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

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Abstract

Some studies 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. The present multilevel Bayesian meta-analysis supports the existence of a small action constraint effect on distance estimation, *Hedge's g* = 0.29, 95% CrI [0.16, 0.47] (N_{studies} = 37, $N_{\text{participants}}$ = 1035). This effect slightly varied according to the action constraint category (i.e., effort, weight, and tool-use) but not according to participants' motor intention. Some authors argued such effects reflect experimental demand biases rather than genuine perceptual effects. Our meta-analysis did not allow to dismiss this possibility, but it did not support it. We provide field-specific conventions for interpreting action constraint effect sizes and minimum sample size to detect them with various levels of power. We encourage researchers to update this meta-analysis using our online repository (https://osf.io/bc3wn/) to send their published or unpublished data.

Keywords: perception-action, visual perception, distance perception, meta-analysis, open science

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

This paper focuses on the visual perception of space; how people visually assess spatial layouts such as distances and slopes. One might intuitively consider that the visual perception of space only depends on visual information conveyed by optical and oculomotor cues (Cutting & Vishton, 1995). However, growing evidence gathered during the two last decades challenged this idea by suggesting that people also perceive space based on 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 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 a target was closer to them when they could use a tool to reach it more easily (i.e., low constraint) than when they could not (i.e., high constraint). These results were 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; the action-specific account, Philbeck & Witt, 2015; for a discussion on these theories, see Morgado & Palluel-Germain, 2016).

Some researchers have questioned the existence and the nature of action constraint effects. First, several studies failed to show a statistically significant effect leading their authors to conclude that it may not be replicable (e.g., Hutchison & Loomis, 2006; Woods, Philbeck, & Danoff, 2009). Second, some authors argued that action constraints influence how people estimate distances (i.e., perceptual judgement), 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 come from an experimental demand bias in that participants would have adjusted their behavior 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[2] by combining all the relevant results we could gather. Because different constraints (e.g., backpack, 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 in that only constraints associated 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 constraints effects, they should vanish when participants are not explicitly instructed to perform an action on a target before or after estimating its distance than when they were instructed to do it.

The other purpose of the present meta-analysis was to investigate two predictions from the experimental demand account. First, some authors argued that participants are more likely to guess the hypothesis in within-subject than in between-subject designs, as they are aware of the different experimental conditions in the research design (Hutchison & Loomis, 2006). Thus, action constraint effects should be larger in within-subject than in between-subject designs. Second, some authors argued that verbal measures are more sensitive to

[1] Alternatively, action constraint effects on judgement might be heuristics with an adaptive interest in everyday life (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).

cognitive biases and voluntary control than other measures (e.g., Woods et al., 2009). Thus, action constraint effects should be larger for verbal measures than for visual and action-based measures.

Method

In the following section, 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 the following keyword strings [“Effort” and “Distance Perception”] and [“Tool-use” and “Distance Perception”] in BibCNRS (PsychARTICLES, Psychology and Behavioral Sciences Collection, PsychINFO, Academic Search Complete) and Google Scholar. By November 2017, this search returned 308 articles published in peer-reviewed journals. We identified 11 additional articles by searching by authors from the field and asking them for additional published or unpublished studies. We ended up with a total of 319 articles.

We only included in our meta-analysis empirical studies in which the independent variable was a manipulation of a physical action constraint (as opposed to affective or social action constraints like falling fear or social support). In a previous version of the manuscript, we included studies based on visuomotor recalibration as constraint manipulation. However, based on the comment of an anonymous reviewer, we decided to exclude them. Indeed, because the visuomotor recalibration (e.g., treadmill manipulation) was often used with blindwalking as a measure of perceived distance, it was not clear whether it influenced perceived distance, walking, or both. Moreover, the visuomotor recalibration literature was beyond the scope of this meta-analysis. We excluded studies using natural variation 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 hill slope served as an effort manipulation because in such studies effort was cofounded with the visual stimulation.

We only included 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 judgements), 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, replies/commentaries, and empirical studies for which sufficient statistics were not available in the article or from the authors. From the 67 studies that passed the inclusion criteria listed above, we only included 37 studies (Figure 1). The complete results of the literature search and the details of the exclusion procedure are reported in the file 'list_inclusion_exclusion_moderators.xls' available in our Open Science Framework Project (https://osf.io/bc3wn/).

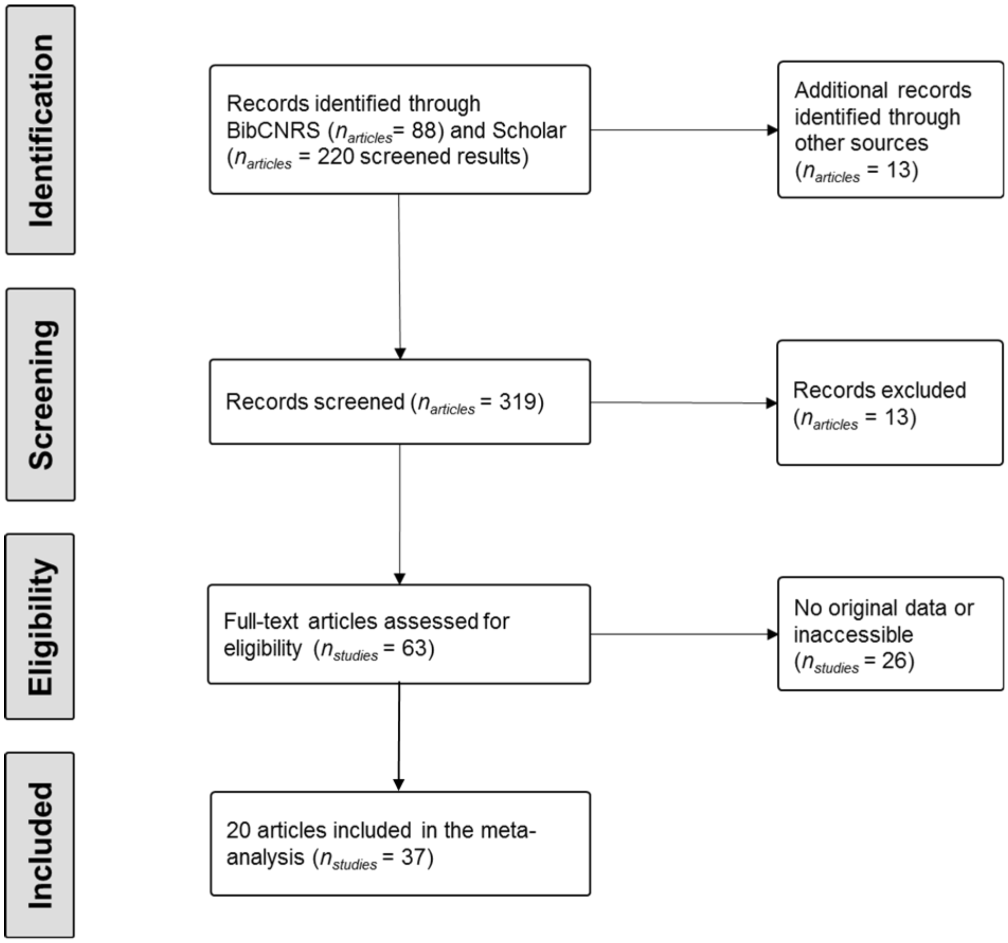

Figure 1. Flow diagram describing the search protocol and workflow used for study selection.

Data Extraction. For each study, RPG, NM, and LM coded independently the following five variables of interest until a consensus was reached (for coding details, see Table 1): the constraint manipulation, the motor intention, the research design, and the measure of distance estimation. 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., blindwalking or blindthrowing to the location of the previously seen target), and an indirect measure of perceived distance (i.e., size estimation). We distinguished between studies following either a within- or between-subject design. Finally, we distinguished studies in which task instructions induced motor intention by prompting participants to perform an action or not.

Table 1

Summary of the studies included in our meta-analysis with the four moderators.

Authors	Experiment	N	Constraint manipulation	Constraint category	Motor intention	Design	Measure
Costello, Bloesch, Davoli, Panting, Abrams & Brockmole (2015)	Exp1 (youth)	32	tool-use (hand pointing vs tool touching)	Tool-use	with intention	within-subject	visual-matching
Durgin & Russell (unpublished, 2008)	Exp.1	28	backpack (with or without)	Weight	without intention	between-subject	verbal
	Exp1_measure1			Weight	without intention		verbal
	Exp1_measure2	24		Weight	with intention	between-subject	action
Hutchison & Loomis (2006)	Exp1_measure3		backpack (with or without)	Weight	without intention		size
	Exp2_measure1	12		Weight	without intention	within-subject	verbal
	Exp2_measure2			Weight			size
Kirsch, Herbort, Butz & Kunde (2012)		24	amplitude of a pointing movement (50% vs. 150% of the distance)	Effort	with intention	within-subject	visual-matching
	Exp1	22		Effort			
Kirsch & Kunde (2013a)	Exp2	23	effort and amplitude	Effort	with intention	within-subject	visual-matching
	Exp3	19		Effort			
Kirsch & Kunde (2013b)	Exp2	23	effort and amplitude	Effort	with intention	within-subject	visual-matching
	Exp3	19		Effort			
Lessard, Linkenauger & Proffitt (2009)		12	ankle weight	Weight	with intention	within-subject	visual-matching
	Exp1			Other			visual-matching
Linkenauger, Bulthoff & Mohler (2015)	Exp2	11	arm lenght (25% larger vs 85% the size of the avatar's arm)	Other	with intention	within-subject	action
	Exp3			Other			visual-matching
	Exp4	12		Other	without intention		
	Exp1_measure1	19		Effort			verbal
	Exp1_measure2			Effort			action
	Exp2_measure1	18		Effort			verbal
Meagher & Marsh (2014)	Exp2_measure2		carry a heavy object alone or with someone else	Effort	with intention	between-subject	action
	Exp3_measure1	19		Effort			verbal
	Exp3_measure2			Effort			action
	Exp4	37		Effort			verbal
	Exp5	60		Effort			verbal
Moeller, Zoppke & Frings (2015)		28	locomotion mode (driving vs. walking)	Effort	without intention	between-subject	visual-matching
Molto, Palluel-Germain, Guinet, Fazioli, Heurley & Morgado (unpublished)		93	tool-use (hand pointing vs tool touching)	Tool-use	with intention	within-subject	visual-matching
Morgado, Gentaz, Guinet, Osiurak & Palluel-Germain (2013)		20	transparent barrier width	Effort	with intention	within-subject	visual-matching
Osiurak, Morgado & Palluel-Germain (2012)	Exp1	21	tool-use (hand pointing vs tool touching)	Tool-use	with intention	between-subject	visual-matching
Proffitt, Stefanucci, Banton & Epstein (2003)	Exp3	24	recalibration optic-flow	Effort	without intention	between-subject	verbal
	Exp1	32	tool-use (hand pointing vs tool touching)	Tool-use			
Witt (2011)	Exp2	16		Tool-use	with intention	between-subject	visual-matching
	Exp3	24	tool-use (laser vs baton)	Tool-use			
Witt, Proffitt (2008)	Exp2	8	tool-use (hand pointing vs tool touching)	Tool-use	with intention	between-subject	visual-matching
Witt, Proffitt & Epstein (2005)	Exp1	16	tool-use (hand pointing vs tool touching)	Tool-use	with intention	within-subject	verbal
	Exp2	8		Tool-use			visual-matching
Witt, Schuck & Taylor (2011)	Exp1	54	swim with or without flippers	Effort	with intention	between-subject	verbal
	Exp1	22	backpack (with or without)	Weight	without intention		
	Exp2	24		Weight			verbal
Woods, Philbeck & Danoff (2009)	Exp3_measure1	24		Weight		between-subject	
	Exp3_measure2		heavy vs light ball throwing	Weight	with intention		action
	Exp4_measure1	24		Weight			verbal
	Exp4_measure2			Weight			action
Zadra, Weltman & Proffitt (2015)		7	caloric supplementation (carbohydrate vs placebo)	Effort	with intention	within-subject	action

Effect size computation. We computed *Cohen's d* using Formulae (1) and (2) for between-subject design (d_s) and within-subject design (d_{rm}), respectively. We compared the mean distance estimations in the high and low constraint conditions. We divided this difference by the pooled standard deviation so that a positive *d* indicated a larger distance estimation in the high than in the low constraint condition. As *d* is biased for small samples, we transformed it into *Hedges' g*, which is commonly used in meta-analyses (Hedges, 1981). To this end, we multiplied *d* by the correction factor *J* (Formula (3). We computed the sampling variance of *g* (Formulae (4) & (5) using formulae from Borenstein, Hedges, Higgins, and Rothenstein (2009). When the correlation between conditions for within-subject designs (*r*) was unavailable, we used the mean value of the available correlations (r = 0.84).

(1) $$Cohen's\ d_s = \frac{meanHC - meanLC}{\sqrt{(nHC-1*sdHC^2+nLC-1*sdLC^2)/(nHC+nLC-2)}}$$

(2) $$Cohen's\ d_{rm} = \frac{meanHC - meanLC}{\sqrt{sdHC^2+sdLC^2-2*r*sdHC*sdLC)/2*1-r}}$$

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

(4) $$V_{d_s} = \frac{n1+n2}{n1n2} + \frac{d^2}{2(n1+n2)}$$

(5) $$V_{d_{rm}} = (\frac{1}{n} + \frac{d^2}{2n})2(1-r)$$

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 perception. We conducted all analyses in R (version 3.4) 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-analyses 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 being independent (Van den Noortgate, López-López, Marín-Martínez, & Sánchez-Meca, 2013). To overcome this non-independence issue, we averaged effect sizes that came from the same study, so that each study yields only one effect size (Cheung, 2014). To model the dependence between studies from the same articles, we included three levels in our model (Figure 2) with participants at Level 1, study at Level 2, and article 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 ($\tau_{article}$), and the between-study variability in the same article (τ_{study}).

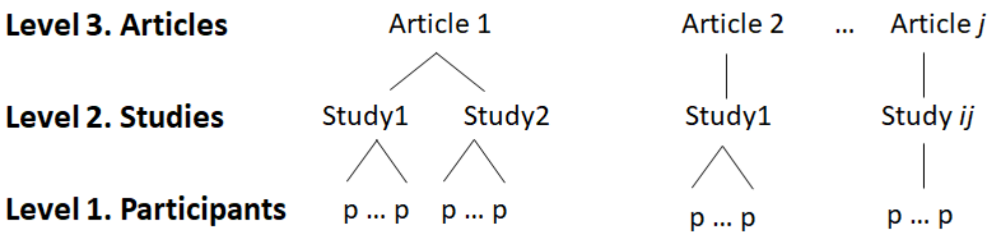

Figure 2. Three-level structure of our meta-analytic model allowing to estimate the between-study variability in the same articles (Level 2) and the between-article variability of the effect size (Level 3).

Bayesian analyses. We conducted all analyses using Bayesian statistics (for an introduction, see Wagenmakers et al., 2018). The main advantage of Bayesian statistics is to consider prior knowledge through the use of prior distributions. Based on 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 α and weakly informative priors on variance components (for R code and mathematical details of this model, see the Supplementary Materials).

Bayesian statistics also allow to quantify the relative evidence for two competing hypotheses. We estimated the relative evidence for the existence of the action constraint effects against its non-existence by comparing a model with the intercept to a model without the intercept. We compared these models using the bayes_factor() method of brms that 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 a hypothesis as anecdotal (BF = [1/3, 1] or [1, 3]), moderate (BF = [1/10, 1/3] or [3, 10]), strong (BF = [1/30, 1/10] or [10, 30]), very strong (BF = [1/100, 1/30] or [30, 100]), or extremely strong (BF < 1/100 or > 100). We also reported 95% credible intervals (CrI), 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 on these intervals, see Nalborczyk, Bürkner, & Williams, 2019). We ran four Markov Chain Monte-Carlo (MCMC) for each model, including each 20,000 iterations with a warmup of 5,000 iterations. We assessed posterior convergence by examining trace plots and the Gelman-Rubin statistic (for R code and technical details, see the Supplementary Materials).

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- 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 symmetric funnel-shaped distribution. If the publication bias is large, more studies should fall on the right of the overall effect size with a high variability, resulting in an asymmetric 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 analysed according to the null-hypothesis significance testing framework. For this reason, we also conducted *p*-curve analyses to test whether a set of p-values has evidential values 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

Dataset

As some authors used multiple effort manipulations in each of their studies (Supplementary Materials), we aggregated their outcomes in order to obtain a single outcome per study. Thus, the resulting full dataset comprised 45 outcomes extracted from 37 studies from 20 articles ($N_{\text{participants}}$ = 1035, $N_{\text{observations}}$ = 1299). In six other studies, the authors used several measures of distance perception, resulting in an effect size estimation (i.e., outcome) per measure (Supplementary Materials). Such multiple-outcome studies weight more in a meta-analysis than single-outcome studies. To avoid this, we rearranged our full dataset by averaging all the outcomes from the same study to include only one outcome per study in our meta-analysis. We used this resulting single-outcome-study dataset (37 outcomes) to estimate the overall effect, the moderator effect of constraint category, and the moderator effect of the research design. It was impossible to estimate the moderator effects of the motor intention and measure after averaging the several outcomes from the same study. Thus, we used our full dataset for these analyses.

Investigating Two Predictions from the 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 effect 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 only constraints associated 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. Thus, we also estimated the moderator effect of instruction-based motor intention on action constraint effects on distance estimation.

Overall effect and moderator effect of constraint category. Figure 3 illustrates the effect size for each article and the overall effect size. Based on our single-outcome-study dataset, the meta-analysis revealed an overall effect of physical action constraints on distance estimation of $g = 0.46$, 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 [0.29, 0.49] range. This analysis revealed an outlier ($g = 2.42$) changing the overall estimate by 36.96% (Supplementary Materials). We decided to discard this study from the subsequent analyses. The updated meta-analysis revealed an overall effect of $g = 0.29$, 95% CrI [0.16, 0.46], $\tau_{article} = 0.18$, 95% CrI [0.01, 0.40], $\tau_{study} = 0.13$, 95% CrI [0.02, 0.26], $BF_{10} = 281.14$. As our posterior distribution was asymmetric 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 [0.16, 0.47] interval (given the prior and the available data). The BF indicated that the data were 281.14 times more likely under the hypothesis of a non-null effect than under the hypothesis of a null effect,[3] which can be interpreted as extremely strong evidence for the existence of the effect relative to its non-existence.

[3] We interpreted all BFs like that. BF_{10}, $p(\text{data}|H_1)/p(\text{data}|H_0)$, and BF_{01}, $p(\text{data}|H_0)/p(\text{data}|H_1)$, are the relative evidence for the presence or the absence of an effect, respectively ($BF_{01} = 1/BF_{10}$ and $BF_{10} = 1/BF_{01}$).

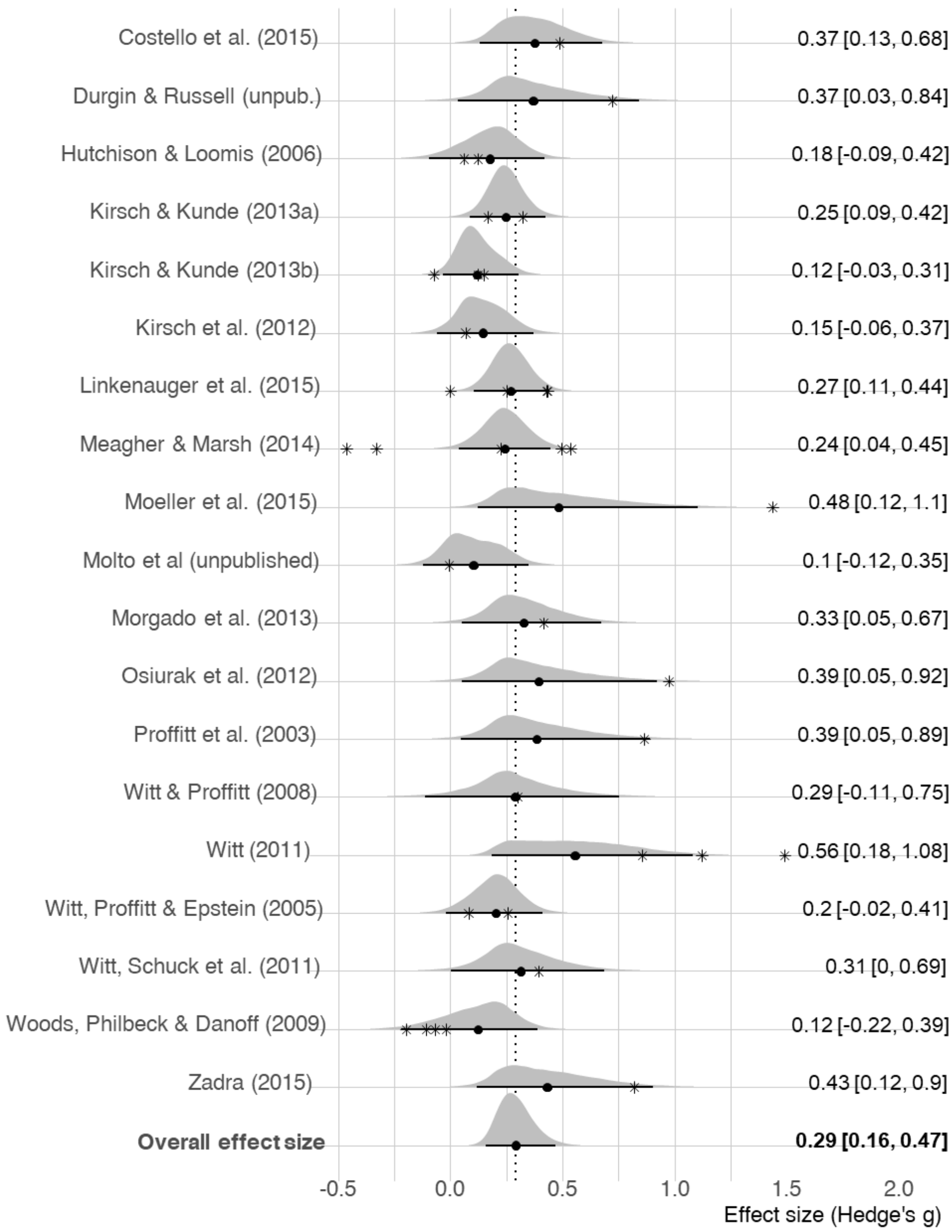

Figure 3. Forest plot of effect sizes. The densities represent the estimations of the model (i.e., the posterior distribution with its mean and 95% credible interval). The stars represent the effect size calculated for each study. Black dots represent the effect size estimated for each article.

As 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 (Figure 4). We discarded four studies from the same articles from this analysis as their manipulation did not fit in any constraint category (Supplementary Materials). Based on our single-outcome-study dataset, 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 ($N_{outcomes}$ = 9, $N_{participants}$ = 250), and strong evidence for an effort effect, g = 0.32, 95% CrI [0.11, 0.59], BF10 = 10.45 ($N_{outcomes}$ = 19, $N_{participants}$ = 416). 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 ($N_{outcomes}$ = 13, $N_{participants}$ = 170).

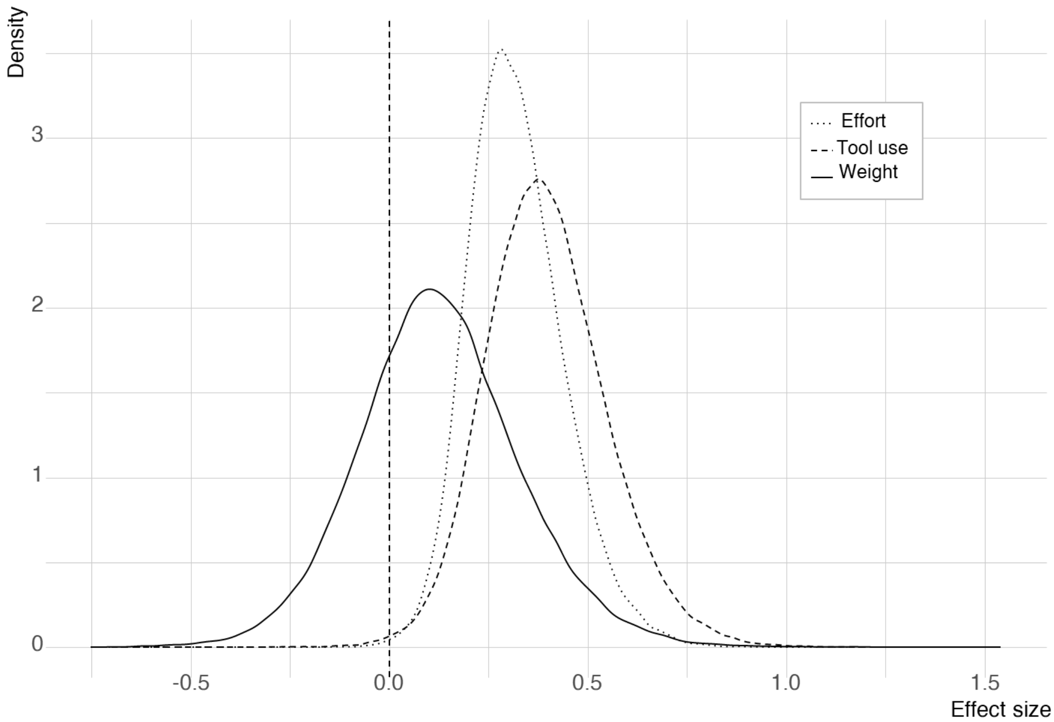

Figure 4. Posterior distribution of effect size according to the constraint manipulation. The x-axis represents the effect size and the y-axis represents the probability density.

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 reported $\hat{\beta}$ indicating the difference between two given constraint categories. A positive $\hat{\beta}$ would indicate a larger effect for effort than tool-use manipulation, for effort than weight manipulation, and for tool-use than weight manipulation (conversely for a negative $\hat{\beta}$). These analyses revealed moderate support for an absence of difference between the three constraints categories.

Table 2

Effect size differences ($\hat{\beta}$) between action constraint manipulations along with their 95% CrI and the BF_{01}.

Contrast	**Estimate ($\hat{\beta}$)**	**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. BF_{01} quantifies the relative evidence for an absence of difference between the conditions (i.e., the reciprocal of BF_{10}, see Footnote 3).

The role of motor intention. Based on our full dataset, we tested whether the action constraint effect from studies in which participants intended to reach the target ($N_{\text{outcomes}} = 35$, $N_{\text{observations}} = 1058$) differed from studies in which they did not ($N_{\text{outcomes}} = 9$, $N_{\text{observations}} = 186$). A positive $\hat{\beta}$ would indicate a larger effect with motor intention than without it (conversely 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], $BF_{01} = 10{,}096.54$.

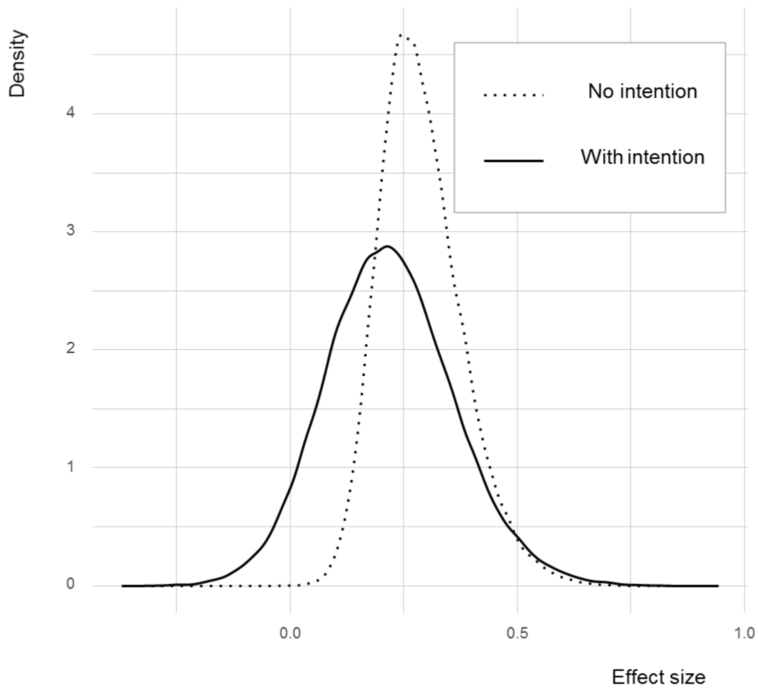

Figure 5. Posterior distribution of effect size as a function of motor intention.

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 confirm them, resulting in a confound which would inflate the effect sizes. The first prediction pertained to the role of research design in action constraint effects. Indeed, some authors argued that hypothesis guessing is easier in within-subject than in between-subject designs (e.g., Hutchison & Loomis, 2006). Thus, we should have observed a larger effect for studies using within-subject designs than for studies using between-subject designs. Thus, we also estimated the moderator effect of research design on action constraint effects on distance estimation. The second prediction pertained to the role of measure in action constraint effects. Indeed, some authors 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. Thus, we also estimated the moderator effect of measure on action constraint effects on distance estimation.

Research Design. Based on our single-outcome-study dataset, we tested whether the action constraint effect was larger for within-subject designs ($N_{studies}$ = 17, $N_{participants}$ = 361) than for between-subject designs ($N_{studies}$ = 19, $N_{participants}$ = 661). A positive $\widehat{\beta}$ would indicate a larger effect for within-subject than between-subject designs (conversely for a negative $\widehat{\beta}$). 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, $\widehat{\beta}$ = -0.26, 95% CrI [-0.54, 0.01], BF_{01} = 1.04.

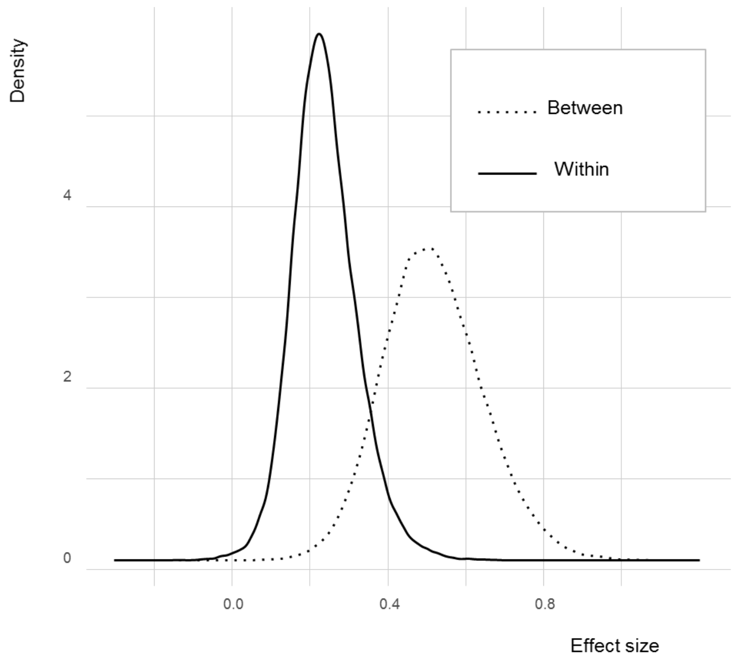

Figure 6. Posterior distribution of effect size as a function of research design.

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

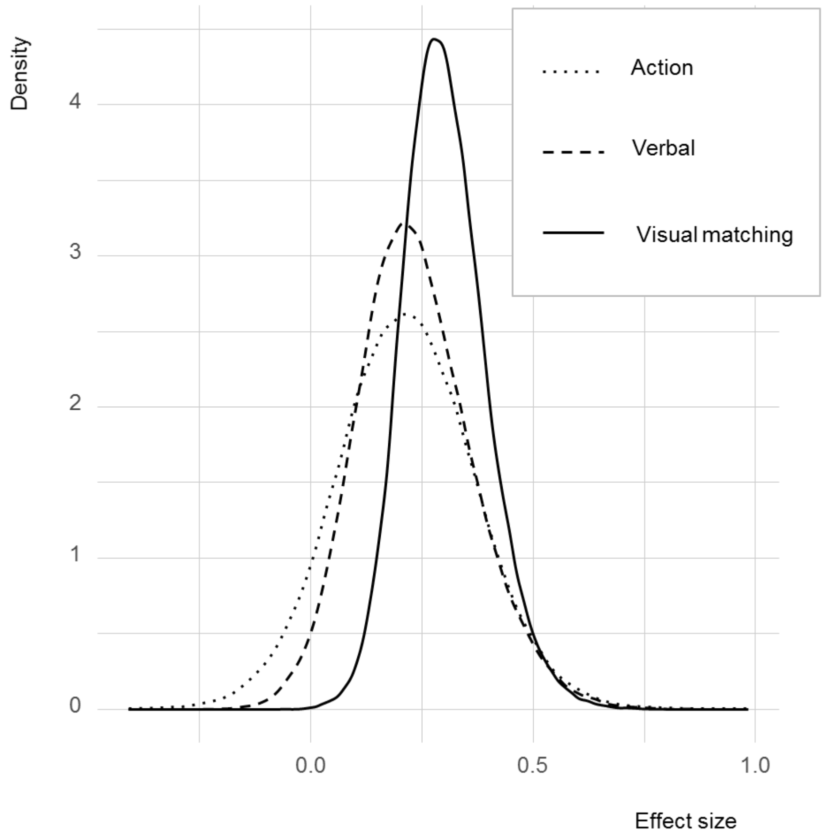

Figure 7. Posterior distribution of effect size as a function of measure.

Table 3

Effect size differences ($\widehat{\beta}$) between measures of distance perception along with their 95% CrI and the BF_{01}.

Contrast	**Estimate ($\widehat{\beta}$)**	**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

Additional analyses

Funnel plots. As for 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 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 (Figure 8). As indicated on the left panel of Figure 8, the funnel plot centred on the overall effect size is roughly symmetrical. This is what one would expect if there was no publication bias based on statistical significance because random variation should result in as many observed outcomes on both sides of the overall effect size (i.e., no correlation between the observed outcome and its standard error). Moreover, our dataset does not seem too heterogenous because most of the observed outcomes fell in the 95% CI represented by the two solid lines.

One limitation of the funnel plot centred on the overall effect is that its asymmetry depends not only on publication bias based on statistical significance but also on the relationship between the observed outcome and its standard error. For instance, Peters et al. (2008) argued that small sample size (i.e., usually large standard error) often relates to poor study design and overestimation of the observed outcome. Thus, the asymmetry should come from a lack of studies showing highly statistically significant effects. In contrast, if there is a publication bias based on statistical significance, the asymmetry should come from a lack of studies showing statistically non-significant effects. Whereas the funnel plot centred on the overall effect does not allow disentangling these potential sources of asymmetry, the contour-enhanced funnel plot (Figure 8, right panel) does. It illustrates the same dataset as the funnel plot centred on the overall effect size, but it is centred on 0 and shows conventional areas of statistical significance through dark-grey contour lines. If there was a publication bias based on statistical significance, one should expect more observed outcomes in the grey and white outer regions and fewer outcomes in the white inner region. This was not the case here.

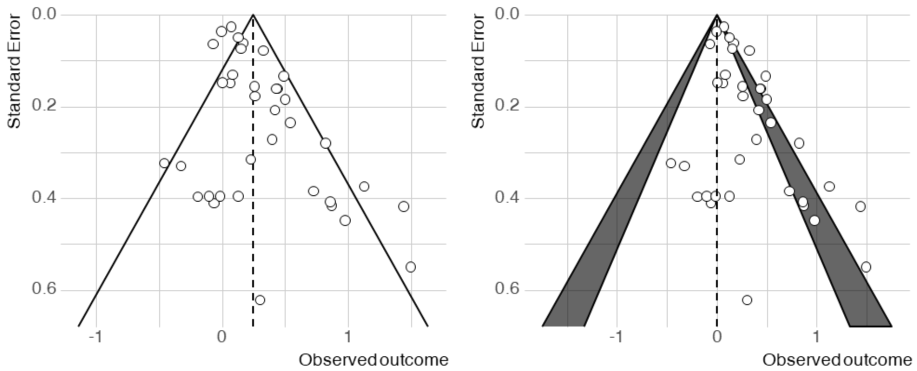

Figure 8. Funnel plot centred on the overall effect size (left panel; 95% CI of the overall effect size represented by the two solid lines) and contour-enhanced funnel plot centred on 0 (right panel; white inner region: $p > .05$, grey region: $p = [.05, .01]$, white outer region: $p < .01$).

***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 (significance threshold = .05) supports the existence of a genuine effect rather than the presence of data snooping (e.g., p-hacking; Simonsohn et al., 2014). One could expect the *p*-curve to be uniform, right-skewed, or left-skewed if the dataset contains evidential values for the absence of an effect, for the presence of an effect, or for data snooping, respectively. Our *p*-curve tends to be right-skewed which might suggest the presence of a genuine effect (Figure 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 the expected *p*-curve for an effect tested with 33% statistical power, which is the arbitrary convention proposed by Simonhson 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 firm conclusions, averaged power = 37%, 90% CI for the averaged power [14%, 62%].

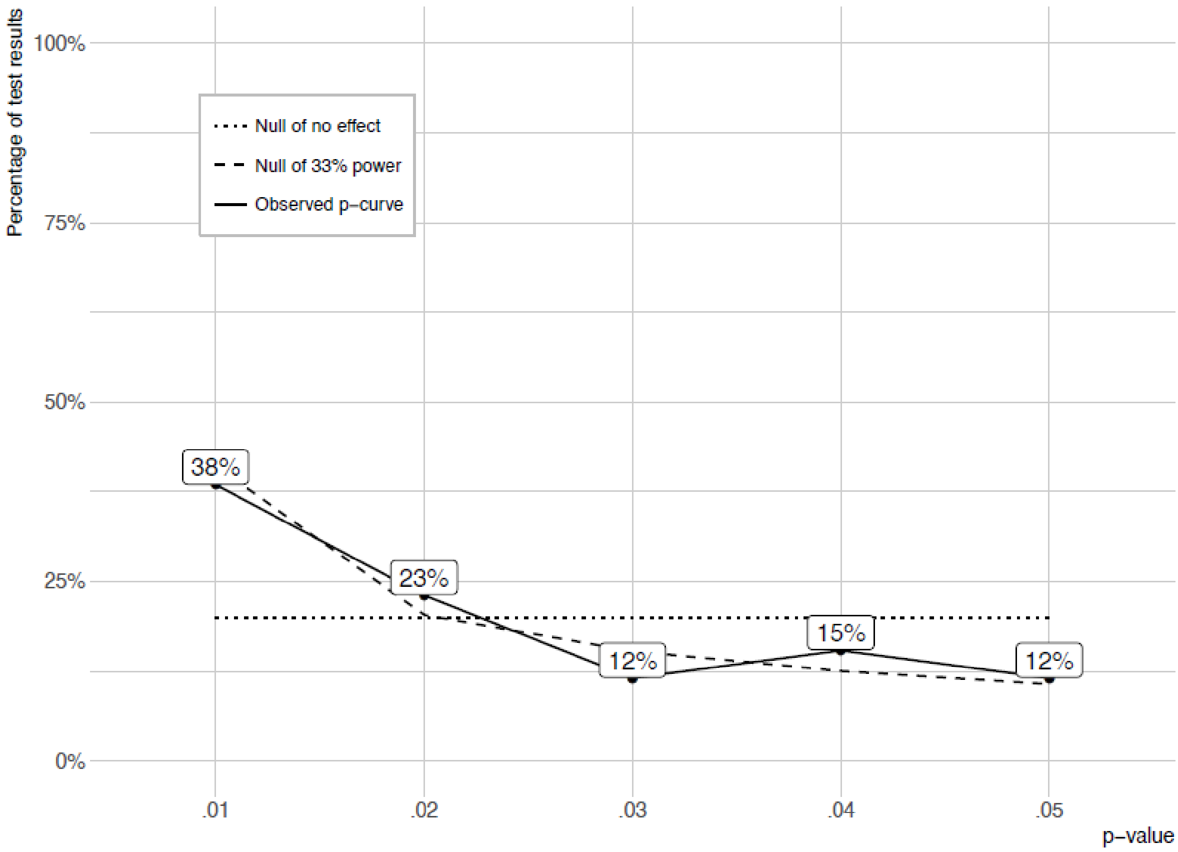

Figure 9. P-curves representing the observed distribution of *p*-values and the expected distributions of *p*-values for a null effect and for an effect tested with 33% power. The

observed *p*-curve includes 27 statistically significant *p*-values ($p < .05$) including 17 p-values under .025. We excluded the statistically non-significant *p*-values associated with the 16 additional outcomes included in the meta-analysis.

Discussion

Our meta-analysis provided extremely strong evidence for the existence of an overall action constraint effect on distance perception. We estimated its size to be $g = 0.29$, with a 95% probability of falling in the range from 0.16 to 0.46 (given the data and the priors). According to Cohen's conventions (1988), this can be considered a small effect in behavioral sciences. Cohen underlined that his arbitrary conventions were relative to his area of interest and recommended to use them "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. As this distribution was slightly asymmetric, we used its mode (i.e., the most probable value) instead. By extension, we also defined extremely small, very small, small, large, very large, and extremely large effects based on the properties of the posterior distribution (Figure 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 moderator role of constraint category.

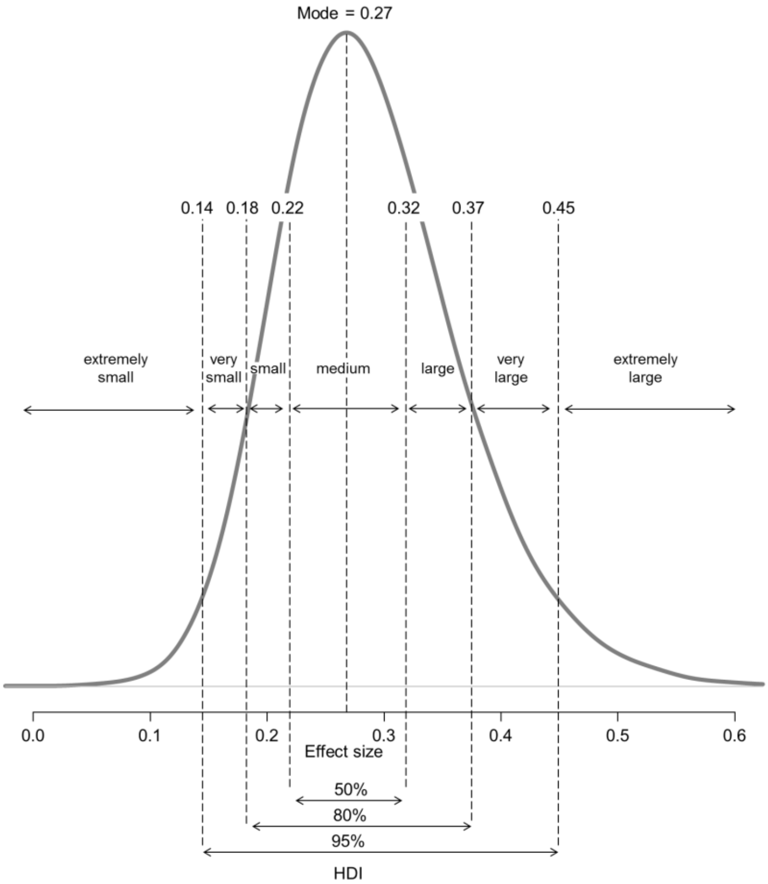

Figure 10. Posterior distribution of the overall effect size with our field-specific interpretative conventions based on highest density intervals of the posterior distribution (i.e., a sort of CrI).

Taken together, these results are consistent with the action constraint theories of perception, which posit that action constraint influence visual perception of space (for a discussion on these theories, see Morgado & Palluel-Germain, 2016). However, weight manipulations (e.g., wearing a heavy backpack or not) 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). As 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 supporting the 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, showing anecdotal evidence against an effect of task instructions. This conclusion should be considered carefully because we know little about 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). Thus, further studies should allow to delineate various levels of intention (e.g., instruction-based intention vs. automatic action potentiation) and their relative role in action constraint effects on distance perception.

Experimental demand bias

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

Although our meta-analysis is not consistent with the experimental demand account, it cannot provide definitive answers about the nature of action constraint effects because this was not its purpose. Indeed, these action constraint effects might be perceptual or post-perceptual (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 judgements 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 judgements as genuine perceptual effects. As 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 post-perceptual, it might be worth studying if they are, for instance, memory effects (e.g., Cooper, Sterling, Bacon, & Bridgeman, 2012) or adaptive judgement 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 modelling, we overcame the limitations of frequentist (e.g., Wagenmakers et al., 2018) and single-level modelling (e.g., Cheung, 2014) approaches.

We also provided field-specific conventions for interpreting effect sizes that are more relevant to the action constraint field than Cohen’s ones. Our conventions are descriptive and allow to assess the typicality of an effect compared with other known effects in the field. However, they are of little help to assess the practical importance of effects for a particular context. For instance, if action constraint effects promote an 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 to determine the minimum action constraint effect size of practical importance for the 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 misestimate the effect size (e.g., Stanley & Spence, 2014). Although these limitations are common to most meta-analyses, we think that the continuous nature of ours should allow to correct this bias as more data is sent to us. To allow this correction, 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/or number of trials per participant (e.g., Forrester, 2015). As null hypothesis significance testing is ubiquitous, we provide guidance for sample size planning for future studies (Table 4). When impossible to achieve by an individual researcher or research team, one should consider to conduct a sequential analysis (e.g., Lakens, 2014) and/or a multi-lab project based on a meta-analytic approach of power analysis (e.g., Cohn & Becker, 2003).

Table 4

Required sample size to detect the action constraint effect with various levels of statistical power for each constraint category depending on research design (two-tailed test, α = .05)

		Between-subject design				**Within-subject design**			
		Power				**Power**			
Constraint Categories	***g* [95% CrI]**	**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]	1860	2128	2490	3078	467	534	624	771

Conclusion

We would like the present paper to mark the first step in a continuous meta-analysis that will allow to monitor the state of the action constraint field as more studies are conducted. We encourage researchers to fuel this continuous meta-analysis by using our online repository at the Open Science Framework (https://osf.io/bc3wn/) to send us their published or unpublished data. This will help to improve the estimation of action constraint effects on distance estimation and may also reveal the role of new moderators. With this paper, 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.

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