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Action Effects on Visual Perception of Distances: A Multilevel Bayesian Meta-Analysis

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Abstract

Previous studies have suggested that action constraints influence visual perception of distances. For instance, the greater the effort to cover a distance, the longer people perceive this distance to be. The present multilevel Bayesian meta-analysis (37 studies with 1,035 total participants) supported the existence of a small action-constraint effect on distance estimation, Hedges's $g = 0.29$, 95% credible interval = [0.16, 0.47]. This effect varied slightly according to the action-constraint category (effort, weight, tool use) but not according to participants' motor intention. Some authors have argued that such effects reflect experimental demand biases rather than genuine perceptual effects. Our meta-analysis did not allow us to dismiss this possibility, but it also did not support it. We provide field-specific conventions for interpreting action-constraint effect sizes and the minimum sample sizes required to detect them with various levels of power. We encourage researchers to help us update this meta-analysis by directly uploading their published or unpublished data to our online repository (https://osf.io/bc3wn/).

Keywords

perception-action, visual perception, distance perception, meta-analysis, open data, open materials

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This article focuses on the visual perception of space—how people visually assess spatial layouts such as distances and slopes. One might intuitively assume that the visual perception of space depends only on visual information conveyed by optical and oculomotor cues (Cutting & Vishton, 1995). However, growing evidence gathered during the last two decades has challenged this idea by suggesting that people also perceive space through variables related to their ability to act (for reviews, see Morgado & Palluel-Germain, 2016; Philbeck & Witt, 2015). Following Sparrow and Newell (1998), we refer to these action-specific variables as *action constraints* (Morgado & Palluel-Germain, 2016).

In a much-cited article, Proffitt, Stefanucci, Banton, and Epstein (2003) studied the influence of action constraints on the visual perception of distances by asking participants to verbally estimate the distance to a target under various levels of action constraint. Participants estimated that the target was farther away when they wore a heavy backpack (i.e., high constraint) than when they did not (i.e., low constraint). Likewise, Witt, Proffitt, and Epstein (2005) observed that participants estimated that a target was closer to them when they could use a tool to reach it more easily (low constraint) than when they could not (high constraint). This result was interpreted as an action-constraint effect on visually perceived distance and led to the emergence of action-constraint theories of perception (e.g., the *evolved-navigation theory*, Jackson & Willey, 2011, and the *action-specific account*, Philbeck & Witt, 2015; for a discussion of these theories, see Morgado & Palluel-Germain, 2016).

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Some researchers have questioned the existence and the nature of action-constraint effects. Several studies have failed to show a statistically significant effect, leading their authors to conclude that the effect may not be replicable (e.g., Hutchison & Loomis, 2006; Woods, Philbeck, & Danoff, 2009). In addition, some authors have argued that action constraints influence how people estimate distances (i.e., make perceptual judgments) but not how they actually see them (e.g., Durgin & Russell, 2008; Woods et al., 2009). According to most proponents of this view (e.g., Durgin et al., 2009; Firestone, 2013), these effects mainly arise from an experimental demand bias: Participants may have adjusted their behavior in response to what they guessed the research hypothesis to be (for a model of demand bias, see Strohmetz, 2008).[1]

One purpose of our meta-analysis was to investigate two predictions from the action-constraint theories of perception. First, we estimated the extent to which action constraints influence the visual perception of distances by combining all the relevant results we could gather.[2] Because different constraints (such as backpacks or tool use) might influence distance perception through different mechanisms, we also estimated the effect size per constraint category (e.g., effort, weight, tool use). Second, several authors have argued that motor intention is a prerequisite for action-constraint effects, because constraints associated only with intended actions would influence distance perception (e.g., Witt et al., 2005). We used task instructions as a proxy for motor-intention induction. If instruction-based motor intention is a prerequisite for action-constraint effects, such effects should vanish when participants are not explicitly instructed to perform an action on a target before or after estimating its distance.

The other purpose of the present meta-analysis was to investigate two predictions from the experimental-demand account. First, some authors have argued that participants are more likely to guess the hypothesis in within-subjects designs than in between-subjects designs, because they are aware of the different experimental conditions in the former (Hutchison & Loomis, 2006). Thus, action-constraint effects should be larger in studies with within-subjects designs than in studies with between-subjects designs. Second, some authors have argued that verbal measures are more sensitive to cognitive biases and voluntary control than other measures are (e.g., Woods et al., 2009). Thus, action-constraint effects should be larger for verbal measures than for visual and action-based measures.

Method

Below, we present the criteria used to select the studies, the formulas used to compute the effect sizes, and the model used to estimate the overall effect size. For each study, we calculated the size of the action-constraint effects on visual distance estimation. We combined all these effect sizes within a three-level Bayesian meta-analytic model to estimate the overall effect size as well as the effect of several moderators.

Data collection and preparation

Literature search and inclusion criteria. To retrieve relevant articles, we used two keyword strings ("effort" and "distance perception"; "tool use" and "distance perception") in PsychARTICLES, Psychology and Behavioral Sciences Collection, PsychINFO, Academic Search Complete, and Google Scholar. By November 2017, this search had returned 308 articles published in peer-reviewed journals. We identified 13 additional articles by searching for authors from the action-constraint field and asking them for additional published or unpublished studies. We ended up with a total of 321 articles.

In our meta-analysis, we included only empirical studies in which the independent variable was a manipulation of a physical action constraint (as opposed to affective or social action constraints, such as fear of falling or need for social support). Following a comment from an anonymous reviewer, we decided to exclude studies based on visuomotor recalibration as a constraint manipulation. Indeed, because the visuomotor recalibration (e.g., a treadmill manipulation) was often used with blind walking as a measure of perceived distance, it was not clear whether it influenced perceived distance, walking, or both.[3] Moreover, the visuomotor-recalibration literature was beyond the scope of this meta-analysis. We also excluded studies that used natural variations of physical action constraints (e.g., participants' weight) and studies in which participants observed someone else performing an action under various action constraints (e.g., tool-use observation). We also excluded studies in which varying the hill slope served as an effort manipulation, because in such studies, effort was confounded with the visual stimulation.

We included only studies in which the dependent variable was a measure of visually perceived egocentric distance. This criterion excluded any other measures of space perception, such as estimations of allocentric distances, affordance judgments (e.g., reachability judgments), or measures of peripersonal space (e.g., line bisection). We included studies using size perception only when the authors explicitly indicated that they used it as an indirect measure of perceived distance. We excluded literature reviews, commentaries and replies, and empirical studies for which sufficient statistics were not available in the article or from the authors. From the 66 studies that met the inclusion criteria listed above, we included only 37 studies (Fig. 1).

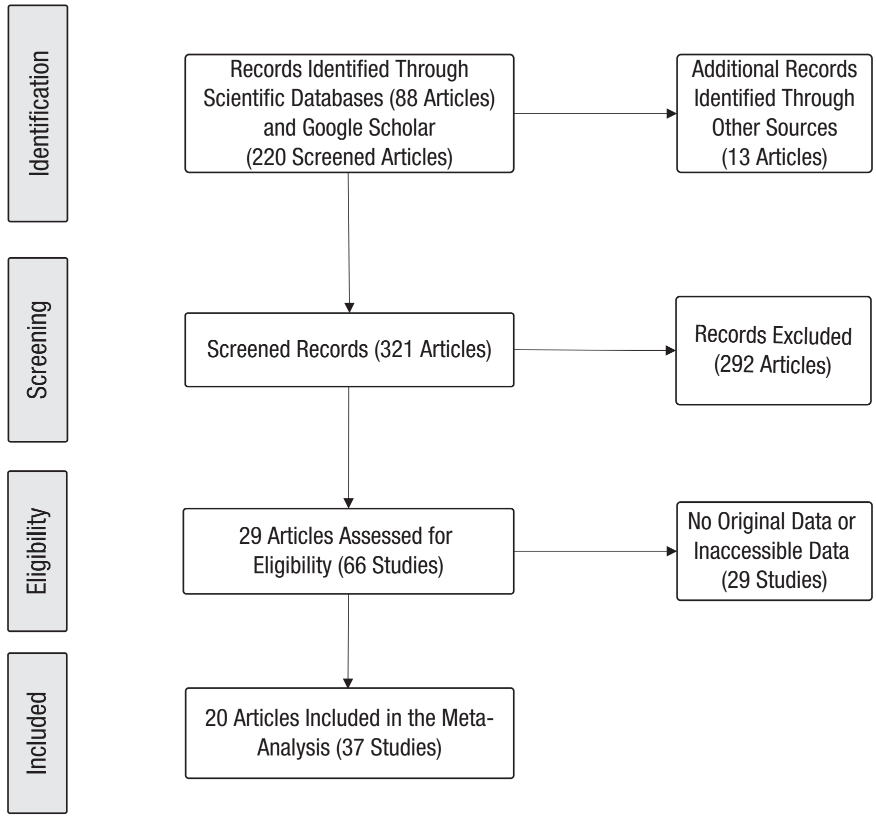

Fig. 1. Flowchart illustrating the search protocol and workflow used for study selection.

The complete results of the literature search and the details of the exclusion procedure are reported in the file "list_inclusion_exclusion_moderators.xls" in our Open Science Framework project (https://osf.io/bc3wn/).

Data extraction. For each study, R. Palluel-Germain, N. Morgado, and L. Molto independently coded the following five variables of interest until a consensus was reached: the constraint manipulation, the motor intention, the research design, and the measure of distance estimation (for coding details, see Table 1). We delineated three categories of constraint manipulations: tool use, weight (e.g., wearing a heavy backpack or not), and various effort manipulations (e.g., swimming with or without flippers; producing a reaching movement under various force levels).

We identified four measures of distance estimation: verbal estimation, visual matching (i.e., matching a comparison distance to a target distance), action-based measures (e.g., blind walking or blind throwing to the location of a previously seen target), and an indirect measure of perceived distance (i.e., size estimation). We distinguished between studies that had within-subjects and between-subjects designs. Finally, we distinguished studies in which task instructions induced motor intention by prompting participants to perform an action from studies that did not prompt participants to perform an action.

Effect-size computation. We computed Cohen's d using separate formulas for between-subjects and within-subjects designs:

Between-subjects Cohen's d_s =

$$\frac{\text{mean(HC)} - \text{mean(LC)}}{\sqrt{\begin{array}{c}(n(\text{HC}) - 1 \times SD(\text{HC})^2 \\ + n(\text{LC}) - 1 \times SD(\text{LC})^2)/(n(\text{HC}) + n(\text{LC}) - 2)\end{array}}}$$

Within-subjects Cohen's d_{rm} =

$$\frac{\text{mean(HC)} - \text{mean(LC)}}{\sqrt{\begin{array}{c}(SD\ (\text{HC})^2 + SD(\text{LC})^2 \\ - 2 \times r \times SD(\text{HC}) \times SD(\text{LC}))/2 \times 1 - r\end{array}}}$$

We compared the mean distance estimations in the high-constraint (HC) and low-constraint (LC) conditions. We divided this difference by the pooled standard deviation so a positive d represented a larger distance

Table 1. Summary of the Studies Included in Our Meta-Analysis

Article and study	N	Constraint manipulation	Moderator			
			Constraint category	Motor intention	Design	Measure
Costello et al. (2015): Study 1 (youth)	32	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Within subjects	Visual matching
Durgin & Russell (2008): Study 1	28	Backpack (with vs. without)	Weight	Without intention	Between subjects	Verbal
Hutchison & Loomis (2006)						
Study 1, Measure 1	24		Weight	Without intention	Between subjects	Verbal
Study 1, Measure 2	24		Weight	With intention	Between subjects	Action
Study 1, Measure 3	24		Weight	Without intention	Between subjects	Size
Study 2, Measure 1	12		Weight	Without intention	Within subjects	Verbal
Study 2, Measure 2	12		Weight	Without intention	Within subjects	Size
Kirsch, Herbort, Butz, & Kunde (2012)	24	Amplitude of a pointing movement (50% vs. 150% of the distance)	Effort	With intention	Within subjects	Visual matching
Kirsch & Kunde (2013a)						
Study 1	22	Force and amplitude	Effort	With intention	Within subjects	Visual matching
Study 2	23	Force and amplitude	Effort	With intention	Within subjects	Visual matching
Study 3	19	Force and amplitude	Effort	With intention	Within subjects	Visual matching
Kirsch & Kunde (2013b)						
Study 2	23	Force and amplitude	Effort	With intention	Within subjects	Visual matching
Study 3	19	Force and amplitude	Effort	With intention	Within subjects	Visual matching
Lessard, Linkenauger, & Proffitt (2009)	12	Ankle weight	Weight	With intention	Within subjects	Visual matching
Linkenauger, Bülthoff, & Mohler (2015)						
Study 1	11	Arm length (25% larger vs. 85% the size of the avatar's arm)	Other	With intention	Within subjects	Visual matching
Study 2	11	Arm length (25% larger vs. 85% the size of the avatar's arm)	Other	With intention	Within subjects	Action
Study 3	11	Arm length (25% larger vs. 85% the size of the avatar's arm)	Other	With intention	Within subjects	Visual matching
Study 4	12	Arm length (25% larger vs. 85% the size of the avatar's arm)	Other	Without intention	Within subjects	Visual matching
Meagher & Marsh (2014)						
Study 1, Measure 1	19	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Verbal
Study 1, Measure 2	19	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Action
Study 2, Measure 1	18	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Verbal

(*continued*)

Table 1. *(continued)*

Article and study	*N*	Constraint manipulation	Moderator: Constraint category	Moderator: Motor intention	Moderator: Design	Moderator: Measure
Study 2, Measure 2	18	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Action
Study 3, Measure 1	19	Carry a heavy object alone vs. with someone else	Effort	With intention	Between-subjects	Verbal
Study 3, Measure 2	19	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Action
Study 4	37	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Verbal
Study 5	60	Carry a heavy object alone vs. with someone else	Effort	With intention	Between subjects	Verbal
Moeller, Zoppke, & Frings (2016)	28	Locomotion mode (driving vs. walking)	Effort	Without intention	Between subjects	Visual matching
Molto et al. (2020)	93	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Within subjects	Visual matching
Morgado, Gentaz, Guinet, Osiurak, & Palluel-Germain (2013)	20	Transparent barrier width	Effort	With intention	Within subjects	Visual matching
Osiurak, Morgado, & Palluel-Germain (2012): Study 1	21	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Between subjects	Visual matching
Proffitt, Stefanucci, Banton, & Epstein (2003): Study 3	24	Recalibration optic flow	Effort	Without intention	Between subjects	Verbal
Witt (2011)						
Study 1	32	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Between subjects	Visual matching
Study 2	16	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Between subjects	Visual matching
Study 3	24	Tool use (laser vs. baton)	Tool use	With intention	Between subjects	Visual matching
Witt & Proffitt (2008): Study 2	8	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Between subjects	Visual matching
Witt, Proffitt, & Epstein (2005)						
Study 1	16	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Within subjects	Verbal
Study 2	8	Tool use (hand pointing vs. tool touching)	Tool use	With intention	Within subjects	Visual matching
Witt, Schuck, & Taylor (2011): Study 1	54	Swim with vs. without flippers	Effort	With intention	Between subjects	Verbal
Woods, Philbeck, & Danoff (2009)						
Study 1	22	Backpack (with vs. without)	Weight	Without intention	Between subjects	Verbal
Study 2	24	Heavy vs. light ball throwing	Weight	With intention	Between subjects	Verbal
Study 3, Measure 1	24	Heavy vs. light ball throwing	Weight	With intention	Between subjects	Verbal
Study 3, Measure 2	24	Heavy vs. light ball throwing	Weight	With intention	Between subjects	Action
Study 4, Measure 1	24	Heavy vs. light ball throwing	Weight	With intention	Between subjects	Verbal
Study 4, Measure 2	24	Heavy vs. light ball throwing	Weight	With intention	Between subjects	Action
Zadra, Weltman, & Proffitt (2016)	7	Caloric supplementation (carbohydrate vs. placebo)	Effort	With intention	Within subjects	Action

estimation in the high-constraint condition than in the low-constraint condition. Because d is biased for small samples, we transformed it into Hedges's g, which is commonly used in meta-analyses (Hedges, 1981). To this end, we multiplied d by the correction factor J:

$$J = 1 - \frac{3}{4df - 1}$$

We computed the sampling variance of g using formulas from Borenstein, Hedges, Higgins, and Rothstein (2009):

$$\text{Between-subjects Cohen's } d: V = \frac{n1 + n2}{n1n2} + \frac{d^2}{2(n1 + n2)}$$

$$\text{Within-subjects Cohen's } d: V = \left(\frac{1}{n} + \frac{d^2}{2n}\right) 2(1 - r)$$

When the correlation (r) between conditions for within-subjects designs was unavailable, we used the mean value of the available correlations ($r = .84$).

Data analyses

The meta-analytic model. We used a three-level Bayesian meta-analytic model to estimate the overall effect of action constraints on distance estimation. We conducted all analyses in R (Version 3.4; R Core Team, 2017), and we used Stan (Stan Development Team, 2018) and the *brms* package (Bürkner, 2017) to fit the model. Some of the included articles contained more than one study, and some studies contained more than one effect size. Articles reporting multiple studies or effect sizes introduce a bias in the meta-analysis by weighting more in the overall effect-size estimation. Therefore, outcomes from the same study or from the same article should not be treated as independent (Van den Noortgate, López-López, Marín-Martínez, & Sánchez-Meca, 2013). To overcome this nonindependence issue, we averaged effect sizes that came from the same study so that each study yielded only one effect size (Cheung, 2014). To model the dependence between studies from the same articles, we included three levels in our model (Fig. 2): participants at Level 1, studies at Level 2, and articles at Level 3. With this model, we estimated the overall effect-size α of action constraint on distance perception (the grand intercept of the model), the between-article variability (τ_{article}), and the between-study variability in the same article (τ_{study}).

Bayesian analyses. We conducted all analyses using Bayesian statistics (for an introduction, see Wagenmakers et al., 2018). The main advantage of Bayesian statistics is that they allow researchers to consider prior knowledge through the use of prior distributions. Given the usual effect sizes observed in psychology, we did not expect the average effect size to be larger than 1.5 (Szucs & Ioannidis, 2017). Therefore, we specified a mildly informative prior on the average effect-size alpha and weakly informative priors on variance components (for R code and mathematical details of this model, see https://osf.io/9zw4j/).

Bayesian statistics also allow researchers to quantify the relative evidence for two competing hypotheses. We estimated the relative evidence for the existence of the action-constraint effects against their nonexistence by comparing models with and without the intercept. We compared these models using the bayes_factor() method in brms, which uses the *bridgesampling* package (Gronau, Singmann, & Wagenmakers, 2017). The Bayes factor (BF) is a ratio of marginal likelihoods, which is similar to a likelihood ratio weighted by the prior predictions of each model. In other words, it indicates the likelihood of the observed data under a given hypothesis (e.g., the effect differs from 0) relative to another hypothesis (e.g., the effect is equal to 0). Although BFs express the relative evidence for a hypothesis in a continuous way, we also followed conventions from Wagenmakers et al. (2018) to make the interpretation easier to unfamiliar readers. We considered the relative strength of evidence for the hypothesis of the existence of an effect to be null ($BF_{10} = BF_{01} = 1$), anecdotal (BF_{10} between 1 and 3; BF_{01} between 1/3 and 1), moderate (BF_{10} between 3 and 10; BF_{01} between 1/10 and 1/3), strong (BF_{10} between 10 and 30; BF_{01} between 1/3 and 1/10), very strong (BF_{10} between 30 and 100; BF_{01} between 1/100 and 1/30), and extremely

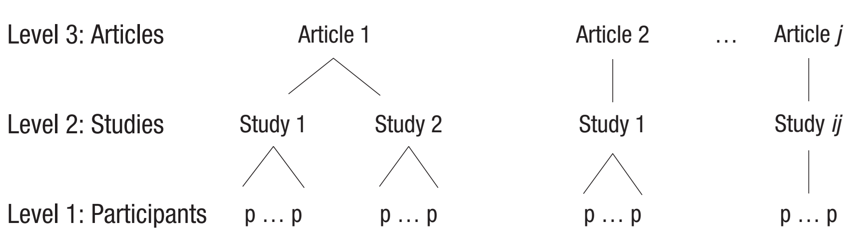

Fig. 2. Three-level structure of our meta-analytic model, which allowed us to estimate the between-study variability in the same articles (Level 2) and the between-articles variability of the effect size (Level 3).

strong ($BF_{10} > 100$; $BF_{01} < 1/100$). We also report 95% credible intervals (CrIs), which are a Bayesian equivalent to confidence intervals except that they have a 95% probability of containing the population value of the parameter (for a discussion of these intervals, see Nalborczyk, Bürkner, & Williams, 2019). We ran four Markov chain Monte Carlo (MCMC) analyses for each model, including each 20,000 iterations with a warm-up of 5,000 iterations. We assessed posterior convergence by examining trace plots and the Gelman-Rubin statistic (for R code and technical details, see https://osf.io/9zw4j/).

Moderator analyses. We fitted separate meta-regression models to evaluate the influence of each moderator. When the moderators had only two levels (e.g., design: within subjects vs. between subjects), we used contrast codes (−0.5, 0.5). When the moderators had more than two levels (i.e., type of manipulation and measure), we fitted models with the moderators as categorical predictors. Then, for each contrast (e.g., verbal vs. visual matching), we computed the posterior distribution of the difference between the two conditions ($\hat{\beta}$).

Additional analyses. We also examined the extent of publication bias using funnel plots (e.g., Peters, Sutton, Jones, Abrams, & Rushton, 2008). A funnel plot depicts the relation between the effect size and its standard error. If publication bias is small, studies should be equally dispersed on both sides of the overall effect size, resulting in a symmetrical funnel-shaped distribution. If the publication bias is large, more studies should fall to the right of the overall effect size, and there should be high variability, resulting in an asymmetrical distribution. This method is limited because other factors can influence the symmetry of the funnel plot (Peters et al., 2008). However, to our knowledge, there is no consensus about the best way to estimate and correct for publication bias (for a comparison of different methods, see Carter, Schönbrodt, Gervais, & Hilgard, 2019).

The results from the studies included in our meta-analysis were originally analyzed using null-hypothesis significance testing. For this reason, we also conducted *p*-curve analyses to test whether a set of *p* values has evidential value for an effect (Simonsohn, Nelson, & Simmons, 2014). If there was no overall effect, *p* values should have been uniformly distributed, whereas if there was an effect, the *p*-value distribution should be right skewed, with more *p* values close to .01 than to .05.

Results

Data set

Because some authors used multiple effort manipulations in each of their studies (Table 1), we aggregated their outcomes in order to obtain a single outcome per study. Thus, the resulting full data set comprised 45 outcomes extracted from 37 studies from 20 articles (participants: $N = 1{,}035$, observations: $N = 1{,}299$). In six other studies, the authors used several measures of distance perception, resulting in an effect-size estimation (i.e., outcome) per measure (Table 1). Such multiple-outcome studies are weighted more in a meta-analysis than single-outcome studies are. To avoid this, we rearranged our full data set by averaging all the outcomes from the same study, so that only one outcome per study would be included in our meta-analysis. We used the resulting single-outcome-study data set (37 outcomes) to estimate the overall effect, the moderator effect of constraint category, and the moderator effect of research design. It was impossible to estimate the moderator effects of motor intention and measure after averaging several outcomes from the same study, so we used our full data set for these analyses.

Investigating two predictions from action-constraint theories of perception

One purpose of the present meta-analysis was to investigate two predictions from the action-constraint theories of space perception. The first prediction pertained to the existence of action-constraint effects on distance estimation, which is one part of the debate surrounding them. Thus, we estimated the overall size of this effect across the action-constraint field and specific effects per constraint categories. The second prediction pertained to the role of motor intention in action-constraint effects on distance estimation. Indeed, several proponents of the action-constraint theories of perception argued that constraints associated only with intended actions would influence distance perception (e.g., Witt et al., 2005). Thus, one could expect a larger action-constraint effect when participants were explicitly instructed to perform an action on a target before or after estimating its distance than when they were not. Consequently, we also estimated this moderator effect of instruction-based motor intention on action-constraint effects.

Overall effect and moderator effect of constraint category. Figure 3 illustrates the effect size for each article and the overall effect size. The meta-analysis on our single-outcome-study data set revealed an overall effect of physical action constraint on distance estimation, $g = 0.46$, 95% CrI = [0.22, 0.72], $\tau_{article} = 0.48$, 95% CrI = [0.26, 0.74], $\tau_{study} = 0.12$, 95% CrI = [0.02, 0.28]. To estimate the influence of each study on this overall effect size, we computed it again by leaving out one study each time. The overall effect size varied within the range of 0.29 to 0.49. This analysis revealed an outlier (Lessard, Linkenauger, &

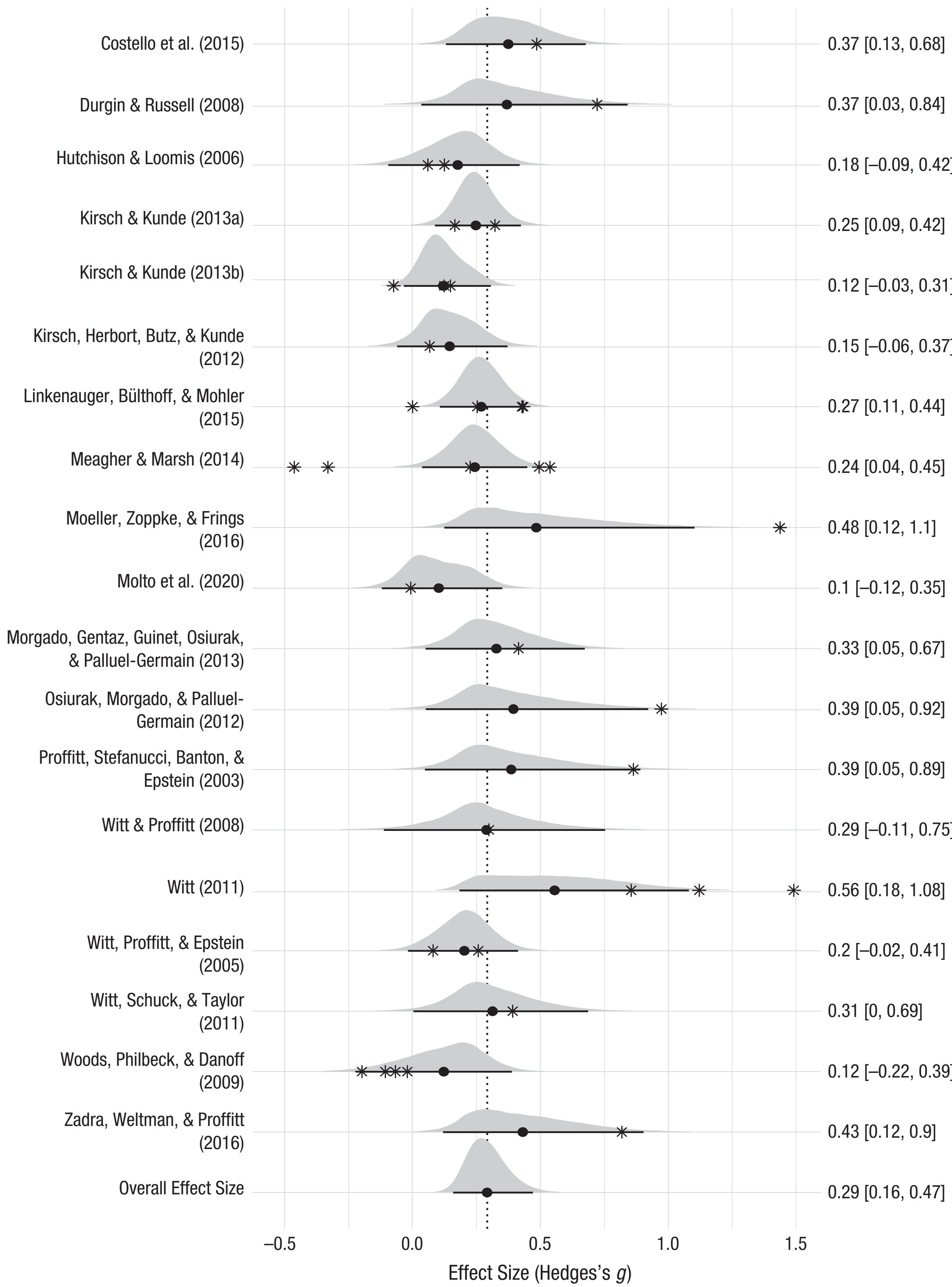

Fig. 3. Forest plot showing effect sizes for each article included in the meta-analysis and the overall effect size. In each row, the gray area represents the posterior distribution, the black dot is the mean, and the errors bars show the 95% credible interval. Asterisks indicate the average effect size for each study in articles reporting multiple studies. The dotted line indicates the overall effect size.

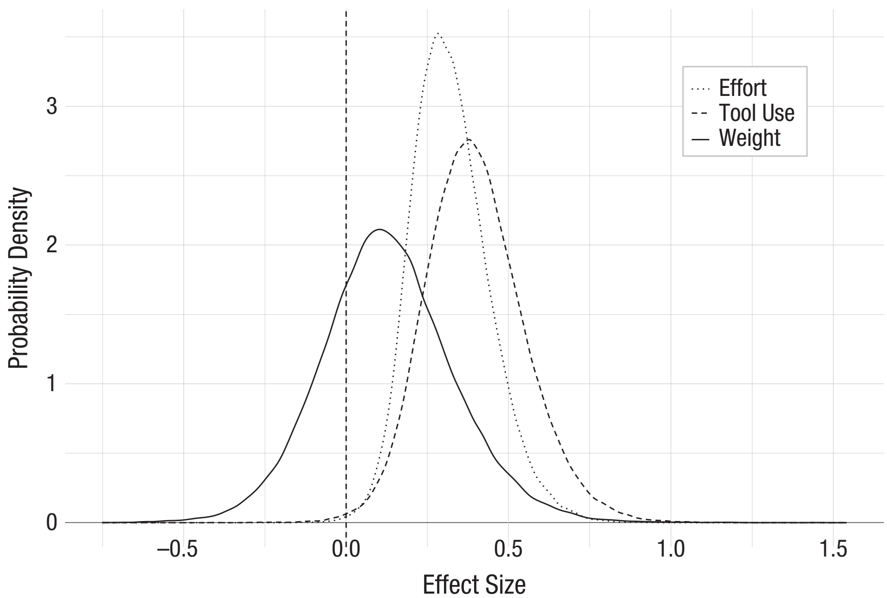

Fig. 4. Posterior distribution of effect sizes as a function of constraint manipulation.

Proffitt, 2009; g = 2.42), changing the overall estimate by 36.96% (see https://osf.io/bc3wn/). We decided to discard this study from the subsequent analyses. The updated meta-analysis revealed an overall effect, g = 0.29, 95% CrI = [0.16, 0.47], τ_{article} = 0.18, 95% CrI = [0.01, 0.40], τ_{study} = 0.13, 95% CrI = [0.02, 0.26], BF_{10} = 281.14. Because our posterior distribution was asymmetrical, the most credible value for the effect size was its mode, g = 0.27, with a 95% probability that the population effect size lies in the interval between 0.16 and 0.47 (given the prior and the available data). The BF indicated that the data were 281.14 times more likely under the hypothesis of no null effect than under the hypothesis of a null effect,[4] which can be interpreted as extremely strong evidence for the existence of the effect.

Because different action constraints (e.g., backpack, tool use) might influence distance perception through different mechanisms, we also computed the effect sizes for each constraint category (Fig. 4). We discarded four studies from the same articles (Linkenauger, Bülthoff, & Mohler, 2015) from this analysis, because the manipulation the authors used did not fit in any constraint category. Using the data from our single-outcome-study data set, we estimated the action-constraint effect for tool use, weight, and effort. Our analysis revealed moderate evidence for a tool-use effect, g = 0.40, 95% CrI = [0.12, 0.72], BF_{10} = 3.81 (9 outcomes, 250 participants), and strong evidence for an effort effect, g = 0.32, 95% CrI = [0.11, 0.59], BF_{10} = 10.45 (19 outcomes, 416 participants). The analysis also revealed moderate evidence for an absence of weight effect, g = 0.13, 95% CrI = [−0.26, 0.55], BF_{01} = 6.16 (13 outcomes, 170 participants).

To directly assess the moderating role of the constraint category, we tested the three contrasts evaluating the differences between the effects of effort, tool use, and weight manipulations (Table 2). For each contrast, we report $\hat{\beta}$ indicating the difference between two given constraint categories. A positive $\hat{\beta}$ indicates a larger effect for effort than for tool-use, for effort than for weight, and for tool use than for weight (the reverse is true for a negative $\hat{\beta}$). These analyses revealed moderate support for an absence of difference among the three constraint categories.

The role of motor intention. Utilizing our full data set, we tested whether the action-constraint effect from studies in which participants intended to reach the target (35 outcomes, 1,058 observations) differed from studies in which they did not (9 outcomes, 186 observations). A positive $\hat{\beta}$ indicates a larger effect with motor intention than without it (the reverse is true for a negative $\hat{\beta}$). Figure 5 illustrates the posterior distribution of effect size depending on motor intention. This analysis revealed extremely strong evidence for an absence of difference between the two conditions, $\hat{\beta}$ = 0.07, 95% CrI = [−0.23, 0.36], BF_{01} = 10,096.54.

Investigating two predictions from the experimental-demand account

The other purpose of our meta-analysis was to investigate two predictions from the experimental-demand account, which posits that action-constraint effects reflect experimental demand bias. Compliant participants who guessed the hypotheses would adjust their response to

Table 2. Effect-Size Differences Between Action-Constraint Manipulations

Contrast	$\hat{\beta}$	95% CrI	BF_{01}
Effort – tool use	–0.08	[–0.43, 0.30]	5.24
Effort – weight	0.19	[–0.26, 0.65]	3.16
Tool use – weight	0.27	[–0.23, 0.77]	3.10

Note: CrI = credible interval. The Bayes factor$_{01}$ (BF_{01}) quantifies the relative evidence for an absence of difference between the action-constraint manipulations (i.e., the reciprocal of BF_{10}; see Note 3).

confirm them, resulting in a confound that would inflate the effect sizes. The first prediction pertained to the role of research design in action-constraint effects. Indeed, some authors have argued that hypothesis guessing is easier in within-subjects designs than in between-subjects designs (e.g., Hutchison & Loomis, 2006). Thus, we should have observed a larger effect for studies using within-subjects designs than for studies using between-subjects designs. Consequently, we also estimated the moderator effect of research design on action-constraint effects. The second prediction pertained to the role of measure in action-constraint effects. Some authors have argued that verbal measures are more sensitive to cognitive biases and voluntary control than other measures (e.g., Woods et al., 2009). Thus, we should have observed a larger effect for studies using verbal measures than for studies using other measures. Consequently, we also estimated the moderator effect of measure on action-constraint effects.

Research design. Using our single-outcome-study data set, we tested whether the action-constraint effect was larger for within-subjects designs (17 studies, 361 participants) than for between-subjects designs (19 studies, 661 participants). A positive $\hat{\beta}$ indicates a larger effect for within-subjects designs than for between-subjects designs (the reverse is true for a negative $\hat{\beta}$). Figure 6 illustrates the posterior distribution of effect size depending on the research design. This analysis revealed anecdotal evidence for an absence of difference between the two types of research designs, $\hat{\beta}$ = –0.26, 95% CrI = [–0.54, 0.01], BF_{01} = 1.04.

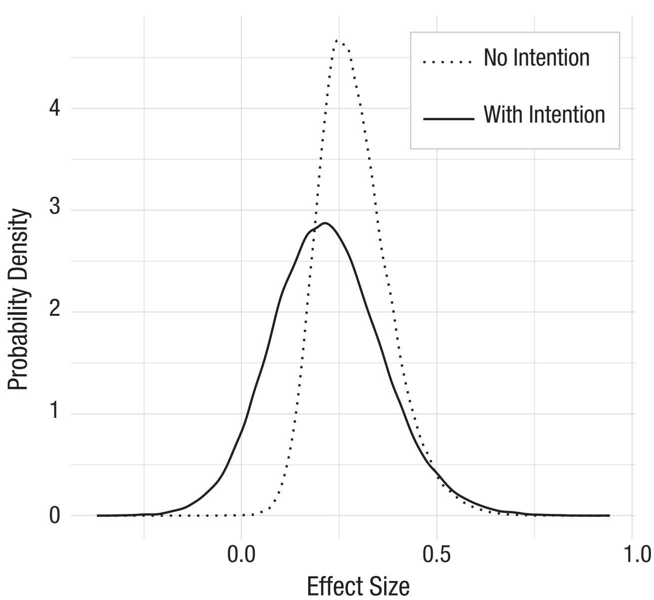

Fig. 5. Posterior distribution of effect sizes as a function of motor intention.

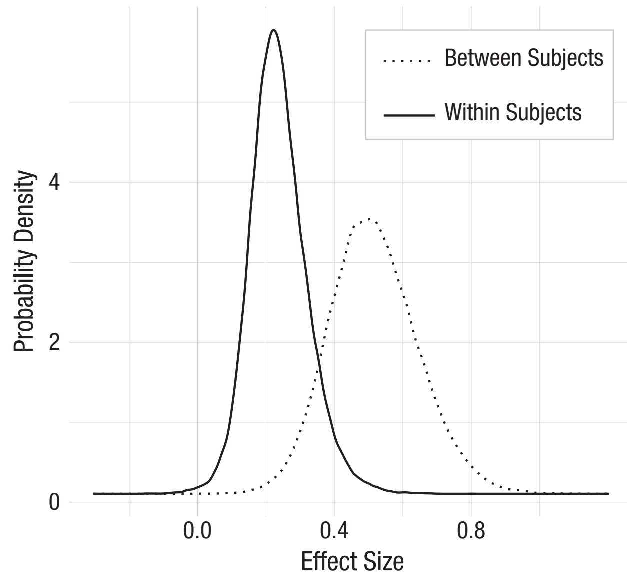

Fig. 6. Posterior distribution of effect sizes as a function of research design.

Measures. For this analysis, we used our full data set, from which we removed two outcomes based on target-size estimation as an indirect measure of perceived distance; we had too few outcomes for this measure compared with the other ones (716 participants, 788 observations). We tested whether the action-constraint effect was larger for the verbal measure (15 outcomes, 560 observations) than for the visual-matching measure (19 outcomes, 444 observations) and for the action measure (8 outcomes, 203 observations). A positive $\hat{\beta}$ indicates a larger effect for the verbal measure than for the others or for the visual-matching measure than for the action measure (the reverse is true for a negative $\hat{\beta}$). Figure 7 illustrates the posterior distribution of effect size depending on the measure. These analyses provided moderate support for an absence of difference among all measures (Table 3).

Additional analyses

Funnel plots. As with all meta-analyses, our conclusions are limited by the fact that we certainly failed to include some relevant studies, because they were unpublished or because the data were unavailable. To estimate

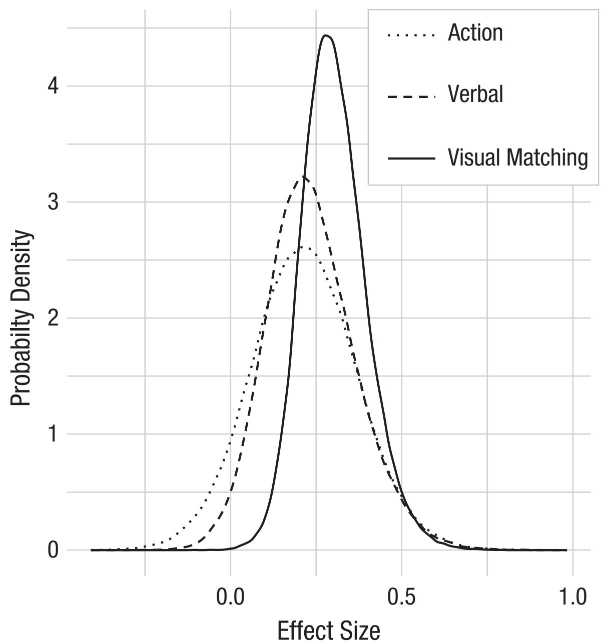

Fig. 7. Posterior distribution of effect sizes as a function of measure.

to what extent a publication bias based on statistical significance could affect our results, we plotted the observed outcome (i.e., effect size) against its standard error (Fig. 8). As can be seen in the left panel of Figure 8, the funnel plot centered on the overall effect size was roughly symmetrical. This is what one would expect if there were no publication bias based on statistical significance; random variation should result in as many observed outcomes on one side of the overall effect size as on the other (i.e., there should be no correlation between the observed outcome and its standard error). Moreover, our data set did not seem too heterogeneous, because most of the observed outcomes fell within the 95% CI represented by the two solid lines.

One limitation of the funnel plot centered on the overall effect is that its asymmetry depends not only on publication bias based on statistical significance but also on the relation between the observed outcome and its standard error. For instance, Peters et al. (2008) argued that small sample size (i.e., in which there is usually a large standard error) often relates to poor study design and overestimation of the observed outcome. Thus, the asymmetry should be the result of a lack of studies showing highly statistically significant effects. In contrast, if there is a publication bias based on statistical significance, the asymmetry should be the result of a lack of studies showing statistically nonsignificant effects. Whereas the funnel plot centered on the overall effect did not allow us to disentangle these potential sources of asymmetry, the contour-enhanced funnel plot did. This funnel plot (Fig. 8, right panel) illustrates the same data set as the funnel plot centered on the overall effect size (Fig. 8, left panel), but it is centered on 0 and shows conventional areas of statistical significance through dark-gray contour lines. If there was indeed a publication bias based on statistical significance, one would expect more observed outcomes in the gray and white outer regions and fewer outcomes in the white inner region. That was not the case here.

Table 3. Effect-Size Differences Between Measures of Distance Perception

Contrast	$\hat{\beta}$	95% CrI	BF_{01}
Verbal – visual matching	−0.07	[−0.35, 0.22]	9.08
Verbal – action	−0.02	[−0.34, 0.29]	6.28
Action – visual matching	−0.09	[−0.42, 0.22]	5.55

Note: CrI = credible interval. The Bayes $factor_{01}$ (BF_{01}) quantifies the relative evidence for an absence of difference between the measures of distance perception (i.e., the reciprocal of BF_{10}; see Note 3).

p-curve. To complement our funnel plots, we conducted a graphically-based *p*-curve analysis to test whether a set of statistically significant *p* values ($p < .05$) supported the existence of a genuine effect rather than the presence of data snooping (e.g., *p*-hacking; Simonsohn et al., 2014). One would expect the *p*-curve to be uniform, right skewed, or left skewed if the data set contained evidential values for the absence of an effect, for the presence of an effect, or for data snooping, respectively. Our *p*-curve tended to be right skewed, which might suggest the presence of a genuine effect (Fig. 9). The slight uptick observed for *p* values of .04 (followed by a slight decrease for *p* values of .05) is not large enough to support the presence of data snooping. However, our observed *p*-curve overlapped nearly perfectly with the expected *p*-curve for an effect tested with 33% statistical power, which is the arbitrary convention proposed by Simonsohn et al. to define low statistical power. This suggests that most of the studies were underpowered and that more *p* values (i.e., outcomes) from properly powered studies should be gathered to allow for firm conclusions (average power = 37%, 90% confidence interval = [14%, 62%]).

Discussion

Our meta-analysis provided extremely strong evidence for the existence of an overall action-constraint effect on distance perception. We estimated its size (*g*) to be 0.29, with a 95% probability of falling in the range between 0.16 and 0.46 (given the data and the priors). According to Cohen's (1988) conventions, this can be considered a small effect in behavioral sciences. Cohen emphasized that his arbitrary conventions were relative to his area of interest and recommended that they be

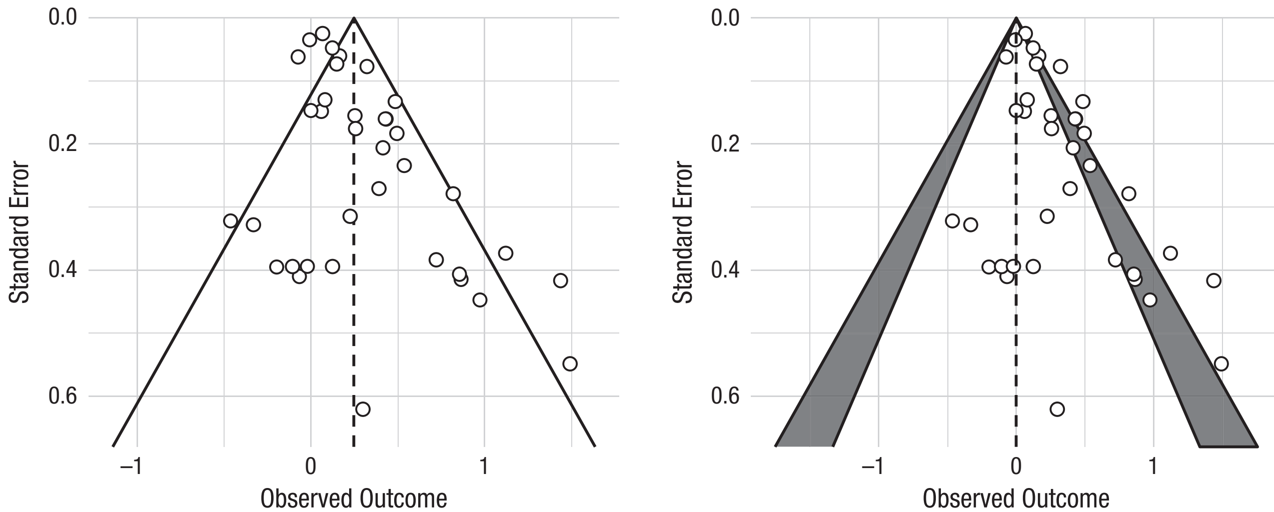

Fig. 8. Funnel plot centered on the overall effect size (left) and contour-enhanced funnel plot centered on 0 (right). Each plot shows standard errors as a function of the observed outcomes. In the left-hand plot, the region between the lines is the 95% confidence interval. In the right-hand plot, shading indicates significance (white inner region: $p > .05$; gray region: $p = .01–.05$; white outer region: $p < .01$).

used "only when no better basis for [interpreting effect size] is available" (p. 25). Thus, we propose new conventions specific to the action-constraint field (see also Funder & Ozer, 2019).

Cohen (1988) based his conventions on "a subjective average of effect sizes such as are encountered in behavioral science" (p. 13). Likewise, we could have considered the average of the posterior distribution of the overall effect size a medium (i.e., typical) effect for the action-constraint field. Because this distribution was slightly asymmetric, we used its mode (i.e., the most probable value) instead. By extension, we also defined

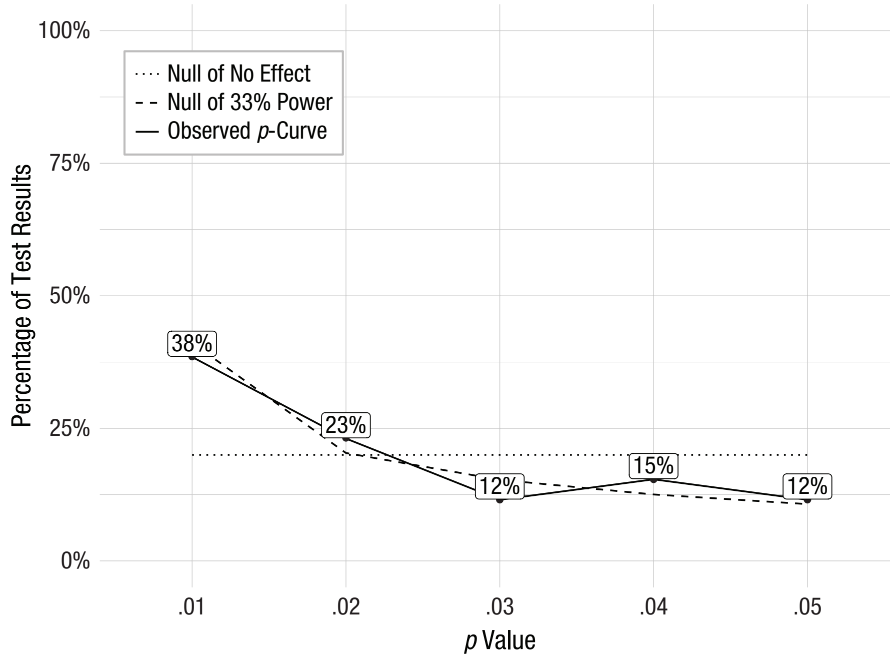

Fig. 9. Observed distribution of p values, the expected distribution of p values for a null effect, and the expected distribution of p values for an effect tested with 33% power. The observed p-curve includes 27 statistically significant p values ($p < .05$), including 17 p values under .025. We excluded the statistically nonsignificant p values associated with the 16 additional outcomes included in the meta-analysis.

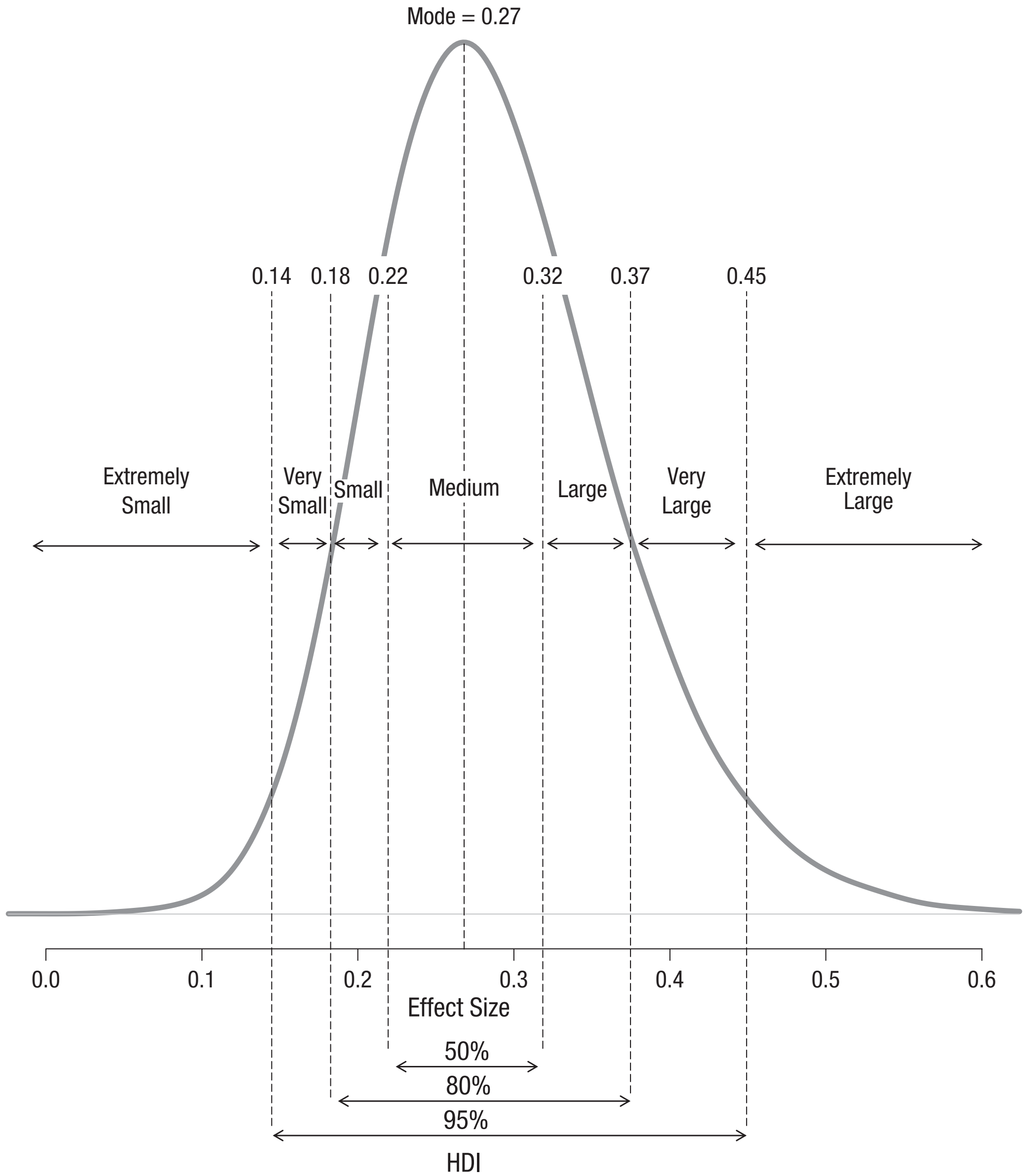

Fig. 10. Posterior distribution of the overall effect size with our field-specific interpretative conventions based on highest density intervals (HDIs) of the posterior distribution (a sort of credible interval).

extremely small, very small, small, large, very large, and *extremely large* effects on the basis of the properties of the posterior distribution (Fig. 10). We hope this will encourage action-constraint researchers to discuss their effect sizes and to do so without relying on Cohen's more general conventions. Our meta-analysis also provided moderate evidence for the existence of a tool-use effect, strong evidence for the existence of an effort effect, and moderate evidence against the existence of a weight effect. Considering our conventions, the tool-use effect, the effort effect, and the weight effect should be considered very large, large, and extremely small, respectively. Despite this, our Bayesian pairwise analyses did not support the moderating role of constraint category.

Taken together, these results are consistent with action-constraint theories of perception, which posit that action constraint influences visual perception of space (for a discussion of these theories, see Morgado & Palluel-Germain, 2016). However, weight manipulations (e.g., wearing vs. not wearing a heavy backpack) might not affect distance perception, as argued by the authors of some replication failures (e.g., Durgin & Russell, 2008; Hutchison & Loomis, 2006; Woods et al., 2009). Because this manipulation is the only one leading to an effect size very close to zero in our meta-analysis, studies using this manipulation should not be used as strong arguments in support of action-constraint theories.

Motor intention

We investigated the role of motor intention in action-constraint effects through the variation of task instructions. We expected a larger effect for studies with task instructions explicitly prompting participants to perform an action than for studies without such instructions. Our analysis did not corroborate this hypothesis, instead showing anecdotal evidence against an effect of task instructions. This conclusion should be considered carefully, because we know little about the task instructions used in most of the articles included in this meta-analysis. Moreover, Proffitt and Linkenauger (2013) proposed that perceiving a spatial property of the environment (e.g., large vs. small distances) would automatically potentiate a relevant action (walking vs. reaching). Further studies should allow researchers to delineate various levels of intention (e.g., instruction-based intention vs. automatic-action potentiation) and their relative roles in action-constraint effects on distance perception.

Experimental demand bias

Some authors have suggested that action-constraint effects might come from experimental demand (Durgin et al., 2009; Firestone, 2013). If this is the case, we should have observed a larger effect for within-subjects designs than for between-subjects designs and for verbal measures than for visual-matching and action measures. Our analyses did not support these hypotheses; instead, they provided anecdotal evidence against an effect of research design and moderate evidence against an effect of measure.

Although the results of our meta-analysis were not consistent with the experimental-demand account, they cannot provide definitive answers about the nature of action-constraint effects. That was not the purpose of the meta-analysis. Indeed, these action-constraint effects might be perceptual or postperceptual (for a discussion, see Philbeck & Witt, 2015). According to Lyons (2015), one approach to visual perception would equate perceptual processes to early vision and post-perceptual processes to late vision, arguing that action constraint influences perceptual judgments but not perception itself. In contrast, another approach to perception would reduce the boundary between perceptual and post-perceptual processes, interpreting action-constraint effects on perceptual judgments as genuine perceptual effects. Because most studies included in our meta-analysis were not designed to address this question, the debate will continue. Nevertheless, even if action-constraint effects are postperceptual, they might be worth studying if they are, for instance, memory effects (e.g., Cooper, Sterling, Bacon, & Bridgeman, 2012) or adaptive-judgment biases (e.g., Haselton et al., 2009; for a computational model, see Shimansky, 2011) rather than mere experimental demand biases.

Strengths, limitations, and recommendations

By focusing only on physical action constraints, we avoided a common shortcut—considering all the action-constraint effects identical and overgeneralizing the conclusions from one to another (for a similar idea, see Proffitt, 2013). Moreover, by using multilevel Bayesian modeling, we overcame the limitations of frequentist (e.g., Wagenmakers et al., 2018) and single-level (e.g., Cheung, 2014) modeling approaches.

We also provided field-specific conventions for interpreting effect sizes that are more relevant to the action-constraint field than Cohen's are. Our conventions are descriptive and allow us to assess the typicality of an effect compared with other known effects in the field. However, they are of little help in assessing the practical importance of effects for a particular context. For example, if action-constraint effects promote adaptive action planning (Proffitt, 2013), it might be useful to show that perceptual differences as large as action-constraint effects can themselves influence action planning (e.g., Gray, 2013). This would allow the determination of the minimum action-constraint effect size of practical importance for action-constraint theories.

We probably failed to include some relevant studies in our meta-analysis either because we missed them or because we could not obtain sufficient statistics to compute effect sizes. This could have led to a publication bias even if our funnel plot makes this possibility unlikely. Moreover, including mainly underpowered studies (see our *p*-curve analyses) in the meta-analysis could have led to misestimation of the effect size (e.g., Stanley & Spence, 2014). Although these limitations are common to most meta-analyses, we think that the continuous nature of our analysis should allow us to correct this bias as more data is sent to us. To assist in this effort, researchers should increase their study quality by using more reliable measures of perceived distance (e.g., Stanley & Spence, 2014), more efficient constraint manipulations and optimal designs (e.g., McClelland, 1997), and larger sample sizes and a greater number of trials per participant (e.g., Forrester, 2015). Because

Table 4. Required Sample Size to Detect Action-Constraint Effects at Various Levels of Statistical Power Using the Two Types of Research Design (Two-Tailed Test, α = .05)

		Between-subjects design				Within-subjects design			
		Power				Power			
Constraint category	*g*	80%	85%	90%	95%	80%	85%	90%	95%
Effort	0.32 [0.11, 0.59]	210	354	414	510	79	90	105	129
Tool use	0.40 [0.12, 0.72]	200	228	266	328	52	59	68	84
Weight	0.13 [−0.26,0.56]	1,860	2,128	2,490	3,078	467	534	624	771

Note: Values in brackets are 95% credible intervals.

null-hypothesis significance testing is ubiquitous, we provide guidance for sample-size planning for future studies (Table 4). When these samples sizes are impossible to achieve, researchers should consider conducting a sequential analysis (e.g., Lakens, 2014) or a multilab project employing a meta-analytic approach to power analysis (e.g., Cohn & Becker, 2003).

Conclusion

We would like the present article to mark the first step in a continuous meta-analysis that will allow us to monitor the state of the action-constraint field as more studies are conducted. We encourage researchers to fuel this continuous meta-analysis by directly uploading their published or unpublished data to our online repository at the Open Science Framework (https://osf.io/bc3wn/). This will help us to improve the estimation of action-constraint effects on distance estimation and may also reveal the role of new moderators. With this article, we wish to stimulate high-quality close and conceptual replications as well as encourage original studies that advance the more general field of action effects on visual perception.

Transparency

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Author Contributions

L. Molto, R. Palluel-Germain, and N. Morgado reviewed all studies and coded those relevant to the meta-analysis. L. Molto, L. Nalborczyk, and N. Morgado analyzed the data. L. Molto, L. Nalborczyk, R. Palluel-Germain, and N. Morgado drafted the manuscript. All the authors approved the final version of the manuscript for submission.

Declaration of Conflicting Interests

The author(s) declared that there were no conflicts of interest with respect to the authorship or the publication of this article.

Open Practices

All data and analysis scripts have been made publicly available via the Open Science Framework and can be accessed at https://osf.io/bc3wn/. The design and analysis plans for this meta-analysis were not registered. The complete Open Practices Disclosure for this article can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797619900336. This article has received the badges for Open Data and Open Materials. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

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Notes

1. Alternatively, action-constraint effects on judgment might be heuristics allowing people to make adaptive decisions (Haselton et al., 2009).
2. We focused only on distance, because more results were available for this spatial property than for others (e.g., slopes).
3. Because this confound issue was present only for blind walking and not for verbal measures, we did not exclude Proffitt et al.'s (2003) Study 3 from the meta-analysis.
4. We interpreted all BFs in this manner. BF_{10}, $p(\text{data} \mid H_1)/p(\text{data} \mid H_0)$, and BF_{01}, $p(\text{data} \mid H_0)/p(\text{data} \mid H_1)$, are the relative evidence for the presence or absence of an effect, respectively ($BF_{01} = 1/BF_{10}$ and $BF_{10} = 1/BF_{01}$).

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