THESIS

WHAT YOU SEE AND WHAT YOU ARE TOLD:

FEEDBACK DOES NOT DIMINISH ACTION-SPECIFIC PERCEPTION

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ABSTRACT

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The action-specific perception account claims that ability to act causes differences in perception of task relevant stimuli. Critics argue that a response bias occurs, not a difference in perception. Feedback has empirical support for affecting response biases and distinguishing between response biases and perceptual biases. Feedback was given during an action-specific perception paradigm to determine how much of the action-specific effect could be explained by response biases. Participants played a computer-based game of tennis. The ball moved across the screen at various speeds between the two anchor speeds on which they were previously trained. Ability to act was manipulated by varying the size of the paddle used to block the ball. After each blocking attempt, participants performed a speed judgment task. Participants typically report the ball as moving faster when using a small paddle than when they use a big paddle. In Experiment 1, participants completed pre-feedback, feedback, and post-feedback phases with interleaved paddle sizes. Paddle size had a significant effect on speed judgments in all three phases and did not diminish when feedback was given. It is possible that participants did not receive enough feedback and so in experiment 2 participants received feedback during the entire experiment. Again, feedback did not eliminate the effect of paddle size on speed judgments. Experiment 3 examined how much of the difference in reported speeds could be explained by response biases. False feedback was used to create a response bias opposing the effect of paddle size on speed. A multilevel linear regression suggests that while both feedback and action ability affects reported speed, in the final model there is no significant interaction between these variables. Results support the claim that action affects perception and is evidence against a primarily response bias account of action-specific effects.

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Modularity of Mind and Vision

Motor information and motivational states have been a source of interest for visual perception research since the beginning of the cognitive revolution (Agnew, Pyke, & Pylyshyn, 1966; Bruner & Goodman, 1947). However, for some, these interests were short lived after the Fodor's Modularity of Mind theory (Fodor, 1983) gained traction within the psychological community. This theory postulates that visual perception is encapsulated. Encapsulation refers to how penetrable the visual perceptual process is. According to this view, visual perception is a result of processing optical stimuli independently of other sensory information and with limited input from prior knowledge. For example, when two circles are of equal diameter but one is surrounded by small diameter circles and the other is surrounded by large diameter circles, the center circles are perceived as different in size despite being the same diameter. This is the Ebbinghaus (or Titchener) illusion and despite knowing the center circles are the same diameter, they are still perceived as different in size. Knowledge about the circles does not change the perception of circle size and so visual perception is encapsulated from this knowledge. Recently, the extent to which sense modalities are encapsulated has been a source of contention.

Action-specific perception research evaluates how factors such as energetic potential, bodily dimensions, and other motor/action factors bias perception (Witt, 2011). For example, players who are batting better than other players during a softball game rate the ball as being larger (Witt & Proffitt, 2005). Action-specific research provides evidence against a modular account of the visual system in that factors other than optical information influences visual processing. However, the idea that action information has an important relationship with perception is not a novel one. Action-specific perception research stems from the theory of affordances. Affordances are those properties of an object or environment that relate to the action capabilities of the perceiver. Within the ecological approach to perception, affordances are the primary units of perception (Gibson, 1978). To have a meaningful discussion of action-specific perception, it is necessary to discuss the tradition from which it developed.

Theory of Affordances

Instead of seeing objects in terms of their distance in feet, meters or other arbitrary distance measuring units, the theory of affordances proposes the brain processes visual information as it relates to one's ability to interact with the world. For example, imagine a cup sitting on a table in front of a perceiver. The theory of affordances suggests that instead of seeing a 10 cm tall cup with a 6 cm diameter that is 50 cm away from the person on the table, the perceiver sees an object that she can easily hold, that would contain a significant amount of water, and that is within reaching distance from where they currently are located. If the cup was 100 cm away instead of 50 cm, then the distance to the cup would no longer afford reaching without movement of the upper torso or locomoting to the object. However, a second perceiver with a longer arm may still perceive the cup as being reachable. The cup also affords other actions. If an intruder breaks into the perceiver's home, the density and size of the cup afford throwing. If a venomous arachnid chooses to meander towards the person, the single opening and sturdiness of the cup affords trapping. Affordance perception is a unique relationship of the actor/perceiver and the environment and as such the specific affordances to which one attunes varies depending on the circumstance.

There are many examples of how humans are perceptually sensitive to the action boundaries that are a distinct characteristic of affordances. Short and tall males identify stairs as unclimbable at differing riser heights (Warren, 1984). The male participants were selected for their height to obtain samples that represented approximately the 2nd (M = 163.7 cm) and 98th (M = 189.8 cm) percentiles of male height. These participants were presented with black and white images of a customized staircase in which the riser height was manipulated to five different heights between 50.8 cm and 101.6 cm. The images were taken and presented from approximately the same height as the participants to provide a more naturalistic viewing setting. These images were presented in a random order and the participants indicated if the stairs were climbable or unclimbable "in the normal way, without using your hands or knees" and how confident they were on a scale from 1-7 of their perceptual categorizations (Warren, 1984; p. 688). The point at which participants responded 50% of the time that the stairs were climbable was used to indicate the point at which the stair height turned from perceptually climbable to perceptually unclimbable. The short group's transition point for climbability was 67.13 cm whereas the tall group's transition was 81.33 cm. What this suggests is that humans' perception of climbability scaled with the height of the participant, which is exactly what you would expect if the theory of affordances were true. There is not an objective height at which stairs become unclimbable; instead this height relates to the perceiver's own body. In follow up experiments, participants identified when riser height for ascending a staircase would be the easiest to climb. This height was found to be the same as the optimal riser height for easiest ascension (riser heights: shorter people: M = 18.85 cm, taller people: M = 22.86 cm). Optimal riser height was a riser height that used minimal oxygen to climb and was kinematically easiest for continual climbing. From this, Warren concluded that humans are perceptually sensitive to environmental characteristics as they relate to our action capabilities. Humans see stairs in terms of our ability to climb them, and this perception allows us to choose optimal paths to transverse.

The human perceptual system is also sensitive to reaching capabilities even across complex reaching scenarios (Carello, Grosofsky, Reichel, Solomon, & Turvey, 1989). Participants were instructed to say whether or not they could reach an object 1) with only shoulder rotation or 2) with bending at the hip and shoulder movement. They made this judgment for objects presented at distances up to 142 cm. When individuals were allowed to use their hips to increase their reaching range, their judgments of reachability also increased. As with stair climbing, there is no specific distance that specifies the boundary of reachability. Instead, this boundary is dictated by the individual perceiver's body. Unexpectedly, there was a significant difference between actual reachability and perceived reachability.

Participant's perceived reachability was longer than their actual reachability with a mean difference of 7.4 cm. This would seem to support the notion that humans are not sensitive to our reaching capabilities when estimating distance; however, participants were more accurate when they were allowed to bend at the hip (mean discrepancy of 4.3 cm) compared to when they could only use their arm (10.5 cm; Carello et al., 1989). This suggests that when individuals are allowed to reach as they would in a naturalistic setting, they are more accurate. It is unlikely that anyone would choose to hold their torso stationary to reach for an object unless they had some sort of injury or were pinned for some reason. This suggests there is a relatively accurate embodiment when performing naturalistic tasks. Individuals are sensitive to reachability boundaries and when allowed to act in naturalistic reaching scenarios are accurate within a few centimeters.

Interestingly, there is some research to suggest that individuals with Autism Spectrum Disorders (ASDs) are not sensitive to affordance information such as step-ability or reachability. When evaluated on a range of affordance perception tasks such as reachability to an object, grasp-ability of an object, and pass-ability of their arm through an aperture opening, the group that had ASDs made significantly more errors on these three tasks (M = 30%) compared to a control group performing the same tasks (M = 9%; Linkenauger, Lerner, Ramenzoni, & Proffitt, 2012). The participant's intelligence quotient (IQ) was not significantly correlated with these errors, and individuals with ASDs often suffer from motor control issues and more difficulty with perceptual-motor integration tasks (Attwood, 2008). What this finding suggests is that motor information, not reasoning ability as indicated by IQ, is vital to affordances.

There are many more examples of affordance perception (for review, see Fajen, Riley, & Turvey, 2009). Methodological differences between ecological approach research and spatial perception research has produced a schism in the communication of affordance perception results to other vision researchers. According to ecological psychologists, perception refers to the perception of affordances and their methods primarily ask people to judge affordances (the step-ability or reachability of an object). Spatial perception research is measured by asking perceivers to estimate the distance to, size of, and speed of an object. This methodological difference, asking for step-ability of a stool versus the height of a stool, has limited vision research. Action-specific perception research is a spatial perception based account that integrates affordance perception research with spatial perception research (Witt, 2011).

An intuitive impression of spatial perception is that it is geometrically veridical such that the distance, size, or speed that is perceived corresponds to measurements as would be taken by a tape measure. The action-specific perception account challenges this impression by demonstrating that affordances penetrate into these seemingly objective percepts of the surrounding environment. In the case of golfers, when golfers putted well they reported seeing the golf hole as smaller than golfers who putted poorly (Witt, Linkenauger, Bakdash, & Proffitt, 2008). Thus spatial perception of distance and size is influenced by affordances. However, the ways that affordances penetrate spatial perception are varied. As a result there are many different categories of action-specific effects.

Types of Action-Specific Effects on Perception

Action effects on perception demonstrated thus far can generally be organized into three broad categories. These three categories are energetics, body dimensions, and task proficiency/skill. In this context, action information are those variables that limit or enhance the effectiveness of a given movement.

Energetics

How much energy is available for you to move limits what actions you can perform. Consequently, when individuals have lower energetic levels, the spatial layout of the environment is perceived based on how difficult a given area is to traverse. For example, when participants are fatigued after running, hills appear steeper than when participants are not fatigued (Proffitt, Bhalla, Gossweiler, & Midgett, 1995). Runners served as participants, and began by estimating either a 5 degree slant hill or a 31 degree slant hill. They made three judgments of slant: verbal estimate, a visual matching estimate, and a haptic estimate using a tilting palm board. They then went for a run. There were no specific requirements for this run besides that they be very tired upon completion and that it should be their hardest run of the week. The participants estimated the other hill, 5 or 31 degrees, upon completion of their run to avoid practice effects on the same hill. As a result the comparison was between subjects for the pre-run and post-run conditions. Individuals fatigued from their run estimated both hills to be steeper than pre-run individuals for the verbal and visual reports. Visual slant reports increased in slant by 50% for the 5 degree hill and 12% for the 31 degree hill and verbal reports increased by 35% and 24% for the 5 degree hill and 31 degree hill respectively. The haptic measure showed no difference in pre and post run estimates and this was interpreted as a relative inertness of haptic perception to difficulty of task, which may be because the haptic task is argued to rely on a different source of visual information than the pathway responsible for conscious perception (see Proffitt et al. 1995 for extended discussion on this topic).

More important for the purposes of this paper, estimates of hill slant were steeper post run compared to pre run. Individuals had less energetic resources post run and this would make climbing a hill more difficult than it was pre-run. As a result, the hill was perceived as steeper. This effect was less extreme for the 31 degree hill, but this makes sense because the 31 degree hill is harder to climb even with access to an abundance of energetic resources. The lack of resources should not play as major of a role in the perception of hill slant because the hill would be difficult regardless of energetic resources. The easier to climb hill, 5 degree hill, showed a much more pronounced slant exaggeration post run because the dearth of energetic resources has a significant impact on the climbability of the hill.

Additionally, when individuals are in a state that requires excess energy to move, such as wearing a heavy backpack, they report hills as being steeper than when they are unencumbered (Bhalla & Proffitt, 1999). What might the visual system be monitoring to evaluate these energetic states? Blood oxygen concentration seems like a likely candidate. Consistent with this notion, individuals with higher maximal oxygen uptake reported hills to be less steep than individuals with lower maximal oxygen uptake (Bhalla & Proffitt, 1999), and when the amount of energy required to traverse an optically specified distance increases, participants reported an object to be further away (White, Shockley, & Riley, 2013). Another source of energetics information might be blood glucose levels. When individuals consume a sugary drink, distances are reported as being closer than when individuals ingest a non-calorically sweetened drink (Schnall, Zadra, & Proffitt, 2010) and individuals who rated stair cases as being steeper would freely choose a more calorically dense drink or snack item (Taylor-Covill & Eves, 2014). The perceptual system is sensitive to how much energy will be expended navigating an environment and how much energy the perceiver has available. By changing perception, the perceptual system can influence navigation to minimize unnecessary energy expenditure (Eves, Thorpe, Lewis, & Taylor-Covill, 2014).

Body Dimensions

Another limit to action is the dimensions of the body of the perceiver. Individuals with broader shoulders report aperture openings through which they would need to pass as being narrower than slim shouldered individuals (Stefanucci & Geuss, 2009). How far an individual can reach should influence the perceived distance from the individual to the object intended to be reached. Likewise, if a person is given something to extend their reach, then objects should appear closer when they use a reach-extending tool than when they do not use the tool. Based on dissociation studies, it is reasonable to conclude that the brain treats reaching tools as an extension of the body, or in other words the tool becomes embodied, and this increases the range of reachable distances (Berti & Frassinetti, 2000). How is perception different when reach is extended via using a tool? The extended reach provided by the tool usage compresses distance to an object within reachable space (Witt, Proffitt, & Epstein 2005). Participants were given a 39 cm long baton and asked to either touch, point to, or provide a verbal estimate of the distance to a white circle that was projected on the table. In this way, the participants actively used the baton but also provided distance estimates. Participants also completed this task with no reach extending tool, i.e. they pointed or touched with an extended finger. Participants estimated the distance to the objects to be closer when they used the tool compared to when they only used their finger. These results were replicated using a visual matching paradigm. Participants reached to touch where a circle had briefly flashed on a table and then two other circles appeared on the table. The participants adjusted the distance between these outside circles until they were the same distance apart as the target circle from a fixed point on the table (Witt et al. 2005). Using this visual matching paradigm, the same effect of tool use on estimated distance was found: the targets looked closer when they could be reached using the reach-extending tool than when the tool was not used. However, it appears that differences in perception is moderated by intention to use the tool to reach. Perception did not differ when the participants simply held the tool and did not use it for reaching to the targets; the participants actually needed to intend to perform an action with the tool to affect perception. In summary, spatial perception differs when a perceiver has a reach extending tool but only when they intend to act with the tool.

Skill and Performance

Finally, the amount of skill one has in a given task should dictate how easily the action can be performed. If an individual is swinging a bat, for example, and that individual has never swung a bat before, their movements will likely be inefficient. Lack of skill could also present significant risk to the individual. If there is a quick route that is hazardous, such as climbing up a cliff face, attempting this route for an unskilled individual would present greater risk for harm than a winding steep hiking trail alternative. Thus, skill and performance on a task are constraints for action capabilities relevant to that task. Perception being sensitive to the skill of the perceiver makes adaptive sense. Consider walking on a log across a river versus walking 100 meters to a bridge. Crossing the log is more skill intensive, but requires less energy walking along the detour to the much safer bridge. If the perceptual system was only sensitive to energetics, the log should appear as a better option, which could be achieved or enhanced with a perceptual fore-shortening of the perceived length of the log. If the perceptual system is sensitive to skill, and the perceiver is inebriated, a perceptual expansion to make the log appear longer or the bridge appear closer would be advantageous and less likely to result in injury. Based on this reasoning, it is not surprising that action-specific effects based on skill have been observed.

Catching or blocking a ball is a skill intensive task. When participants use a small paddle that causes them to have poor performance when blocking a virtual ball, they report the virtual ball as moving faster than when they use a bigger paddle (Witt & Sugovic, 2010). Participants played a modified version of the classic computer game Pong in which the ball bounced across the screen at one of six speeds and they tried to block the ball with a small, medium, or large paddle. Initially, participants were trained on the fastest and slowest speeds of the ball. During test trials, after each attempt to block the ball, they would decide if the ball moved more like the slow speed on which they were trained or more like the fast speed on which they were trained. This decision is referred to as a **speed bisection task**. This process was repeated for a total of 360 trials with every combination of speed and paddle being presented 20 times. Participants were poor at blocking the ball with the small paddle (*M* = 60% of balls blocked) and excellent at blocking the ball with the large paddle (*M* = 98% of balls blocked). From the speed bisection data, the point of subjective equality (PSE) was calculated for each paddle size. The PSE is the speed at which the ball looked equally fast and slow. The mean PSE when participants used the small paddle was 1.24 m/s whereas the mean PSE for the large paddle was 1.35 m/s. This result means that when the participants used the large paddle they saw the ball as moving faster compared to when they used the small paddle (Witt & Sugovic, 2010). I will refer to this trend in the future as the **paddle effect**. The paddle effect may seem confusing because the PSE is higher for the large paddle condition, but the proper way to think about PSE is that the higher the PSE, the more speeds are identified as slow. There were more speeds below the PSE, i.e. slower, when using the large paddle compared to when the participants used the small paddle. If we created a bar graph with number of speeds identified as slow by the participant, the bar graph would be taller, thus a bigger number, for the large paddle than for the small paddle. The paddle effect was replicated in a follow up experiment with the same trend in results (small paddle PSE: *M* = 1.26 m/s, large paddle PSE: *M* = 1.36 m/s).

This same trend in results was once again found when a bar was placed behind the paddle. This bar ran the vertical length of the projected area. The addition of this bar minimized the optical differences between the paddles. Specifically, in the original experiment, there was more luminance contrast between the ball and the big paddle due to the larger surface area than between the ball and the small paddle. Given the stance that perception is encapsulated and should therefore not be influenced by information such as a person's ability to act, one must consider alternative explanations for these effects on perceived speed. Perhaps the differences in luminance contrast accounts for why the ball appeared slower when playing with the big paddle. By adding the bar, this eliminated differences in luminance contrast. However, it did not eliminate the effect of paddle size on perceived speed: the ball still appeared slower when playing with the bigger paddle. This result is consistent with the claim that ease to block the ball influences perceived speed. Since this original set of studies (Witt & Sugovic, 2010) the pong paradigm and the basic finding that performance influences perceived speed has been replicated and expanded upon (Witt & Sugovic, 2012, 2013a, 2013b, 2013c; Witt, Sugovic, & Taylor, 2012; Witt, South & Sugovic, 2014).

Other realms in which performance and skill influence perception includes golf (Witt, Linkenauger, Bakdash, & Proffitt, 2008; Witt, Linkenauger, & Proffitt, 2012; Wood, Vine, & Wilson, 2013; Canal-Bruland, Zhu, van der Kamp, & Masters, 2011), darts and target shooting (Canal-Bruland, Pijpers, & Oudejans 2010; Wesp, Cichello, Grazia, & Davis, 2004; Canal-Bruland & van der Kamp, 2009; Lee, Lee, Carello, & Turvey, 2012) and goal kicking both in American football and soccer (Witt & Dorsch, 2009; Jin & Lee, 2013). In each of these cases, athletes who were playing better than others perceived their target to be bigger.

Non-perceptual Accounts of Action-Specific Effects

So far action-specific effects have been discussed in terms of action's effect on spatial perception. However, some researchers argue many of these effects can be explained via non-perceptual mechanisms. Instead of changing perception, participants are responding to experimental demands or outside pressure to change their responses. A classic example of this would be the Asch (1956) conformity studies. Participants judged the length of lines as same or different. Some participants changed their response based on the judgments of confederates who were thought to be co-participants. While their response during the task changed, there was no evidence to suggest their perception of the line changed. Action-specific effects are sometimes argued to be the same as the effect found in this study. Participants are responding how they believe they should respond or based on pressures to respond in a specific manner, but perception itself is unchanged. This is referred to as a response bias (see *Figure 1*). If this concept is yet unclear, perhaps another example will help clarify. McGinnies (1949) presented individuals with taboo or neutral words and had them say the word when the recognized the word. When reaction times were analyzed, he found that participants took longer to respond to taboo words compared to neutral words. He interpreted this as the visual system protecting the mind from potentially harmful stimuli by delaying the processing of the stimuli. Perceptual defense as it was termed was exciting for many researchers until Zajonc (1962) found that the slower reaction time was due to the participants' hesitancy to say the taboo words out loud. Thus, perceptual defense is an example of when the participants' responses are biased (slower responses due to unease) but their perception (recognition of the word) was not affected.

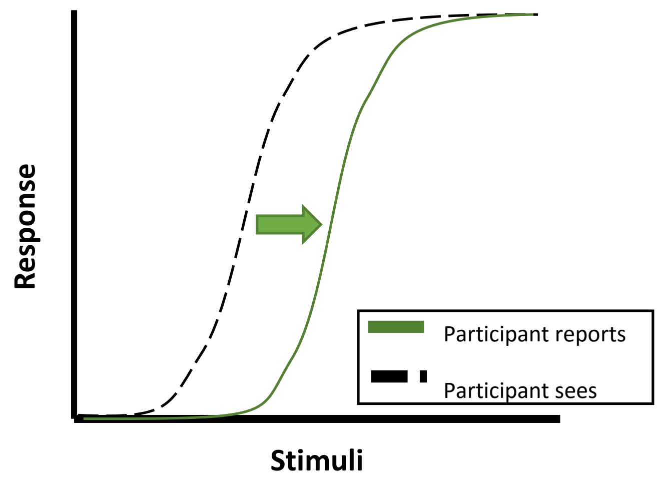

Figure 1: This figure shows hypothetical data in a generic perception experiment. In typical experiments, the stimuli vary along a given dimension, such as distance or speed, and this is usually plotted along the x-axis. For each instance, participants make some kind of response such as whether the length of a line is long or short or whether the speed of the ball is fast or slow. In experiments such as these with binary responses, the y-axis is proportion of times the participants responded, e.g., long or fast, and the data tend to follow the ogive curves shown here. Response biases (green arrow) alter how participants respond. So despite participants seeing the stimuli as represented by the dashed line, participants' responses are different and are represented by the green line. The critical issue is to determine the extent to which the response (green solid line) is dictated by perception (dashed black line) versus by response bias (green arrow).

The perceptual nature of action-specific effects has come into question. Answering this question has many important consequences. The answer to this question either supports or is evidence against a modular mind. If action information causes differences in visual perception, then the view that only optical information is processed to form visual perception is incorrect. This has consequences to how researchers theorize about how the brain operates and how researchers design perceptual experiments. Action information changing visual perception also has an important impact on careers in architecture, health and exercise science, ergonomics, and industrial engineering among many others (Witt, Linkenauger, & Wickens, in press). The perceptual nature of action-specific perception effects is an important question and as a result it is necessary to address challenges to the perceptual nature of these effects.

Some researchers have suggested that cognitive cues, such as feeling a hill would be hard to climb, could bias people's responses (Woods, Philbeck, & Danoff, 2009). In this scenario, an individual pre workout and post workout would look at a hill and perceive it the same way, but because they feel like the hill would be harder to climb post run, they report the hill being steeper than they actually see it. This effect would be especially present if the participants are encouraged to account for these cognitive states such as if the researcher said "In your current state, could you climb the hill? As a follow up, how steep does the hill look in degrees?" Less obvious cues such as researcher affect may give the participant some clue as to how they want the participant to respond (Woods et al., 2009). An easy way to avoid this issue is to give clear instructions that do not lead the participant to respond in any specific manner and to present the instructions for a given task at the beginning of the experiment with randomized within subject blocks. This allows for uniform instructions to a single participant for both conditions of a given experiment.

Even without misleading instructions, however, a participant may understand what the researcher wants based on the demands put on the participant. For example, when participants wore a heavy backpack and estimated hill slants but the backpack was either explained away as necessary for carrying equipment (Durgin, Baird, Greenburg, Russell, Shaughnessy, & Waymouth, 2009) or instructions indicated to completely ignore the backpack when making estimates (Durgin, Klein, Spiegel, Strawser, & Williams, 2012) there was no effect of backpack on hill. Inclusion of experimental demands in a linear hierarchical model also explained more variance for the given hill slant estimates than a model without experimental demands (Durgin et al., 2009). This suggests that the effect of backpacks on estimated hill slant was not perceptual, but a response-based effect. The participants "figured out" how they should respond to satisfy the researcher's expectations and did so. A similar result was found as a potential explanation for glucose's effect on hill slant. Belief of whether or not sugar should affect perception of hill slants was a more significant predictor of hill slant estimates than actual sugar consumption (Shaffer, McManama, Swank, & Durgin, 2013). This result suggests that what people believe affects their responses. The change in responses due to belief is a response bias alternative to action affecting perception.

However, experimental demands do not appear to sufficiently explain action-specific effects in all domains. Within the pong paradigm, participants were explicitly instructed to be very careful to classify all fast speeds as fast or all slow speeds as slow, thus purposefully instilling experimenter demands so as to measure the participants' willingness to change their answers. Even after accounting for willingness to alter answers based on instructions, the paddle effect was still found (Witt & Sugovic, 2013b). There was an overall shift in PSEs for the slow instruction group compared to the fast instruction group, but the difference in PSEs between the large paddle and the small paddle, the paddle effect, did not interact with instructions. The paddle effect was even present in those individuals that were classified as noncompliant, i.e. less willing or unwilling to make adjustments to their speed estimates based on instructions. Compliancy was determined by a median split in mean PSE divided by instruction type. For example, if someone was instructed to be sure to identify the fast speeds and had a lower mean PSE than the median PSE for individuals with the same instructions then they were classified as compliant because they followed instructions and classified the speeds accordingly. Participants in the fast instruction group whose mean PSE was greater than the group's median PSE were classified as non-compliant because they did not classify speeds according to the instructions. The paddle effect was equally present in both compliant and non-compliant participants, and was still present in those individuals who refused to alter their responses based on the demands of the experimenter (Witt & Sugovic, 2013b). What this suggests is that although people are sensitive to experimental demands, this does not account for the paddle effect. Additionally, if the paddle effect was only due to experimenter expectancy effects, then participants should show the same trend in responses when watching a computer as they do when they watch a partner play the pong paradigm. Yet, that is not the case (Witt et al., 2012). Participants did not show the paddle effect when watching a computer perform the pong paradigm, yet all the same experimental demands are present. The task, visually speaking, is the same as when they watch a co-participant perform the task and when they perform the task themselves. All the information that the participant would have in the computer scenario to see what the experimenter wants is present to the same degree as in the non-computer scenarios. This does not mean that there could not be some complex form of experimenter expectancy or response bias present, but it does cast doubt on this account of action-specific effects.

Additionally, studies using quasi-experimental designs, which therefore minimize experimenter expectancy effects, also show action-specific effects on perceptual judgments. In one experiment, some participants chose a snack item before making a staircase slant estimation. Others made the slant estimation and then chose a snack item. Regardless of order, individuals who estimated the stair case to be steeper chose more calorically dense snacks than individuals who rated the stairs as less steep (Taylor-Covill & Eves, 2014). If the participants were only responding to experimental demands, then this relationship should not be present when the participants choose their snack item before making slant judgments because they are unaware of the slant estimation task. It may also be difficult for individuals to calculate the caloric intake of the food items to purposefully bias their responses. While the current project addresses response bias accounts of action-specific perception effects, it is important to acknowledge these are not the only challenges leveled against the action-specific account.

While response bias accounts of action-specific perception are the primary challenges to action-specific perception being addressed, it is important to spend some time acknowledging other challenges. One such challenge is how researchers can properly measure a distortion in perception (Firestone, 2013; Firestone & Scholl 2014). For example, a common way to measure visual perception is via visual matching. This measurement asks the participant to make distance A perceptually equal to distance B, or adjust line A until it is as long as line B. Sometimes the participant instructs an experimenter who adjusts the distance, size, or any other perceptual property of interest. If perception is different and the experimenter tries to have individuals visually match what they are seeing to another object, that other object should have the same perceptual differences. If this occurs, then there should be no readily apparent evidence of any perceptual differences because such an effect could only be measured by comparison. For example, when participants hold a rod that limits their pass-ability through an aperture and make perceptual judgments on another aperture manipulated by an experimenter as a comparison, then there should be no action-specific effect because the width of both apertures should be scaled to account for the limited pass-ability. However, such an effect was found using the exact scenario just described (Firestone & Scholl, 2014). The reasoning for why this should not occur is sound, however when other studies control for the comparison stimuli properly, action-specific effects occur. For example, when reaching with tool is evaluated using perceptual matching the dimension of evaluation is changed from the y-axis along which the perception is occurring to the x-axis along which the tool should have no effect and thus no distortions should occur (Witt et al., 2005).

Another concern is how one can compare actions when the distances for each action are scaled by different metrics (Firestone, 2013). If one is deciding between whether to reach for an object or walk to said object, say the eternal struggle of the remote being on the coffee table instead of on the arm rest, how can the brain properly compare the distances between two intended actions when the walking distance is scaled for walking and the reaching distance is scaled for reaching? Essentially, what is happening in this scenario is the perceiver is comparing 12 inches to 30 centimeters without having a conversion between the two. While this concern is valid, in realistic scenarios it is unclear if this is an actual issue. In order to be energetically efficient, reaching distance would first be evaluated and scaled to arm length, obstacles, etc. Once scaled, if the perceptual system represents the remote as farther than your arm can reach efficiently, then the natural course of action would be to either walk to the coffee table or possibly discover a new love for a T.V. show that at first seemed irritating by remaining were you are. The comparison between two dissimilar action scenarios is resolved when one scenario is not viable.

As a follow up challenge, it is unclear if these distortions in perception are enough to change behavior (Firestone, 2013). In situations such as taking the stairs or a readily accessible elevator, similar action scenarios, scaling based on action abilities may be a deciding factor. In this scenario, both should be scaled to things like energy expenditure, stairs: high expenditure, escalator: low. Indeed, in naturalistic settings individuals who take stairs perceptually report the stairs to be less steep than those who take an escalator (Eves et al., 2014). What this finding demonstrates is that there is a relationship between how steep stars are and the behavior of climbing the stairs.

Are all concerns for action-specific perception resolved by this line of reasoning? They are most definitely not, but the beauty of the scientific system is that theory yields to data. If the data supports an account, that account is further tested until the theory no longer is accurate and a new, more robust theory takes its place. Even if the perfect formulation of how action scales perception is not currently at hand, there is an abundance of evidence supporting there is some effect present and more evidence will lend credence to one theoretical account or an alternative. However, while these concerns are important to acknowledge and are a fruitful well of ideas for future research, they are not the focus of the current project. The current project addresses the claim that response biases are responsible for action-specific effects.

Despite previous attempts to limit response bias via experimental design, it is possible that response biases are still present. Experimental demands and response biases can never fully be accounted and it would be illogical to say that there are no response bias effects present within action-specific studies. The question is, to what extent can action-specific effects be explained by response bias versus genuine effects on perception? To further investigate this question, I drew from the literature on crossmodal perception, which has had to wrangle with the same question.

Crossmodal Perception and the Use of Feedback

Crossmodal perception research finds that information from one sense can systematically bias another sense instead of being processed independently of one another. For example if a light source varies in brightness, then individuals have an easier time discriminating differences in pitches of simultaneously presented sounds. This only occurs if the brightness increases as pitch increases. If pitch decreases and brightness increases then participants' pitch discrimination decreases (Marks, Ben-Artzi, & Lakatos, 2003). Other effects such as audition influencing vision (Abadi & Murphy, 2014; Lovelace, Stein, & Wallace, 2003; Vroomen & de Gelder, 2000) and touch influencing vision (Matsumiya, 2013) have been observed.

Much like action-specific perception, crossmodal research has many skeptics. Some researchers claim that crossmodal effects are nothing more than response biases (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. Action-specific researchers can borrow from these strategies in order to address the same question – is the effect truly perceptual – with respect to effects of action.

One such technique that is particularly relevant for the current paper is the inclusion of accuracy feedback on the perceptual judgments of the participants. Accuracy feedback is providing information to the participant on the accuracy of their responses. When researchers provide feedback, they underscore the importance of accuracy over other demands. Although this method is not unique to crossmodal research, it has been used by researchers within this field to distinguish response bias effects from true perceptual effects. As stated before, the basic idea is that response biases change how people respond but not what they see. Feedback alters responses by changing the decision criterion. Decision criterion are factors that the decider uses to arrive at a specific decision. If the criterion currently used is to meet implicit experimental demands, feedback changes that criterion to meet the demands of the feedback. Thus, feedback changes decisions. Consequently, if an effect persists despite the inclusion of feedback, the effect is considered to be perceptual.

An example of the use of feedback to address the issue of whether an effect is perceptual or due to response bias was used in the flash-sound effect. Two concurrent auditory beeps when presented with a single flash of light biases individuals to report seeing two flashes of light (Shams & Shimojo, 2000). This flash-sound effect withstands accuracy feedback (Rosenthal, Shimojo, & Shams, 2009). Individuals sat in front of a computer screen and were told to identify how many flashes occurred on the screen, one or two flashes. At the same time, there was no sound, one beep, or two beeps. The participants were told that this sound was not important to the visual task and that the sound would be "distracting" and were told to ignore it. After participants judged the number of flashes, they were provided immediate accuracy feedback with the words "right" or "wrong" appearing on the screen. When receiving accuracy feedback information, participants were just as likely to report two flashes when a single flash was presented with two beeps as participants who received no feedback. Receiving feedback that their judgments were wrong did not detract from the effect of two beeps on reporting the perception of two flashes. This finding suggests that the auditory beeps influenced perception of the flashes, rather than influencing the responses.

The failure of feedback to reduce the sound-flash effect was also present when the sound trials were presented blocked instead of randomly and when feedback was made a within-subjects factor. When participants were monetarily incentivized to change their responses based on feedback, accuracy feedback did diminish the number of reported "two flashes" when two beeps was present; however, the illusory effect was still present even if it was diminished. Additionally, multiple participants reported “notic[ing] a subtle phenomenological difference between the percepts induced by the actual and illusory flashes and learned to discriminate between them…, though they continued to perceive the illusory flash” (Rosenthal et al., 2009, p. 192). Based on these results, the authors argued that sound can alter the processing of optical information in some circumstances and that feedback was an effective means of altering responses but not perception.

Feedback's Influence on Responses and Perception

There are numerous different forms of feedback. For the purpose of this study, I focused on immediate accuracy feedback. Immediate means the participants received feedback right after making judgments and accuracy specifies that the feedback was on the accuracy of their judgments. Immediate feedback as opposed to delayed feedback has been shown to be more effective in altering responses for the research design I am employing (Maddox, Ashby, & Bohil, 2003). There are different forms of immediate accuracy feedback. The feedback can either be true or false and symmetrical or unsymmetrical. Before diving into the nuances of true versus feedback, it is important to clarify symmetrical versus asymmetrical.

Symmetrical feedback is feedback that is provided across all conditions. For example, if a researcher investigated shape discrimination under blue, red, yellow, and green illumination, symmetrical feedback would mean feedback was provided in all four illumination conditions. Asymmetrical feedback would mean that feedback was provided in some conditions but not others, such as the blue and red condition but not the green and yellow. Now we are left with two final questions on feedback: What is true feedback and false feedback, and what effects do these types of feedback have on participant responses?

Immediate symmetrical true accuracy feedback and immediate asymmetrical false accuracy feedback were used during the current experiments. For brevity I will use the terms true feedback and false feedback and will note when the feedback was anything other than immediate symmetrical true accuracy or immediate asymmetrical false accuracy. True feedback is often used in decision making literature to remove response biases (Koehler & Harvey, 2004). True feedback is when feedback provides veridical information on that decision. For example, if participants had to judge the shape of an object, and responded with octagon when it was a hexagon, the true feedback response would be to say that their decision was incorrect.

True feedback has been shown to nullify response biases (Smillie, Quek, & Dalgleish, 2014; Tindale, 1989). See *Figure 2* for a visualization of how feedback affects responses. After creating a response bias which lowered accuracy during a Vernier task, see *Figure 3,* the participants were given true feedback (Herzog & Fahle, 1999). After they received true feedback, their accuracy scores increased. This is argued to be a removing of a response bias instead of creating one because when participants adjust their responses in accordance with the feedback, their responses become more veridical.

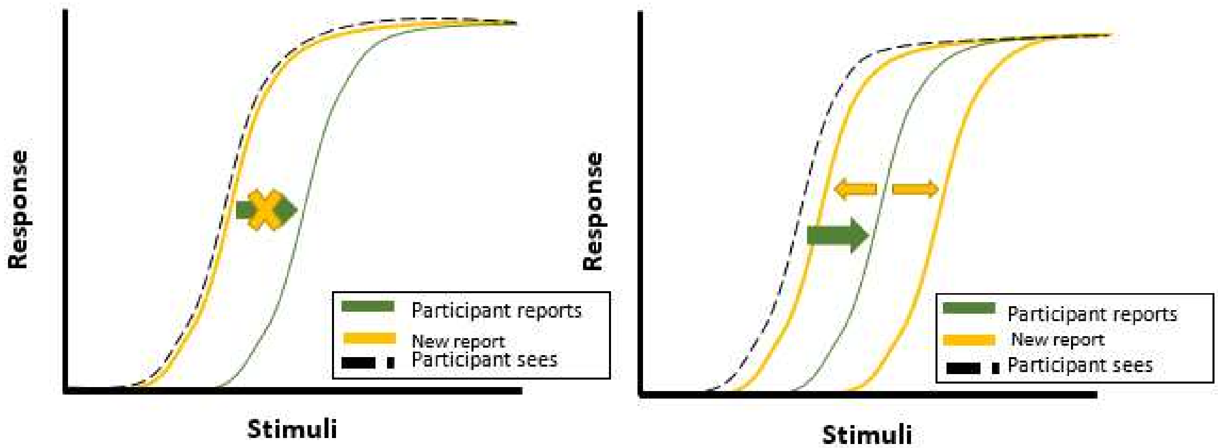

Figure 2: This figures show hypothetical data in a generic perception experiment. In typical experiments, the stimuli vary along a given dimension, such as distance or speed, and this is usually plotted along the x-axis. For each instance, participants make some kind of response such as whether the length of a line is long or short or whether the speed of the ball is fast or slow. In experiments such as these with binary responses, the y-axis is proportion of times the participants responded, e.g., long or fast, and the data tend to follow the ogive curves shown here. These particular graphs also show the distinction between what is seen (dashed black line) is and how the participant responds (green line). The green arrow represents the shift in responses due to respond bias. The two figures also show the two potential effects of feedback. Feedback can alter responses by eliminating response biases altogether (left figure) or causing an additional response bias (yellow arrows) in the same or in the opposite direction (right figure). The goal of true feedback is to remove response biases in order to get an unbiased measure of what the participant sees (left). The goal of false feedback is to create a response bias in the opposite direction or in the same direction to detract from or enhance a response bias (right).

Figure 3: The typical Vernier task presents two lines slightly offset from each other. Typical lines are shorter and thinner than the example ones provided. The participant has to say to which side the top line is offset from the bottom. This judgment is made from 2 meters away. This process is repeated 10,000 times in the case of Herzog and Fahle (1999). The right side shows the same stimuli rotated 90 degrees.

False feedback is when the participant is given nonveridical information to alter their responses. Referring back to the example shape judgment experiment, if the participant saw an octagon and said hexagon, the false feedback response would be to say that the decision was correct. False feedback is not always entirely false. If nonveridical information is only presented on the octagon trials but veridical information is presented on the circle or triangle trials, this is still considered false feedback. The key to false feedback is that it is trying to shift the participant's responses away from what is veridical. False feedback was shown to instantiate response biases (Herzog & Fahle, 1999). When performing the Vernier task, participants will alter their responses and have lower accuracy when given false feedback (Herzog & Fahle, 1999). This result was interpreted as the creation of a response bias via false feedback because the responses moved further from the true answer.

Interestingly participants' accuracy increased, or rebounded, once the false feedback was removed (Herzog & Fahle, 1999). While other researchers have not used a specific term to refer to this rebound, I will refer to this as a **feedback recovery**. Feedback recovery occurs when participants cannot maintain their change in responses due to a response bias once the false feedback is removed. The false feedback caused a change in responses, but without the constant information provided by the false feedback the participant cannot maintain those responses. Instead they revert back to responding how they see the stimuli. Feedback recovery was slow without any correcting feedback, but when participants are given true feedback after false feedback, their feedback recovery was quicker compared to when no feedback was given post false feedback (Herzog & Fahle, 1999). If the new way of responding that is achieved from feedback can be maintained in the absence of feedback, this would be evidence that feedback eliminated the previous response bias. In the case of true feedback, this would mean participants are responding to how they see the stimuli and not implicit experimental demands. However, this reasoning is only true if feedback does not alter perception. The logical next question is then, "does feedback affect perception?"

Generally, it appears that feedback can affect perception under two circumstances: 1) feedback allows the individual to attune or pay attention to the relevant stimuli that might not be easily apparent (Biederman & Shiffrar, 1987) and 2) feedback may facilitate a reorganization or reweighting of visual information (van der Kamp, Withagen, & de Wit, 2013). When participants were given feedback on what visual information helped characterize one class, male chicks, from another class, female chicks, the participants' performance on categorizing chicks improves (Biederman & Shiffrar, 1987). When chickens are born, identifying males from females can be quite difficult because of their underdeveloped sex organs. However, within large farms/factories, day old chicks must be sorted by sex with high accuracy to avoid unwanted copulation. People who are experts at this task are called sexers, and the task requires perceptual expertise because it is very difficult. When naïve individuals are asked to perform this task using general intuition, they perform at a mean accuracy of 60.5%. When these same individuals are given instructional feedback as to what distinguishes males from females based on expert sexers' descriptions, their performance increases to 84% accuracy, outperforming the experts who had 72% accuracy. The feedback provided by the sexers improved perception of chick sex because it helped the naïve participants to attune to key characteristics that were diagnostic of the sex of the chick. This refined attuning has been used as a classic example of how feedback can affect perception.

Feedback can also prompt a restructuring of the visual elements within a given scene. When participants are provided true feedback as to their judgments on the midpoint of Judd illusions, the illusionary bias as to the midpoint of the horizontal line disappears (van der Kamp et al., 2013). The fact that this effect persisted beyond the feedback period suggests that the feedback facilitated a difference in perception of the stimuli. This was interpreted as allowing the participants to reorganize/reweight the illusory visual information to increase their accuracy. It is unclear if this is a different process from attuning to different stimuli. However, one factor that may differentiate these effects is the time period over which these effects are studied. Generally those effects that report changing the weighting of visual stimuli occur over multiple days and the effects are relatively long lasting. Effects that often refer to a better attunement of visual stimuli occur within a single day and the longevity of these effects are unclear. Because of these differences in how feedback affects decision making and perception, it is important to examine the effects of feedback on a given effect as well as to examine feedback recovery, responses after feedback has been removed, in order to disentangle perceptual effects from response-based effects. Even if feedback eliminates an effect, if the effect re-emerges after feedback is removed, this is still evidence for a perceptual effect.

Feedback and Action-Specific Perception

To evaluate how much of action-specific effects are driven by response biases versus differences in perception, feedback was applied to the pong paradigm to see if the inclusion of feedback removes the paddle effect. As a reminder, the paddle effect is the difference in judged speed, i.e. PSEs, when participants use the large paddle versus the small paddle. When participants make speed judgments using the speed bisection task within the pong paradigm, feedback as to the accuracy of their judgments should reduce or eliminate the paddle effect if the paddle effect is driven by response bias. If the difference in reported speed is driven by experimental demands then the experimenter emphasis on accuracy should remove the reported speed difference. However, because there is no change in the speed of the ball between paddles, feedback should not affect the paddle effect if it is perceptual in nature. Furthermore, feedback recovery was used to test whether paddle size influences perception or responses. There is the issue of the paddle size being different, but when the bar behind the paddle is used, the visual differences are minimal. Thus, feedback's effect on perception should be minimal and any effects feedback has on the paddle effect are better explained via a change in response bias versus a difference in perception. True feedback should remove any response biases present and should also remove experimental demand effects. By providing feedback, the participant should assume what the researcher wants is for them to be as accurate as possible. By making feedback incentivizing, there should also be an internal desire to be as accurate as possible. In three experiments, the extent to which action effects can be explained by response biases was be evaluated. In Experiment 1, participants received true feedback along with pre and post feedback phases. In Experiment 2, participants received a full experimental session of true feedback to determine how much of difference prolonged feedback training had on speed judgments. Finally, Experiment 3 used asymmetric false feedback in an attempt to instill a response bias to counteract the paddle effect and used a multilevel linear regression model to examine the unique effects of feedback, paddle size, and their interaction on PSEs.

Experiment 1

This experiment explored the effects of feedback on the paddle effect in the pong paradigm. There were three phases: pre-feedback, feedback, and post-feedback. The pre-feedback phase establishes a baseline paddle effect for comparison to the feedback and post-feedback phases. The feedback phase when compared to the pre-feedback phase evaluates how much of the paddle effect can be explained by response bias effects. Finally, the comparison of post-feedback to the previous two phases determines if feedback recovery occurred. If feedback reduces the paddle effect, and there is no feedback recovery, this is strong evidence for a response bias account of the paddle effect. If feedback does not reduce the paddle effect or there is large feedback recovery then a primarily perceptual account of the paddle effect is more likely.

Method

Participants and design. Twenty-four (12 females) students participated in the experiment for course credit. There were two paddle length conditions (small and large) and three feedback phases (pre-feedback, feedback, and post-feedback) and all participants were exposed to every phase in that order.

Stimuli and apparatus. Stimuli were presented on a computer screen (1280 X 1024) with a 60 Hz refresh rate on 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 in any manner, such as a chin rest. A keyboard was directly in front of the monitor. The white ball was a circle 1 cm in diameter. The ball always moved from the left side of the screen to the right side of the screen at speeds ranging from 26.2 cm/s to 74.2cm/s. A white rectangle represented the paddle and was 0.86 cm wide and one of two heights: 1.86 and 9.28 cm. The paddle was on the right side of the screen overtop of a .86 cm wide and 18 cm tall white bar. This second bar covered the entire height of the screen. 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. This minimizes the visual discrepancies between the two paddle conditions.

Procedure. Participants completed the experiment one at a time. After giving consent, participants were seated in front of the screen. The participant was first exposed to the fastest (74.2cm/s) and slowest speeds (26.2 cm/s) three times each in a random order. Before each presentation the white words “this is the slow speed” or “this is the fast speed” appeared on the screen. After this initial exposure phase, the participants were tasked with identifying the fast and slow speeds. The participants began these training trials by pressing the trigger button on the joystick. After the ball completed its movement, the participants were prompted to identify whether the ball moved at the slow speed or the fast speed by the text “slow or fast” appearing on the screen. The participants indicated their response by pressing the corresponding labeled button on the joystick. After responses are made, the participants received feedback as to whether they made the correct speed identification with green “correct” text or red “incorrect” text appearing on the screen. Then the next trial begins. There were three trials each of the slow and fast speeds presented in random order. During the initial exposure and training phases the ball only moved horizontally across the screen. During the rest of the experiment, the ball moved both horizontally and vertically as if it was “bouncing” across the screen.

After the initial exposure and training phases, the participants completed the pre-feedback phase. There were 96 trials, 8 trials each of every paddle length and speed (26.2, 33.5, 41.5, 50.0, 58.7, and 67.5 cm/s) combination. Every combination of paddle length and ball speed was presented twice in a random order before the being presented again in random order. At the beginning of the trial, the ball, paddle, and bar appeared on the screen. The participant initiated the ball movement by clicking the trigger on the joystick. They then attempted to block the ball by moving the paddle vertically using the joystick. When the ball came into contact with the paddle or exited off the screen, the blocking task ended and the participants were prompted with the words "fast or slow". At this point, the participant indicated whether the ball they saw during this trial was more like the slow speed they were trained on or more like the fast speed. After making their response, no feedback was given and the next trial began.

The participant then completed the feedback phase. The feedback phase was exactly the same as the pre-feedback phase except after making their speed bisection judgment they received true feedback as to the correctness of their judgment. Of the six speeds, speeds 26.2, 33.5 and 41.5 cm/s are the speeds that are more like the slow speed whereas speeds 50.0, 58.7, and 67.5 cm/s are the speeds that are more like the fast speed. When participants incorrectly categorized a speed, such as saying the 33.5 cm/s ball moved more like the fast speed when in reality it moved more like the slow speed, the red text "incorrect" appeared on the screen along with the sound buzz.wav found on windows sound. Participants only received feedback when they were incorrect.

The participant then completed the post-feedback phase. The post-feedback phase was exactly the same as the pre-feedback phase in that no feedback was given about their speed bisection responses. After completion of this phase, the experiment ended and the participant was debriefed and thanked for their time.

Results and Discussion

Participants had lower performance on the blocking task when using the small paddle (*M* = 54.8% balls 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 manipulated performance.

Binary regressions of the speed judgments in SPSS were used to compute the point of subjective equality (PSE) for each participant for each paddle size for each phase of the experiment. This is the point at which participants reported the ball as moving equally fast and slow. As a reminder, a higher PSE indicates reporting the ball as slower. 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, $F(1, 23) = 63.96$, $p < .001$. With 95% confidence, the PSE for the large paddle is between 47.03 cm/s and 49.88 cm/s, and the PSE for the small paddle is between 43.02 cm/s and 45.14 cm/s. The mean difference between the large paddle and the small paddle was 4.38 cm/s. Participants had lower ball-blocking performance when using the small paddle and had lower PSEs which corresponds to more "fast" responses compared to when they used the large paddle. The perceptual account of this effect argues that the performance information alters the perceived speed of the ball. This is contrasted against the response bias account which suggests that the discrepancy in PSEs is from response bias and experimental demand.

There was a statistically significant difference between the PSEs for each phase, $F(2, 46) = 7.625$, $p = .01$ with the feedback phase ($M = 44.94$ cm/s, $SE = 0.48$) being faster than both the pre-feedback phase (46.28 cm/s, $SE = 0.71$) and the post-feedback phase (47.05 cm/s, $SE = 0.71$) (see *Figure 4*). Pairwise comparisons revealed that the feedback phase PSEs were significantly faster than both the pre-feedback (*Mean difference* = -1.88 cm/s, $SE = .578$, $p = .003$), and post feedback phases (*Mean difference* = -2.11 cm/s, $SE = .619$, $p = .002$). This means that while receiving feedback, participants reported more balls as moving faster than before and after feedback. This suggests that participants were attempting to incorporate feedback information into their PSE estimates and is suggestive of the feedback manipulation being successful. However, it is not clear, nor was it predicted, as to why feedback produced this specific pattern. The pattern does not seem to replicate in Experiment 2 and thus will not be further discussed.

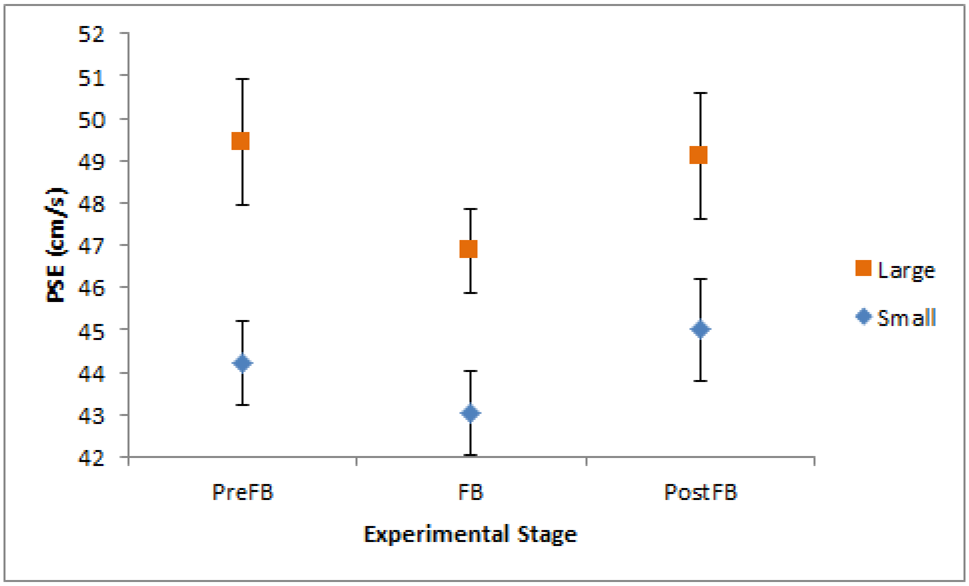

Figure 4: PSEs plotted for each of the three phase (PreFB: pre-feedback, FB: feedback, and PostFB: post-feedback) and both paddle sizes (orange: large paddle, blue: small paddle). Within subject error bars are standard error of the mean. Although feedback shifted overall responses, the effect of paddle on perceived speed (i.e. the difference between the large paddle and the small paddle in each phase) is still significant.

The critical analysis was of the interaction. If response biases account for the paddle effect, there should have been a significant interaction between feedback and paddle length. There was no significant interaction between paddle and phase, $F(2, 46) = 1.19$, $p = .25$. What this result suggests is that although feedback had an effect on PSE scores as indicated by the main effect for phase, feedback did not significantly alter the PSE differences when participants used the small paddle compared to the large paddle. This result demonstrates that feedback did not reduce the paddle effect. Participants still reported the speeds as moving slower in the large paddle condition ($M = 46.85$ cm/s, $SE = 1.04$) compared to the small paddle condition ($M = 43.02$, $SE = 0.97$) in the feedback phase of the experiment, $t(23) = -6.76$, $p < .001$. This supports a primarily perceptual account of the paddle effect over a primarily response bias account. If the response bias account was true, we should see an interaction between paddle length and feedback and the t test for the differences in PSEs between the small and large paddles in the feedback phase should be insignificant.

That feedback did not eliminate the paddle effect supports a perceptual account over a response-based account. However, another explanation is that participants ignored the feedback altogether. To examine whether the feedback was ignored or effective in altering responses, the raw data was examined instead of the PSEs. From the raw data, accuracy scores for each trial were calculated. If the ball was moving at one of the slower three speeds, the response was coded as correct if the response was "slow" and incorrect if the response was "fast" and vice-versa for the three fastest speeds. Mean accuracy was calculated for each participant for each quartile of trials for each phase (see Figure 5). To account for time to adapt to the feedback and for potential carry-over effects from phase to phase, the mean accuracy score for the last 3 quartiles for each phase was computed. Feedback improved mean accuracy scores (M = 88.7%, SD = 5.0%) more than the pre-feedback (M = 85.6%, SD = 4.3%), and post feedback (M = 86.7%, SD = 5.1%) phases' speed judgments F (2, 46) = 3.38, p = .04 (see Figure 5). Pairwise comparisons revealed that there was a significant difference between the pre-feedback and feedback phase, p = .03, but not between the feedback and post feedback phases, p = .19. This improvement in accuracy scores indicates that feedback successfully altered responses in the expected direction. Thus, feedback affected responses. As a reminder, feedback was used to remove response biases and experimenter expectancy effects. If these effects were the primary contributor to the paddle effect, the paddle effect should have diminished or disappeared during the feedback trials and not reemerged during post feedback. The fact that it did not paired with the evidence that feedback successfully altered responses suggests that the paddle effect is primarily due to perceptual differences in perceived ball speed.

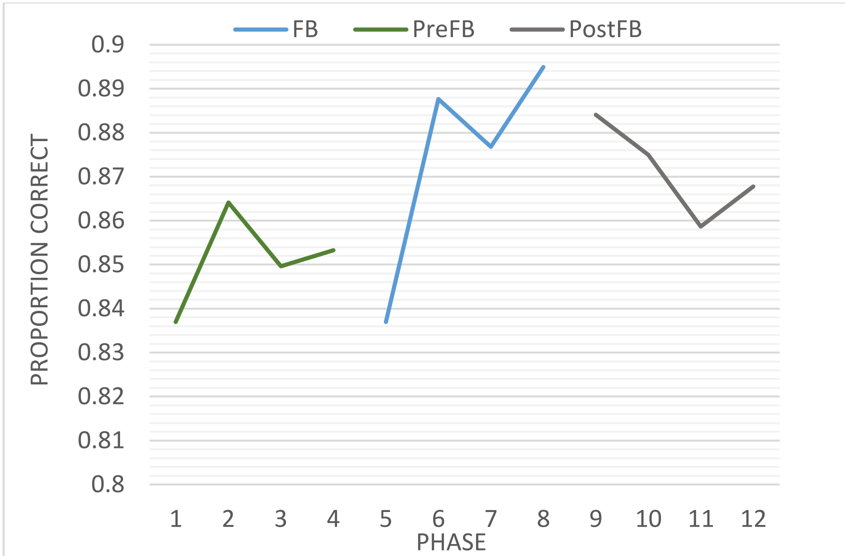

Figure 5: Accuracy scores for each phase (Fb: feedback, PreFB: pre-feedback, and PostFB: post-feedback) split into fourths for a total of 12 phases. Accuracy increased in phases 6-8 which correspond to the last 3/4ths of the feedback trials. Accuracy decreased again during the last 3/4ths of post-feedback trials (9-12).

Experiment 2

It might be the case, that while feedback was effective in producing some change to responses, it still might not have been effective enough to override any task demands due to paddle size. This may result from a) lack of motivation to incorporate feedback information into their responses or b) not enough trials to see the full effects of feedback. To address these issues, in Experiment 2 participants received a full experimental session of true feedback along with a brief question at the end evaluating how incentivized they were by the feedback.

Method

Participants and design. Seventeen participants (eight females) completed this experiment in exchange for course credit. There were two paddle length conditions (small and large) and participants received true feedback on the accuracy of their judgments throughout all experimental trials.

Stimuli and apparatus. The stimuli and apparatus were the exact same as Experiment 1.

Procedure. The procedure for this experiment was the same as Experiment 1 except for the following changes. Instead of 96 trials each for three phases, Experiment 2 only has one phase consisting of 288 trials. The participants received true feedback in the same manner as Experiment 1 except throughout the entire experiment. 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. Participants were able to respond with any U.S.

currency amount ranging from $0 to $5. Participants reported the amount they would be willing to return to the researcher, who recorded the value alongside participant demographic information.

Results and Discussion

PSEs were calculated in the same manner as Experiment 1 for each participant and each paddle size. These PSEs were then submitted to paired samples t-test. The effect of paddle on PSE was significant, $t(16) = -5.934$, $p < .001$, with PSEs when using the large paddle ($M = 49.4$ cm/s, $SE = .54$) being higher than PSEs when the small paddle was used ($M = 45.6$ cm/s, $SE = .51$). This suggests that feedback did not significantly change the PSEs between the two paddle conditions. It may be the case that the effects of feedback took time to become effective. Trials were split into three phases corresponding to the first third of trials, second third, and last third. A repeated measures ANOVA was conducted with paddle length (small vs. large), and phase (first, second, and third) as within subject factors. Paddle length had significant main effect on PSEs, $F(1, 16) = 35.11$, $p < .001$. There was no main effect for phase, $F(2, 32) = 1.94$, $p = .16$, with the mean PSE for phase one being 47.3 cm/s, phase two being 47.3 cm/s, and phase three being 48.2 cm/s. This suggests that participants' mean PSEs for each phase remained relatively consistent across time. Critically, the interaction between paddle length and phase was also not significant, $F(2, 32) = 1.57$, $p = .22$. *Figure* 6 displays the results of Experiment 2 split by phase and Experiment 1 results split by phase. This result suggests that even after approximately half an hour of accuracy feedback, participants were unable to become accurate enough to eliminate the paddle effect. Once again, if the paddle effect was primarily a response bias and not a perceptual bias, then true feedback should cause the mean PSEs for the two paddle conditions to become equal. Thus, there should be no difference in the mean PSEs between the large paddle and small paddle conditions if the paddle effect is a response bias. The results of the current experiment supports a primarily perceptual account of the paddle effect rather than a response bias account because even during the last third of the experiment at which point the participant has received 192 trials of possible feedback and continues to receive feedback, the paddle effect is still present.

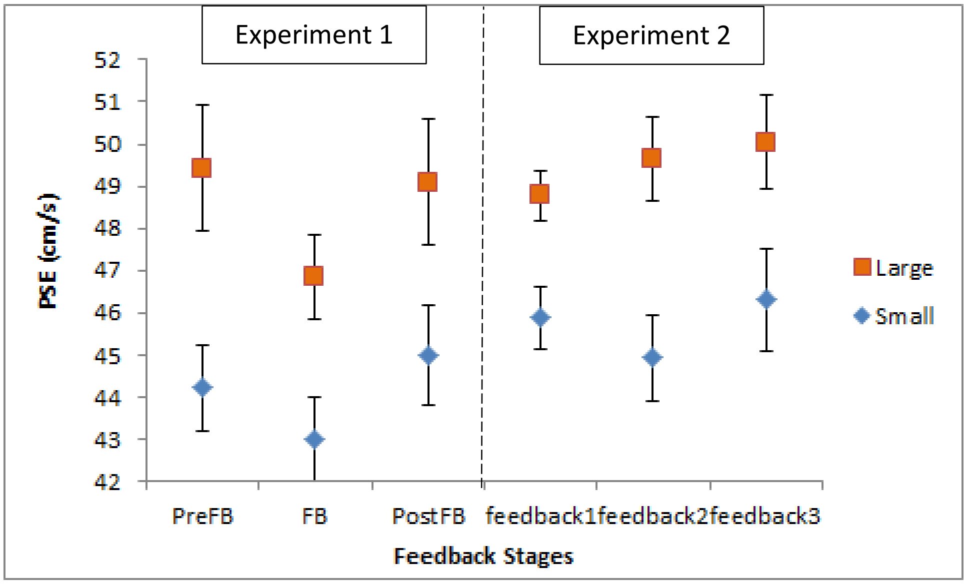

Figure 6. PSEs for Experiment 2 (feedback 1: first third of feedback trials, feedback 2: second third, and feedback 3: last third) and paddle plotted with Experiment 1 results. Error bars are 1 SEM calculated within-subjects.

Albeit unlikely, it is possible that participants were motivated enough to change their responses based on a purported response bias due to paddle size but not motivated enough to alter their responses based on the feedback. In this scenario, the paddle effect is primarily due to response bias and although participants have the means to change their responses, they do not feel the need to alter their responses based on the feedback. To address this potential concern, the question asked at the end of the experiment evaluated motivation directly. On average, participants were willing to return just over half of the offered money to remove the annoying buzzing sound that accompanied incorrect responses (*M* = \$2.94, *SD* = \$1.85). Additionally, there was a negative correlation between the amount of money a participant was willing to return and the accuracy of their judgments, $r(17) = -.48$, $p = .05$. This result means that individuals who heard the buzzing noise more often, as a result of their lower accuracy, were willing to part with more free money to get rid of the noise compared to individuals who heard the noise less often. The fact that individuals were willing to give back a mean of $2.94 dollars, more than half the money, back to the researcher to remove the noise suggests that participants were not wanting to hear the noise. Because they did not want to listen to the noise, the feedback should have had a high incentive. Simply by following the feedback instructions and changing their responses, the participants could have removed the noise from the experiment by performing perfectly and thus not have listened to it again. The results of Experiment 2 support a perceptual account of the paddle effect over a response bias account. Even with 288 trials of incentivizing true feedback, participants still reported the ball as moving faster when using the small paddle compared to when they use the large paddle.

Experiment 3

In Experiment 3 participants received false feedback in an attempt to directly counteract the paddle effect. In this scenario, participants received feedback in the large paddle condition that would cause them to rate more speeds as faster whereas they received feedback in the small paddle condition that would cause them to rate more speeds as slower. By manipulating the feedback in this manner, the participants were not asked to be as accurate as possible. Instead, the easiest way to not hear the buzzing noise would be to respond with more "slow" responses whenever the participant used the small paddle and with more "fast" responses whenever they used the large paddle. By pitting feedback against the paddle effect, I was able to examine the amount of variance explained independently by the paddle size, the feedback given to the participant, and the interaction between them. If the interaction term explains more variance than either paddle size or feedback independently, this would suggest that feedback and paddle length are using the same mechanism to alter responses. This would support a primarily response bias account of the paddle effect. If paddle size explains more variance than the interaction term, this is evidence towards a predominantly perceptual account of the paddle effect.

Method

Participants and design. Twenty-four participants (11 females) completed this experiment in exchange for course credit. This was a 2X2 factorial design with two paddle length conditions (small and large) and two feedback phases (feedback and post-feedback). Participants were exposed to each phase in that order.

Stimuli and apparatus. The stimuli and apparatus for this experiment were the exact same as Experiments 1 and 2.

Procedure. The procedure for this experiment was the same as Experiment 2 except for the following changes. Instead of one feedback phase consisting of 288 trials, there was two phases, feedback and post-feedback, each consisting of 144 trials. Feedback was given during the feedback phase but not during the post-feedback phase. Unlike in the previous experiments, here the feedback was asymmetric false feedback instead of true feedback. When using the small paddle, participants received feedback that they were incorrect when they responded “fast” for the 4 slowest speeds (speeds 26.2, 33.5, 41.5, and 50.0 cm/s), and no feedback for any other responses. This means that although the speed 50.0 cm/s is 1 of the 3 fast speeds, a “fast” response was presented as incorrect. When using the large paddle participants received feedback that they were incorrect when they replied “slow” for the 4 fastest speeds (speeds 41.5, 50.0, 58.7, and 67.5 cm/s.) Note that participants received opposite feedback for the two middle speeds (41.5 and 50.0 cm/s) depending on which paddle was being used. In order for feedback to be effective, it must be at least moderately believable, which is why false feedback was only given for these 2 speeds. If when using the large paddle participants received feedback saying that their estimate of the 26.2 cm/s speed as being more like the slow speed was incorrect, the feedback may not be seen as valid. In such a scenario, the participant may disregard the feedback all together. The feedback design used struck a balance between being believable and encouraging participants to lower their speed estimates when using the small paddle and raise their speed estimates when using the large paddle.

Results and Discussion

PSEs were calculated using the same technique as Experiments 1 and 2 for each participant, each paddle length, and the first and second half of each phase of feedback. This means there was a total of eight PSEs for each participant. In order to evaluate the contributions of paddle length, feedback, and the interaction between them on PSEs, a multilevel multivariate linear regression model was built. This model treated correlation among PSEs from the same individual as a random effect. A base model (Model 1) of just the intercepts was first constructed, followed by Model 2 that included the key factors of paddle size and feedback, but not their interaction, and a block variable which controlled for differences in responses over time. Feedback is coded such that no feedback is 0 and feedback is 1. Paddle is coded with small paddle being 0 and large paddle being 1. Multiple coding methods were tested for the block variable since time is intrinsically linked with the feedback manipulation, i.e. feedback always came before no feedback. This included coding each block in ascending order (0,1,2,3), coding the first and second half of each feedback phase (0,1,0,1) and coding for when the paddle effect was predicted to be larger versus smaller (0,1,1,0). There were no significant changes in the key factors of the analysis based on the way the block variable was coded. As a result, the block variable was coded 0 for first quarter, 1 for second, 2 for third, and 3 for fourth quarter. A final model (Model 3) that included the interaction term was also constructed. Lower levels of AIC and BIC indicate a better model fit with BIC being stricter. The variables for all models are presented in *Table 1*.

Table 1

Multilevel Models and Variables

Variables	Model 1 *B*	Model 1 *SE*	Model 2 *B*	Model 2 *SE*	Model 3 *B*	Model 3 *SE*
Fixed Effects						
Intercept	46.60	0.08	41.84	1.49	42.11	1.51
Block			1.51**	0.50	1.51**	0.50
Paddle			2.75***	0.50	2.21**	0.70
Feedback			2.25*	1.11	1.71	1.22
Paddle X Feedback					1.08	0.99
Variance Components						
Individual intercept *SD*	3.46		3.43		3.43	
Residual *SD*	3.82		3.41		3.39	
Fit Statistics						
AIC	1111.52		1080.42		1081.21	
BIC	1121.28		1099.97		1104.01	

Table 1: Variables for the three models constructed are in this table. AIC is Akaike information criterion and BIC is Bayesian information criterion. Variance components are the standard deviation of the residuals, what is not included in the model, and standard deviation for how much the intercept varies between individuals. All values (except fit statistics) are cm/s. Model 2 provides the best fit, but for theoretical reasons Model 3 was used as the final model.*$p < .05$. ** $p < .01$. ***$p < .001$.

Comparison of the model that includes the interaction variable to a model that does not include it (comparison of Model 2 and Model 3) suggests that the inclusion of the interaction variable does not provide a better model fit, X^2 difference (1) = 1.22, *p* = .27. This suggests that there is such a small interaction between paddle length and feedback factors that it should not be included in the final model trying to predict PSE. This result is evidence that there is such a small portion of the paddle effect that is explained by response effects that it is not even worth including as a predictor variable. However, because this interaction variable has theoretical implications, the final model, Model 3, included the interaction variable.

In Model 3, paddle length was a significant predictor of PSE, *t*(164) = 3.13, *p* = .002. Holding all other variables in the model constant, when the paddle size increased from the small paddle to the large paddle the estimated PSE for any given participant increased by 2.21 cm/s. With 95% confidence, the difference between the large and small paddle holding all other factors constant is between 0.84 and 3.57 cm/s. This means that holding feedback, and the interaction between feedback and paddle length constant, the effect of paddle size on PSEs is significant. This suggests that the difference in PSEs due to paddle size cannot be completely explained by response bias because if it was then the paddle length variable would not be significant after accounting for feedback and the interaction variable. Feedback in the final model was not significant, *t*(164) = 1.40, *p* = .16. With 95% confidence and holding all other factors constant, feedback changes overall PSE scores by between -0.66 and 4.08 cm/s. This result means that according to our final model, feedback did not make a significant difference in the participant's PSEs.

The feedback and paddle length interaction was not a significant predictor of PSEs, *t*(164) = 1.08, *p* = .28 and with 95% confidence and holding all other variables constant, the difference in the paddle effect when feedback was given compared to when it was not given is between -0.85 and 3.02 cm/s. Figure 7 shows the mean PSEs for the big paddle and small paddle conditions split by phase of feedback.

These results suggest that paddle length affected PSEs, but this effect was not moderated by feedback. A moderation of the paddle effect by feedback would support a primarily response bias account of the effect whereas the current result supports a primarily perceptual account of the influence of paddle length on PSEs. If the paddle effect, which is the paddle length variable in this model, was primarily a response bias then feedback should have reduced or eliminated the paddle effect. This would have emerged in the regression model as the interaction between feedback and paddle length. There was no statistically significant interaction and thus the paddle effect is not best explained by a response bias.

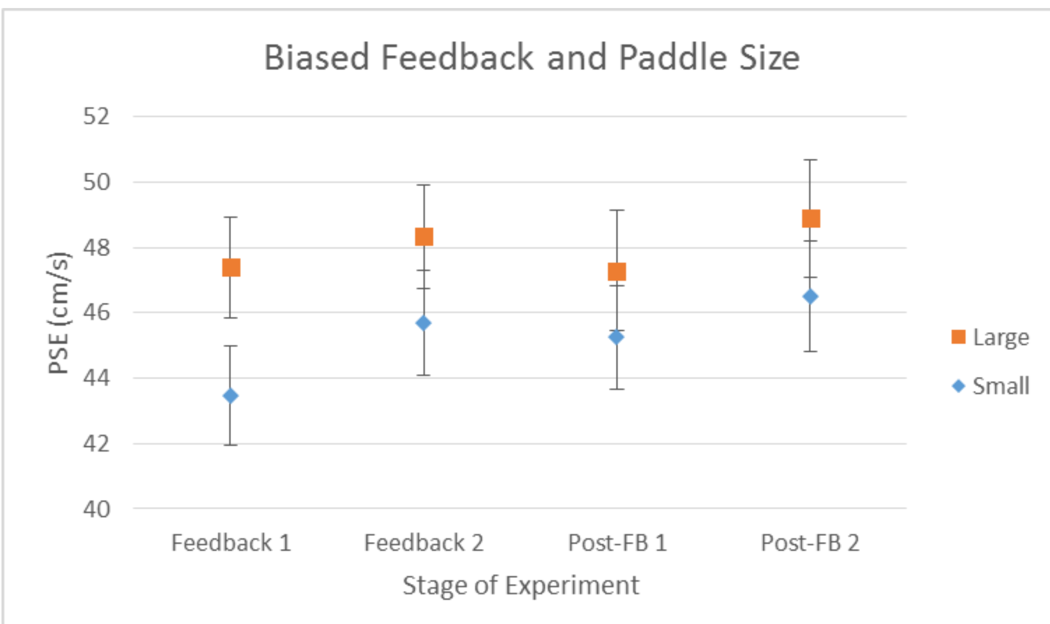

Figure 7: Mean PSEs for each paddle condition and feedback phase. Feedback 1 is the first half of the feedback phase and feedback 2 is the second half. Post-FB 1 is the first half of the no feedback phase and Post-FB 2 is the second half. Within subject error bars are standard error of the mean. Overall, the large paddle condition had significantly higher PSEs than the small paddle condition. There was no main effect for feedback nor was there an interaction between feedback and paddle length.

Partial correlations of paddle length, feedback, and their interaction variable with PSE suggests that paddle size uniquely accounted for approximately 7.48% of the variance within PSEs, $pr(192) = .27$, *p* < .001, whereas feedback uniquely only accounted for approximately 1.08% of the variance within PSEs, *pr*(192) = .10, *p* = .15, and the interaction variable only uniquely explained approximately 0.31% of the variance in PSEs, *pr*(192) = .06, *p* = .44. This means that of these three variables, paddle size accounts for a much higher proportion of the variance of PSEs than either of the other two once all other factors are controlled. The fact that paddle length accounts for more of the variance than the paddle and feedback interaction suggests that most of the paddle effect, the effect that paddle length has on PSE, cannot be accounted for by response bias, which the interaction variable represents. This supports a perceptual account of the paddle effect within the pong paradigm as opposed to a response bias account. It may seem surprising that paddle length accounts for such a greater amount of the variance compared to the interaction variable, but this is supported by the fact that there was not a significant interaction between paddle length and feedback in the first two experiments. This experiment found a much smaller paddle effect compared to the previous two experiments (Experiment 3 *M* paddle effect: 2.21 cm/s, Experiment 2 *M* paddle effect: 3.80 cm/s, Experiment 1 *M* paddle effect: 4.38 cm/s. However the 95% confidence interval for Experiment 3 which had the lowest mean difference (0.84 and 3.57 cm/s) and Experiment 1 which had the highest mean difference (1.80 and 6.86 cm/s) overlap. As a result, differences in the size of the paddle effect might simply be due to natural variance within the samples from the three experiments.

General Discussion

Do affordances penetrate spatial perception or are the studies that report such findings a result of response biases? These two accounts were pitted against each other in three separate experiments to evaluate the perceptual nature of action-specific effects on perception. Experiment 1 demonstrated that true feedback had a significant effect on accuracy scores but did not diminish the action-specific perception effect (the paddle effect). This finding suggests that while responses can be changed by giving true feedback, the action-specific effect did not diminish. If the action-specific effect did diminish, a response-bias account of the action-specific effect would be supported. The observed finding suggests that response bias effects are separate from action-specific perception effects and this supports an action-specific perception account.

Experiment 2 tripled the number of trials on which the participant could receive true feedback. By providing feedback, the participants could infer that the experimenter wanted them to be as accurate as possible. Participants were also motivated by the feedback, as evaluated in Experiment 2. However, even after 288 trials of motivating feedback, the paddle effect was still present. The fact that true feedback is effective at reducing response biases and the paddle effect was still observed is strong evidence in favor of a perceptual account of action-specific perception effects.

To further vet the paddle effect, false feedback was given in Experiment 3 that contradicted the paddle effect. This meant that if any part of the paddle effect was malleable to response biases, the paddle effect should diminish. Experiment 3 also calculated partial correlations to evaluate how much variance could be uniquely explained by the paddle effect and false feedback. Although the observed paddle effect was indeed smaller, the multilevel linear regression suggests that it is not due to the influence of feedback. The 95% confidence for the paddle effect for Experiment 3 overlapped with the

95% confidence interval for Experiment 1, suggesting similarly sized effects. Stacked on top of this mounting evidence is the fact that the paddle effect explained more variance than did the feedback and the interaction between the paddle effect and feedback was negligible. Experiment 3 also provides strong evidence that response biases cannot explain the action-specific effect observed in this study.

Feedback was a novel manipulation for action-specific perception studies. While previous studies have used various forms of experimental design to control or manipulate response bias effects, none have so directly tackled the response bias account. True feedback is a well-tested means of reducing response biases, and pitting feedback against purported action-specific perception effects provided corroborative evidence that the paddle effect is perceptual. This does not mean that there are no response biases in our experimental design. The important and exhilarating point is that response biases are not the primary explanatory factor of action-specific effects.

Having such strong and novel support for the action-specific perception account has far reaching implications. Perceptual researchers should turn a more critical eye to the Modularity of the Mind Theory (Fodor, 1983). A modular mind in which only optical information and limited prior knowledge combines to form visual perception is not supported by the current studies. Instead, a formulation of the mind in which visual perception receives input from adaptive sources such as action is more likely. The brain is not a computer with nodes that perform a set function regardless of the overall goal, but instead has regions that are dynamic and adaptive. Unlike a computer, the brain has a purpose. A computer has no purpose besides the wide and varied demands of the operator. The brain on the other hand has a very distinct purpose: survival. To that end, an evolved and adaptive perceptual system is not an all-purpose computing module. The perceptual system serves one purpose, survival, and integrating action information into sensory systems has adaptive advantages. It allows for easy, safe navigation and conservation of energy. Notice this is not an argument that everything influences visual perception. Only those factors that provide a meaningful adaptive advantage such as action information. This is also not a complete refutation of modularity, but rather demonstrates a need for the theory of modularity to be revised and expanded. Like any good theory, modularity should yield to data.

This research directly addresses the perception vs. response bias controversy within the action-specific perception field in a novel manner using established manipulations and thus pushes the argument further away from “WHAT are action-specific perception effects?” to “HOW does action effect perception?” and that transition is one full of possibilities. This does not mean that research should cease independently seeking out answers to the “what” question, but it does provide researchers with cause to investigate “how.” Up until now, investigating “how” could be costly for effects that may not have been perceptual, but hopefully with validation that action-specific effects are perceptual, more researchers and research can confidently investigate action-specific perception effects.

Beyond the implications for theory and research, the current findings that not all action-specific effects can be explained by response biases also have important applications. The United States is currently struggling with health and obesity. Much of the population lives in urban areas with confined avenues for movement and sparse opportunity for exercise. If action and perception have a relationship, then changing the design of parks to seem more easily navigable or making stairs visually appear easier to climb could have positive outcomes (Eves et al., 2014). For individuals undergoing physical therapy, making rehabilitation areas appear easier to navigate should increase the likelihood of the patient performing the necessary exercise for recovery. Work related injuries may be reduced by engineering consoles or cockpits not only around optical information and action information, but also by how those two key factors interact, i.e. action-specific information (Witt et al., in press).

While the current experiments are novel and informative, they cannot definitively answer the question of “are action-specific perception effects perceptual in nature?” However, they do provide robust evidence that action-specific effects are perceptual. People see in terms of their abilities to act.

This has widespread implications for perceptual research, applied psychological research, and various other fields such as engineering, health science, and architecture.

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