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RESEARCH ARTICLE

Rate of recalibration to changing affordances for squeezing through doorways reveals the role of feedback

John M. Franchak[1] · **Frank A. Somoano**[1]

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Abstract

Recalibration of affordance perception in response to changing motor abilities can only occur if observers detect appropriate perceptual information. Recent work suggests that although many affordances can be recalibrated without practicing the specific action to gather outcome feedback—information about whether the attempted action succeeded or failed—calibration of other affordances might depend on outcome feedback (Franchak, Attent Percept Psychophys 79:1816–1829, 2017). However, past work could not rule out the possibility that practicing the action produced perceptual–motor feedback besides outcome feedback that facilitated recalibration. The results of two experiments support the hypothesis that recalibration in a doorway squeezing task depends on outcome feedback as opposed to perceptual–motor feedback. After putting on a backpack that changed participants' doorway squeezing ability, affordance judgments were uncalibrated and remained so even after making repeated judgments. However, after practicing the action, which produced outcome feedback, judgments rapidly calibrated. Moreover, the order of feedback information directly impacted participants' judgments: Participants did not recalibrate if they received only success experience or only failure experience. Recalibration only occurred after participants received both types of feedback experiences, suggesting that outcome feedback is necessary for recalibration in the doorway squeezing task. More generally, the results of the current study support a key tenet of ecological psychology—that affordance perception depends on action-specific information about body–environment relations.

Keywords Affordances · Recalibration · Exploration · Feedback · Perception and action

Introduction

According to the ecological approach to perception (Gibson 1979), perceiving whether actions are possible—that is, perceiving affordances—means detecting information about the fit between the actor's body and the environment (Mark et al. 1990; Warren and Whang 1987). Perceptual information for affordances is thus action-referential (Thomas et al. 2016), and different affordances (i.e., possibilities for different types of actions) are perceived via different informational variables. In contrast, according to computational approaches to perception (e.g., Kording and Wolpert 2006; Marr 1982), observers detect metric properties about the environment that are general-purpose, and thus action-neutral. Decisions about action possibilities are based on a comparison between body representations and representations of environmental properties. From a computational perspective, a single percept of an environmental property can be used broadly for decisions about any actions that involve that property. For example, metric doorway width could be used to judge possibilities for a variety of different actions: walking straight through the doorway (walking task) versus squeezing through the doorway in a sideways position (squeezing task).

However, from the ecological perspective, different informational variables would be needed perceive those affordances if each affordance entails a different body–environment relation. For example, eye-height scaled visual information relating shoulder width to doorway width allows observers to detect affordances in the walking task (Warren and Whang 1987). But whereas affordances in the walking task depend on shoulder width relative to doorway

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✉ John M. Franchak
franchak@ucr.edu

[1] University of California, Riverside, 900 University Avenue, Riverside, CA 92521, USA

width, affordances in the squeezing task involve different relations—torso size and rigidity relative to doorway width (Comalli et al. 2013). Accordingly, recent evidence suggests that eye-height information alone may be insufficient for perceiving affordances for squeezing through doorways (Franchak 2017; Franchak and Adolph 2014).

This alleged dissociation in perception for two actions (walking versus squeezing) that involve the same environmental property (doorway width) supports the ecological claim that perception is action-referential rather than action-neutral. The goal of the current studies is to strengthen that claim by demonstrating that the process of recalibrating in the squeezing task differs from the walking task because perceiving each affordance depends on different action-referential information. Affordance perception presents a continual challenge because changes in the body and abilities alter affordances, requiring recalibration of affordance perception. If affordance perception depends on detecting action-referential, body-scaled information specific to a particular affordance, it follows that the recalibration processes for different affordances should vary depending on how observers learn to detect different types of information. Notably, the computational approach does not make this prediction. There is no a priori reason to expect different recalibration processes in the walking and squeezing tasks if perception depends on action-neutral percepts of doorway width.

Perceptual information for recalibrating to different affordances

Eye-height scaled information supports affordance perception in the walking task (Warren and Whang 1987). In that study, altering eye height information by covertly changing the apparent horizon systematically changed affordance judgments. In the related barrier task (avoiding collision while navigating under barriers), patterns of optic flow generated from postural movements while viewing barriers allow observers to perceive affordances for rolling under barriers in a wheelchair (Stoffregen et al. 2009; Yu et al. 2011; Yu and Stoffregen 2012). Beyond fitting tasks, eye-height scaled information is critical for judging whether seats of different heights afford sitting (Mark et al. 1990; Stoffregen et al. 2005). In the sitting task, perception was calibrated only when participants could generate optic flow information—when standing normally or when allowed to walk in between trials (Mark et al. 1990). When recalibrating to wearing platform shoes that increased participants' height, initial affordance judgments were uncalibrated but gradually improved over trials. However, when participants' ability to generate optic flow was disrupted by adopting an awkward stance, judgments did not improve over trials and remained uncalibrated. Thus, gradual improvement is typical of a recalibration process involving eye-height scaled information from optic flow.

Importantly, outcome feedback—information about successfully performing the action versus failing to perform the action—is not required for recalibration in the above tasks. Practice rolling under barriers does not result in better calibration compared to general wheelchair experience (Stoffregen et al. 2009). Similarly, practicing sitting on seats of different heights does not improve recalibration in the sitting task (Mark et al. 1990). Although these tasks do not require outcome feedback, they do require perceptual–motor feedback, specifically, postural movements to generate eye-height information through optic flow. In this paper, the term perceptual–motor feedback will refer to any non-outcome information generated by the observer.

Recalibration in the squeezing task might depend on outcome feedback instead of or in addition to perceptual–motor feedback (Franchak 2017; Franchak and Adolph 2014). For the walking task, the ratio of shoulder width to doorway width determines whether actors can walk straight through or whether they need to turn to avoid collision (Franchak et al. 2012; Higuchi et al. 2011; Warren and Whang 1987). But in the squeezing task, the sagittal (sideways) width and rigidity of the torso determine whether fitting is possible (Comalli et al. 2013; Franchak et al. 2010). A different body–environment relationship in the squeezing task should necessitate different information for perceiving affordances compared to the barrier and walking tasks. Whereas shoulder width and observer height are static properties that are scaled to eye-height, the rigidity of the body is not. Affordances that involve dynamic properties such as rigidity, friction, or throwability may not be perceived solely based on visual information (Franchak 2017; Franchak and Adolph 2014; Joh et al. 2006, 2007; Zhu and Bingham 2010). Feedback about outcomes may be needed to calibrate visual perception in these instances (e.g., Zhu and Bingham 2010).

Indeed, participants in the squeezing task made uncalibrated judgments after putting on a a backpack that changed affordances even when they had unhindered ability to generate eye-height information, suggesting that eye-height information was insufficient (Franchak 2017). Moreover, providing other types of perceptual–motor feedback without outcome feedback, such as walking around a room and pressing the backpack against a surface, failed to improve calibration. Calibration in the squeezing task only improved after receiving outcome feedback in one of two ways: (1) practicing squeezing through doorways of different sizes, and (2) making verbal judgments and receiving verbal feedback about whether judgments were correct.

Current studies

The current studies examine whether the process of recalibration in the squeezing task differs from recalibration in the walking and barrier tasks by determing the roles of outcome feedback versus perceptual–motor feedback. Such evidence would support a key tenet of the ecological approach—that perception involves detecting action-referential information rather than action-neutral information.

Methodological issues in past work with the squeezing task leave open the possibility that some initial, but incomplete, recalibration occurs in the absence of outcome feedback (Franchak 2017; Franchak and Adolph 2014). Prior work with barrier and sitting tasks collected affordance judgments over repeated trials to study the time course of recalibration and found that errors reduced gradually over trials (Mark et al. 1990; Yu and Stoffregen 2012). In contrast, prior work with the squeezing task calculated judgment error by aggregating across an entire block of yes/no judgments (Comalli et al. 2013; Franchak 2017; Franchak and Adolph 2014; Franchak et al. 2010), making it impossible to know whether calibration changed during repeated judgments. This was addressed in the current studies by testing the time course of learning in the squeezing task in a set of perceptual experience trials during which no outcome feedback was provided. If recalibration requires outcome feedback, judgments should not improve.

Second, past work suggested that outcome feedback was responsible for recalibration in the squeezing task (Franchak 2017) but could not fully disentangle the roles of perceptual–motor feedback and outcome feedback. This limitation stemmed from the dual nature of practice: practice squeezing through the doorway provides simultaneous perceptual–motor feedback (e.g., optic flow from walking to the doorway, haptic information from squeezing through the doorway) and outcome feedback (e.g., recognizing whether the attempt succeeded or failed). Past work attempted to isolate particular types of perceptual–motor feedback, such as optic flow, to show that they were not sufficient for recalibration in the absence of outcome feedback. However, it would be impossible to rule out every type, combination, and amount of perceptual–motor feedback to conclusively demonstrate that outcome feedback is necessary. Thus, whether practice recalibrates perception through outcome feedback or perceptual–motor feedback is an open question.

The current studies take a different approach by comparing the rate of recalibration from practicing in the squeezing task to the rate of recalibration from eye-height information in the walking, barrier, and sitting tasks. Gradual (linear) recalibration is characteristic of recalibration through perceptual–motor feedback in each of those tasks (Mark et al. 1990; Yu et al. 2011). On the other hand, learning from outcome feedback may be more rapid and occur after only a few trials such as in studies of haptic perception of length (Wagman et al. 2001, 2008; Withagen and Michaels 2005). Thus, a set of outcome experience trials assessed affordance judgments as participants practiced. Rapid learning during outcome experience trials would suggest that outcome feedback, not perceptual–motor feedback, drives recalibration from practice in the squeezing task.

Experiment 2 further tested whether outcome feedback and/or perceptual–motor feedback lead to recalibration from practice by varying the information available from outcome feedback. The theory of direct learning states that for calibration to change, there must be feedback information to specify how perception should change to achieve better calibration (Jacobs and Michaels 2007). For example, feedback about the accuracy of rod length judgments in studies of dynamic touch move observers to detect perceptual variables that specify rod length (Wagman et al. 2001). In the squeezing task, outcome feedback only specifies how perception should change if observers experience both successful and failed practice outcomes. In contrast, experiencing only failed outcomes (or only successful outcomes) is nonspecifying: Several practice trials that all result in failure might indicate that the observer's perception is no longer calibrated and that larger doorways are required to fit, but these failures alone would not specify the magnitude and direction of the required change in calibration. If participants can calibrate when experiencing only successes or only failures, it would suggest that perceptual–motor feedback rather than outcome feedback from practice recalibrates perception. Alternatively, requiring both successes and failures for recalibration would indicate that outcome information, not perceptual–motor feedback, recalibrates perception.

Experiment 1

Experiment 1 tested whether the time course of recalibration in the squeezing task resembles the gradual recalibration observed in barrier and sitting tasks. Perceptual experience trials were modeled after the procedure introduced by Mark (1987): participants made repeated affordance judgments while standing in place without outcome feedback. If the same perceptual information supports recalibration in the squeezing task as in the barrier and sitting tasks, judgment errors should gradually reduce over time. However, if participants require outcome feedback, judgments should not change.

Experiment 1 also measured the rate of learning during practice to determine whether practice recalibrates perception through perceptual–motor feedback versus outcome feedback. After the perceptual experience trials, participants completed a set of outcome experience trials that alternated blocks of practice trials with affordance judgment trials. If practice recalibrates perception via perceptual–motor feedback gathered while walking through the doorway, errors should gradually decline across outcome experience trials. However, if practice recalibrates perception through outcome feedback, errors should be rapidly reduced.

Methods

Participants

Participants were 25 undergraduate students (17 female, 8 male) from the University of California, Riverside. Mean age was 20.5 years (SD 2.6). Informed consent was obtained from all participants included in the study. Participants were recruited from the psychology department participant pool and received course credit. One additional participant was recruited but failed to follow instructions; his data were excluded from analysis. Participants averaged $M = 165.3$ cm (SD 10.9) in height and $M = 69.9$ kg (SD 18.9) in weight.

Apparatus

Doorways of different widths were presented with the same adjustable doorway apparatus used in Franchak (2017) (Fig. 1). The apparatus had a sliding door (185 cm tall × 100 cm wide) attached to a track contained within a free-standing support structure. On the left side of the support structure, a stationary wall (182 cm tall × 62 cm wide) was positioned perpendicular to the sliding door. The sliding door could be locked in place to ensure that the doorway width remained unchanged as participants squeezed through. Calibration markings (0–70 cm in 0.5 cm increments) were captured by a miniature camera and displayed to the experimenter so that the doorway size could be accurately set on each trial. Participants began each trial behind a starting line 320 cm from the sliding door. The wall behind the doorway (when viewed from the starting line) was completely covered in a large sheet of white paper to obscure landmarks that might aid perceptual judgments.

As in Franchak (2017), participants wore a backpack that weighed 1.1 kg and measured 43 cm tall × 25 cm wide × 12 cm deep. The backpack was worn on the back with waist and sternum straps securing the backpack in place. Rigid cardboard inside the backpack ensured that the backpack stayed the same size as participants squeezed through the doorway. The backpack was weighted evenly throughout and was sufficiently heavy to be perceived haptically by participants.

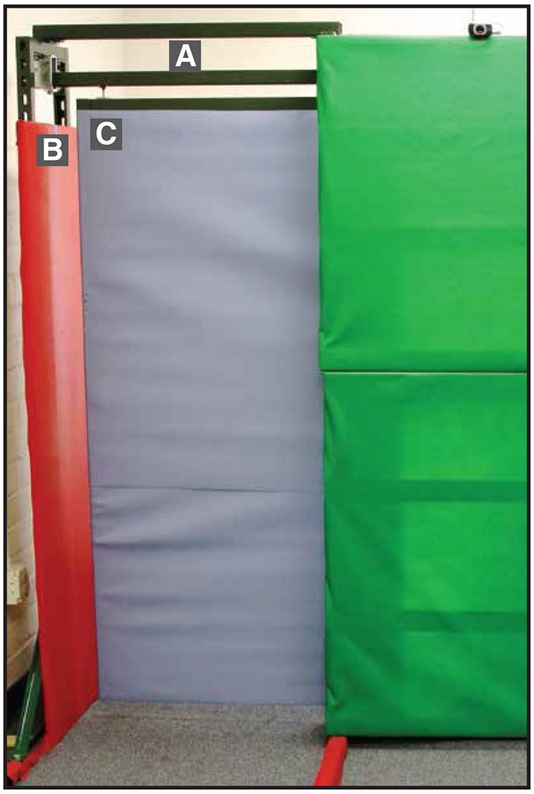

Fig. 1 Adjustable doorway apparatus. A free-standing metal frame (**a**) held a fixed wall (**b**) and a trolley on which a moveable wall (**c**) was mounted. The space between the two walls was adjusted to create doorways of varying width

Procedure

Participants put on the backpack and then completed two experimental phases. The perceptual experience trials (PETs) consisted of 24 perceptual judgments while standing at the starting line, replicating the procedure of Mark (1987). Outcome experience trials (OETs) consisted of four sets trials to test the time course of learning from practice (Fig. 2a). Each OET set contained 5 practice trials followed by 4 perceptual judgments. Data from practice trials in which participants were asked to attempt to fit through the doorway were used to determine each participant's affordance boundary—the smallest doorway they successfully squeezed through while wearing the backpack.

For each affordance judgment, participants were asked to indicate the smallest doorway they could squeeze through sideways with the backpack. A method of limits procedure was used (e.g., Mark 1987), with successive judgments alternating between ascending and descending trials. On ascending trials, the experimenter slowly opened the doorway until the participant told the experimenter to stop moving the doorway. The participant was encouraged to ask the experimenter to increase or decrease the doorway size until

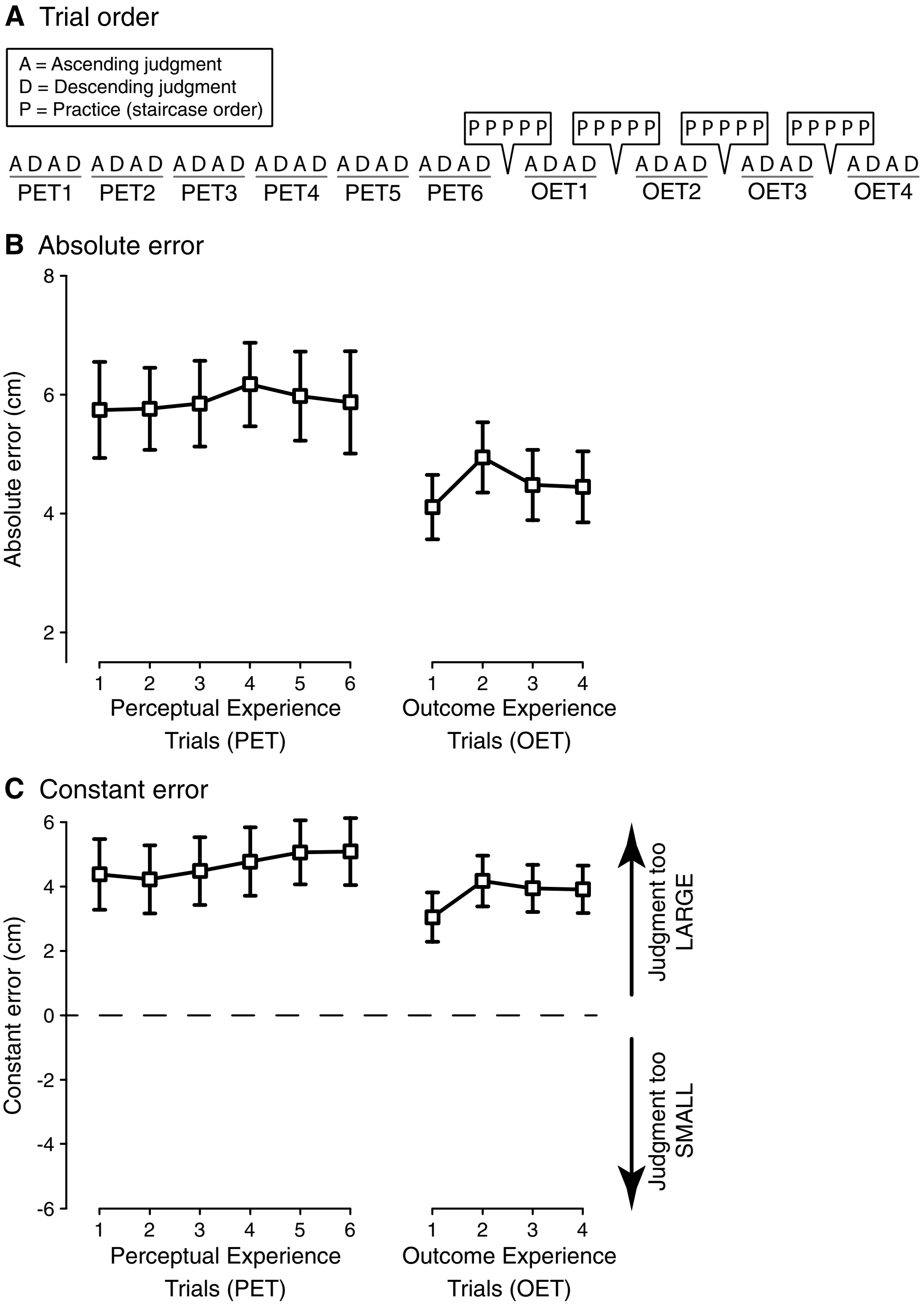

Fig. 2 **a** Schematic of trial procedure in Experiment 1. A perceptual experience phase consisting of 24 judgments was followed by an outcome experience phase which contained alternating blocks of 5 practice and 4 judgment trials. Each set of 4 judgments were averaged into a single error score. Changes in **b** absolute error and **c** constant error are shown over perceptual experience trials (PET) and outcome experience trials (OET). Error bars show ± 1 SE

it resembled the smallest doorway the participant could squeeze through. For descending trials, the doorway started from the largest possible increment (70 cm) and decreased in size.

During practice trials, participants attempted to squeeze through the doorway sideways. Doorway sizes for the 20 practice trials were determined according to a staircase procedure (Cornsweet 1962), starting from 32 cm on trial 1. Subsequent doorway sizes were determined based on the outcome of the prior trial. Doorway sizes were decreased by 2 cm after each success and increased by 1.5 cm after each failure. The staircase algorithm ensured that participants were exposed to both successful and failed outcomes over the 20 trials and that trials were placed near the affordance boundary. The affordance boundary was defined as the smallest doorway that the participant successfully squeezed through during the 20 practice trials.

Results

Variations in participants' body size mean that affordance boundaries vary. Thus, errors were measured for each individual participant relative to their affordance boundary. Absolute error measured the magnitude of error and was calculated as the absolute value of the difference between perceptual judgments and affordance boundaries. Constant error measured the bias in judgments and was calculated by subtracting each participant's affordance boundary from their judgments. Negative constant errors indicate a negative bias, meaning participants reported they could fit through small doorways that were impossibly small. Positive constant errors indicate a positive bias, meaning participants reported needing unnecessarily large doorways.

Each set of 4 judgments (2 ascending and 2 descending) were averaged, yielding 6 perceptual experience trial (PET) scores and 4 outcome experience trial (OET) scores (Fig. 2a). Each OET score reflects the 4 judgments made following one set of practice trials. Absolute and constant error were analyzed in separate ten trial set (PET and OET sets) repeated-measures ANOVAs that tested whether errors changed across trials. Four planned contrasts addressed the following questions. First, did error change after the introduction of outcome feedback (comparing the 6 PET errors to the 4 OET errors)? Second, did error change during perceptual experience trials in the absence of outcome feedback (linear contrast on PET errors)? Third, did error change after the presentation of outcome feedback (linear contrast on OET errors)? Finally, did error change after the first set presentation of outcome feedback (comparing the last PET error to the first OET error)?

Absolute error

Judgment errors improved after outcome experience was introduced (Fig. 2b), as confirmed by a significant contrast comparing PET and OET errors, $F(1, 24) = 4.74$, $p = .04$, partial-$\eta^2 = 0.17$. However, a linear contrast showed no change from perceptual–motor feedback during the PET block, $F(1, 24) = 0.06$, $p = .81$, partial-$\eta^2 = 0.003$, nor within the OET block, $F(1, 24) = 0.09$, $p = .77$, partial-$\eta^2 = 0.004$. The overall change in error resulted from a single decrease that occurred between the last perceptual experience trials ($M = 5.87$ cm, SD 4.3) and the first outcome experience trials ($M = 4.11$ cm, SD 2.7), $F(1, 24) = 8.01$, $p = .009$, partial-$\eta^2 = 0.25$.

Constant error

Positive constant errors in the two blocks of trials indicated that most participants erred by making overly large affordance judgments (Fig. 2c). Mean constant errors were positive for 21/25 participants during PET and 22/25 participants during OET. Thus, the improvements in absolute error were achieved by participants decreasing estimates while maintaining the same direction of bias. However, constant error did not significantly change from PET to OET, $F(1, 24) = 1.70$, $p = .21$, partial-$\eta^2 = 0.07$. Like absolute error, there was no evidence of change in constant error within PET, $F(1, 24) = 0.86$, $p = .36$, partial-$\eta^2 = 0.04$, or OET, $F(1, 24) = 0.98$, $p = .33$, partial-$\eta^2 = 0.04$. Constant error significantly decreased from the last PET set ($M = 5.09$ cm, SD 5.2) to the first OET set ($M = 3.04$ cm, SD 3.2), $F(1, 24) = 1.70$, $p = .21$, partial-$\eta^2 = 0.07$.

Discussion

As predicted, participants did not recalibrate until they received outcome feedback from practice walking through doorways, as in past work (Franchak 2017; Franchak and Adolph 2014; Franchak et al. 2010; Yasuda et al. 2014). Both absolute and constant error remained unchanged over the PET phase, indicating that perceptual–motor feedback did not calibrate participants' perception as it did in eye-height scaled tasks (Mark 1987; Mark et al. 1990; Stoffregen et al. 2005). Recalibration occurred during the OET phase with the change occurring rapidly rather than gradually, which suggests a recalibration process dependent on outcome feedback rather than (or in addition to) perceptual–motor feedback. Judgments did not change as participants accrued more practice. Instead, outcome feedback from the first five practice trials led participants to reach ceiling accuracy.

Experiment 2

Experiment 1 suggests that outcome feedback from practice led to a rapid decrease in error. Experiment 2 was designed to further examine the roles of outcome feedback and perceptual–motor feedback when learning from practice.

If outcome feedback rather than perceptual–motor feedback is responsible for recalibration, recalibration should depend on whether outcome feedback specifies how calibration should change (Jacobs and Michaels 2007). Specifically, participants should only be able to recalibrate if they experience both success and failure outcomes. Partial support for this prediction comes from the work of Yasuda and colleagues (2014). Participants who practiced fitting through doorways of different sizes, which resulted in both success and failure, were compared to participants who practiced walking only through large doorways that resulted in success. Participants who received only successes did not recalibrate, whereas those who received both success and failure information did. However, this does not rule out the possibility that participants would have recalibrated if they experienced successes that were more informative, that is, closer to their affordance boundaries.

The current study tested the role of success and failure feedback by comparing three trial order conditions—failure

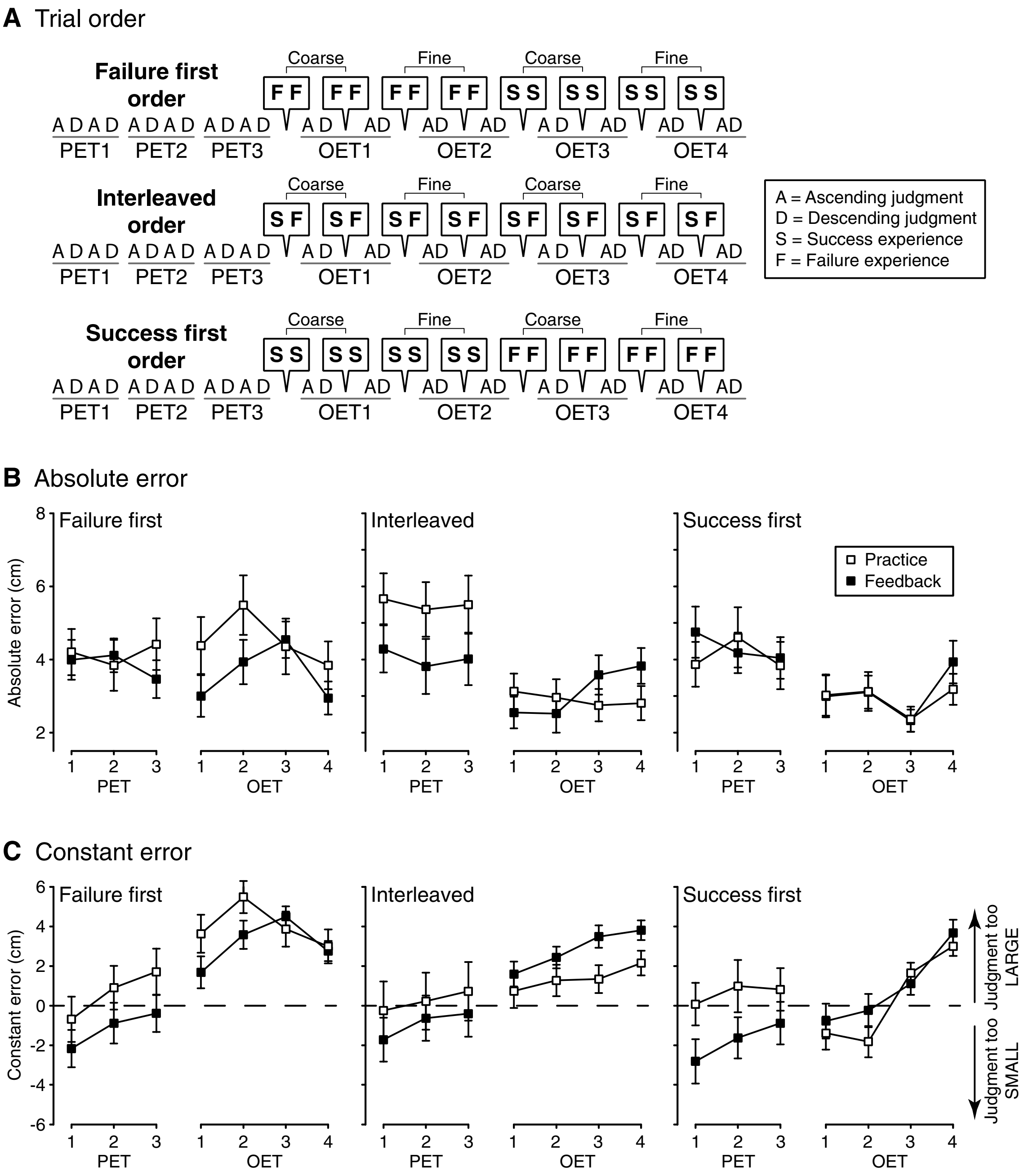

Fig. 3 **a** Schematic of trial procedure in Experiment 2. A perceptual experience trial (PET) phase of 12 judgments was followed by an outcome experience trial (OET) phase which contained alternating blocks of 2 experience trials and 2 judgment trials. Three trial order conditions (failure first, interleaved, or success first) differed only with respect to the ordering of success (S) and failure (F) experience trials, highlighted in bold type. Regardless of order, each set of four experience trials alternated between coarse-grained (very large and very small doorways) and fine-grained trials (doorways closer to the affordance boundary). Each set of four judgments were averaged into a single error score. Changes in **b** absolute error and **c** constant error are shown over perceptual experience trials (PET) and outcome experience trials (OET). Error bars show ± 1 SE

first, interleaved, and success first (Fig. 3a). Failure first participants received a block of only failure experiences (squeezing through impossibly small doorways) followed by a block of only success experiences (fitting through doorways that are sufficiently wide). Participants in the interleaved condition received alternating experience with successes and failures. Success first participants received a block of only success experiences followed by a block of only failure experiences. If outcome feedback is required, recalibration should only be achieved once participants have experienced both success and failure.

Experiencing trials close to the affordance boundary might lead to quicker recalibration compared to experiencing trials farther from the affordance boundary. Indeed, participants in Experiment 1 recalibrated after the first block of practice trials, which, due to the staircase procedure, included trials close to the affordance boundary. Such quick recalibration would interfere with testing the effect of different trial orders, thus, in Experiment 2 we purposefully presented trials far from the affordance boundary (termed "coarse" trials) before presenting trials close to the affordance (termed "fine" trials).

Even if outcome feedback is required for recalibration as hypothesized, perceptual–motor feedback might provide an additional influence. Past work could not determine whether recalibration differed between practice (which provides both outcome and perceptual–motor feedback) and verbal judgments with outcome feedback provided by the experimenter (no perceptual–motor feedback from practice) (Franchak 2017). Endpoint calibration was similar between practice and verbal feedback conditions, but it is possible that observers recalibrated at different rates depending on whether perceptual–motor feedback was available. Thus, Experiment 2 compared the rate of recalibration in practice and verbal feedback conditions. If recalibration is equivalent between the two conditions, it is unlikely that perceptual–motor feedback affects recalibration from practice. However, if the rate of recalibration differs between the practice and verbal feedback conditions, recalibration from practice is affected by both outcome and perceptual–motor feedback.

Method

Participants and design

Participants were 120 undergraduate students (65 female, 55 male) aged $M = 19.7$ years (SD 1.1). Informed consent was obtained from all individual participants included in the study. Participants were recruited from the University of California, Riverside psychology department participant pool and received course credit. Four additional participants were recruited but were excluded from analyses for failure to follow instructions (2 participants) or equipment failure (2 participants). Participants averaged $M = 166.5$ cm (SD 9.9) in height and $M = 68.2$ kg (SD 18.1) in weight. Participants were assigned to one of three trial orders (practice first, interleaved, and failure first) and one of two experience types (practice, verbal feedback). All participants completed perceptual experience trial and outcome experience trial phases. Thus, there were 20 participants in each of the 6 combinations of experience type and order in a 3 Trial Order (between-subjects) × 2 Experience Type (between-subjects) × 2 Trial Phase (within-subjects) design.

Apparatus and procedure

The same doorway apparatus and backpack were used as in Experiment 1. Participants completed three experimental phases while wearing the backpack: affordance boundary trials, perceptual experience trials (PET), and outcome experience trials (OET).

Fifteen affordance boundary trials at the start of the session determined which doorway sizes to present in later OETs to achieve the desired trial order (success first, interleaved, or failure first). Affordance boundaries were also required to provide outcome feedback for participants in the verbal feedback condition. Affordance boundary trials were similar to practice trials in Experiment 1—participants squeezed through different doorway sizes as determined by a staircase procedure. Participants were required to keep their eyes closed during affordance boundary trials, which has been shown to prevent recalibration (Franchak 2017). Participants stood one step away from the doorway with their eyes closed. After setting the doorway size, the experimenter asked participants to squeeze through, guiding themselves through the doorway with their hands. Once they passed through the doorway or became stuck, the experimenter opened the doorway to its widest dimension before participants opened their eyes.

Afterwards, PETs were completed as in Experiment 1. Because judgments did not change over the PET in Experiment 1, the phase was abbreviated to 12 judgment trials.

Finally, participants completed OETs, which consisted of 8 sets of 4 trials (2 experience trials followed by 2 judgments) (Fig. 3a). Trial orders and experience conditions differed only in the experience trials, with the key procedural differences outlined in the sections below. All participants received the same set of doorway sizes during experience trials with respect to their individual affordance boundaries. Participants received eight success experiences—doorways that were 2, 3, 4, 5, 6, 7, 8, and 9 cm larger than their affordance boundary—and eight failure experiences—doorways that were 2, 3, 4, 5, 6, 7, 8, and 9 cm smaller than their affordance boundary. For example, a participant with an affordance boundary of 30 cm would receive failure experiences with doorways from 21 to 28 cm in width and success experiences from 32 to 39 cm in width. Based on their proximity to the affordance boundary, doorways 2–5 cm from the affordance boundary were termed “fine-grained” and doorways 6–9 cm away were termed “coarse-grained”. Doorways were presented from coarse to fine to make the task more challenging for participants compared with Experiment 1, in which recalibration occurred after the first practice block.

Failure first and success first orders

Failure first participants received eight failure trials before receiving eight success trials (Fig. 3a). Within the failure set and the success set, trials proceeded from coarse to fine, so the complete order was: 4 coarse failures, 4 fine failures, 4 coarse successes, 4 fine successes. The order of 4-trial block was randomized. For example, a failure first participant with an affordance boundary might receive the following trials ordered as follows: eight failures (22, 23, 24, 21, 26, 27, 28, 25) followed by eight successes (39, 36, 37, 38, 35, 33, 32, 34).

The success first order was simply the opposite of the failure first order: Participants received eight successes (4 coarse then 4 fine) followed by eight failures (4 coarse then 4 fine).

Interleaved order

In the interleaved condition, each successive pair of trials contained one success and one failure. To mirror the other two orders, trials 1–4 and 9–12 were coarse and trials 5–8 and 13–16 were fine. As with the other order conditions, exemplars were randomly ordered. For example, an interleaved participant with an affordance boundary of 30 cm might receive the following order of alternating success and failure trials: 38, 23, 36, 21, 33, 28, 34, 25, 39, 22, 37, 24, 32, 27, 35, 26.

Experience conditions

Participants in the practice condition practiced during experience trials by attempting to fit through the doorway, identical to the practice trials in Experiment 1. In the verbal feedback condition, participants were shown a doorway and asked to make a yes/no judgment about whether they could squeeze through the doorway. The experimenter provided verbal feedback for each judgment by saying “Correct” if the participant said “Yes” to a doorway larger than their affordance boundary or said “No” to a doorway smaller than their affordance boundary. Otherwise, the experimenter responded “Incorrect”.

Results

Absolute and constant errors were calculated as in Experiment 1: scores were condensed for analysis by averaging each set of four judgments (Fig. 3a). This resulted in 3 sets of perceptual experience trials (PETs) and 4 sets of outcome experience trials (OETs). Each OET set reflects judgments made following one set of experience trials. For example, in the success-first order OET 1 indexes judgments following coarse-grained successes and OET 4 indexes judgments following fine-grained failures.

Three sets of analyses were conducted. The first set tested overall change in error from perceptual experience to outcome experience trials. However, because participants received outcome feedback in different orders, it would be inappropriate to test recalibration based on the OET average. Thus, overall recalibration was assessed by comparing the final PET error with the final OET error, at which point participants had all received the same experiences.

The second and third sets of analyses tested for changes in error within PET and within OET, respectively. Although nearly all participants in Experiment 1 were biased in the same direction (e.g., judgments were too large) and bias did not qualitatively change over time, visual inspection of Fig. 3c indicated that biases in Experiment 2 varied considerably and changed over the course of the two phases. Thus, trial-by-trial changes in absolute error were uninterpretable because they could result from either a change in bias, a change in error magnitude, or both. Consequently, analyses of change within PET and OET were conducted only on constant error. PET constant errors were tested using linear contrasts. Change in OET errors were tested with linear and quadratic trend contrasts to investigate the shape of change; pairwise comparisons compared errors between successive trial sets to determine when changes occurred.

Change from perceptual experience to outcome experience

Figure 3b shows that absolute errors decreased from the end of PET ($M = 4.9$ cm, SD 2.6) to the end of OET ($M = 3.7$ cm, SD 2.1) across order and experience type. The 2 Trial Set (PET 3, OET 4) × 3 Order × 2 Experience Type ANOVA revealed only a significant main effect of Trial Set, $F(1, 114) = 8.71$, $p = .004$, partial-$\eta^2 = 0.07$.

Constant error differed compared to Experiment 1. Whereas PET errors were positively biased in Experiment 1, PET errors in Experiment 2 were negatively biased ($M = -0.39$ cm, SD 5.0). When averaged across all three PET sets, 57.5% of participants were biased towards reporting that they could fit through impossibly small doorways. Constant errors increased from the end of PET ($M = -1.26$ cm, SD 5.2) to the end of OET ($M = 3.1$ cm, SD 2.8) (Fig. 3c), revealing change in the opposite direction of that observed in Experiment 1. A significant main effect of trial set was revealed in a 2 Trial Set (PET 3, OET 4) × 3 Order × 2 Experience Type ANOVA, $F(1, 114) = 40.5$, $p < .001$, partial-$\eta^2 = 0.26$. The only other significant effect was a trial set × experience type interaction, $F(1, 114) = 7.1$, $p = .009$, partial-$\eta^2 = 0.06$. Despite identical perceptual experience trial procedures, the practice condition had larger PET errors ($M = 1.1$ cm) compared to the verbal feedback condition ($M = -0.6$ cm), and this pattern reversed in OET with larger errors for verbal feedback ($M = 3.4$ cm) compared with practice ($M = 2.7$ cm). Pairwise comparisons, however, did not reveal any significant difference between the conditions for PET ($p = .086$) or OET ($p = .173$).

Change within perceptual experience trials

Unexpectedly, constant errors increased during PET (Fig. 3c). A significant linear contrast was confirmed in a three PET Trial Set × 3 Order × 2 Experience Type ANOVA, $F(1, 114) = 40.65$, $p < .001$, partial-$\eta^2 = 0.26$. There were no significant main effects of order and experience. Moreover, neither order nor experience type interacted with the linear effect of trial set, which was expected given that the perceptual experience trial procedures were identical across all groups.

Change within outcome experience trials

Changes in constant errors during the OET depended on the specific combination of trial order and experience type. A 4 Trial Set × 3 Order × 2 Experience Type ANOVA on constant error confirmed a significant 3-way interaction, $F(6, 342) = 2.79$, $p = .012$, partial-$\eta^2 = 0.05$, and a significant Trial Set × Order interaction, $F(6, 342) = 16.8$, $p < .001$, partial-$\eta^2 = 0.28$. In addition, there were significant main effects of Trial Set, $F(3, 342) = 28.29$, $p < .001$, partial-$\eta^2 = 0.20$, and Order, $F(2, 114) = 11.5$, $p < .001$, partial-$\eta^2 = 0.17$.

To follow-up on the 3-way interaction, data were split by trial order and then analyzed in three separate 4 Trial Set × 2 Experience Type ANOVAs. For each trial order, linear and quadratic trend contrasts determined the shape of change, and Sidak-corrected pairwise comparisons between successive trial sets determined which changes were significant.

For failure first participants, a significant main effect of trial set confirmed that errors changed over trials, $F(3, 114) = 8.28$, $p < .001$, partial-$\eta^2 = 0.18$. Regardless of experience type, errors were positive, with errors initially increasing before decreasing on later trials. This was confirmed by a significant quadratic trend contrast, $F(1, 38) = 37.85$, $p < .001$, partial-$\eta^2 = 0.50$. However, despite the overall similarity in the shape of change for practice and verbal feedback, the timing of change differed by experience type, significant trial set × experience type interaction $F(3, 114) = 3.89$, $p = .011$, partial-$\eta^2 = 0.09$. Practice participants significantly increased error from OET 1 ($M = 3.63$ cm) to OET 2 ($M = 5.48$ cm), $p = .035$, then significantly decreased error from OET 2 to OET 3 ($M = 3.87$ cm), $p = .009$. Error marginally decreased from OET 3 to OET 4 ($M = 2.99$ cm), $p = .053$. The timing of change differed for verbal feedback participants: Errors significantly increased from OET 1 ($M = 1.68$ cm) to OET 2 ($M = 3.58$ cm), $p = .029$, but did not significantly change from OET 2 to OET 3 ($M = 4.49$ cm), $p = .31$. Errors did significantly decrease from OET 3 to 4 ($M = 2.75$ cm), $p < .001$.

In contrast, errors for the interleaved order increased linearly and experience type did not matter. But, like the failure first condition, mean errors were positively signed throughout the OET phase. The only significant effects were a main effect of trial set, $F(3, 114) = 12.12$, $p < .001$, partial-$\eta^2 = 0.24$, and a significant linear contrast, $F(1, 38) = 22.68$, $p < .001$, partial-$\eta^2 = 0.37$. However, pairwise comparisons between successive trial sets (OET sets 1–2, 2–3, 3–4) were non-significant (ps > 0.15), suggesting that the changes over time were not robust.

Participants in the success first condition followed yet another pattern. Errors, which were initially negative, remained similar over the first two trial sets before increasing sharply over the final two trial sets to become positive. Experience type had no effect. The ANOVA revealed a main effect of trial set, $F(3, 114) = 32.91$, $p < .001$, partial-$\eta^2 = 0.24$, a significant linear contrast, $F(1, 38) = 40.68$, $p < .001$, partial-$\eta^2 = 0.52$, and a significant quadratic contrast, $F(1, 38) = 16.53$, $p < .001$, partial-$\eta^2 = 0.30$. Across experience types, errors in the first two trial sets were negative (OET 1, $M = -1.09$ cm; OET 2, $M = -1.03$ cm) and did not significantly differ ($p = .99$). However, errors became positive in the second two trial sets (OET 3, $M = 1.38$ cm; OET 4, $M = 3.33$ cm) through significant increases from OETs 2–3 and 3–4 (ps < 0.001).

Discussion

Like Experiment 1, absolute error decreased from perceptual experience trials to outcome experience trials. By the final OET, when all participants received both successful and failed outcome feedback, participants were better calibrated regardless of order or experience type.

Consistent with direct learning theory (Jacobs and Michaels 2007), full recalibration depended on receiving feedback to specify how perceptual calibration needed to change. For this task, that meant receiving both success and failure outcome feedback. Interleaved participants recalibrated similarly to those in Experiment 1 who received a mix of successful and failed experiences from the staircase procedure—errors improved at the beginning of the OET phase and changed little after subsequent feedback. In contrast, failure first and success first participants followed different patterns. Before receiving success feedback, failure first participants responded by increasing their judgments and erred by reporting that they needed much larger doorways than they truly did. After receiving success feedback, judgments decreased and became more calibrated. The opposite was true of success-first participants—their estimates were too small until they experienced failure.

Experience type moderated change in calibration depending on trial order, revealing that perceptual–motor feedback influenced judgments when available. Failure first participants had access to different information depending on whether they practiced or received verbal feedback. Practice generated haptic information in addition to outcome feedback on failure trials when trying to squeeze through, whereas verbal feedback on failure trials only provided outcome feedback. Calibration changed differently in the practice condition compared to the verbal feedback condition, indicating that perceptual–motor feedback did affect recalibration. Experience type did not have an effect in the interleaved or success first orders. For the interleaved order, haptic information likely did not have an effect because success and failure feedback were sufficient to calibrate judgments in the first few trials. For the success first order, practice and verbal feedback did not differ, likely because fitting through a doorway much larger than the body provides little, if any, haptic information.

PET errors in Experiment 2 differed from those in Experiment 1 in two unanticipated ways: PET errors were negatively rather than positively biased, and judgments changed by becoming larger, and thus better calibrated. Because the PET phases in Experiments 1 and 2 used nearly identical procedures, the affordance boundary procedure that preceded the PET phase in Experiment 2 likely accounts for these differences. Before PET judgments, participants in Experiment 2 squeezed through the doorway with eyes closed so that affordances could be measured. Although this manipulation did not affect absolute error in past work (Franchak 2017), it led to more participants becoming negatively biased (reporting that they could fit through impossibly small doorways). One possible explanation is that affordance boundary trials changed participants' bias from positive (as in Experiment 1) to negative before the PET phase. The increase in judgments over the course of the PET phase might reflect a return to the expected positive bias, suggesting that the improvement in calibration was incidental.

General discussion

Two experiments indicate that the squeezing task involves a different recalibration process compared with other fitting tasks. Experiment 1 showed that calibration did not change in the absence of outcome feedback in the context of this particular squeezing task. Calibration improved rapidly rather than gradually following practice, suggesting that outcome feedback, not perceptual–motor feedback, was responsible for recalibration while practicing. Experiment 2 showed that recalibration depended on the experience of both success and failure feedback, supporting the claim that outcome feedback is responsible for recalibration. Finally, the difference between practice and verbal feedback conditions in Experiment 2 shows that although outcome feedback is primarily responsible for recalibration in the squeezing task, perceptual–motor feedback can influence judgments when available.

These results indicate that task variations—walking through versus squeezing through—mean that different aspects of the body-environment relation determine affordances, which in turn creates differences in the informational requirements for affordance perception. For the walking and barrier tasks, relations between static body properties (shoulder width, height) and environmental properties (doorway width, barrier height) mean that eye-height scaled information is sufficient (Stoffregen et al. 2009; Warren and Whang 1987; Yu et al. 2011). In contrast, torso rigidity helps determine affordances for squeezing, meaning that affordances for squeezing are not scaled to static body dimensions. The current study provides the strongest evidence yet (rapid recalibration that depends on success and failure information) that outcome feedback rather than perceptual–motor feedback is required for recalibration in the squeezing task.

What general principle, if any, determines when outcome feedback is required? The dissociation between the squeezing, walking, and barrier tasks shows that functional similarity of actions has no bearing on what information is required. At first glance, it appears that affordances depending on static properties (shoulder width, height, leg length) do not require outcome feedback (Mark et al. 1990; Stoffregen et al. 2009; Warren and Whang 1987) whereas affordances that depend on dynamic properties (torso rigidity) do. However, a study of maximum leaping height, a dynamic property, found that affordance judgments improved without explicit feedback (Ramenzoni et al. 2010). Other investigations of leaping found benefits of practice but did not rule out whether outcome feedback rather than perceptual motor feedback from practice was responsible (Cole et al. 2013; Day et al. 2015). Complicating matters further, neither perceptual–motor feedback nor outcome feedback from 8 days of practice is sufficient for calibrating perception for rolling through horizontal barriers in a wheelchair (Higuchi et al. 2004), despite the apparent similarity to the walking and barrier tasks. Possibly, attempts to discern a simple answer to the question of when outcome feedback is necessary is misguided if affordance perception operates on multidimensional and multisensory informational variables detected in the global array (Stoffregen and Bardy 2001).

Indeed, the current results suggest that participants take advantage of multiple sources of information when available. Recalibration followed a different time course for participants who practiced compared to those who received verbal feedback, despite both groups receiving the same outcome feedback. Specifically, participants who received failure experience when practicing felt the squeeze of the doorway on each failed attempt but participants who made verbal judgments only received outcome feedback. Accordingly, participants who practiced, and in doing so received haptic information, changed their judgments more rapidly than those who did not practice. Such flexibility in detecting different types of available information within the confines of a single task is consistent with the idea of soft-assembly of perceptual systems (Wagman and Hajnal 2014, 2016). If smart perceptual mechanisms are spontaneously assembled to fit the constraints of a particular task, observers may readily detect other relevant types of information that are available in the global array.

The results of the current study with respect to past literature support the ecological approach claim that perception depends on action-referential information about body-environment relations. It is difficult to explain from a computational perspective why perception of affordances for walking versus squeezing through doorway would differ if observers perceived action-neutral properties such as metric doorway size. However, regardless of theoretical perspective, the results of the current study present a challenging problem. If functionally-similar affordances, such as walking versus squeezing, depend on different recalibration processes, what are the consequences for real-world perception and action? Because information is actively generated through observers' exploratory behaviors—postural sway while standing generates eye height information and practicing squeezing through doorways generates outcome feedback—it is crucial that observers know how to explore in the appropriate way for the task at hand. If some task variations result in different informational variables required for perceiving affordances (e.g., walking without turning versus squeezing sideways through a doorway), but others do not (e.g., walking without turning versus walking without ducking), the challenge for observers is immense. Future work on the soft-assembly of perceptual systems, particularly on spontaneous exploratory behavior, will help reveal how observers meet this challenge.

Compliance with ethical standards

Ethical approval All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

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