Published in final edited form as:
J Exp Psychol Hum Percept Perform. 2009 August ; 35(4): 1104–1117. doi:10.1037/a0013622.

The Various Perceptions of Distance: An Alternative View of How Effort Affects Distance Judgments

Adam J. Woods,
Department of Psychology, The George Washington University

John W. Philbeck, and
Department of Psychology, The George Washington University

Jerome V. Danoff
Department of Exercise Sciences, The George Washington University

Abstract

D. R. Proffitt and colleagues (e. g., D. R. Proffitt, J. Stefanucci, T. Banton, & W. Epstein, 2003) have suggested that objects appear farther away if more effort is required to act upon them (e.g., by having to throw a ball). The authors attempted to replicate several findings supporting this view but found no effort-related effects in a variety of conditions differing in environment, type of effort, and intention to act. Although they did find an effect of effort on verbal reports when participants were instructed to take into account nonvisual (cognitive) factors, no effort-related effect was found under apparent- and objective-distance instruction types. The authors' interpretation is that in the paradigms tested, effort manipulations are prone to influencing response calibration because they encourage participants to take nonperceptual connotations of distance into account while leaving perceived distance itself unaffected. This in no way rules out the possibility that effort influences perception in other contexts, but it does focus attention on the role of response calibration in any verbal distance estimation task.

Keywords

egocentric distance perception; effort; calibration; visual perception; instruction type

Space perception researchers commonly encounter people who say, "You should study me—my distance perception is terrible!" In experimental settings, however, the average participant can demonstrate remarkably good distance perception by walking accurately without vision to objects initially seen at distances up to 22 m or more (e. g., Loomis, Da Silva, Fujita, & Fukusima, 1992; Rieser, Ashmead, Talor, & Youngquist, 1990; Thomson, 1980, among a host of others). Informal discussion often reveals that people directly equate "poor distance perception" with their sense of unfamiliarity with assigning numbers to distances. Researchers, on the other hand, typically do not conceive of perceived distance as being so narrowly tied to one specific type of behavioral response. In fact, researchers have used a variety of behavioral methods to measure perceived distance in addition to blindfolded walking and verbal reports (see Loomis, Da Silva, Philbeck, & Fukusima, 1996, and Da Silva, 1985, for reviews). This illustrates that nonspecialists may have very different interpretations of "distance" and

Correspondence concerning this article should be addressed to Adam J. Woods, Department of Psychology, The George Washington University, 2125 G St., NW, Washington, DC 20052. ajwoods@gwmail.gwu.edu or woodsaj@gmail.com.
Jerome V. Danoff is now at the Rehabilitation Medicine Department, National Institutes of Health Clinical Center, Bethesda, Maryland.

"perceived distance" than researchers do. This article explores some possible conceptualizations of distance and their implications for space perception research.

For researchers, perceived egocentric distance (or simply perceived distance) is a representation of the distance between oneself and an object. Some previous models of visual space perception (e.g., Foley, 1991; Gogel, 1990; Landy, Maloney, Johnson, & Young, 1995) have conceived of perceived distance as the result of a series of processing stages: taking a set of stimulus cues as inputs, weighting these cues according to their reliability, and then combining the resulting weighted stimulus information. If an overt indication of distance is required, the perceived distance becomes the input to processes that generate and calibrate an appropriate behavioral response. If two behavioral measures happen to yield different patterns of responses, this kind of model could account for such differences in one of two ways: (a) One possibility is that changes in perceived distance were responsible. In this view, the two response measures may have somehow induced participants to weight the stimulus cues differently for each response type or differentially impacted the visual information their eyes picked up (e.g., by altering the pattern of eye movements). (b) A second possibility is that perceived distance was the same regardless of which response was used but that the responses were calibrated differently. In light of these two possibilities, differing response patterns for two behavioral measures should not automatically be interpreted as reflecting changes in perceived distance. Before reaching this conclusion, one must consider possible differences in response calibration. Although several factors have been suggested to influence response calibration (e.g., memory, context, and instruction type; Bingham, Zall, Robin, & Shull, 2000; Carlson, 1977; Foley, 1991; Lappin, Shelton, & Rieser, 2006; Tyer, Allen, & Pasnak, 1983), this issue has not been explored extensively, particularly in the context of verbal reports of egocentric distance.

Recently, Proffitt and colleagues have proposed that perceived distance is influenced by one's current physiological potential (perceived anticipated effort, or simply effort) to perform an intended action (Proffitt, 2006a, 2006b; Proffitt, Stefanucci, Banton, & Epstein, 2003; Witt, Proffitt, & Epstein, 2004; see also Fajen, 2005; Land, 2006; Yarbus, 1967). In this view, as effort associated with acting on a target (e.g., walking or throwing) increases, perceived target distance also increases. If this theory is true, a host of heretofore unrecognized effort-related factors might influence perception (e.g., sleep deprivation, emotional state, fatigue, age, injury, and so on). Proffitt's theory of distance perception differs from the serial, weighted averaging model presented earlier. He has argued that there are many reciprocal interconnections, functionally and neutrally, linking incoming visual information, the control of behavior, and nonvisual (cognitive) factors (Proffitt, 2006b); this being the case, he has posited that the mechanisms underlying distance perception should not be conceived as a series of encapsulated, sequential processing stages. Regardless of whether perceived distance is best conceptualized as a serial process or something more complex and interactive, there is little debate that nonperceptual (cognitive) factors can, in principle, influence overt behavioral indications without influencing perceived distance (Proffitt, 2006b).

In the current study, we examined the role of effort in distance perception in more detail. We hypothesized that at least in some circumstances, anticipated effort influences the calibration of responses, rather than perceived distance per se. In this article, we will distinguish among several possible positions concerning the role of effort and response calibration on judgments of egocentric distance. (a) At one extreme, which we will call the *effort-only* position, effort directly influences perception, and effort manipulations never result in changes in response calibration. Proponents of this position would no doubt accept the notion that behavioral responses must be calibrated in some way and, thus, that response calibration could in principle play a role. However, effort-only proponents would hold that the role of calibration is negligible in explaining effort manipulation effects. (b) At the other extreme, the *calibration-only* position, effort never directly influences perception, and all work by Proffitt and others supporting an influence of effort on perception is due to changes in response calibration. Both of these extreme positions are difficult to definitively rule out. We explicitly disavow both of these positions, however. Instead, we favor a more moderate, third position: (c) In this view, which we will call the *effort– calibration hybrid* position, effort does not unavoidably impact perception, but it certainly can influence it if conditions are favorable. This view not only admits the possibility of effort-related effects on perception but also the possibility that changes in response calibration might mimic effort-related effects while leaving perceived distance unchanged (a prospect first expressed by Proffitt; 2006b). This view does not deny effort-related influences on perception but instead focuses on determining the conditions under which such influences are manifested. This is the view we adopt in this article. Although we examine the role of response calibration in two particular methodologies that have produced effects of effort manipulations in the past, this should in no way be taken to imply that we believe calibration accounts for all effort-related effects.

To date, several published studies have produced results consistent with an effect of perceived anticipated effort on perception (Bhalla & Proffitt, 1999; Creem-Regehr, Willemsen, Gooch, & Thompson, 2005; Proffitt et al, 2003; Witt et al., 2004). Many of them have used methods designed to minimize changes in response calibration as an explanatory factor (e.g., by manipulating effort between groups or by using distractor tasks, so as not to call attention to the importance of the effort manipulation). Proffitt (2006b) mentioned that using converging methods is another way to deal with the response calibration issue: If effort legitimately influences perception, effort-related effects should be found with a variety of different effort manipulations and a variety of response measures (see also Loomis & Philbeck, 2008). This is certainly important to verify and may ultimately prove to be the strongest evidence in support of the effort–perception linkage. Of course, the converging methods might themselves be subject to response calibration changes. Even if multiple studies apparently converge to show effort-related effects on perception, this convergence would not necessarily lend force to the case in favor of effort effects if response calibration has not been ruled out in each of the converging methodologies. Taken together, these studies are suggestive that effort indeed affects perception, but the possibility that response calibration is changing in these situations is difficult to definitively exclude.

Response calibration could change in an experimental setting due to a variety of factors: error feedback, perceptuomotor adaptation, viewing context, safety concerns, social desirability, and so on. It might be accompanied by an explicit decision to alter one’s behavior, or it might occur entirely without awareness. One particular source of changes in response calibration (though certainly not the only one) is demand characteristics (e.g., Carlson, 1977; Tyer et al.,1983). The term *demand characteristics* refers to conditions in which the experimental predictions are transparent to participants, either because the experimenter or design somehow communicates the predictions to the participants or because participants’ independent hypotheses about the predicted outcomes happen to match those of the experimenter. In either case, participants could respond in a way that supports these predictions without there being any change in perceived distance per se. Thus, experiments must be conducted in such a way that demand characteristics are minimized or eliminated as a possible influence on the data. Experimenters can minimize demand characteristics by giving the same, neutral instructions to all participants and by remaining neutral in affect when interacting with participants. Using a double-blind methodology, in which both the participants and the experimenters are naïve concerning the experimental predictions, can also lend confidence that experimenters are not consciously or unconsciously communicating the predictions to participants.

To complicate matters, however, demand characteristics might influence the data even if all participants are given the same, neutral instructions. One way this might happen is if participants do not interpret the instructions in the way the experimenters intend. In the context of distance perception research, the seemingly straightforward instruction to report the distance of an object can be interpreted in a variety of ways by naïve observers. When driving toward a mountain that one knows from experience is *physically* 40 miles away, the mountain might *visually appear* to be only 5 miles away. However, when stuck in traffic, one may *feel* that the mountain is 80 miles away in a more abstract, cognitive sense. In an experimental setting, if the instructions do not specify which connotation of distance should be considered when responding, observers are left to their own assumptions about which interpretation of distance the experimenters might mean (Carlson, 1977). Depending on their assumptions, observers could produce systematically different responses (i.e., change their response calibration) even though their perceived distance remains constant. We propose that, at least in some situations, effort manipulations can affect observer assumptions about how to interpret the task when responding, and changes in response calibration based on these assumptions must be carefully considered when investigating the potential influence of effort on perception.

The following five experiments tested these ideas. In two experiments, we tried and failed to replicate key experimental results interpreted as evidence that effort directly affects perceived distance. In two further experiments, we evaluated two factors (intention to act and testing environment) that might explain our inability to replicate the findings of the Proffitt group (Proffitt et al., 2003; Witt et al., 2004). We were still unable to replicate an effect of effort on distance perception but found results indicative of a change in calibration of verbal responses. In the fifth experiment, we tested whether experimental instructions might produce effort-related effects by emphasizing nonvisual (cognitive) interpretations of the distance estimation task. We found an effect of effort only when observers were instructed to take into account nonvisual factors. These changes in verbal distance estimates were not reflected in blind-throwing performance, suggesting that only the calibration of verbal estimates was altered, not perceived distance.

Following Proffitt et al. (2003), we balanced sex between groups in all experiments and included this as a between-groups variable. A total of 168 individuals (mean age, 19.4 years; range, 18–28 years) volunteered to participate in exchange for course credit. All were naïve concerning the purpose of the studies, and all participated in only one study apiece. We report partial eta-square values in the statistical analyses to give an indication of effect sizes.

Experiment 1

Proffitt et al. (2003) found that verbal distance estimates were larger when participants wore a heavy backpack (from 1/5 to 1/6 of their body weight) than when they did not. Previous findings by Corlett, Byblow, and Taylor (1990) are potentially inconsistent with these findings, however. Corlett and colleagues evaluated the effect of perceived locomotor constraint on distance perception (as indicated by blindfolded walking to previously viewed targets). They manipulated constraint, and thus effort, via application of resistance with a rubber band attached to the participant. They found that participants under low levels of resistance (presumably analogous to the effect of wearing a heavy backpack) did not differ in their accuracy in blind walking to a previously viewed target. Under higher resistance, blind walking tended to result in participants undershooting the targets (not overshooting, as one might expect if increasing effort increased perceived distance). These results suggest that the mere presence of increased walking effort did not exert a direct effect on perceived distance. As Proffitt (2006b) has pointed out, if effort influences perceived distance, this effect should be observable in different kinds of behavioral indications of distance. Hutchison and Loomis (2006) attempted to evaluate the effect of effort using multiple behavioral indicators of perceived distance. They performed two experiments using the same backpack manipulation as Proffitt et al. (2003). In the first experiment, they used a between-subjects backpack manipulation of effort, in which participants gave three types of responses for a given target: (a) verbally reporting target distance, (b) verbally reporting target size (which was taken to be an indirect measure of perceived distance), and (c) blindwalking in a direction 30° oblique to the previously viewed target. In the second experiment, they attempted the same procedure (with the exclusion of blindwalking) using a within-subjects design. Hutchison and Loomis failed to find evidence of an effect of effort in either experiment. Proffitt, Stefannuci, Banton, and Epstein (2006) suggested, in a response to Hutchison and Loomis, that several methodological differences between the two laboratories likely explained Hutchison and Loomis' failure to replicate an effect of effort on distance perception: (a) The within-subjects design in Experiment 2 of Hutchison and Loomis (2006) may have provided participants with insight into the intent of the experiment; (b) relatively few practice and test trials were used by Hutchison and Loomis (2006), which may have led to more variable verbal estimates; and (c) Proffitt and colleagues had previously found evidence that the effect of effort on distance perception depends on the next action a participant anticipates performing (Witt et al., 2004). Proffitt et al. (2006) argued that participants in Hutchison and Loomis' (2006) study did not anticipate acting on the target distance (e.g., walking directly to the target) because verbal estimates were preceded by other types of estimates (reporting size judgments and blind walking in a direction oblique to the target) that are not directly associated with an "intention to act" on the target.

To evaluate more directly effort's effect on distance perception, we performed a strict methodological replication of Proffitt et al.'s (2003) backpack experiment. The target distances, experiment setting (large grassy field), instructions, stimuli (presented along six radii), design, number of trials, and the number of subjects were closely matched to those used by Proffitt et al (2003). Therefore, our results should not have been hindered by any of the factors that Proffitt et al. (2006) suggested may have reduced Hutchison and Loomis' ability to replicate an effect of effort on distance perception. If effort has the robust effect on distance perception reported by Proffitt et al. (2003), we predicted that participants wearing a weighted backpack would verbally report distances as being significantly further away than those not wearing a backpack.

Method

Design—Twenty-four participants were alternately assigned to either a backpack or no-backpack condition. Each participant made 24 distance estimates. Following Proffitt et al. (2003), we divided these estimates into two blocks of six practice trials (with participants being informed that only the first of these blocks was practice), followed by two blocks of six test trials. Six stimulus distances were presented in random order in each block (Table 1, Experiment 1). The practice trials were intended to encourage participants to adopt a consistent response strategy prior to the test trials, but no error feedback was given. The target was a 0.23-m-tall cone. On the basis of Proffitt et al.'s (2003) results, we did not anticipate that there would be a significant effect of sex, but we followed their lead and included this as a within-subjects factor. Although Proffitt et al. used a block design, they did not include test block as a factor in their study. We included test block as a within-subject factor to evaluate the possible effect of increasing fatigue across blocks.

Apparatus—The experiment took place on a flat, grassy field (120 m × 100 m). Golf tees marking target locations for experimenters were not visible to participants. Backpack participants wore a backpack weighing between one fifth and one sixth of their reported weight throughout the experiment. All participants were informed that the experiment investigated how college-age participants perceive distances. Following Proffitt et al. (2003; J, Stefanucci, personal communication, September 11, 2007), backpack participants were told that they would wear a backpack because most college students wear a backpack while walking around campus. No-backpack participants wore no backpack and did not report their weight prior to testing. None of the weights used in the backpack condition were visible in the no-backpack condition. This precaution was taken to prevent influencing participants' responses via knowledge of the between-subjects manipulation (i.e., knowledge of an alternative weighted-backpack group).

Procedure—Participants held a 12-inch ruler as a scale reference throughout testing. Participants stood at a central location for the duration of the study; targets were presented in one of six possible directions (0°, 30°, 60°, 120°, 150°, 180°). The target direction on each trial was randomized to minimize the participants' use of environmental cues as a reference for distance. None of the distances was presented in the same direction more than once. On each trial, participants faced away from the field while the target was being placed. They then turned to view the target and verbally reported the distance (in feet and inches) from themselves to the target. We then converted these distances into meters.

Results

We conducted a 2 (backpack) × 2 (sex) × 2 (test block) × 6 (distance) repeated-measures multivariate analysis of variance (MANOVA). Test block and distance were varied within subjects, while backpack and sex were manipulated between subjects. Two of 24 participants gave substantially larger estimates than other participants (1 backpack participant, mean response = 27.4 m; 1 no-backpack participant, mean response = 18.1 m) and were classified as outliers (using the same outlier method as Witt, Proffitt, & Epstein, 2005, $M \pm 1.5\ SD$; outlier classification limits for all 24 subject data set: backpack $M \pm 1.5\ SD = 8.9\text{m} \pm 7.62$, and no-backpack $M \pm 1.5\ SD = 8.37 \pm 6.67$). We performed analysis on both the full 24-participant data set and also on the data set after removing the 2 outliers (22-participant data set). Descriptive statistics are provided in Table 2. In contrast to Proffitt et al. (2003) and regardless of analysis technique, we found no effect of backpack—for the full data set: $F(1, 20) = 0.07$, $p = .79$, partial $\eta^2 = 0.004$; with the outliers removed: $F(1, 20) = 0.15$, $p = .7$, partial $\eta^2 = 0.008$; Figure 1). Other than a main effect of distance, Fs(1, 20) > 22.4, ps < 0.001, partial η^2s > 0.53), there were no other main effects or significant interactions.

Discussion

Might we have failed to replicate Proffitt et al.'s (2003) results because our data were more variable? Although those authors did not explicitly report between- and within-subjects variability, we estimated their between-subject standard errors by physically measuring the error bars given in their Figure 2, which shows backpack group versus no-backpack group performance. Averaging over target distance, our estimates of Proffitt et al.'s (2003) standard errors were 0.30 m in the no-backpack condition and 0.48 m in the backpack condition. Standard errors for our no-backpack participants were slightly larger than those reported by Proffitt et al., but those of our backpack participants were comparable. This suggests that our null results were not due to increased variability in our data relative to theirs. It is interesting that Proffitt et al.'s (2003) no-backpack group tended to underestimate target distances by approximately 26%, considerably less accurate than is typical for verbal estimates obtained under similar viewing conditions (which average about 17% undershooting; Da Silva, 1985). Proffitt et al. (2003) did not discuss this apparent departure from the typical pattern. Our observers underestimated by approximately 17%, a finding that fits well with results reported in previous literature outside the realm of effort manipulations.

Experiment 2

Witt et al. (2004) found that verbal distance estimates were larger after participants tossed a heavy ball than after they tossed a lighter ball. The authors interpreted this as evidence that the effort associated with tossing the ball influenced perceived distance. The exchange between Hutchison and Loomis (2006) and Proffitt et al. (2006) suggests that Proffitt et al.'s (2003) effort-related effects using the backpack manipulation may be particularly sensitive to specific methodological conditions. By contrast, Witt et al.'s finding of effort-related effects in five experiments suggests that effort effects may be more robust for throwing methods. In our Experiment 2, we again attempted to replicate an effect of effort on distance perception applying the methods used in Witt et al.'s (2004) first experiment. Target distances, size and weight of the balls, number of trials, number of participants, and instructions were identical to those used by Witt et al (2004). There were three differences between Witt et al (2004) and the present experiment: (a) Experiment 2 was performed indoors in a space that contained some irregularly spaced permanent floor markings instead of outdoors; (b) Experiment 2 presented targets along six radii, as in Witt et al. (2004), but our radii were divided across two starting locations (in Experiment 4, we evaluated whether testing environment influenced our ability to replicate an effort effect on distance perception). (c) In Witt et al (2004), the experimenter threw the ball directly back to the participant after each successive throw by the participant. Because experimenters must exert effort to throw a ball, just as participants do, seeing the experimenter exert effort might draw participants' attention to the ball weight and influence response calibration. To guard against this possibility, in Experiment 2, we had the experimenter roll the ball back to a second experimenter, who then handed the ball to the participant for the next throw.

Method

Design—Twenty-four participants were assigned in alternating order to either a heavy-ball or a light-ball condition, with each participant being exposed to only one ball weight. Following Witt et al. (2004), each participant made 12 estimates (four practice trials and two blocks of four test trials), with four stimulus distances in each block (Table 1, Experiment 2) presented in random order.

Apparatus—Target locations were marked for experimenters with clear tape in an empty dance studio (22 m × 15 m). Numerous other tape markers were placed haphazardly on the floor to disguise the target markers. There were some irregularly spaced permanent floor markings, but participants never saw a particular target distance twice in the same direction. This design was intended to reduce the effectiveness of particular floor markings as relative distance cues. (We checked this manipulation in Experiment 4, which was performed in a grassy field with no such ground markings. As we will later discuss, Experiment 4 showed that the effort manipulation was not influenced by the testing environment.) The target was a disc cone (0.19 m diameter × 0.05 m tall). The heavy and light balls had masses of 0.91 kg and 0.32 kg, respectively (with diameters of 0.19 m and 0.18 m, respectively). Tape markings were arranged into six radii (Table 1). To accommodate the range of stimulus distances in the dance studio, we centered the radii at two possible starting locations.

Procedure—Participants started at one of two locations, depending on the target direction for a given trial (Location 1 = 0°, 30°, and 60°; Location 2 = 120°, 150°, and 180°) and either moved to the other starting location or continued standing at their present location for the next trial. None of the target distances was presented in the same direction more than once. Participants threw underhanded with the dominant hand three times in a row to the target. After the third throw, participants gave a verbal estimate of the target distance from their current location, using feet and inches (later converted to meters). While the next target was placed, participants lowered a blindfold or were prompted to walk to the other starting location. Targets were not placed until the participant was standing at the correct starting location with the blindfold down. Participants were notified when practice was over, and the experimental trials began. No accuracy feedback was given.

Throwing distances were measured to the nearest quarter inch using an electronic measuring device (Combo PRO; SONIN, Inc., Charlotte, NC). One participant gave extremely inaccurate responses during practice trials (e.g., 8-m target distance reported as 150 ft [46 m]). This participant was encouraged to think of his or her height and then to imagine how many times he or she would need to lie down to reach the target. We felt that large inaccuracies of this type were more likely due to atypical verbal calibration than to inaccurately perceived distance, and the feedback was intended to minimize this source of between-subject variability in the subsequent test trials. No such instruction was given during test trials.

Results and Discussion

We performed a 2 (ball weight) × 2 (sex) × 2 (test block) × 4 (distance) repeated-measures MANOVA. Test block and distance were varied within subjects, and ball condition and sex were manipulated between subjects. Descriptive statistics are given in Table 2.

Sighted throwing was highly correlated with target distance ($r = .91$, Table 2), confirming that participants were expending appropriate amounts of effort to complete the throwing task. More important, however, unlike Witt et al. (2004), we found no effect of ball weight on verbal distance estimates, $F(1, 20) = 1.3$, $p = .26$, partial $\eta^2 = 0.06$ (Figure 2). If anything, our heavy-ball participants gave slightly smaller verbal estimates than the light-ball group (averaging 5.78 m vs. 6.35 m, respectively). By contrast, mean verbal responses for the light-ball and heavy-ball participants in Witt et al. were 5.22 m and 6.57 m, respectively. There were no other significant between-subjects effects, and distance was the only significant within-subjects effect, $F(1, 20) = 126.7$, $p < .001$, partial $\eta^2 = 0.86$.

Might we have failed to replicate Witt et al.'s (2004) findings because our data were more variable than theirs? Those authors did not explicitly report between- and within-subjects variability, so we estimated their standard errors given in their Figure 1 (Witt et al., 2004, Experiment 1) showing performance of their heavy-ball versus light-ball groups. Averaging across targets, our estimates of Witt et al.'s (2004) standard errors were 0.35 m for the heavy-ball and 0.40 m for the light-ball groups. Our standard errors were comparable, suggesting that the differences in our data were not due to increased response variability in our study relative to theirs.

It is possible that differences in our methodology (i.e., indoors testing, rolling the ball back to the participant, or splitting the six radii) could have contributed to our null effects of ball weight. We evaluated several of these possibilities in the next two experiments.

Experiment 3

Given Witt et al.'s (2004) robust ball-throwing findings, our failure to replicate their results despite using virtually identical methods is striking. Proffitt (2006a, 2006b) and Witt et al. (2004) have suggested that effort-related changes in perceived distance are action specific. This may explain the apparent discrepancy in our results. In this view, manipulations of effort via ball throwing should be most likely to change perceived distance when participants are intending to throw a ball to the target. Witt et al.'s (2004) Experiment 1 showed an apparent effect of effort even though participants did not intend to throw immediately after giving their verbal estimates. We replicated this methodology without finding any effort-related effects. Nevertheless, including the intention to act may intensify the influence of effort. In Experiment 3, we evaluated (a) whether intention to act was the essential missing component preventing replication in our Experiment 2 and (b) what effect the inclusion of such a component had on subject responses (Experiment 2 vs. Experiment 3). Target distances, stimuli, number of trials, number of participants, and instructions were identical to those used by Witt et al (2004). The differences in Experiment 3 were the same as noted in Experiment 2.

Method

Design—Twenty-four participants were assigned in alternating order to either a heavy-ball or a light-ball condition. The design was the same as in Experiment 2.

Apparatus—The laboratory and effort manipulation were the same as in Experiment 2.

Procedure—Procedures were the same as in Experiment 2, with one exception: After verbally estimating a particular target distance, participants lowered a blindfold and immediately attempted to throw to that target. This same procedure was used in Experiment 4 of Witt et al. (2004) and was done to ensure that participants' ball-throwing "intention" was appropriately matched to the target distance, which they were asked to verbally estimate.

Results

We performed a 2 (ball weight) × 2 (sex) × 2 (test block) × 4 (distance) repeated-measures MANOVA. Test block and distance were varied within subjects, and ball weight and sex were manipulated between subjects. Descriptive statistics are given in Table 2.

Verbal estimations—Despite the inclusion of an intention to act component, we again found no effect of ball weight on verbal estimates, $F(1, 20) = 0.44$, $p = .52$, partial $\eta^2 = 0.02$. Our estimates of Witt et al.'s (2004, Experiment 1) mean verbal reports and within-subjects standard errors were 6.57 m ± 0.35 m for heavy-ball participants and 5.22m ± 0.4 m for light-ball participants, averaging across target distance. By contrast, our mean verbal reports and standard errors were 6.72 m ± 0.44 m for heavy-ball participants and 7.14 m ± 0.43 m light-ball participants. Heavy-ball participants again gave slightly smaller verbal estimates than light-ball participants.

Blind-throwing performance—Blind throwing after giving verbal reports can be used as an independent measure of perceived distance, provided that target distances do not exceed participants' physical abilities (Sahm, Creem-Regehr, Thompson, & Willemsen, 2005).[1] Witt et al. (2004) did have their participants make blind throws after giving a verbal estimate, but they did not report the results of the blind-throwing performance. In our analysis, data from both blind and sighted throwing showed a Distance × Sex interaction (both Fs > 8.3, ps < 0.001, partial η^2s > 0.49), with women tending to undershoot significantly more for 8-m and 10-m target distances. The fact that this undershooting appeared under both blind and sighted throwing suggests that it was due primarily to motoric factors. Concentrating on just the 4-m and 6-m distances (in order to focus on a region less susceptible to motor constraints), we found that although men generally threw farther, $F(1, 20) = 16.7$, $p = .001$, $d' = 0.46$, partial $\eta^2 = 0.02$, there was no overall effect of ball weight on blind throwing, $F(1, 20) = 0.08$, $p = .77$, partial $\eta^2 = 0.45$. This mirrors the lack of effect in the verbal estimates.

Intention versus no intention—To evaluate whether there was an effect of intention on verbal estimates of distance, we performed a repeated-measures MANOVA on data from both Experiments 2 and 3, including intention versus no intention, ball weight, and sex as between-subject variables. Distance and test block were the within-subject variables. The only significant between-subject effects were for intention, $F(1, 40) = 5.63$, $p = .02$, $d' = 0.64$, partial $\eta^2 = 0.12$. Results showed that the inclusion of an intention component significantly increased

[1]An influential framework espoused by Milner and Goodale (1995), among others, has proposed that visual information is processed in distinct cortical pathways depending on whether it is used for visual object recognition (and presumably other forms of conscious visual perception) versus on-line control of actions. Blind throwing to a previously viewed target is a visually directed action and therefore, in principle, might be controlled by a special-purpose action-based representation rather than by visual perception. However, Sahm et al. (2005) found that blind throwing and blind walking are tightly linked across a variety of viewing conditions. On the basis of this finding, they have argued that blind throwing is indeed responsive to perceived distance.

verbal estimates of distance (Figure 3a), but most important for our purposes, there was no main effect of ball weight, $F(1, 40) = 1.6$, $p = .21$, partial $\eta^2 = 0.03$, and no Intention × Ball Weight interaction, $F(1, 40) = 0.08$, $p = .78$, partial $\eta^2 = 0.002$. Although the overall increase in verbal reports could reflect a direct influence of perceived distance based on intention to act, we found no significant effect of ball condition in Experiment 3 on either verbal estimates or blind throwing. Our interpretation is that intention to act influenced the calibration of verbal estimates without altering distance perception, per se. Examining in more detail the possibility of intention to act as either a direct or indirect mediator of perceived distance is beyond the scope of the current article but remains an interesting topic for future work.

Experiment 4

Although inclusion of an intention to act component was associated with a general increase in verbal estimates, we were still unable to replicate Witt et al.'s (2004) and Proffitt et al.'s (2003) findings with regard to the ball-weight manipulation. Another possible reason that we were unable to replicate their results pertains to the testing environment in which the studies were conducted. Witt et al. (2004)'s ball-throwing study was conducted in a grassy field, whereas ours was conducted on an indoor surface containing visible floor markings. To evaluate the possible effect of testing environment, we replicated Experiment 3 in an outdoor environment. Testing environment (large grassy field, six-radii presentation), target distances, stimuli, number of trials, number of participants, and instructions were identical to those used in Witt et al. (2004). The only difference between Experiment 4 and the methods described in Witt et al.. (2004) was that we continued to roll the ball back to a second experimenter rather than throw it directly back to the participant.

Method

Design—Twenty-four participants were assigned in alternating order to either a heavy-ball or a light-ball condition. The design was identical to that of Experiment 3.

Apparatus—The effort manipulation was the same as in Experiment 3. We conducted the experiment in the same outdoor testing environment used in Experiment 1.

Procedure—Procedures were the same as in Experiment 3 with the exception that targets were randomly presented on one of six possible radii centered on the observer (following Witt et al., 2004).

Results

Verbal estimates and blind-throwing performance—After giving a verbal estimate, participants made a final blind throw to the target. There was again no effect of the effort manipulation on verbal estimates, $F(1, 20) = 0.06$, $p = .8$, partial $\eta^2 = 0.003$, or blind throws, $F(1, 20) = 0.008$, $p = .9$, partial $\eta^2 = 0.001$). There were no other between-subjects effects, and distance was the only significant within-subjects effect for both verbal estimates and blind throws, Fs > 141, ps < 0.001, partial η^2 s > 0.87. Our estimate of Witt et al.'s (2004, Experiment 1) mean verbal reports and standard errors were 6.57 m ± 0.35 m for heavy ball and 5.22 m ± 0.4 m for light ball. By contrast, our mean verbal reports and standard errors were 5.73 m ± 0.44 m for heavy ball and 5.77 m ± 0.42 m for light ball. Descriptive statistics are given in Table 2.

Indoor versus outdoor—To evaluate whether testing environment has an effect on verbal estimates of distance and blind-throwing performance, we performed a repeated-measures MANOVA on data from both Experiments 3 and 4, including testing environment (indoor vs. outdoor), ball conditions, and sex as between-subject variables. Distance and test block were

NIH-PA Author Manuscript

NIH-PA Author Manuscript

the within-subject variables. For verbal reports, the only significant between subject effect was for testing environment, $F(1, 40) = 8.99$, $p < .005$, $d' = 0.83$; partial $\eta^2 = 0.18$ (Figure 3b). Results showed that participants who were tested indoors ($M = 6.95$ m, $SE = 0.27$ m) gave significantly larger verbal estimates of distance than participants who were tested outdoors ($M = 5.76$ m, $SE = 0.28$ m); however, there was no main effect of ball weight, $F(1, 40) = 0.41$, $p = .52$, partial $\eta^2 = 0.01$, and no Testing Environment × Ball Weight interaction, $F(1, 40) = 0.07$, $p = .78$, partial $\eta^2 = 0.002$. Distance was the only within-subjects effect, $F(3, 120) = 3.20$, $p < .001$, partial $\eta^2 = 0.89$.

Concentrating only on the 4-m and 6-m distances, we found the only significant between-subjects effects for blind-throwing performance were sex, $F(1, 40) = 7.18$, $p = .01$, $d' = 0.74$, partial $\eta^2 = 0.15$, and a Sex × Testing Environment interaction, $F(1, 40) = 6.0$, $p = .02$, $d' = 0.67$, partial $\eta^2 = 0.13$. On average, male participants ($M = 5.18$ m, $SE = 0.09$ m) threw significantly farther than female participants ($M = 4.85$ m, $SE = 0.07$ m). The Sex × Testing Environment interaction arose from a tendency for men to throw farther in the indoor environment ($M = 5.4$ m, $SE = 0.12$ m) than in the outdoor environment ($M = 4.96$ m, $SE = 0.13$), while there was no such tendency for women. Distance, $F(1, 40) = 5.18$, $p < .001$, partial $\eta^2 = 0.93$, and Test Block, $F(1, 40) = 4.67$, $p = .037$, partial $\eta^2 = 0.1$, were the only within-subjects interactions. The latter arose from a tendency for participants to throw farther in the second block of test trials ($M = 5.1$ m, $SE = 0.07$) than in the first block ($M = 4.95$, $SE = 0.06$). This suggests that fatigue was not the source of the significant block effect. It is important to note that when Experiment 3 or 4 was considered singly, there was no effect of block in either, so the Sex × Testing Environment interaction may be the result of Type 1 error.

The increase in verbal reports in the indoor environment could reflect bona fide differences in perceived distance that depended on testing environment. The distance cues for each target distance were presumably the same in the two environments, but it may be that the proximity of the walls in the indoor environment, or some other contextual aspect of the environment, differentially affected perceived distances relative to the same target distances outdoors (see Andre & Rogers, 2006; Lappin et al., 2006). It is interesting that we found no significant effect of testing environment on blind-throwing performance. Further research would be required to fully explain why only verbal reports were affected by the testing environment. One possibility is that the throwing responses reflected perceived distance, which presumably was unchanged in the two environments for a given target and that the testing environment systematically influenced the calibration of verbal reports without changing perceived distance per se. More important for our present purposes, however, was the fact that we failed to find a significant effect of ball condition or a Ball Condition × Testing Environment interaction, suggesting that the use of an indoor versus outdoor testing environment was not the critical factor preventing us from replicating Witt et al.'s (2004) findings concerning the effects of effort on perceived distance.

Experiment 5

According to the effort– calibration hybrid hypothesis, in some situations, changes in response calibration could mimic effort-related influences on perception without perception itself being affected. In this view, the ball-weight manipulation may influence response calibration in this particular paradigm, while under other circumstances, there may be genuine effort-related effects on perception. This hypothesis focuses on determining what circumstances tend to influence response calibration while leaving perceived distance untouched. In this regard, it is currently unclear what participants' intuitions might be concerning the connection between ball weight and distance estimates. If participants demonstrate that their intuitions match the predictions of the effort hypothesis, the possibility that changes in response calibration are responsible for effort-related effects in the ball-throwing paradigm becomes a much more pressing concern.

People certainly share intuitions about how effort and the ability to act might influence the experience of distance under some circumstances. Just as a mountain that visually appears close might “seem” farther away in an abstract, cognitive sense if one is stuck in traffic, a destination might also seem farther away if one is carrying heavy grocery bags. Our hypothesis was that participants in the ball-throwing paradigm sometimes adopt a response attitude that takes this kind of nonperceptual factor into account rather than basing their judgments exclusively on perceived distance. Even if exactly the same instructions and methodology are used for the heavy- and light-ball groups, participants might pick up subtle (and perhaps unconscious) cues from the experimenter that encourage them to take into account their intuitions about the connection between ball weight and distance when responding.

This hypothesis rests on the assumption that if participants adopt a response attitude focusing on relatively abstract, cognitive connotations of distance, their verbal distance estimates will indeed be influenced by ball weight in a manner consistent with the effort hypothesis. Our goal in Experiment 5 was to test this assumption. To do this, we explicitly pointed out three possible response attitudes (or conceptualizations of distance) for participants. After being briefed on the difference among these response attitudes, participants were instructed to adopt one of them when verbally or physically (i.e., throwing a ball) indicating the distance to a target. One group was asked to focus on apparent (perceived) distance, and another on objective (physical) distance (e.g., Carlson, 1977). A third group was instructed to take into account any “nonvisual factors” that might influence where they felt the target was located. This category was analogous to our earlier example of how we may “feel” that a mountain is farther away if we are stuck in traffic, even if the distance to the mountain does not appear to change. Each of these three instruction groups was further subdivided into a heavy-ball group and a light-ball group. Observers then threw a ball three times to each target, gave a verbal estimate of the target distance, and finally threw the ball to the target without vision, using the response attitude specified in the instructions. This methodology allowed us to probe the effects of adopting a nonvisual factor response attitude in a way that did not invite an explicit comparison between heavy and light balls; as in the original ball-throwing paradigm of Witt et al. (2004), heavy-ball participants were only exposed to the heavy ball. In fact, ball weight was not mentioned in any of the groups’ instructions.

Previous work involving instructional manipulations has shown that if observers are instructed to report on the objective, or physical, distance of an object under reduced-cue conditions, they produce systematically different responses than if they are instructed to report on the perceived, or apparent, distance of the object (for a review, see Carlson, 1977). The instruction to report the physical location (as opposed to the apparent or perceived location) is thought to yield responses based on a composite of perceived distance and more abstract, cognitive factors such as the observer’s memory or expectations about the stimulus environment (Carlson, 1977; Tyer et al., 1983). However, under the well-lit, multicue conditions that have been used in the ball-throwing paradigm, there is little difference in verbal distance estimates under apparent- versus objective-distance instructions (Da Silva, 1985). Presumably, the abundance of visual-distance information reduces the discrepancy between perceived distance and the perception– cognition composite. On the basis of this past research and our contention that ball weight does not affect perceived distance per se, we predicted that there would be little difference between verbal estimates for the objective- and apparent-distance instruction groups, with no effect of ball weight in either. By contrast, we predicted that when participants were explicitly instructed to take nonvisual factors into account, the heavy-ball group would produce significantly larger distance responses than the light-ball group. If the data did not support this prediction, it would show that this paradigm is relatively insensitive to uncontrolled variations in response attitude.

If the predictions were supported, it would suggest that uncontrolled influences on response attitude could potentially account for effort-related effects in previous studies in which the ball-throwing manipulation was used (Witt et al., 2004).

Method

Design—Participants were assigned to one of three instruction groups (objective distance, apparent distance, and nonvisual factors; $n = 24$ in each). Within each group, participants were further subdivided, in alternating order, into heavy- or light-ball groups. It is important to note that although the experimenters were aware of the ball conditions and instruction groups, they were kept blind to the specific experimental hypotheses and trained to maintain a neutral and consistent affect with participants to avoid unintentional biasing of participants. A double-blind methodology was implemented to minimize the possibility that the experimenters might unintentionally communicate the experimental predictions to the participants. Otherwise, the design was the same as in Experiment 3.

Apparatus—The laboratory and effort manipulation were the same as in Experiment 3.

Procedures—Procedures were generally the same as in Experiment 3 (with blind throws following each verbal estimate). The major difference was that in this experiment three sets of instructions were used. All instructions contained an initial portion describing all three possible factors that might influence verbal distance judgments: (a) objective (physical) distance, (b) apparent (perceived) distance, and (c) nonvisual factors (see Table 3). Real-world examples were given for each factor. None of the instructions mentioned effort. Each instruction contained a final portion outlining the procedure. The only difference in the instructions was the inclusion of one of three paragraphs directing participants as to which conceptualization of “distance” to consider when giving their verbal estimates: objective, apparent, or nonvisual factors. Heavy- and light-ball participants within a given instruction group received the same written instructions.

Results

We performed a 2 (ball weight) × 2 (sex) × 3 (instruction group) × 2 (test block) × 4 (distance) repeated-measures MANOVA. Test block and distance were varied within subjects. Instruction group, ball weight, and sex were between-subjects factors. Descriptive statistics are given in Table 2.

Verbal estimates—A strong trend for an Instruction × Ball Weight interaction, $F(2, 59) = 2.95$, $p = .06$, suggests that at least one instruction group likely demonstrated an effect of ball weight on verbal estimates. Therefore, we analyzed each instruction group using a repeated-measures MANOVA: 2 (ball weight) × 2 (sex) × 2 (test block) × 4 (distance). Neither the objective-distance nor apparent-distance instruction groups demonstrated any between-subjects effects, Fs < 0.85, ps > 0.36, partial η^2s < 0.029. Distance was the only within-subjects main effect, Fs > 138, ps < 0.001, partial η^2s > 0.84. However, the nonvisual-factors group demonstrated a between-subjects effect of ball weight, $F(1, 20) = 5.5$, $p = .03$, $d' = 0.60$, partial $\eta^2 = 0.22$ (Figure 4). Consistent with Witt et al. (2004), heavy-ball participants gave significantly larger verbal distance estimates than light-ball participants (averaging 6.7 m vs. 5.1 m, respectively). Distance, $F(3, 60) = 116$, $p < .001$, partial $\eta^2 = 0.86$, and a Distance × Ball Weight interaction, $F(3, 60) = 3.77$, $p = .01$, partial $\eta^2 = 0.16$, were the only significant within-subjects effects. Our estimate of Witt et al.’s (2004, Experiment 1) average standard errors for verbal estimates were 0.35 m for heavy ball and 0.40 m for light ball. Our average standard errors for verbal estimates were comparable (see Table 2), which suggests that nonsignificant findings in the apparent-distance and objective-distance instructions groups were not due to increased variability. Results from our nonvisual-factors group and Witt et al.'s (2004) average results were almost identical, with comparable means and only a slight elevation in standard error for our data.

Blind-throwing performance—Repeated-measures MANOVA involving the 4-m and 6-m blind-throwing trials showed no ball weight effect in any of the instruction groups, *F*s < 2.5, *p*s > 0.14, partial $\eta^2 < 0.1$. This is strong converging evidence that perceived distance was not affected by the effort manipulation. Consistent with Experiment 2, sighted throwing for all instruction groups demonstrated a Sex × Distance interaction, $F(3, 59) = 34.8$, $p < .001$, partial $\eta^2 = 0.37$.

Discussion

Consistent with previous research (Da Silva, 1985; Rogers & Gogel, 1975), we found no effect of apparent-distance versus objective-distance instructions under well-lit conditions. However, our nonvisual-factors instruction did yield significantly different verbal responses than did the other two instruction conditions and was the only condition associated with effort-related effects. When participants were overtly cued to focus on nonvisual factors, without being provided any indication how these factors should affect their responses, both the direction and magnitude of the effects matched the effort effects reported by Witt et al. (2004). This suggests that participants in the ball-throwing paradigm are indeed able to intuit the predicted connection between effort and distance and will produce responses consistent with this prediction if somehow encouraged to take nonperceptual factors into account. In this experiment, we used written instructions to explicitly suggest that participants do that, but this method is by no means the only possible way. As we will discuss shortly, experimenters could provide this suggestion in a variety of subtle ways without their own awareness.

Why were Proffitt and colleagues' (2004) effort manipulations associated with changes in verbal estimates, and why did we not find the same effects? Our Experiments 2–4 closely replicated Witt et al.'s (2004) published methodology in most respects. The two most obvious differences are that (a) our experimenters strove to remain neutral in affect when interacting with participants, whereas experimenter affect in Witt et al. was not reported and thus may not have been entirely neutral, and (b) our experimenter rolled the balls back to participants, whereas the experimenter in Witt et al. tossed the balls back. There are good reasons to think that either of these two differences could explain why our results generally did not replicate those of Witt et al.

Experimenter affect—Carlson (1977) suggested that if instructions do not explicitly specify which aspect of distance (e.g., physical vs. apparent) should be considered when generating responses, participants are left to rely on their own interpretation of the task. Participants in Witt et al. (2004) were instructed to report on the distance to the target, and we followed suit in our experiments. These instructions could be classified as "neutral objective," in that they encouraged a response attitude focusing on the physical target distance, but no additional instructions were given to firmly distinguish this attitude from "apparent" or "nonvisual factors" response attitudes. Under these circumstances, participants may be especially sensitive to subtle cues from the experimenter (perhaps emitted without the experimenter's awareness) about what aspect of distance should be assumed when generating responses. Providing additional encouragement to participants who express frustration at their throwing performance is one way that experimenters might inadvertently suggest that nonperceptual factors should be taken into account when responding. If one ball-weight group expresses more frustration than the other (perhaps because the ball is heavy and they tend to throw more inaccurately), they may tend to receive more encouragement, and this could alter the extent to which they calibrate their responses to take their intuitions about the role of effort into account. We took two steps to minimize the potential biasing effects of experimenter affect: (a) In Experiments

1–5, our experimenters maintained a neutral affect throughout the experiment, and (b) in Experiment 5, the experimenters were kept blind as to the experimental predictions. Experimenter affect in Witt et al. (2004) may not have been entirely neutral, and this might well play a powerful role in this paradigm.

Throwing versus rolling the ball back—In Witt et al.'s (2004) ball-throwing study, the experimenter threw the balls back to participants between trials. Experimenters must exert effort to throw the balls back; if participants see the experimenter exerting effort to throw a heavy ball, for example, this may well subtly suggest to them that they should take their own ball-throwing effort into account when generating their verbal responses. (In a similar way, if an experimenter struggles to put a heavy backpack on a participant, this could suggest that the weight of the backpack is important for the study and that the verbal estimate should be adjusted to reflect this; Proffitt, 2006a.) In addition, experimenter throwing accuracy was not measured in Witt et al. (2004). If experimenter throwing accuracy is not 100% consistent between groups (e.g., it could easily vary as a function of ball weight), the groups would differ systematically in terms of the overall amount of experimenter error they see. This could influence the extent to which participants calibrate their distance estimates to take their throwing effort into account.

It is interesting that Witt et al. (2004) also found effort-related effects in a visual-matching task; these effects appeared in only one of two closely matched conditions, despite enhanced statistical power, but these results could hint that uncontrolled experimenter interactions with participants have unanticipated effects even on nonverbal visual-matching responses. Biersdorf, Ohwaki, and Kozil (1963), for example, found that the wording of experimenter instructions affects performance in a visual-matching task.

Because experimenter affect and experimenter throwing accuracy were not recorded in Witt et al. (2004), a direct test of the role of these factors would be problematic. Nevertheless, given that our methods were otherwise virtually identical, it seems very likely that the appearance of effort-related effects in this paradigm hinges crucially on some aspect of these differences.

Finally, we note that verbal reports typically underestimate target distances somewhat (by approximately 17%) under conditions comparable to those studied here (see Da Silva, 1985, for an extensive review). The vast majority of the studies in the existing literature did not manipulate effort, so there are abundant "no-effort" baseline data. It is reasonable to expect Witt's (2004) and Proffitt's (2004) results to be understandable in the context of this extensive previous work. If increasing effort indeed increases perceived distance, one would expect both light- and heavy-ball conditions to result in larger distance estimates than is typical for no-effort conditions. Instead, Witt and Proffitt's light-ball estimates were considerably smaller than typical no-effort data (with average undershooting on the order of 26%). Our results in Experiments 2, 3, and 4 and in the apparent-distance and objective-distance instruction groups in Experiment 5 mesh well with typical full-cue verbal reports (approximately 13% average undershooting across experiments), whereas light-ball participants in our nonvisual factors condition performed similarly to Witt and Proffitt's light-ball group, showing significant underestimates on the order of 26.5%. Our interpretation is that the ball-weight manipulation affected the relative calibration of verbal responses rather than perceived distance per se.

General Discussion

Our results are significant on three levels. First, on an empirical level, our experiments add to a growing literature suggesting that at least some of Proffitt's (Profitt et al., 2003; Witt, Profitt, & Epstein, 2004) effort-related findings, though providing valuable insight into the factors that affect overt distance judgments, may be more fragile than has been heretofore appreciated (Corlett et al., 1990; Hutchison & Loomis, 2006). If ball throwing or backpack wearing indeed influences egocentric distance perception, such effects occur under a relatively limited set of circumstances. This conclusion is based on our results from 96 participants in Experiments 1–4, in whom we found no effort-related effects. This null result held true in both indoor and outdoor environments, for different manipulations of effort (wearing a backpack, throwing a ball), and with or without intention to act on the part of the participants.

Second, on a methodological level, our work highlights the vital importance of providing explicit instructions concerning what response attitude should be adopted and being sure that participants understand the difference among different connotations of distance. Our work also emphasizes the importance of ensuring that experimenters interact with participants neutrally and consistently. The use of a double-blind methodology, as in Experiment 5, provides a particularly convincing demonstration that the experimenter is not unconsciously behaving in a way that communicates the experimental predictions to participants. At a minimum, however, researchers using effort-related manipulations should take care to treat all participants consistently and should explicitly report that they have done so when describing their work.

Third, on a theoretical level, the fact that our findings differed so dramatically from Proffitt's indicates that some factor, heretofore underappreciated, exerts a powerful influence on distance judgments. Anticipated effort cannot be the sole factor: In our first four experiments, we manipulated effort in the same way it had been manipulated in Proffitt's group, but only Proffitt's group showed differences in verbal reports. Our hypothesis is that this powerful influence, at least in the context of backpack and ball-throwing manipulations, is the result of the multiple connotations of distance. If instructions do not adequately specify how the term should be interpreted, participants must rely on their own intuitions concerning what interpretation the experimenters intend. This, in turn, opens the door for experimenter–participant interactions to play a larger role in influencing how participants go about generating their responses. We have argued that these effects in ball-throwing studies, and presumably also in backpack-encumbrance studies, do not alter the perceived distance of objects per se but instead the calibration of responses. The results of our intention versus no-intention analysis in Experiment 3, our indoor versus outdoor analysis in Experiment 4, and our instructional manipulations in Experiment 5 support this notion. In addition, these results highlight the importance of explicitly considering the possible influence of anticipated effort on response calibration in future work.

To reiterate our effort– calibration hybrid viewpoint expressed earlier, we do not interpret these experiments as evidence that the framework espoused by Proffitt and colleagues is globally wrong or that effort never influences perception. Instead, our interpretation is that effort likely does influence perceived distance under some circumstances, but that ball throwing and backpack encumbrance do not strongly elicit these effects. If these manipulations are representative of effort as it occurs in more natural, everyday situations, our results suggest that the real-world consequences of anticipated effort on space perception may be more minimal than has previously been assumed. It is also possible, however, that effort does influence perceived distance in a variety of real-world situations, perhaps because people implicitly plan actions and engage in motor simulations when judging distances (Witt & Proffitt, in press), but that ball-throwing and backpack-encumbrance manipulations are relatively poor examples of this influence.

Recently, Proffitt (2006a) has expressed the notion that the perceived distance of an object is intimately coupled with the possibilities that exist for acting upon the object—in Gibson's terminology (1979), the “affordances” provided by the object (p. 127). It was suggested to us that task demands in some effort manipulations might implicitly encourage participants to judge the affordances that a target distance provides for action rather than perceived distance per se. Such affordances would indeed encompass nonvisual factors such as weight, energy, and so forth, and this would explain the directionality of the ball-weight effects that we observed. It may be that our nonvisual-factors instructions in Experiment 5 had the effect of biasing participants' responses more toward their assessment of the affordances associated with the target, rather than toward perceived (or physical) target distance; in a similar vein, the other two instruction groups may have tended to ignore affordances when responding.

Direct judgments of spatial relations are key to a variety of research domains, both inside and outside the discipline of psychology (e.g., spatial cognition, neuropsychology, exercise science, medical diagnosis, human factors). Thus, the lessons learned from this work have implications extending well beyond visual space perception.

Acknowledgments

This work was supported by a National Science Foundation Graduate Research Fellowship awarded to Adam J. Woods and National Institutes of Health Grant RO1 NS052137 awarded to John W. Philbeck. We thank Jeanine Stefanucci and Jessica Witt for their advice in replicating their methods and Deanne Adams, Shayna Thomas, Thom Gennaro, Jessica Huang, Richard Lewis, Zita Patai, Margaret Cerullo, and Petra Zdenkova for assistance in data collection.

References

Andre J, Rogers S. Using verbal and blind-walking distance estimates to investigate the two visual systems hypothesis. Perception and Psychophysics 2006;68:353–361. [PubMed: 16900829]

Bhalla M, Proffitt DR. Visual–motor recalibration in geographical slant perception. Journal of Experimental Psychology: Human Perception and Performance 1999;25:1076–1096. [PubMed: 10464946]

Biersdorf WR, Ohwaki S, Kozil DJ. The effect of instructions and oculomotor adjustments on apparent size. American Journal of Psychology 1963;76:1–17. [PubMed: 13968206]

Bingham GP, Zall F, Robin D, Shull JA. Distortions in definite distance and shape perception as measured by reaching without and with haptic feedback. Journal of Experimental Psychology: Human Perception and Performance 2000;26:1436–1460. [PubMed: 10946724]

Carlson, VR. Instructions and perceptual constancy judgments. In: Epstein, W., editor. Stability and constancy in visual perception: Mechanisms and processes. New York: Wiley; 1977. p. 217-254.

Corlett JT, Byblow W, Taylor B. The effect of perceived locomotor constraints on distance estimation. Journal of Motor Behavior 1990;22:347–360. [PubMed: 15117664]

Creem-Regehr SH, Willemsen P, Gooch AA, Thompson WB. The influence of restricted viewing conditions on egocentric distance perception: Implications for real and virtual indoor environments. Perception 2005;34:191–204. [PubMed: 15832569]

Da Silva JA. Scales for perceived egocentric distance in a large open field: Comparison of three psychophysical methods. American Journal of Psychology 1985;98:119–144. [PubMed: 4003616]

Fajen BR. Perceiving possibilities for action: On the necessity of calibration and perceptual learning for the visual guidance of action. Perception 2005;34:717–740. [PubMed: 16042193]

Foley, JM. Binocular space perception. In: Regan, D., editor. Binocular vision. London: Macmillan Press; 1991. p. 75-92.

Gibson, JJ. The ecological approach to visual perception. Boston: Houghton Mifflin; 1979. p. 127-146.

Gogel WC. A theory of phenomenal geometry and its applications. Perception and Psychophysics 1990;48:105–123. [PubMed: 2385484]

Hutchison JJ, Loomis JM. Does energy expenditure affect the perception of egocentric distance? Spanish Journal of Psychology 2006;9:332–339. [PubMed: 17120712]

Land MF. Eye movements and the control of actions in everyday life. Progress in Retinal and Eye Research 2006;25:296–324. [PubMed: 16516530]

Landy MS, Maloney LT, Johnson EB, Young MJ. Measurement and modeling of depth cue combination: In defense of weak fusion. Vision Research 1995;35:389–412. [PubMed: 7892735]

Lappin JS, Shelton AL, Rieser JJ. Environmental context influences visually perceived distance. Perception and Psychophysics 2006;68:571–581. [PubMed: 16933422]

Loomis JM, Da Silva JA, Fujita N, Fukusima SS. Visual space perception and visually directed action. Journal of Experimental Psychology: Human Perception and Performance 1992;18:906–921. [PubMed: 1431754]

Loomis JM, Da Silva JA, Philbeck JW, Fukusima SS. Visual perception of location and distance. Current Directions in Psychological Science 1996;5:72–77.

Loomis, JM.; Philbeck, JW. Measuring perception with spatial updating and action. In: Klatzky, RL.; Berhmann, M.; MacWhinney, B., editors. Embodiment, ego-space, and action: The 34th Carnegie Symposium on Cognition. New York: Psychology Press; 2008.

Milner, DA.; Goodale, MA. The visual brain in action. Oxford, UK: Oxford University Press; 1995.

Proffitt DR. Distance perception. Current Directions in Psychological Science 2006a;15:131–135.

Proffitt DR. Embodied perception and the economy of action. Perspectives on Psychological Science 2006b;1:110–122.

Proffitt DR, Stefanucci J, Banton T, Epstein W. The role of effort in perceived distance. Psychological Science 2003;14:106–112. [PubMed: 12661670]

Proffitt DR, Stefanucci J, Banton T, Epstein W. Reply to Hutchison and Loomis. Spanish Journal of Psychology 2006;9:340–342.

Rieser JJ, Ashmead DH, Talor CR, Youngquist GA. Visual perception and the guidance of locomotion without vision to previously seen targets. Perception 1990;19:675–689. [PubMed: 2103000]

Rogers SP, Gogel WC. Relation between judged and physical distance in multicue conditions as a function of instructions and tasks. Perceptual & Motor Skills 1975;41:171–178. [PubMed: 1178401]

Sahm CS, Creem-Regehr SH, Thompson WB, Willemsen P. Throwing versus walking as indicators of distance perception in real and virtual environments. ACM Transactions on Applied Perception 2005;2:35–45.

Thomson JA. How do we use visual information to control locomotion? Trends in Neurosciences 1980;3:247–250.

Tyer ZE, Allen JA, Pasnak R. Instruction effects on size and distance judgments. Perception & Psychophysics 1983;34:135–139. [PubMed: 6634369]

Witt JK, Proffitt DR. Action-specific influences on distance perception: A role for motor simulation. Journal of Experimental Psychology: Human Perception and Performance. (in press).

Witt JK, Proffitt DR, Epstein W. Perceiving distance: A role of effort and intent. Perception 2004;33:577–590. [PubMed: 15250663]

Witt JK, Proffitt DR, Epstein W. Tool use affects perceived distance, but only when you intend to use it. Journal of Experimental Psychology: Human Perception & Performance 2005;31:880–888. [PubMed: 16262485]

Yarbus, A. Eye movements and vision. New York: Plenum Press; 1967.

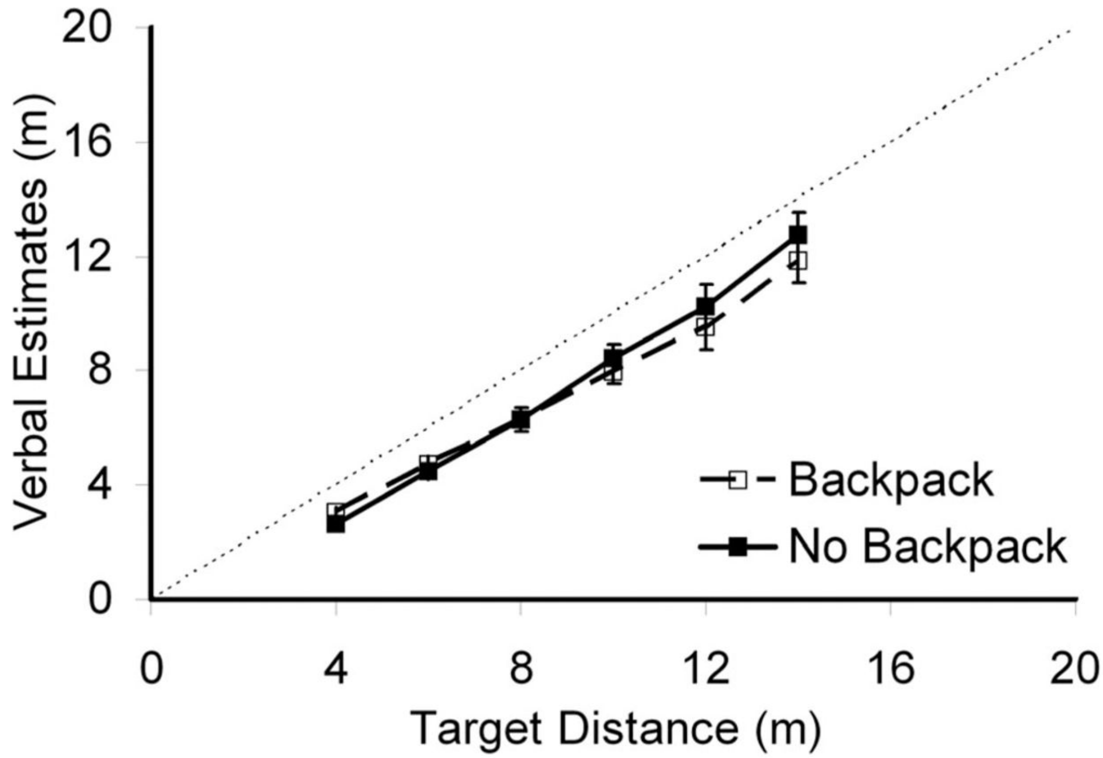

Figure 1.
Results of verbal estimates of distance to targets for backpack versus no-backpack participants in Experiment 1 with outliers removed. Dotted line represents physical distance of targets.

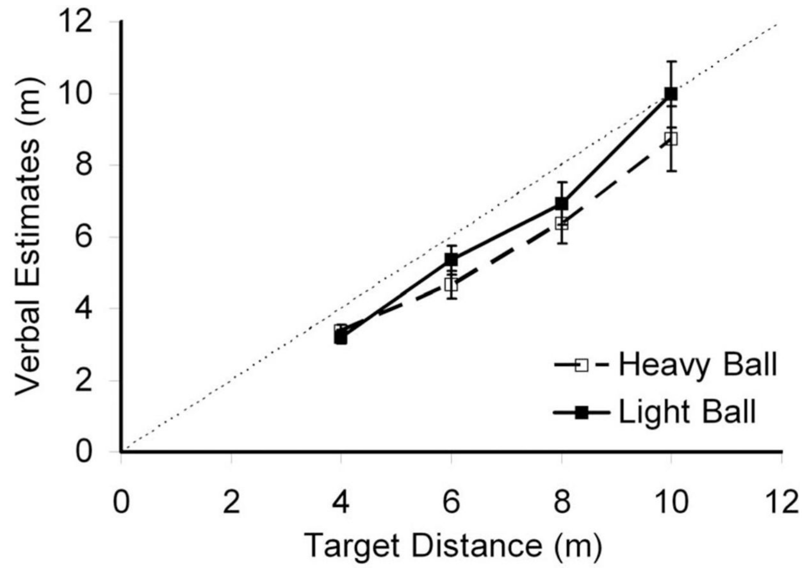

Figure 2.
Results of verbal estimates of distance to targets for heavy-versus light-ball participants in Experiment 2. Dotted line represents physical distance of targets.

NIH-PA Author Manuscript
NIH-PA Author Manuscript

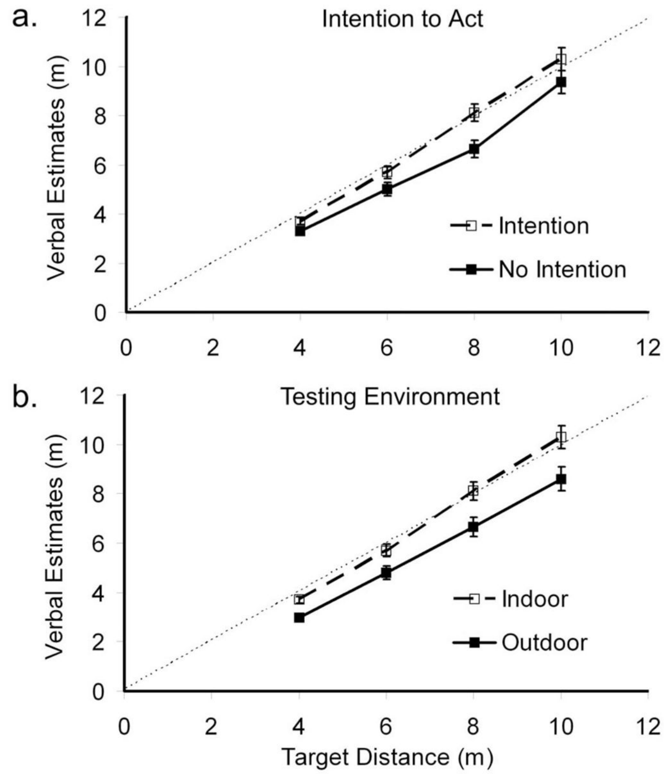

Figure 3.

a. Results of verbal estimates of distance to targets for Experiment 2 (no intention to act component) versus those for Experiment 3 (inclusion of an intention to act component—blind throwing following verbal estimates). b. Results of verbal estimates of distance for Experiment 3 (indoor testing environment) versus those for Experiment 4 (outdoor testing environment). Dotted line represents physical distance of targets.

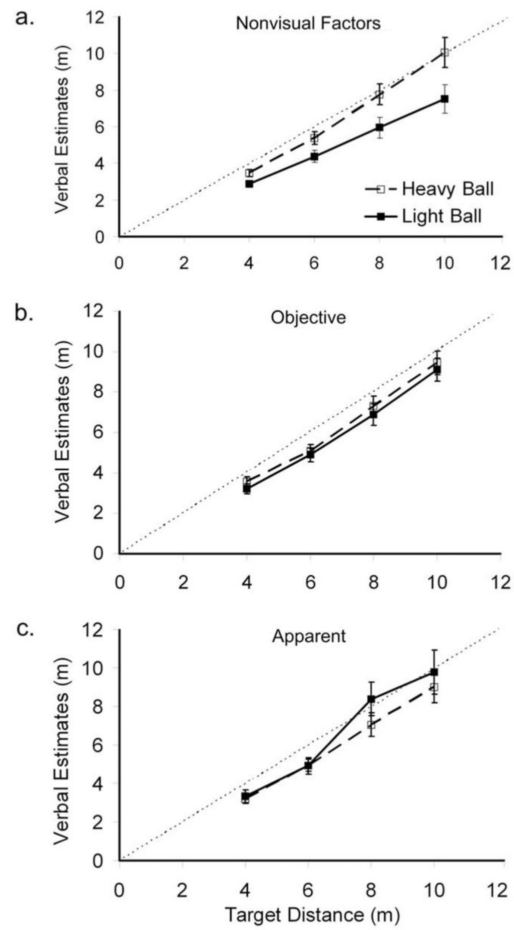

Figure 4.

Results of verbal estimates of distance to targets for heavy- versus light-ball participants from each instruction group in Experiment 5: (a) nonvisual factors, (b) objective distance, and (c) apparent distance. Dotted line represents physical distance of targets.

Table 1

Target Distances for All Experiments

Experiment 1			Experiments 2–5	
Practice block				
1	2	Test blocks 1 and 2	Practice block	Test blocks 1 and 2
1 m	2 m	4 m	3 m	4 m
3 m	5 m	6 m	5 m	6 m
7 m	7 m	8 m	9 m	8 m
11 m	11 m	10 m	11 m	10 m
15 m	13 m	12 m	—	—
17 m	16 m	14 m	—	—

All distances and blocked designs were identical to those used in Proffitt et al. (2003) and Witt et al. (2004).

NIH-PA Author Manuscript

NIH-PA Author Manuscript

Table 2

Means, Within- & Between-Subject Standard Deviations, Correlations, and Slopes for All Experiments

	Heavy ball or backpack					Light ball or no backpack				
		SD					*SD*			
Variable	***M***	**Within**	**Between**	***r***	**slope**	***M***	**Within**	**Between**	***r***	**slope**
Verbal reports										
Experiment 1										
All participants	8.92	5.08	9.40	0.47	1.29	8.38	4.46	5.83	0.70	1.19
W/o 2 outliers	7.24	3.33	3.52	0.84	0.86	7.50	3.79	3.99	0.86	1.00
Experiment 2	5.83	2.33	2.54	0.79	0.90	6.36	2.83	3.05	0.80	1.09
Experiment 3	6.72	2.69	3.02	0.78	1.05	7.14	2.98	3.10	0.84	1.16
Experiment 4	5.73	2.13	2.63	0.74	0.87	5.77	2.37	2.55	0.85	0.96
Experiment 5										
Apparent distance	6.05	2.47	2.90	0.76	0.98	6.48	2.69	3.06	0.79	1.07
Objective distance	6.35	2.64	2.78	0.80	0.99	6.02	2.44	2.73	0.81	0.98
Nonvisual factors	6.49	2.60	3.05	0.78	1.06	5.17	1.98	2.40	0.72	0.77
Sighted throws—Experiment 2	6.31	1.87	1.86	0.92	0.76	6.51	2.05	2.03	0.92	0.84
Blind throws—4 m & 6 m										
Experiment 3	5.07	0.87	0.88	0.78	0.68	5.07	1.08	1.11	0.79	0.87
Experiment 4	4.93	1.09	1.06	0.87	0.91	4.96	1.09	1.08	0.84	0.89
Experiment 5										
Apparent distance	4.86	1.01	1.00	0.82	0.81	4.65	0.94	0.95	0.69	0.65
Objective distance	5.08	0.91	0.98	0.69	0.67	4.74	1.16	1.12	0.85	0.94
Nonvisual factors	4.77	0.91	0.94	0.79	0.73	4.86	1.04	0.99	0.87	0.85

Note. Within = within-subject; between = between-subject; *r* = Pearson's correlation coefficient; slope = slope of the best fitting line between participants' responses and physical target distances (see Table 1 for values).

Table 3

Instruction Types in Experiment 5

Instructions for experiment 5
Instructions common to all participants:
In this experiment we'll ask you to estimate how far away things are. There are several possible interpretations of this, so first we want to make it clear what we want you to estimate. Imagine you're looking at a mountain that you know is 40 miles away. If you have ever seen a far-away mountain, you may have thought it looked much closer than it really was. This shows that there can be a difference between (a) how far away something is in reality and (b) how far away our eyes tell us it is—where it appears to be visually. (c) Nonvisual factors can also play a role. For example, if you're traveling in a fast car, it might feel as if the mountain is closer than if you're in a car stuck in traffic, even though visually the mountain may not appear to be closer. If you're in a hurry, the mountain might feel farther away, even though it may not visually appear farther.
When we look at things in the nearby environment, these same factors can apply. If we're looking at a basketball, there may be things in the environment that make it visually appear to be closer or farther away than its real distance (the distance that a tape measure would show). Nonvisual factors (things that are going on with you at the moment, for example) may also make you feel as if the object is at a different distance.
Insert condition-specific instructions (see next section):
You'll start each trial wearing a blindfold. When you're asked to lift the blindfold, you'll see an orange cone on the floor. Your task will be to throw a ball to the cone. Try to be as accurate as possible in throwing to the cone. Throw the ball underhanded with your dominant hand (the hand you write with). You will throw the ball three times. After the third throw, you'll be asked to give a verbal estimate in feet and inches of how far the cone is from where you're standing. After your verbal estimate, you will be asked to pull the blindfold down and throw without vision to the cone one last time. After the last throw, leave the blindfold down, and the cone will be placed for the next trial. Between some trials, you may be asked to move to another starting location before starting the next trial.
Condition-specific instructions:
Objective distance
In this study, there are two ways we will ask you to indicate distances: either by throwing a ball or by giving a verbal estimate in feet and inches. When you indicate distance (verbally or by throwing), base your response on how far away you think the object really is. If you think that the object appears to be at a different distance than you think it really is or if you feel that the object is at a different distance (for whatever reason), ignore those other things, and just base your answer on where you think the object really is. This means that if you're answering verbally, imagine there's a tape measure stretching out to the object, and you're reading off the measurement. If you're throwing, try to make the ball land exactly where the object was.
Apparent distance
In this study, there are two ways we will ask you to indicate distances: either by throwing a ball or by giving a verbal estimate in feet and inches. When you indicate distance (verbally or by throwing), base your response on how far away the object visually appears. If you think your eyes are playing tricks on you (that is, you think the object is in reality at a different distance than it visually appears to be) or if it feels as if the object is (nonvisually) at a different distance, ignore those other things and just base your answer on where your eyes tell you the object appears to be.
Nonvisual factors
In this study, there are two ways we will ask you to indicate distances: either by throwing a ball or by giving a verbal estimate in feet and inches. When you indicate distance (verbally or by throwing), base your response on how far away you feel the object is, taking all nonvisual factors into account. If you think that the object appears to the eye to be at a different distance than it feels (taking nonvisual factors into account), just base your answer on where you feel the object is.