to the paddle effect. However, although this difference was not statistically significant, we did not have sufficient power to find an effect this small as being significant (over 1000 participants would have been required), the Bayes factor was not decisively in favor of the null hypothesis (it was greater than 0.33), and the figure suggests some role for feedback in reducing the paddle effect. The magnitude of the decrease due to feedback was approximately 1 cm/s (a 25% decrease from the pre-feedback paddle effect). However, even if this had been a statistically significant effect, the results still support the claim that the paddle effect is perceptual, with at least 75% of the magnitude of the effect being attributable to perceptual differences.

A potential concern with the current paradigm is whether responses are affected by ease to block the ball (manipulated via paddle size) or trial outcome, which refers to whether or not the ball was successfully blocked on a given trial. After missing the ball, participants might try to excuse the poor performance by inferring that the ball was fast (cf. Firestone & Scholl, 2016; Wesp & Gasper, 2012). This pattern of responding would still produce a paddle effect because participants miss the ball more frequently with the small paddle than with the big paddle. But the effect would not be perceptual because success on a given trial is unknown until after or shortly before the ball stops moving. Thus, an effect on trial outcome cannot be a perceptual effect.[2]

To explore the relative contribution of the paddle effect (differences in estimated speed across paddle sizes) and the trial outcome effect (differences in estimated speed between misses and successful blocks), we computed PSEs for the small paddle when participants missed the ball, PSEs for the small paddle when they successfully blocked the ball, and PSEs for the big paddle when participants successfully blocked the ball for each experiment (collapsed across all phases for Experiment 1 due to not enough trials to perform these calculations within phase). Participants did not miss the ball frequently enough with the big paddle to have enough data

[2] An exception to this is that post-events can influence perception, which is known as postdiction. Thus, it is possible that trial outcome could affect perceived ball speed. We are unaware of any techniques to separate postdictive explanations from judgment-based explanations and thus take the more conservative view that any effects of trial outcome are due to response biases or judgment-based effects rather than being genuinely perceptual. An argument that the effect of trial outcome is perceptual would be consistent with the action-specific account of perception (for extended discussion on this issue, see Witt, Tenhundfeld, & Bielak, 2017).

to compute PSEs for the big paddle when they missed. As shown in Fig. 9, the paddle effect (measured, in this case, as the difference between the PSEs when successful with the small paddle versus the big paddle) was similar across experiments. These difference scores were submitted to an independent-samples t test with experiment as the between-subjects factor, and showed that experiment was not a significant factor, $t(35) = 0.82$, $p = 0.42$, $d = 0.10$. As shown by paired t tests for each experiment, the difference between PSEs on successful trials between the big and small paddles was significant for both Experiment 1, $t(22) = 5.40$, $p < 0.001$, $d_{\mathrm{RM}} = 0.94$, and Experiment 2, $t(13) = 5.03$, $p < 0.001$, $d_{\mathrm{RM}} = 1.64$. In contrast, there was a significant difference in the trial outcome effect across experiments. The trial outcome effect was computed as the difference in PSEs between the small and missed trials and the small and blocked trials. The trial outcome effect was bigger for Experiment 1 than for Experiment 2, $t(35) = 2.42$, $p = 0.021$, $d = 0.30$. In addition, paired-samples t tests for each experiment comparing PSEs with the small paddle when missed versus blocked showed that the trial outcome effect was significant for Experiment 1, $t(22) = 4.75$, $p < 0.001$, $d_{\mathrm{rm}} = 0.87$, but not for Experiment 2, $t(13) = 0.76$, $p > 0.46$, $d_{RM} = 0.20$.

These findings are important for two reasons. First, the paddle effect was still significant even after accounting for potential effects of trial outcome. This rules out an explanation of the paddle effect based on an obvious judgment-related factor, namely trial outcome. This result is thus consistent with the idea that the paddle effect is perceptual. Second, the increased amount of feedback in Experiment 2 was effective at eliminating the trial outcome effect. This pattern is consistent with the claim that the amount of feedback, at least in Experiment 2, was sufficient to eliminate judgment-based responses. Consequently, the lack of influence of the feedback on the paddle effect is further support for a perceptual explanation of the paddle effect.

General discussion

Action-specific effects on perceptual judgments indicate that a person's potential to perform an action influences spatial perception (Proffitt, 2008; Witt, 2011, 2016c). The action-specific approach to perception is one of many theories proposing links between action and perception. For example, the theory of event coding posits that there are shared representations that code both perception and action (Hommel et al. 2001). As a result, action's influence on perception can be seen when performing and planning an action (e.g., Kirsch & Kunde, 2014; Lindemann & Bekkering, 2009; Musseler & Hommel, 1997). Despite these well-established effects and other kinds of perception–action relations (for review, see Witt, 2016b), the claim that action-specific effects are perceptual has been met with much resistance. If the perceiver's potential for action truly influences spatial perception, the findings have extensive implications for theories of spatial vision. Many theories consider spatial vision to be largely a function of optical information, with only limited influences from non-optical information such as crossmodal influences from other senses and natural constraints or priors. Action-specific effects open up a new category of information for spatial perception. This category, furthermore, is action which is generally considered to be the end-point of cognition, and thus not a source for the starting point of perception. Action-specific perceptual effects, like the theory of event coding and other theories of perception–action, also challenge the assumption of sequential cognitive processing from see to think to act.

A challenge for the action-specific account of perception has been to assess whether particular action-specific effects are truly perceptual. Coupled with previous research that addressed other alternative interpretations, the current studies make a compelling case for a perceptual interpretation of the action-specific effect of paddle size on estimated speed (the paddle effect). Because perception cannot be measured directly and instead must be inferred based on observable behaviors such as judgments, proof that an effect is perceptual requires the process of eliminating alternative explanations.

Some alternative explanations involve straight-forward tests. For example, to assess whether the ease with which a ball could be blocked influences perceived speed or immediate memory for ball speed can be addressed by conducting the Pong experiments such that participants make speed judgments while the ball is still visibly moving. Additionally, previous research eliminated many potential alternative explanations by documenting that the effect of ease to block a ball on estimated ball speed is not due to effects in memory (Witt & Sugovic, 2012, 2013a), differences in the allocation of attention (Witt, Sugovic, & Dodd, 2016), or low-level visual differences across condition (Witt & Sugovic, 2012; Witt et al. 2012). Two recent papers provide detailed and brief reviews of this literature and how it addresses alternative explanations (Witt, 2016a; Witt, Sugovic, Tenhundfeld, & King, 2016; respectively).

In contrast, other alternative explanations are especially challenging to eliminate, such as assessing whether the effects are due to response bias and/or judgment-based processes. In the field of distance perception, the primary strategy to rule out post-perceptual processing has been to examine convergence across a variety of different kinds of measures (cf. Foley, 1977; Gogel, 1990; Loomis & Philbeck, 2008). The paddle effect has been found across a wide range of perceptual measures including visual matching tasks (Witt & Sugovic, 2012) and action-based measures (Witt & Sugovic, 2013a), thus demonstrating the convergence necessary for making claims of a perceptual effect. Another technique has been to explicitly measure the extent to which participants are willing to alter their responses to comply with demand characteristics, and account for this compliancy when measuring the paddle effect. For example, in one study, we instructed one group of participants in a way to bias their responses to be slow and another group to be fast (Witt & Sugovic, 2013b). We measured the extent to which participants complied with these instructions, and compared the paddle effect across participants who were more compliant versus less compliant. The paddle effect was significant and equivalent across both groups. The results suggest that the paddle effect is independent of compliance (e.g., a willingness to change one's responses to conform with demand characteristics), and thus is independent of response bias.

Firestone and Scholl (2016) distinguish active modulation of responses from judgment-based effects for which participants genuinely believe their judgments, but their judgments are based on inferences, thoughts, and conclusions rather than purely perceptual experience. To differentiate between these judgment-based effects and perceptual effects, they recommended a strategy put forth by Wesp and Gasper (2012) to use a cover story to account for poor performance. The idea is that participants might account for poor performance by inferring that the target was smaller or faster than they had perceived it, and then reported their beliefs about target size or speed rather than their perception. However, if these participants are given a reason for poor performance, such as being told that the darts they were throwing were faulty, participants would not have to find a reason to account for their poor performance and would report on what they saw, instead of what they inferred. The evidence for the effectiveness of this strategy to differentiate judgments from perception was that dart throwing performance correlated with perceived size for those not given a cover story, but the correlation was not significant when participants were told about the faulty darts. It is unclear as to why participants who threw well with the 'faulty' darts (the darts were actually the same in both groups), did not infer or conclude that the target must have been bigger to account for such good performance with such poor equipment, and this possibility was not discussed by either Wesp and Gasper (2012) or Firestone and Scholl (2016).

We implemented a similar strategy in the Pong task (Witt et al., 2017). Participants in one group were told that the task was especially difficult because the ball would bounce at random. Another group was told the task was relatively easy because they had full control over the paddle. Despite these cover stories, the paddle effect was equivalent for both groups. Thus, the cover story did not eliminate the paddle effect, which is consistent with the idea that the paddle effect is perceptual. In contrast, for participants who were better than others at blocking the ball, the cover story that the task was difficult successfully eliminated the effect of trial outcome on speed judgments. The trial outcome effect is the effect of a successful block or a miss on a given trial on judged ball speed. The trial outcome effect is considered a judgment-based effect rather than perceptual, because the information related to trial outcome (block versus miss) is unknown until the end of the trial and thus is unlikely to influence the perception of ball speed. The cover story eliminated the judgment-based effect of trial outcome's influence on estimated speed, but not the paddle effect, is further support that the paddle effect is not due to judgment-based processes such as participants reporting on their inferences about ball speed, rather than their perception.

Despite the implementation of these strategies to address issues of response bias and judgment-based influences, questions still remain as to whether the paddle effect is truly perceptual. Thus, a new strategy was implemented in the current experiments, namely to give participants direct and explicit feedback about the accuracy of their speed judgments. Participants classified the speed of balls as being more like the slow anchor speed or more like the fast anchor speed. For the three slowest ball speeds, participants were given feedback that their responses were incorrect if they classified the balls as fast, and vice versa for the three fastest ball speeds. Given that participants were more likely to classify the slow balls as "fast" when playing with the small paddle, and more likely to classify the fast balls as "slow" when playing with the big paddle, the result of the feedback was continuous information that these classifications were incorrect. Yet, participants continued to classify the ball speeds differently depending on the size of the paddle.

One possible explanation for the lack of effect of feedback on the paddle effect is that the participants ignored the feedback. It seems counterintuitive to think that participants are savvy enough to pick up subtle cues about complex experimental predictions but intentionally ignore the direct and explicit feedback provided in this experiment. Nevertheless, the impact of the feedback is still worth considering. There are several reasons to think that participants did not ignore the feedback. In Experiment 2, 84% of all participants were willing to give back at least $1 of 'free money' to remove the feedback, indicating that they found the feedback unpleasant. In addition, with the extended feedback provided in Experiment 2, this eliminated the effect of trial outcome, which was significant in Experiment 1. Thus, feedback was successful at eliminating a judgment-based effect, but not the paddle effect, which we claim is perceptual.

Provided that the paddle effect is perceptual, one might question why feedback did not eliminate it given that feedback can be useful for perceptual learning. However, as discussed in the introduction, for feedback to affect the perceptual learning, there must be substantially more trials than what was presented here. With enough exposure, feedback could possibly lead to perceptual learning that could alter the paddle effect. But given that our goal was to use feedback to differentiate between a response-bias or judgment-based effect from a genuine perceptual effect, we ensured that the number of trials would not lead to perceptual learning.

Building on past research, the current results substantiate perceptual claims within this one particular action-specific paradigm. That a person's ability to act genuinely impacts perception of ball speed indicates that it is time for theories of spatial vision to accommodate a person's potential for action as an influential source of information that affects one's spatial perceptions. Furthermore, the 10% difference in perceived speed across the two paddle conditions is notable. A 10% difference in physical speed separates professional baseball pitchers from high school pitchers; for a professional batter to see the ball as moving 10% slower would be akin to putting him in a high school game. If only we could figure out how to give the Chicago Cubs the equivalent of the "big paddle", one of the authors (JKW) would not have to wait another 108 years for a Cubs World Series title!

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Compliance with ethical standards

All procedures were in accordance with the ethical standards of the institutional research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. Informed consent was obtained from all individual participants included in the study.

Conflict of interest The authors declare that they have no conflict of interest.

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