



Registered Report

A registered re-examination of the effects of leftward prism adaptation on landmark judgements in healthy people



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ABSTRACT

It has long been known that active adaptation to a shift of the visual field, caused by laterally-displacing prisms, induces short-term sensorimotor aftereffects. More recent evidence suggests that prism adaptation may also stimulate higher-level changes in spatial cognition, which can modify the spatial biases of healthy people. The first reported, and most replicated, higher-level aftereffect is a rightward shift in the point of subjective equality (PSE) for a perceptual bisection task (the landmark task), following adaptation to leftward prisms. A recent meta-analysis suggests that this visuospatial aftereffect should be robustly induced by an extended period of adaptation to strong leftward prisms (15°, ~26.8 prism dioptres). However, we have been unable to replicate this effect, suggesting that the effect size estimated from prior literature might be over-optimistic. This Registered Report compared visuospatial aftereffects on the landmark task for a 15° leftward prism adaptation group ($n = 102$) against a sham-adaptation control group ($n = 102$). The effect size for the comparison was Cohen's $d = .27$, 95% CI [-0.01, .55], which did not pass the criterion set for significance. A Bayesian analysis indicated that the data were more than 4.1 times as likely under the null than under an informed experimental hypothesis. Exploratory analyses showed no evidence for a rightward shift of landmark judgements in the prism group considered alone, and no relationship between sensorimotor and visuospatial aftereffects. We further found no support for previous suggestions that visuospatial aftereffects are modulated by a person's baseline bias (leftward or rightward) for the landmark task. Null findings are also presented for a preliminary group of 62 participants adapted to 15° leftward prisms, and an additional group of 29 participants adapted to 10° leftward prisms. We do not rule out the possibility that leftward prisms might induce

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higher-level visuospatial aftereffects in healthy people, but we should be more sceptical about this claim.

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1. Introduction

If a person looks through laterally-displacing prisms, the alignment between the body and vision is altered, and initial attempts to act on visual targets will err in the direction of the prismatic displacement. By receiving visual feedback on these errors, the person can adapt to the altered alignment, and restore accurate performance. If the prisms are then removed, and the person reaches again for a visual target, they will now err in the *opposite* direction. This is the *sensorimotor aftereffect*, which reflects the compensatory adaptation for the prior prismatic displacement. Provided that normal visual feedback is available, this aftereffect will in turn be extinguished rapidly, returning the person to an un-adapted state. This classical cycle of prism adaptation is a compelling demonstration of the short-term plasticity of the human sensorimotor system.

In the past 20 years, the classical story has been extended through a new wave of research, sparked by the discovery that adaptation to a rightward optical shift of 10° (~17.5 prism dioptres) could temporarily reduce the symptoms of left neglect (Rossetti et al., 1998). Neglect is a pathological imbalance of spatial attention, cognition and behaviour, associated with damage to the right hemisphere, often following stroke. The amelioration of these symptoms far outlasted the expected sensorimotor aftereffects, persisting for hours and even days after prism exposure (McIntosh, Rossetti, & Milner, 2002; Rossetti et al., 1998). Moreover, the benefits were found to generalise to a range of visuospatial tasks (for a review, see Pisella, Rode, Farnè, Tiliakete, & Rossetti, 2006), and to non-visual tasks based on haptic exploration (McIntosh et al., 2002) and mental representation (Rode, Rossetti, & Boisson, 2001; Rossetti et al., 2004). These findings suggest that, in addition to the sensorimotor aftereffects, prism adaptation may have previously unsuspected, longer-lasting aftereffects on spatial cognition. Prism adaptation may thus be a powerful tool for understanding the relation between low-level sensorimotor mappings and higher-level spatial cognition, and could potentially offer a simple, non-invasive therapy for neglect. However, although initial clinical trials did suggest some benefits over standard care (for summaries, see Fasotti & van Kessel, 2013; Kerkhoff & Schenk, 2012), larger-scale trials have yet to confirm reliable functional benefits of prism therapy for neglect (for an overview, see Ten Brink et al., 2017; for a recent meta-analysis, see Longley et al., 2021).

Nonetheless, the existence of the proposed *cognitive* aftereffects is supported by a parallel wave of research in healthy people, showing similar, albeit subtler, changes in spatial cognition following prism adaptation. Contrary to patients with left neglect, who are biased strongly to the right side of space, healthy people typically have a slight bias to the left, known by analogy as ‘pseudoneglect’ (after Bowers & Heilman, 1980; for a review and meta-analysis, see Jewell &

McCourt, 2000). Where adaptation to rightward prisms had been found to reduce the rightward bias of neglect, adaptation to leftward prisms was found to reduce (or reverse) the leftward bias of pseudoneglect. This change in visuospatial bias was observed initially on a horizontal length estimation task, known as the landmark task (Colent, Pisella, Bernieri, Rode, & Rossetti, 2000). In the landmark task, a pre-transected line is presented, and the participant must judge which side is longer (or shorter). A gross measure of visuospatial bias is sometimes taken from the proportion of trials in which an accurately bisected line is judged to be shorter on the left (or right) (Harvey, Milner, & Roberts, 1995), but a more precise metric can be obtained by fitting a psychophysical function to responses across multiple transection positions, and extracting the point of subjective equality (PSE). Colent et al. (2000) reported that the PSE was shifted rightward after adaptation to leftward prisms; a similar result was then shown for a manual bisection task, in which participants actively transect horizontal lines (Colent et al., 2000; Michel, Rossetti, et al., 2003).

This rightward shift in visuospatial perception after leftward prism adaptation has been interpreted as an experimental model for neglect (Michel, 2006; Michel, Pisella, et al., 2003). Subsequent studies reported similar shifts on other visual and non-visual spatial tasks that are commonly affected by neglect (e.g., Girardi et al., 2004; Loftus, Nicholls, Mattingley, & Bradshaw, 2008; Loftus et al., 2009). Leftward prism adaptation may even induce other changes reminiscent of right hemisphere dysfunction, such as an increased tendency to focus on local details at the expense of global forms (Bultitude & Woods, 2010), and an impaired ability to remap spatial representations across saccadic eye-movements (Bultitude, Van der Stigchel, & Nijboer, 2013). However, the empirical evidence is not wholly consistent, and null findings have been reported for other measures and tasks that are often disturbed in neglect, such as temporal order judgements, space- and object-based attention, saccadic latencies and visual search (for a review, see Michel, 2006). Amongst the cognitive aftereffects that have now been investigated in healthy people, we will focus on the first, and most-often replicated finding of a rightward shift in the perceived midpoint of horizontal lines. To distinguish this from other cognitive changes, and from low-level sensorimotor aftereffects, we will refer to this shift as a *visuospatial aftereffect* of prism adaptation.

1.1. Meta-analysis of visuospatial aftereffects of prism adaptation in healthy people

We have been unable to replicate the visuospatial aftereffects of prism adaptation on landmark judgements (or line bisection responses) in healthy people, despite several (unpublished) attempts. We subsequently completed a meta-analysis of these aftereffects, motivated in part by the desire to understand our own null results (McIntosh, Brown, & Young,

2019). Two previously unpublished null results were included, along with 16 published experiments for the landmark task, and 11 for the line bisection task, from a total of 12 sources (see Table 1, McIntosh et al., 2019). One salient conclusion was that the quality of evidence was higher for the landmark task than for line bisection. We further suggested that manual line bisection is a poor task choice for the study of visuospatial aftereffects, because the manual response to mark the perceived midpoint may be influenced by the low-level sensorimotor aftereffect. For these reasons, our focus in the present paper will be exclusively on visuospatial aftereffects in the landmark task.

Fig. 1a shows a funnel plot of the standardized effect size (Cohen's d) of the shift in bias on the landmark task induced by a period of adaptation to leftward prisms. The y-axis represents the standard error of the estimate, which is a function of sample size [$1/\sqrt{n}$], so that higher points on the plot are from experiments with larger sample sizes (n ranges from 7 to 40, median 12). The largest effect sizes, on the right of the plot, tended to be from studies with smaller sample sizes ($n = 7–15$), but were also associated with long exposures (≥ 10 min) to strong prisms (15°). Assuming that these differences in adaptation parameters, rather than sample size, might be responsible for effect size variations, we included duration of exposure (short, long) and prism strength as moderators in a random-effects meta-analysis. Fig. 1b shows the moderated funnel plot of residual effect sizes; that is, the portion of the observed effects that could not be accounted for by these methodological factors. Moderate heterogeneity remained between studies, which might relate to other variations in the implementation of the landmark task (e.g., number of trials and transection positions) and/or chance factors; but the model was overall highly significant.

Fig. 1c illustrates this model, showing the relation between the predicted effect size and prism strength, assuming a long period of prism exposure (≥ 10 min). There appears to be a dose–response relationship, with stronger prisms inducing larger visuospatial aftereffects. On this basis, we concluded that “the visuospatial aftereffects of leftward prism adaptation are real and robust” (p. 271, McIntosh et al., 2019), and we attributed previous failures to replicate these effects to the use of insufficiently powerful prisms (e.g., 10° rather than $\geq 15^\circ$), and/or brief adaptation protocols (< 10 min). We recommended that future studies should adapt participants to 15° (or higher) prisms for at least 10 min, with upward of 250 pointing movements, an amount of exposure that has been suggested to lead to consolidated, longer-lasting adaptation (Inoue et al., 2014). The predicted effect size for a long period of exposure to 15° prisms is very large ($d = .94$, 95% CI [.64, 1.24]) (Fig. 1c).

1.2. A further failure to replicate

Following the meta-analysis, we have conducted a further study, as part of an undergraduate dissertation project at the University of Edinburgh (see Acknowledgements). The original purpose was to examine whether the visuospatial aftereffects of prism adaptation are modulated by the participant's initial perceptual bias on the landmark task, as some authors

have suggested (e.g., Goedert, Leblanc, Tsai, & Barrett, 2010; Herlihey, Black, & Ferber, 2012; Schintu et al., 2017). To assess initial bias, we included a baseline block of the landmark task, followed by pre- and post-adaptation blocks separated by a period of prism adaptation. To ensure robust visuospatial aftereffects, we used the adaptation parameters recommended by our meta-analysis, exposing participants to 15° prisms, for a total of 350 pointing movements (~ 10 min). The methods were pre-registered at the open science framework, and the raw data are archived there (<https://osf.io/f8b72/>).

However, not only did we observe no modulation by initial perceptual bias, we were unable to confirm any effect of prism adaptation on landmark bias at all, even considering all 62 participants together. This total sample size is more than 50% higher than that of any prior published study, and it should have had near-perfect power (.9996 at $\alpha .05$, one-tailed) to detect the lower-bound effect-size predicted from the meta-analysis ($d = .64$). But the shift in the PSE was indistinguishable from zero (Fig. 2a). This unexpected result cannot be attributed to unsuitability of the landmark task, which was sufficiently sensitive to show clear pseudoneglect on average, and which had high test-retest reliability across the three blocks (Cronbach's alpha of .87). Nor can it be attributed to a failure to adapt participants sufficiently to the prismatic shift, because the sensorimotor aftereffect, measured by open-loop pointing, was very robust, with a mean shift of 9.63° (SD 1.96, $d = 4.9$), equivalent to 64% of the prism strength (Fig. 2b).

1.3. The need for unbiased evidence

The above null result is hard to ascribe to a lack of statistical or prismatic power, but an alternative explanation could be that the targeted effect size ($d = .64$), estimated from prior literature, was over-optimistic. A meta-analysis enables a weighted overview of the available evidence on a question, but if the evidence is biased, or highly heterogeneous, then the overview may be distorted. At the same time, meta-analytic methods such as funnel plot visualization, can aid in the identification of such problems. In an unbiased literature, larger samples, at the top of the funnel plot, should give convergent estimates of the true effect size, and the spread of estimates should increase symmetrically around this value for progressively smaller sample sizes, lower in the plot. If the studies are relatively homogeneous, then around 95% of data points should fall within the triangular region. In Fig. 1a, the heterogeneity between prism adaptation studies is high, but this could be related to differences in the adaptation procedure between studies. Once prism strength and exposure duration are included as moderators, the residual effect sizes show much less heterogeneity (Fig. 1b).

The funnel plot can also be useful for identifying potential publication bias, which would be indicated by a lateral asymmetry of the distribution of estimates with respect to the triangular region. The prototypical bias would be the non-publication of small sample studies that fail to show a significant (positive) effect, leading to a sparsity of data for the lower left portion of the triangle. This would encourage a negative relationship between sample size and effect size.

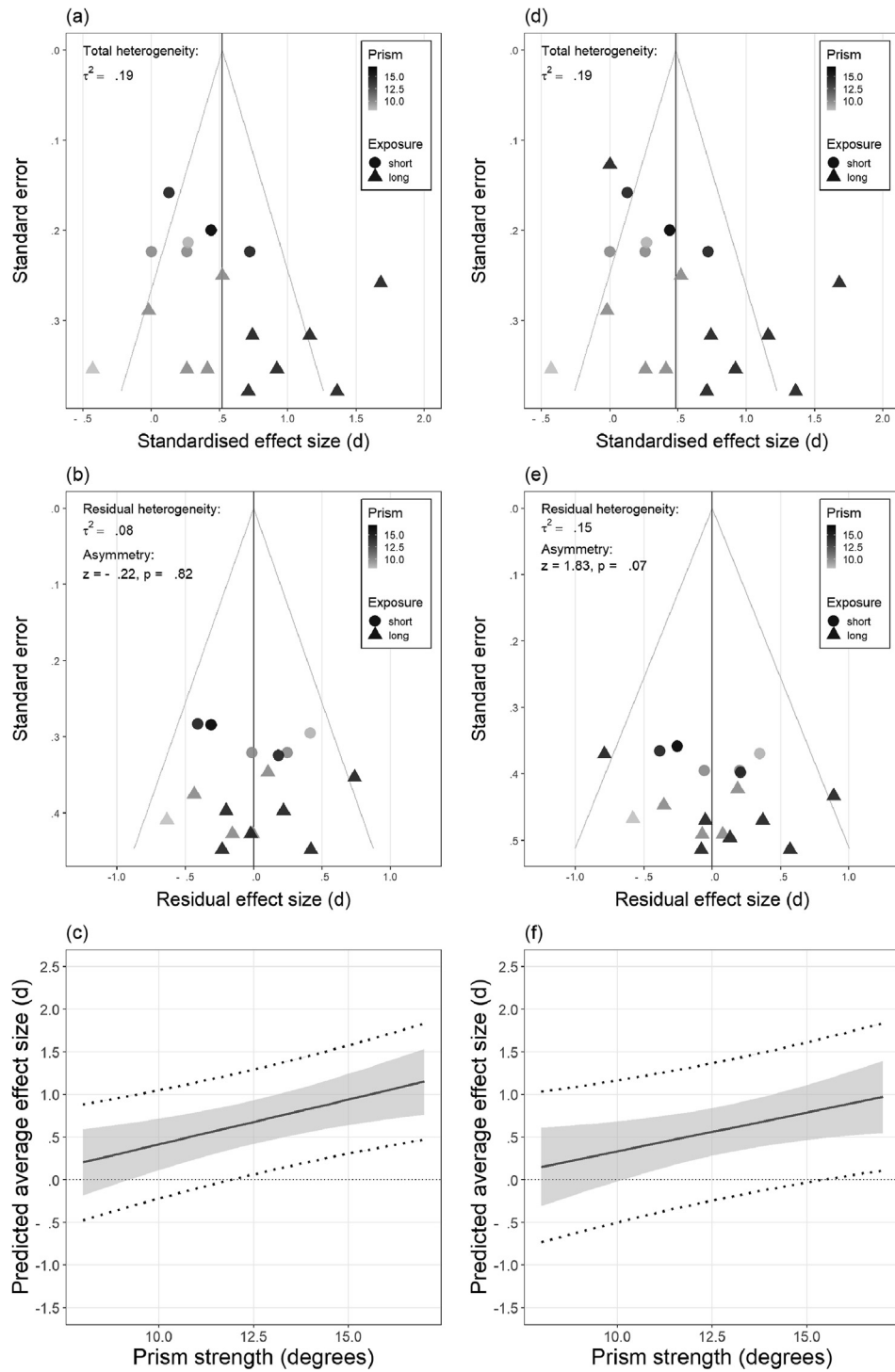


Fig. 1 – The panels in the left column (a–c) summarise the random effects meta-analysis of the visuospacial aftereffect of prism adaptation on landmark bias, and those in the right column (d–f) show the corresponding plots updated to include the data from the study reported in Section 1.2 (a, d) The unmoderated random-effects funnel plot of standardised effect size by standard error (larger studies are higher in plot). The triangular region follows the 95% confidence region at each level of standard error, and is centred on the meta-estimate of average effect size (b, e) The moderated funnel plot of residual effect size, after prism strength and exposure duration (short, long) have been accounted for, where a long period of prism exposure is defined as ≥ 10 min (c, f) Predicted average effect size by prism strength, assuming a long exposure. The grey shaded region shows the 95% confidence intervals, and the dotted lines show the 95% prediction intervals.

Common tests of publication bias, such as Eggers test of asymmetry, are based on the evaluation of (appropriate transformations of) this general relationship.¹ As Fig. 1b indicates, our random-effects meta-analysis of visuospatial aftereffects of prisms in the landmark task did not suggest any obvious publication bias.² Fig. 1d-f, on the right side of Fig. 1, show how the meta-analysis would be altered by updating it to include our study reported above (Section 1.2). Compared with Fig. 1b, Fig. 1e shows more residual heterogeneity and an increased degree of asymmetry, albeit not exceeding the threshold for significance.

Another possible form of publication bias can be evaluated by visualising the timeline of reported effect sizes by date of publication. A *decline effect* may sometimes be seen, if an effect enters the literature with inflated estimates of effect size, followed by later studies finding more modest effects (e.g., de Bruin & Della Sala, 2015). Fig. 3a shows that the visuospatial aftereffects of leftward prism adaptation on the landmark task are subject to an apparent decline effect, perhaps related also to a tendency for increasing sample sizes in later studies. Rather than basing our view of the visuospatial aftereffects of prism adaptation too firmly on a literature that may be biased, a productive way forward would be to use this literature to frame a novel attempt to obtain an unbiased estimate of the magnitude of these effects.

1.4. The present study

In this study, we aim to obtain an unbiased estimate of the effects of leftward prism adaptation on PSE in the landmark task. The methods are an elaborated version of those of our study reported above (Section 1.2), using strong wide-field wedge prisms (15°) and an extended adaptation procedure (350 movements). In addition to a leftward prism group, the study will include a control group exposed to a sham adaptation procedure, to control for possible non-prism-specific effects. For instance, the adaptation protocol involves repetitive movements of the right arm, but limb movements may differentially activate the contralateral hemisphere and thereby affect the lateral allocation of attention (Jewell & McCourt, 2000); unilateral limb activation has even been applied as a rehabilitation strategy in neglect (Robertson & Hawkins, 1999). Moreover, the evaluation of visuospatial aftereffects is based on a comparison of pre- and post-adaptation blocks of landmark trials, separated by a lengthy adaptation block, but there is evidence that landmark PSE can be shifted rightward by reductions in generalised arousal and alertness, due for instance to time on task, or tiredness (Benwell, Thut, Learmonth, & Harvey, 2013; Dufour, Touzalin, & Candas, 2007; Manly, Dobler, Dodds, & George, 2005). It

seems essential to control for any such generalised effects, in order to isolate the effect of prism adaptation itself.

Surprisingly, only one study of the visuospatial aftereffects of prism adaptation on landmark PSE has included a sham control group, and this study did not observe a PSE shift in either group (Experiment 1, McIntosh et al., 2019). Slightly more common has been the inclusion, in five studies, of a rightward prism adaptation comparison group (Berberovic & Mattingley, 2003; Colent et al., 2000; Schintu et al., 2014, 2017; Striemer, Russell, & Nath, 2016). It is sometimes claimed that such studies have shown that the visuospatial aftereffects of prism adaptation are specific to leftward prisms (see e.g., Michel, 2016); but this conclusion has only been inferred from significant effects of leftward prisms in the context of null effects of rightward prisms, and never from a direct statistical comparison between groups. Moreover, Berberovic and Mattingley (2003) unexpectedly found that, for one version of the landmark task (in extrapersonal space), rightward prism adaptation induced a significant shift in PSE in the same direction as that induced by leftward prisms, a result that defies easy interpretation. If the data are gathered from all of these studies, and plotted together, it is not at all clear that the visuospatial aftereffects of leftward prisms differ from those of rightward prisms (Fig. 3b). The specificity of visuospatial aftereffects to the leftward direction of prisms, would thus be interesting to test further. However, it is secondary to the more fundamental issue of whether the visuospatial aftereffects of leftward prisms are themselves robust. We prioritise the inclusion of a sham adaptation condition, over a rightward adaptation condition, in order to focus resources on the more fundamental question.

The present study is proposed as a Registered Report, which seems well-suited to furnish unbiased data on this question. There is sufficient prior literature to enable informed predictions about the expected effect size, yet sufficient doubt about the true effect size that the question is worth asking. The literature has a convergent set of methods, so it is relatively straightforward to specify an appropriate design. At the same time, the Registered Reports process, by putting peer review before data collection, maximises the chance that any undesirable idiosyncrasies of our design, which might reduce its ability to elicit the aftereffects of interest, can be identified and amended in advance. Finally, to reduce the possibility of inscrutable lab-specific effects, the study is a collaboration across two sites. Both teams have published positive findings of visuospatial aftereffects of prism adaptation, in healthy people (e.g., Bultitude, Van der Stigchel, & Nijboer, 2013; Bultitude & Woods, 2010; Girardi et al., 2004)³ and in patients with right hemisphere lesions (e.g., Bultitude, Rafal, & List, 2009; Nijboer, McIntosh, Nys, Dijkerman, & Milner, 2008; Schindler et al., 2009). However, we both also have null results in our respective file drawers. This alone gives us cause to believe that this literature is subject to at least some degree of publication bias, making a fully-preregistered investigation all the more relevant.

¹ Egger's test tests whether the y intercept departs from zero, for a linear regression of standardised effect size on precision (reciprocal of the standard error of the effect size estimate) (Egger, Davey Smith, Schneider, & Minder, 1997).

² This was not true for the meta-analysis of manual line bisection studies, where a significant asymmetry was found, even after moderation (see McIntosh et al., 2019). This is another reason for preferring to focus on the landmark task in the present study.

³ Though not only positive results (Bultitude, Downing, et al., 2013; Bultitude, List, et al., 2013; Dijkerman et al., 2003; Ten Brink et al., 2017).

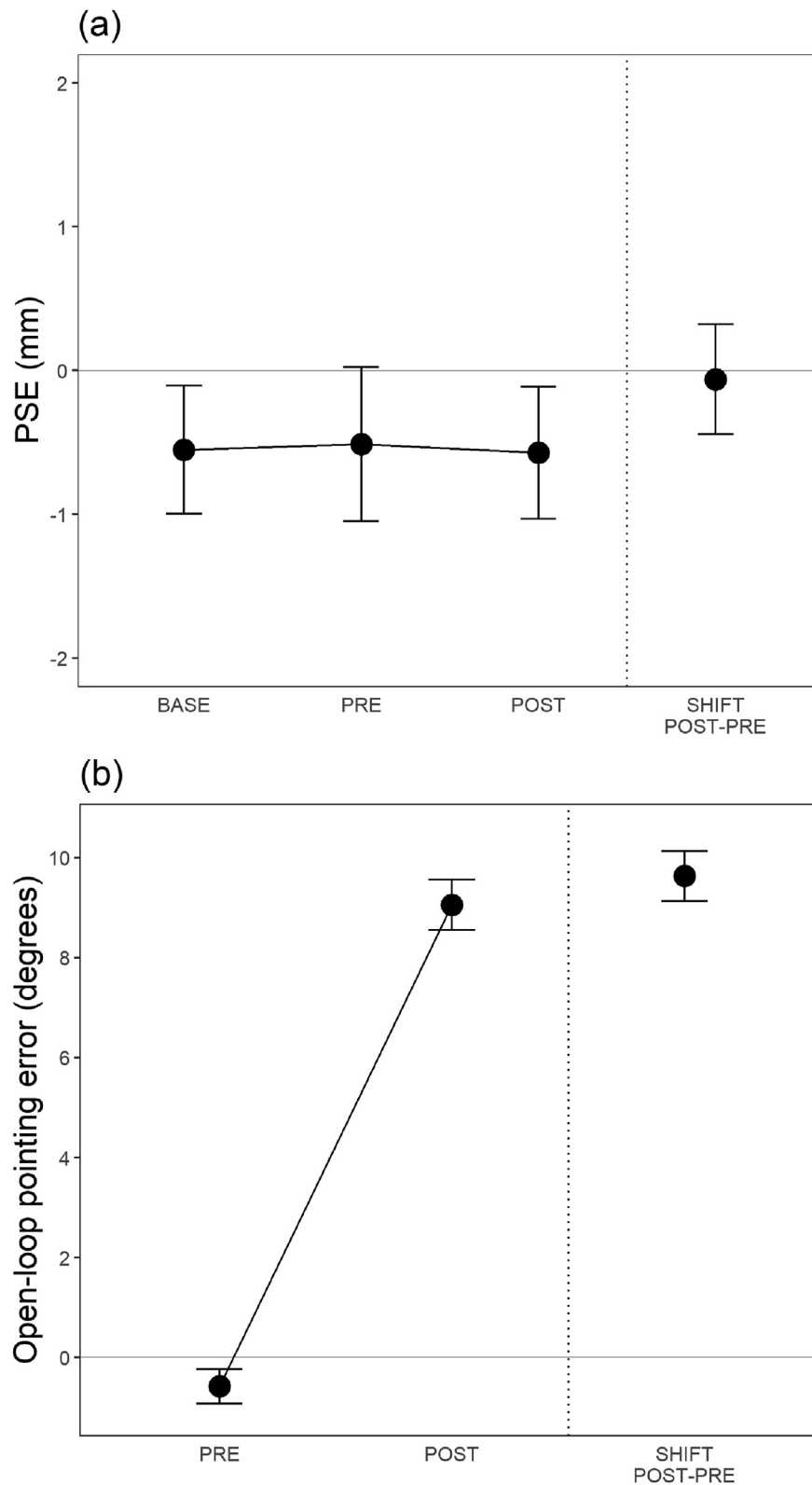


Fig. 2 – Summary of results for the study reported in Section 1.2. (a) Landmark PSE of 62 participants in the baseline, pre-adaptation and post-adaptation blocks. The shift is the subtraction of pre-from post-adaptation PSE, and represents the visuospatial aftereffect. (b) Open loop pointing error in the pre- and post-adaptation blocks. The shift is the subtraction of pre-from post-adaptation pointing error, and represents the sensorimotor aftereffect. Error bars represent 95% confidence intervals.

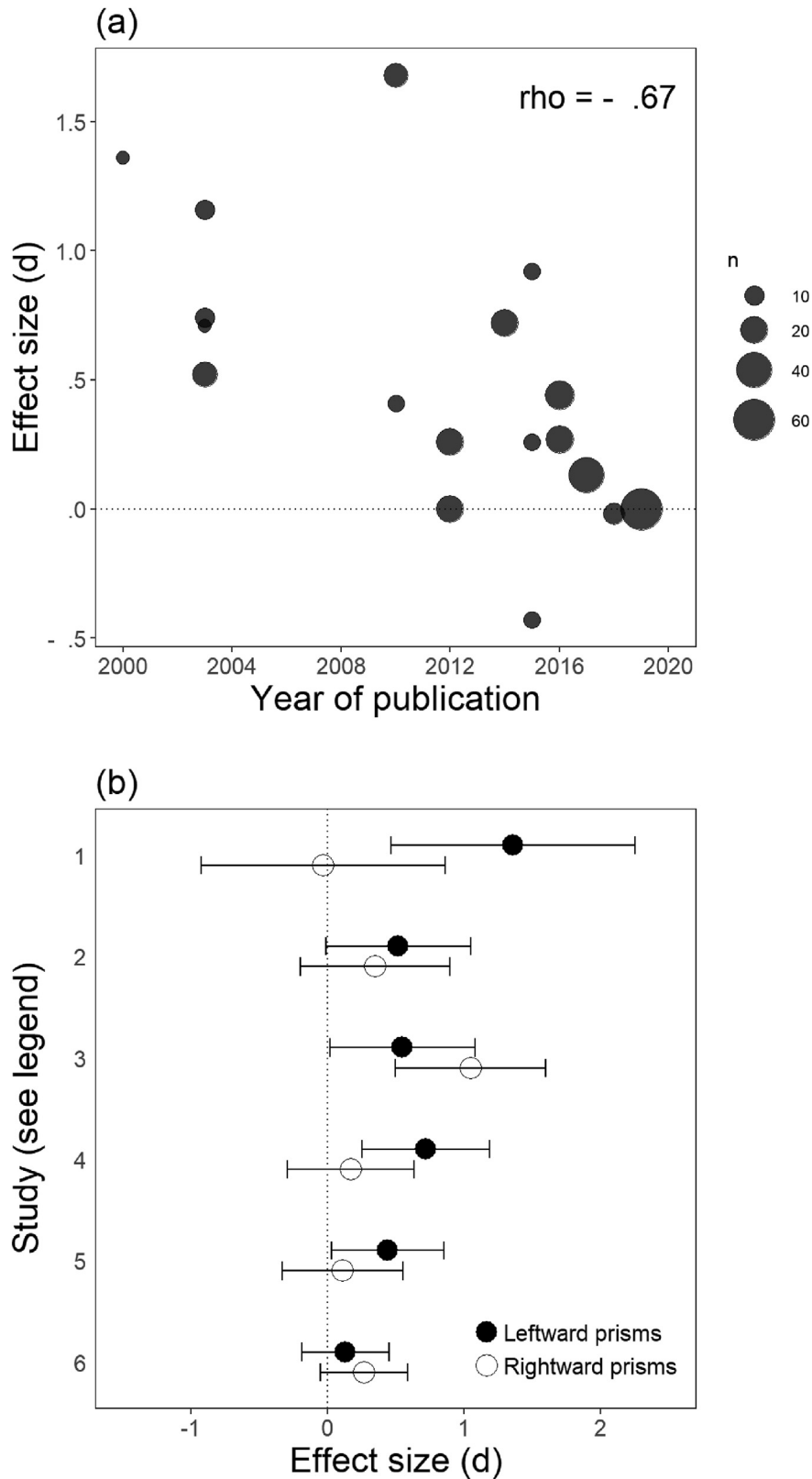


Fig. 3 – (a) Effect size by year of publication, showing a decline effect with time. The point size is scaled by sample size. The plot includes the study reported in Section 1.2 (rightmost point). (b) Effect size, for left and right prism adaptation groups, for five experiments that have included groups adapted to opposite directions of shift: (1) Colent et al. (2000, 15° prisms); (2) Berberovic and Mattingley (2003, peripersonal space, 10° prisms); (3) Berberovic and Mattingley (2003, extrapersonal space, 10° prisms); (4) Schintu et al. (2014, p. 15° prisms); (5) Strimer et al. (2016, p. 17° prisms); (6) Schintu et al. (2017, p. 17° prisms). Positive effect sizes represent a rightward shift in landmark bias. Error bars represent 95% confidence intervals.

2. Methods

2.1. Participants

Two hundred and four participants were planned to be included, assigned equally to two adaptation condition groups: prism adaptation and sham adaptation. Our initial plan was that 51 participants would be tested for each condition at each site, assigned sequentially to alternating groups, though the eventual numbers at each site would depend on recruitment and testing capacity.

In practice, this assignment plan encountered a problem, because the first 30 prism participants tested at the Bath site were mistakenly adapted to 10° leftward prisms, instead of 15° leftward prisms. These 10° prism participants are excluded from the registered experiment; an exploratory analysis of their data will be presented in Results. In order to avoid having to discard the 30 sham control participants tested up to that point, and to be able to continue collecting control data whilst we sourced a pair of 15° prisms, we sought a protocol amendment to allow for non-alternating allocation to groups.⁴ At the Bath site, then, 40 sham control participants were tested before the first 15° prism participant could be tested, and participants thereafter were allocated in a 5:1 ratio of prism to sham, up to a final total of 103 (30 × 10° prism, 28 × 15° prism, 45 × sham). At the Edinburgh site, participants were tested in an alternating sequence up to the originally-planned half-sample ($n = 102$), and thereafter in a counter-balanced schedule to make up for the shortfall in the Bath sample, and for participant exclusions. A final total of 140 participants were tested at the Edinburgh site (79 × 15° prism, 61 sham). The full testing order across sites is recorded in the open data.

Initial criteria for recruitment were: age between 18 and 40; self-reported right handedness with normal mobility in the right hand and arm; self-reported fluency in English, to ensure understanding of instructions; ability to read normal text at 50 cm viewing distance without glasses (contact lenses are allowed); and no reported history of neurological injury (e.g., stroke) or illness (e.g., multiple sclerosis). Recruited participants were excluded and replaced if the laterality quotient from the Edinburgh Handedness Inventory (EHI: Oldfield, 1971) was negative ($n = 1$), if they failed our near-vision screening test ($n = 0$) (see Section 2.2), if they did not complete the entire session ($n = 1$), or if a significant binomial logistic regression could not be fit to their responses in one or more blocks of the landmark task ($n = 7$).

Participants were rewarded by £7 payment ($n = 170$) or course credits ($n = 73$), and gave written informed consent before data collection began. The experiment was approved by the Psychology Ethics Committees of the Universities of Bath and Edinburgh. The onset of data collection was delayed from March 2020 to October 2021 by the COVID-19 pandemic. The original ethics approval was modified to specify new ventilation and hygiene measures, which included the requirement for the experimenter and the participant (unless

exempt) to wear face masks.⁵ This was made optional by a relaxation of restrictions (from 19 May 2022) for the last 41 participants tested at the Edinburgh site. Face-mask compliance is recorded along with the participant information in the open data.

2.2. Procedure

The participant first stood to face a far wall (>2 m distant) and, with both eyes open, held their index finger approximately 20 cm in front of their eyes so that they saw it aligned with a dark vertical stripe on the wall. They were then guided through the Porta test, closing first one eye and then the other to determine which eye's view was most similar to the binocular view. Based on the participant's reports, the experimenter recorded eye dominance as left, right or mixed. The participant then sat at a table, with their head in a chin-rest, centrally facing a touchscreen (Bath site, active display 442 × 248 mm, resolution 1600 × 900 pixels; Edinburgh site, active display 525 × 297 mm; resolution 1680 × 1050 pixels),⁶ tilted slightly back from vertical, with a viewing distance of 500 mm to the screen centre. The participant was then shown a white screen with five letters in black Sloan font, 2.3 mm high. Provided that the participant read all five letters correctly at the first attempt, they were allowed to progress to the main experiment.⁷ They then completed a computerised version of the EHI, to provide a measure of hand dominance.⁸

The room lighting was then dimmed to a low ambient level. On the table in front of the chin-rest, there was a start button for the right hand, with a direct reach path to the screen centre of 450 mm. A black shelf (160 mm deep), just below the chin-rest, blocked the direct view of the hand on the desk, and occluded the first half of the reach path to the screen. In the open-loop pointing and prism adaptation blocks, the participant used the right hand, keeping the left hand in their lap. In the landmark task, the participant responded using foot pedals, keeping both hands in their lap. The participant was unable to see either hand at any time, except during the prism adaptation procedure.

The testing session included pre- and post-adaptation blocks of landmark judgements, and open-loop pointing, in

⁵ The use of face-masks was not part of the original Stage 1 protocol, which received in principle acceptance prior to the COVID-19 pandemic. Protocol amendment approved 27 September 2021.

⁶ The touchscreen used at the Bath site has different dimensions and resolution to that used at Edinburgh, and specified in the original Stage 1 protocol. Protocol amendment approved 27 September 2021.

⁷ This is not a formal test of visual acuity, but a simple screening step to confirm that there is adequate acuity at 500 mm viewing to resolve the landmark stimuli clearly. The letters presented subtend 0.26°, and the gap size that must be resolved in order to identify the letters in Sloan font is 1/5th of this value (0.05°). This approximates a visual acuity of 0.32 (LogMAR 0.5), which is at the lower boundary of near-normal vision.

⁸ The order (Porta test, visual acuity check, EHI) differs from that stated in the original Stage 1 protocol (EHI, Porta test, visual acuity check), as the revised order was found during piloting to be more practical. Protocol amendment approved 27 September 2021.

⁴ Protocol amendment approved 6 February 2022.

order to measure visuospatial aftereffects and sensorimotor aftereffects respectively. The immediate pre- and post-tests, around the adaptation procedure, form the hypothesis-testing core of the experiment. A baseline block of landmark trials was also included at the beginning, to allow for exploratory investigations of the possible moderating influence of baseline perceptual bias (Goedert et al., 2010; Herlihey et al., 2012; Schintu et al., 2017). A late block of landmark trials was also performed after the core experiment, to allow for exploration of visuospatial aftereffects over a longer post-prism period (cf. Schintu et al., 2014). Finally, a late open-loop pointing block was added at the end to allow us to probe the state of adaptation at the end of the experiment. The session thus had 11 phases: baseline landmark task (~6 min); pre-prism landmark task (~6 min); pre-prism open-loop pointing (~1 min); prism adaptation (≥ 12 min); post-prism open loop pointing (~1 min); post-prism landmark task (~6 min); late landmark task (~6 min); late open-loop pointing (~1 min). The procedure for each task is described below.

Landmark Task: The participant sat with their hands in their lap, and their two feet resting on identical foot pedals, 300 mm either side of the midline. The participant was shown a series of horizontal lines, against a black background, in mid-level grey (colour code # 969696). Each line was 250 mm long and 1 mm thick, transected by a 15 mm vertical line, 1 mm thick. Participants were instructed to indicate which side of the line was longer (or shorter), by pressing the pedal under the left or right foot. At each testing site, the judgement required (longer/shorter) was counterbalanced within each adaptation group, in order to counterbalance the effects of any consistent response bias to favour the left or right pedal. The line remained on the screen until a response was made, upon which the screen was filled by a greyscale white noise pattern for 500 ms, to reduce retinal persistence from the previous trial, and then a black field for 500 ms, before the next trial. Any responses made in under 200 ms were ignored as anticipations, and the participant needed to make another pedal press to register the response.⁹ Lines were transected at .5, 1, 2, 4 or 8 mm to the left or right of the centre with 16 lines for each of these conditions. Four of these 16 lines were centred on the screen, and two each were shifted by 3, 6, and 9 mm to the left and to the right. The task comprised 160 lines in total, with the order of trials shuffled randomly.

Open-loop pointing: The participant made five pointing movements from the start button towards a (10 mm) grey dot at the centre of the screen. They were instructed to make smooth, fast movements, and to try to arrive at the screen in synchrony with an auditory tone (100 ms, 500 Hz), which onset 400 ms after button release. For this task, the participant wore LCD glasses and pressed the start button to clear the glasses and show each dot. The glasses became opaque on button release, occluding visual feedback from the entire movement. The next trial began when the button was pressed and at least

1600 ms had elapsed since the end of the previous trial. Before the first assessment, participants were given a short practice session, without LCD glasses, to familiarise with the procedure, and timing requirements.

Prism Adaptation (closed-loop pointing): The participants in the prism adaptation group wore goggles with 15° leftward, wide-field wedge prisms; those in the sham adaptation group wore glasses with plain lenses. The participant made pointing movements towards a (10 mm) grey dot, appearing at the vertical midline of the screen, and at a random horizontal coordinate within 100 mm ($\sim 11^\circ$) to either side of the horizontal midline. The hand was occluded by the shelf during the first half of the reach path, with visual feedback available for the second half (i.e., the standard ‘concurrent’ feedback arrangement used in almost all prior studies on this topic; see McIntosh et al., 2019). Participants were instructed to make smooth, fast movements, and to try to arrive at the screen in synchrony with an auditory tone (100 ms, 500 Hz), which onset 400 ms after button release. They were asked not to deliberately correct for any errors observed; this was to discourage conscious compensation for the prismatic shift, to encourage the prioritisation of pointing speed, and to allow terminal errors to occur that might drive sensorimotor adaptation. The dot disappeared once the screen was touched, and the next trial began when the button was pressed and at least 1600 ms had elapsed since the end of the previous trial. The maximum pointing rate was thus once every 2 s; and 350 pointing movements were made in total (minimum exposure duration ~ 12 min).

2.3. Dependent variables

Landmark task. For each block of the landmark task, a binomial logistic regression was fitted to model the probability of a left-is-shorter (\equiv right-is-longer) response according to the transection location. If the fit was significant (Wald test, $p < .05$) then the model was used to calculate the point of subjective equality (PSE; the transection point in mm at which the probability of a left-is-shorter response is .5) and the Just Noticeable Difference (JND; half of the transection distance in mm between .75 and .25 probability of a left-is-longer response). PSE and JND represent the bias and sensitivity of landmark judgements respectively. The critical dependent variable is the shift in PSE following prism adaptation, calculated as the PSE in the pre-adaptation block subtracted from that in the post-adaptation block. A negative value indicates a leftward shift in PSE, and a positive value a rightward shift.

Open-loop pointing. For each pointing trial, the horizontal displacement of the touch response from the target centre was calculated, with leftward error signed negative and rightward error positive. For each block separately, the mean error was calculated, and expressed in degrees of visual angle. The critical dependent variable was the immediate sensorimotor aftereffect, which is the shift in error following prism adaptation, calculated as the open-loop pointing error in the pre-adaptation block subtracted from that in the post-adaptation block. The late sensorimotor aftereffect was also calculated, as the error in the pre-prism block subtracted from that in the late block, in order to assess the state of adaptation at the end of the experiment.

⁹ The original Stage 1 plan was to recycle trials with anticipation responses (RT < 200 ms) to the end of the block, but it was found to be more practical simply to ignore anticipatory responses and require a subsequent response on the same trial. Protocol amendment approved 27 September 2021.

Prism Adaptation (closed-loop pointing): Pointing error (horizontal displacement from target centre) was also be recorded for closed-loop pointing, in order to track error reduction during the prism-adaptation procedure.

2.4. Statistical analysis

Main hypothesis. The critical inferential analysis relates exclusively to the immediate pre- and post-prism blocks of the landmark task, at the core of this experiment. We will compare the shift in PSE between groups (prism, sham), using an independent t-test, with a one-tailed alpha criterion of .02. This will test the hypothesis that the prism adaptation group show a significant rightward shift of landmark PSE, by comparison to the sham adaptation group.

This analysis will be supplemented by a Bayesian independent t-test, performed in JASP (JASP Team, 2019). The Bayes Factor will estimate the relative strength of evidence for the predicted rightward shift of PSE in the prism group, over the null hypothesis of no difference from the sham group. The shift hypothesis will be represented by an informed prior, based on the meta-analysis of McIntosh et al. (2019), updated to include our subsequent study (Section 1.2). For a prism strength of 15°, the updated meta-analysis predicts an average effect size of .79 (SE .17 CI .47 to 1.11) (Fig. 1f). The informed prior will be a normal distribution, centred on this predicted effect size (.79), with a standard deviation equal to the standard error of the meta-analytic estimate (.17).

Outcome-neutral criterion. We will require evidence that the prism adaptation group adapted sufficiently to the prisms. Facchin, Folegatti, Rossetti and Farnè (2019) have estimated the average sensorimotor aftereffect at around 38% of prism strength. Our subsequent study, using an adaptation protocol similar to that of the present experiment, found a sensorimotor aftereffect of 64% (Fig. 2b). Our operational criterion will be that an average differential immediate sensorimotor aftereffect of at least 5° (one-third of the prism strength) must be observed in the prism group, compared to the sham group, in order to allow for a meaningful interpretation of the main analysis.

Exploratory analyses: PSE in the baseline block of the landmark task will enter into an exploratory analysis of the possible modulatory influence of baseline bias on visuospatial aftereffects (e.g., Goedert et al., 2010; Herlihey et al., 2012; Schintu et al., 2017). PSE in the late block will be included in this analysis, to assess the possibility that visuospatial aftereffects vary with time post-adaptation (Schintu et al., 2014). JND will also be subjected to an exploratory analysis of prism aftereffects, and the late sensorimotor aftereffect will be used to assess the state of adaptation at the end of the experiment. We may also explore the inter-correlations between sensorimotor aftereffects and visuospatial aftereffects.

2.5. Power

The sample size was calculated to provide .9 power to detect the effect targeted by the main hypothesis, with an alpha criterion of .02 (one-tailed). The expected effect size was drawn from the meta-analysis of McIntosh et al. (2019),

updated to include our subsequent study (Section 1.2). We targeted the lower bound value of the expected effect size for a prism strength of 15° (.79, SE .17, CI .47 to 1.11). For this lower-bound effect size ($d = .47$), the required power is achieved at a group size of 102, with balanced allocation to the two groups. We thus included 204 participants in total, with 102 per group.

This sample size would, of course, be more than adequate for the outcome-neutral criterion of a 5° differential sensorimotor aftereffect, were this criterion to be put to a statistical test. In our recent study, using a similar adaptation procedure, the standard deviation of the sensorimotor aftereffect was 1.96°. Assuming a similar standard deviation, a 5° sensorimotor aftereffect would have a standardised effect size of around 2.55, so that 5 participants per group would achieve .90 power, with an alpha criterion of .02 (one-tailed).

3. Results

3.1. Demographic characteristics

The prism group had 102 full valid datasets: 77 female and 25 male participants, with a mean age of 22.23 years (SD 4.40), and a median EHI laterality quotient of 100 (range 22–100); 79 were right-eye dominant, 20 left-eye dominant, and 3 mixed. The sham group had had 102 full valid datasets: 82 female and 20 male participants, with a mean age of 21.39 years (SD 3.44), and a median EHI laterality quotient of 100 (range 25–100); 72 were right-eye dominant, 28 left-eye dominant, and 2 mixed.

3.2. Preregistered hypothesis test

The point of subjective equality (PSE) for each group for each block of the landmark task is depicted in the left panel of Fig. 4a. The mean shift in PSE in the post-prism and in the late block, relative to the pre-prism block, is shown in the right panel. The critical outcome is the shift in PSE in the post-prism block. This shift was close to zero in the prism group (mean = .04 mm, SD 1.39), and slightly negative in the sham group (mean = -.33 mm, SD 1.39). An independent t-test to judge whether the shift was more rightward in the prism group than in the sham group returned a p -value above our preregistered threshold of .02 ($t = 1.92$, $df = 202$, one tailed $p = .028$, Cohen's $d = .27$, 95% CI [-0.01, .55]). A Bayesian independent t-test, with an informed prior of .79 (SD .17) returned a Bayes factor (BF_{01}) of 4.11, indicating that the data were more than four times as likely under the null hypothesis. We do not reject the null hypothesis that leftward prism adaptation does not shift landmark judgements to the right in neurotypical participants.

3.3. Outcome-neutral criterion

The null visuospatial aftereffect of prism adaptation occurred in the context of a strong sensorimotor aftereffect of prism adaptation, shown in Fig. 4b. Open-loop pointing error shifted rightward in the prism group by an average of 9.63° (SD 1.69),

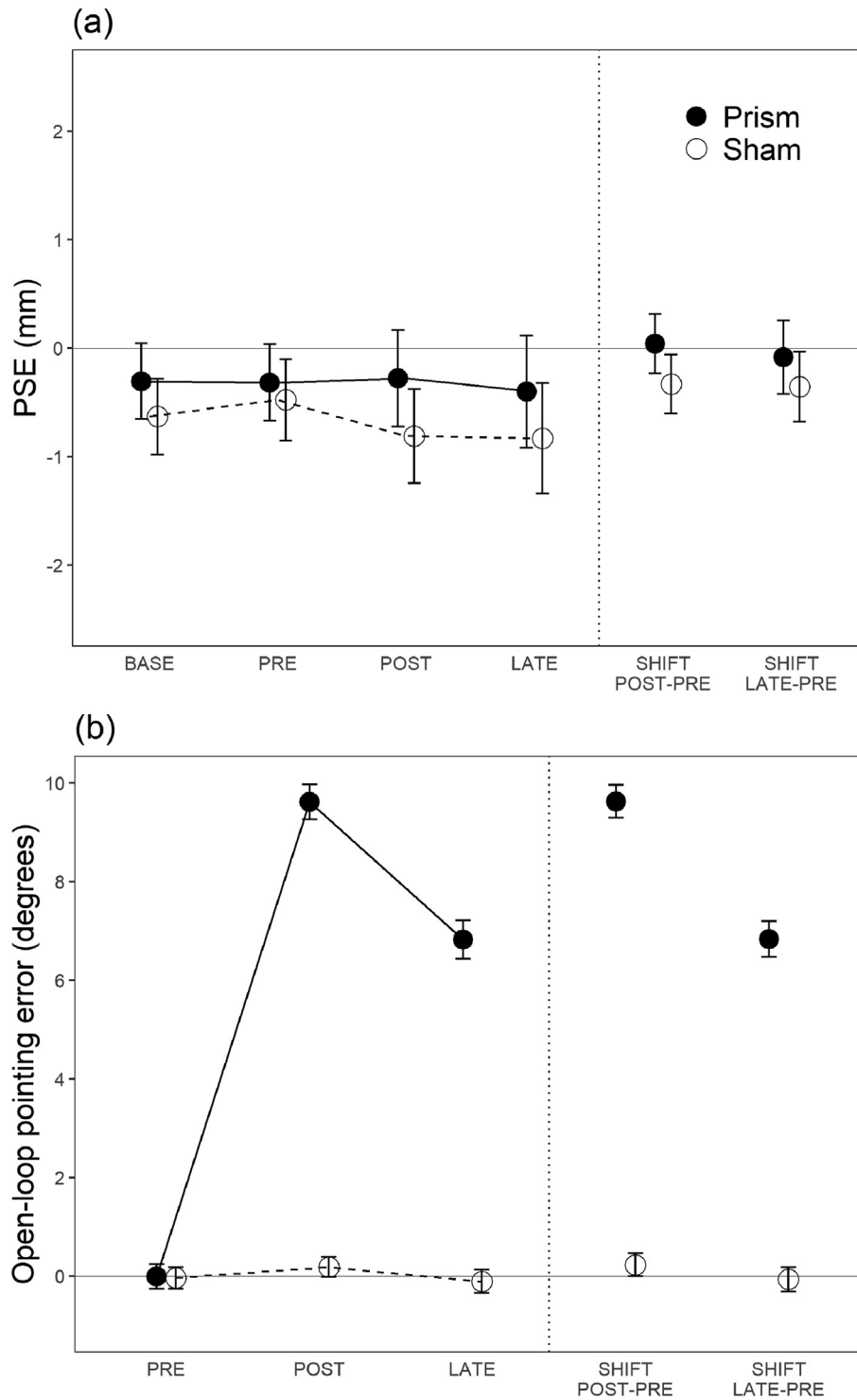


Fig. 4 – (a) The left side shows mean PSE for the 15° prism group ($n = 102$) and the sham group ($n = 102$), and the right side shows mean shifts in PSE from the pre-prism block to the post-prism and late-blocks. The preregistered hypothesis test is on the difference between groups in the post-prism shift. **(b)** The left side shows mean open-loop pointing error for each group, and the right side shows mean shifts in pointing error from the pre-prism block to the post-prism and late-blocks. Error-bars represent 95% confidence intervals.

compared with .23° (SD 1.19) in the sham group. The difference in immediate sensorimotor aftereffect between prism and sham groups was 9.40° (63% of the prism strength), far exceeding our preregistered outcome-neutral criterion of one

third of prism strength (5°). Even in the late open-loop block, at the end of the experiment, the same criterion was comfortably exceeded, with a differential sensorimotor aftereffect of 6.90°. The failure to observe a significant *visuospatial*

aftereffect is not due to a failure to adapt participants sufficiently to the prisms.

3.4. Further outcome-neutral quality checks

Given the null finding for the critical hypothesis test, it is worth reporting some further, non-preregistered quality checks, which provide reassurance that the landmark task was sensitive and reliable as measure of visuospatial bias. First, the left panel of Fig. 4a shows a general tendency toward a slight leftward PSE. The mean PSE calculated across groups for the baseline and pre-prism blocks was -0.43 mm (SD 1.68, 95% CI $[-0.67, -0.20]$). This reproduces the expected pattern of pseudoneglect (see Jewell & McCourt, 2000), indicating that the task was a valid assessment of visuospatial bias. Second, this measure of bias was highly reliable, with the correlation between PSE in the critical pre- and post-prism blocks for the prism and sham groups respectively at $r = .79$ (95% CI $[\.70, .85]$) and $.78$ (95% CI $[\.69, .85]$). Cronbach's alpha across all four blocks indicated excellent reliability for both the prism ($\alpha = .90$, 95% CI $[\.88, .93]$) and the sham group ($\alpha = .90$, 95% CI $[\.87, .93]$); the full inter-block correlation table is provided in Supplementary materials (Table S1). Third, the precision of landmark performance was generally good, with a grand mean just noticeable difference (JND) of 2.41 mm, SD .93. Descriptive analyses of JND, and representative psychophysical functions, are provided in Supplementary Materials. Overall, the landmark task was sensitive to the expected visuospatial bias, and highly reliable, so it should have been capable of detecting visuospatial aftereffects of prism adaptation if they were present.

3.5. Exploratory analyses

3.5.1. Evaluation of late visuospatial aftereffects

To explore the time-course of any visuospatial aftereffects, we included a late block of landmark judgements (Fig. 4a). The late shift was slightly negative in both prism and sham groups. Although the late shift was slightly more positive in the prism (mean -0.08 mm, SD 1.73) than in the sham group (mean -0.36 , SD 1.65), the difference between groups did not depart from zero (Cohen's $d = .16$, 95% CI $[-.12, .44]$). There is thus little evidence of a visuospatial aftereffect within the full time-course of the experiment.

3.5.2. Relationship between sensorimotor and visuospatial aftereffect

Because visuospatial aftereffects of prism adaptation in neurotypical individuals are a consequence of sensorimotor adaptation, we might expect a correlation between the magnitude of the sensorimotor aftereffects and visual aftereffects on spatial tasks. Several studies have tested for this expected relationship, but it has not yet been found (see Michel, 2016, for a review). Fig. 5 plots the relationship between the shift of open-loop pointing performance (sensorimotor aftereffect), and the shift of landmark PSE, at both the immediate post-prism and late blocks. There is more between-participant variability in the late test, but there is no suggestion of any relationship between sensorimotor and visuospatial aftereffects. For the prism group, the correlation was $r = -.13$, 95% CI $[-.32, .06]$ for the post-prism shift, and

$r = .00$, 95% CI $[-.19, .20]$ for the late shift. For comparison, the equivalent correlations for the sham group were $r = -.11$, 95% CI $[-.29, .09]$ and $r = -.12$, 95% CI $[-.30, .08]$.

3.5.3. Modulation of visuospatial aftereffects by baseline bias

It has been suggested that the visuospatial aftereffects of leftward prisms are modulated by baseline bias for the landmark task, such that only participants with an initial leftward bias will show a rightward shift of PSE following leftward prism adaptation (e.g., Goedert et al., 2010; Herlihey et al., 2012; Schintu et al., 2017). We included a baseline block to estimate each participant's initial bias. The correlation between PSE in the baseline block and the post-prism shift in the prism group was $r = .06$ (95% CI $[-.13, .26]$); for comparison, the correlation for the sham group was $r = .12$ (95% CI $[-.07, .31]$). This suggests no substantive dependency of the visuospatial aftereffects of prism adaptation on baseline bias. To explore this further, we followed the methodology of previous studies by sub-dividing participants by baseline bias, according to the sign of their PSE in the baseline block. Because of the preponderance of pseudoneglect, the left baseline bias subgroup was larger than the right baseline bias subgroup for both the prism group ($n = 58$ vs 44) and the sham group ($n = 67$ vs 35). The prism group, in the upper panel of Fig. 6a, showed no evident modulation of visuospatial aftereffects by baseline bias.

Unlike the above analysis, previous studies have not had a measure of baseline bias that is independent of the post-prism shift in PSE (Goedert et al., 2010; Herlihey et al., 2012; Schintu et al., 2017). Baseline bias has always been estimated from the pre-prism PSE, which is also subtracted from post-prism PSE to calculate the visuospatial shift. This renders the result prone to artefactual patterns due to regression to the mean (Campbell & Kenny, 1999): a group selected for larger leftward baseline errors is likely to show a more rightward shift as an artefact of this selection criterion. We can illustrate this artefact by calculating the shift between the baseline block and the pre-prism block. Pearson's correlations show a negative relationship between baseline bias and the pre minus baseline shift in both the prism ($r = -.33$, 95% CI $[-.50, -.15]$) and the sham group ($r = -.32$, 95% CI $[-.48, -.13]$). This negative relationship is not surprising; indeed it is almost guaranteed, because we have effectively correlated X with Y minus X . Fig. 6b visualises the data according to the subgroup split between rightward and leftward baseline bias. The leftward baseline bias group show a more rightward shift of PSE. This is consistent with a regression-to-the-mean artefact, induced by the methodological circularity that the same estimate of PSE has been used both to define baseline bias and to calculate the shift in PSE.

3.5.4. Additional sub-group analyses

There are two more specific sub-sets of the data worth exploring. First, as reported in Methods, an initial series of 30 prism participants at the Bath site were erroneously adapted to 10° prisms (instead of 15° prisms), in alternation with 30 sham controls. The data from these 60 participants constitute an unintended but well-counterbalanced experiment testing for visuospatial aftereffects of 10° leftward prisms. One 10° prism participant was excluded due to a poor psychophysical fit in the late landmark block, leaving 29 participants in the 10° prism group (21 female, 8 male, mean age 20.34 years, SD

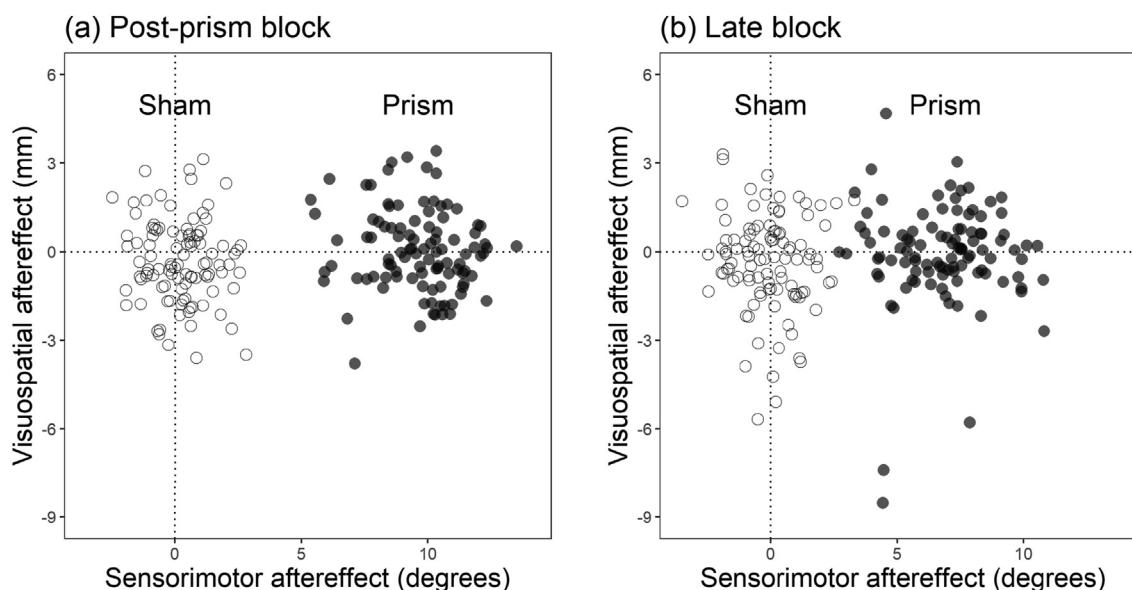


Fig. 5 – Exploratory scattergrams showing the (lack of) relationship between the sensorimotor and visuospatial aftereffects induced by prism adaptation. In both the post-prism block and the late block, the sensorimotor aftereffect is strong in the prism group, but there is no discernible visuospatial aftereffect on landmark judgements.

2.42), and 30 participants in the sham group (29 female, 1 male, mean age 20.10 years, SD 2.25). This is not a highly-powered experiment, particularly given the expectation of smaller visuospatial aftereffects for 10° prisms than for 15° prisms (see Fig. 1f). Nonetheless, the 10° prism group has more than double the median sample size of the preceding literature ($n = 12$; see McIntosh et al., 2019). Panels a and b of Fig. 7 show the results for the landmark task for this 10° prism experiment, suggesting negligible or non-existent visuospatial aftereffects, despite robust sensorimotor aftereffects. For visuospatial aftereffects, the difference between groups did not depart from zero for either the post-prism shift (Cohen's $d = .08$, 95% CI $[-.44, .60]$) or the late shift (Cohen's $d = .11$, 95% CI $[-.42, .63]$).

Finally, the delayed onset of testing with 15° prisms at Bath meant that the alternating sequence of allocation to groups was disrupted at this site. The Bath site also consequently tested more sham participants ($n = 42$) than prism participants ($n = 25$), so that the groups were not balanced against site for the experiment overall. There is no reason to think this would bias the data with respect to the main hypothesis, but it is nonetheless worth considering the data for the Edinburgh site separately, where these problems did not occur. More participants at Edinburgh were tested in the prism condition ($n = 74$) than in the sham condition ($n = 60$)—to compensate for the imbalance at Bath—but the allocation sequence was rotated appropriately at all stages. The Edinburgh dataset is therefore close to a counterbalanced implementation of the original protocol, albeit with lower statistical power (for these group sizes, power for the registered hypothesis test would be .74). Panels a and b of Fig. 7 show the results for the Edinburgh site alone. For visuospatial aftereffects, the difference between groups did not depart from zero for either the post-prism shift (Cohen's $d = .11$, 95% CI $[-.24, .45]$) or the late shift (Cohen's $d = -.08$, 95% CI $[-.42, .26]$).

4. Discussion

4.1. Major outcomes

This Registered Report was a stringent, high-powered test of the claimed effect of leftward prism adaptation on landmark task judgements in healthy adults. Based on an updated meta-analysis of prior literature (see Section 2.5), we targeted a minimum effect size of $d = .46$, with a significance criterion of .02 (in accordance with Cortex guidelines). This threshold was not met, so we do not reject the null hypothesis that leftward prism adaptation does not shift landmark judgements right. The fact that the observed p value was lower than the more conventional threshold of $p < .05$ is irrelevant to this decision, because this was not our criterion.¹⁰ However, there are other ways that we could weigh the evidence that might suggest more moderate conclusions. Our Bayesian t -test indicated that the data were 4.11 times more likely under the null than

¹⁰ Readers may wish to reach their own interpretation of the observed p value of .028. However, it should be emphasised that the experiment was highly powered (.90) to detect the smallest effect of interest with the significance criterion set to $p < .02$. A true effect would be expected to pass this criterion on 90% of occasions. If we instead adopted a more conventional significance criterion of $p < .05$, for the same sample size, power would be .955. This implies that a p -value above the set criterion but below the conventional criterion ($.02 > p < .05$) would be expected on 5.5% of occasions that the experimental hypothesis is true (95.5%–90%). By definition, 3% of p -values would be expected to fall within the same range if the null hypothesis is true. So, in the context of the present experiment, a p -value above the set criterion but below the conventional .05 level is only marginally more likely under the experimental hypothesis than under the null hypothesis, and we would not regard it as good evidence for the former.

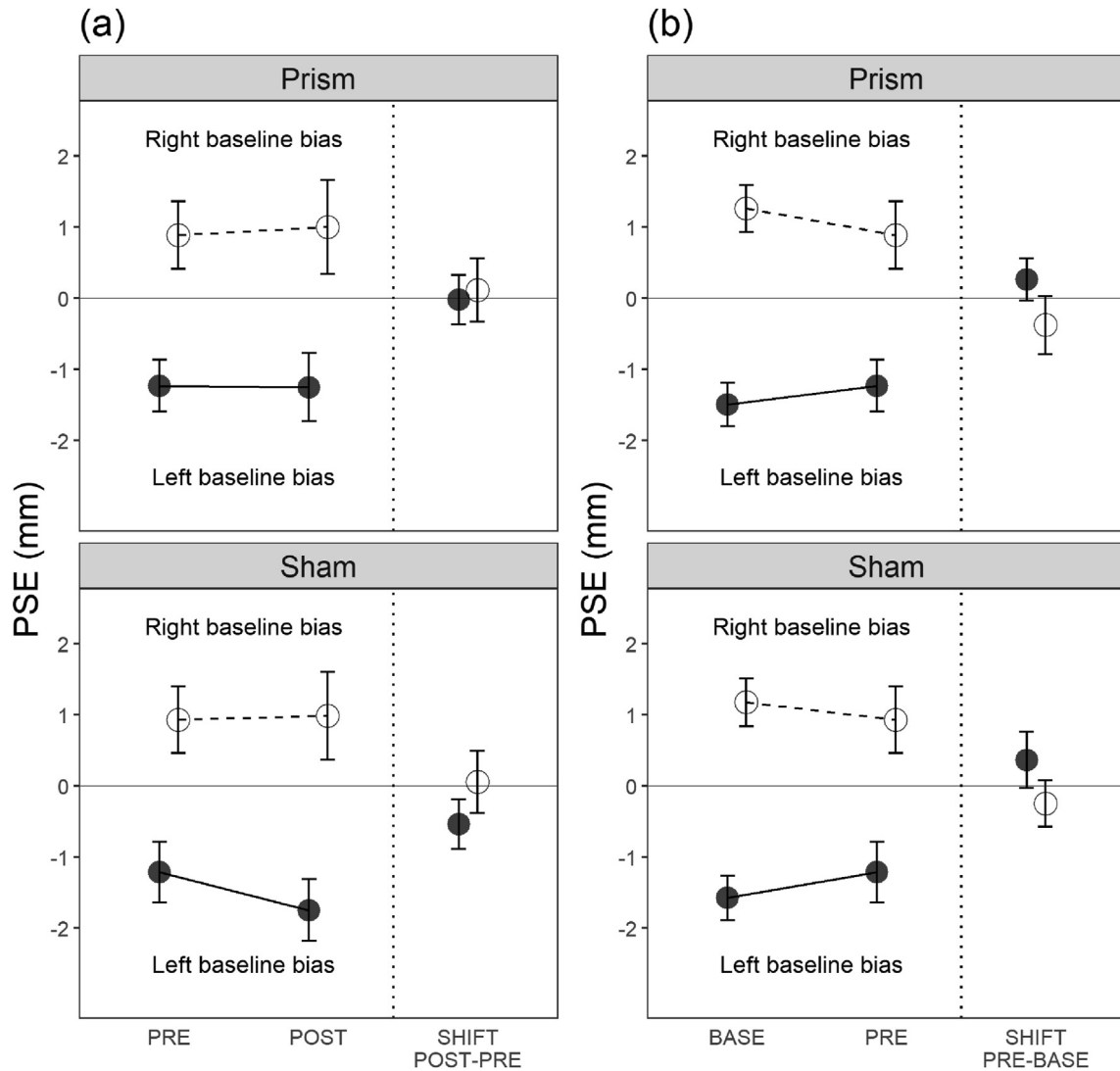


Fig. 6 – (a) Exploratory sub-group analyses with each group sub-divided into participants with a rightward PSE at baseline (prism $n = 44$; sham $n = 35$) and participants with a leftward PSE at baseline (prism $n = 58$; sham $n = 67$). The left side shows the mean PSE in the pre- and post-prism blocks, and the right side shows the mean shift in PSE between these blocks. (b) The same analysis is repeated but instead focusing on the shift in PSE between baseline and pre-prism blocks. This is methodologically circular because baseline bias is now being used to define the independent variable (assignment to sub-groups) and to calculate the dependent variable (shift from baseline). This makes the analysis prone to regression-to-the-mean artefacts, whereby the left baseline bias group will appear to shift rightward and the right baseline bias subgroup will appear to shift leftward (see text for details). Error-bars represent 95% confidence intervals.

under our informed experimental hypothesis. This level of relative evidence for the null may be considered ‘substantial’ ($BF_{01} > 3$) but not ‘strong’ ($BF_{01} > 10$) (Jeffreys, 1939). We might also note that the effect size estimate from the comparison between prism and control groups ($d = .27$, 95% CI $[-.01, .55]$) is compatible at the upper end with an effect that exceeds the targeted minimum effect size, and would be far from negligible. Thus, we do not rule out the possibility that leftward prisms might induce rightward shifts in landmark judgements, but we should be more sceptical about this claim.

There are at least three other reasons for scepticism. First, the non-significant numerical trend towards a rightward shift for the prism relative to the sham condition was not driven by a rightward shift in the prism group, but by an unexpected

(and unexplained) trend towards a leftward shift in the sham group (see Fig. 4b). With one exception (Experiment 1, McIntosh et al., 2019), no previous study on this question has included a sham control group; the claimed visuospatial aftereffects have always been based on a rightward shift in the prism group alone. A comparable estimate of the visuospatial aftereffect for the prism group alone in the present study would be $d = .03$, 95% CI $[-.17, .22]$, suggesting no directional shift at all.¹¹ A Bayes Factor for these data indicates 3445 times

¹¹ If we include the data from the 62 participants reported in Section 1.2, then the overall ($n = 164$) effect size estimate for the visuospatial shift following adaptation to 15° prisms is $d = .00$, 95% CI $[-.15, .15]$.

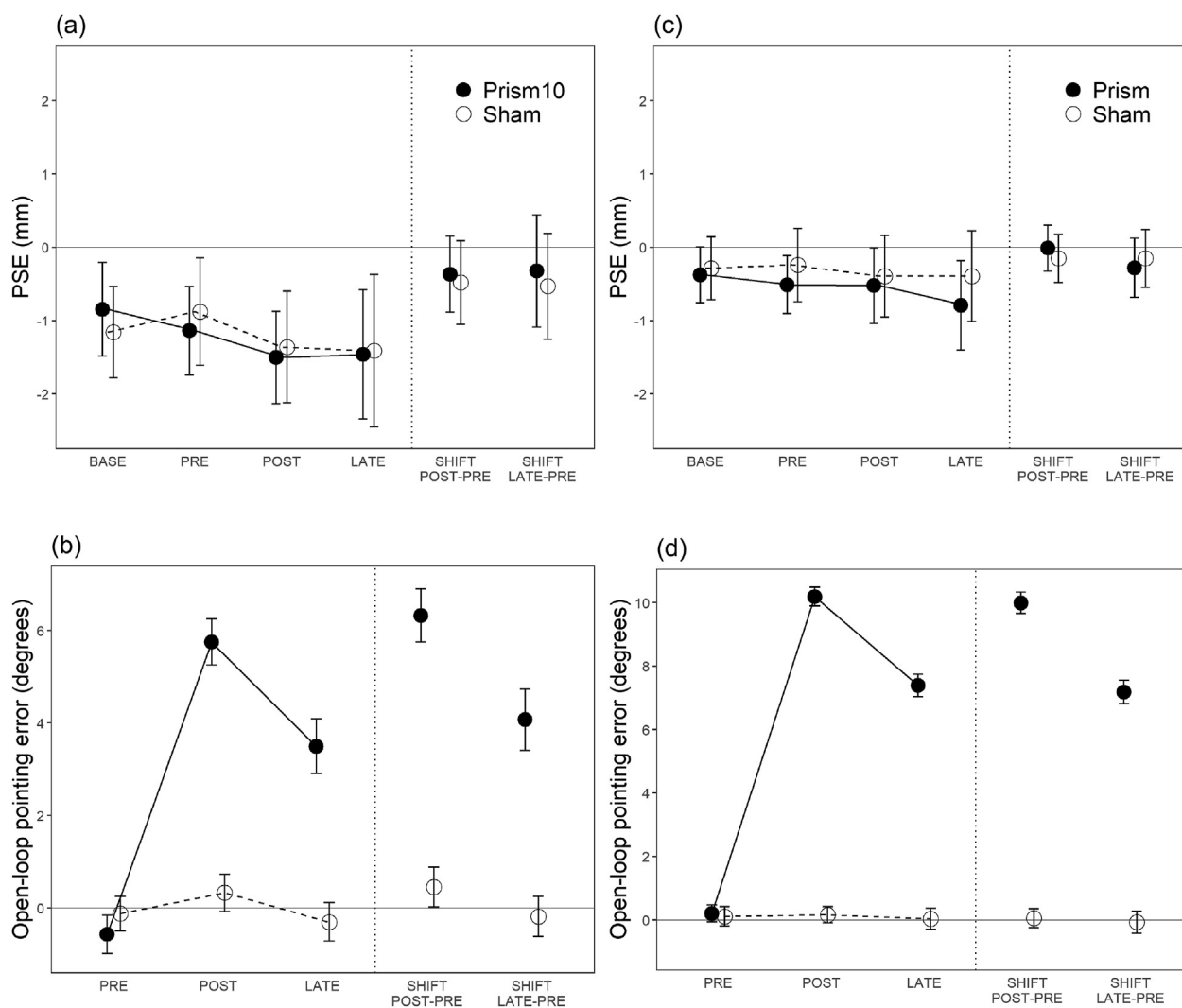


Fig. 7 – Exploratory analyses of two sub-sets of the total data, which form counterbalanced experiments (see text for details). (a) and (b) show landmark PSE and open-loop pointing errors for a group of 29 participants tested at the Bath site with 10° leftward prisms, and the 30 sham controls tested in alternation. There are no discernible visuospatial aftereffects (panel a right side), despite robust sensorimotor aftereffects in the prism group (panel b right side). (c) and (d) show landmark PSE and open-loop pointing errors for a group of 74 participants tested at the Edinburgh site with 15° leftward prisms, and the 60 sham controls tested in alternation/rotation. There are no discernible visuospatial aftereffects (panel c right side), despite robust sensorimotor aftereffects in the prism group (panel d right side). Error-bars represent 95% confidence intervals.

more evidence for the null than for the informed experimental hypothesis. If the specific claim from prior literature is that landmark judgements shift rightward following leftward prism adaptation, we find decisive evidence against this claim for the prism group.

Second, there is still no clear empirical or theoretical link between the classical sensorimotor aftereffects and the proposed visuospatial aftereffects. [Facchin et al. \(2019\)](#) estimated the sensorimotor aftereffect to be 38% of prism strength on average (and 50% at best), so our sensorimotor aftereffect of 9.4°, reflecting 63% the prism strength, is very strong. Not only did we detect no visuospatial counterpart to this strong sensorimotor adaptation at either the post-prism or late test, but there was no discernible relationship between sensorimotor and visuospatial aftereffects across participants ([Fig. 5](#)).

Even in studies where visuospatial changes have been claimed at the group level, a correlation with sensorimotor aftereffects has never been confirmed (see [Michel, 2016](#)). This independence is hard to reconcile with the idea of a causal link between low-level sensorimotor plasticity and high-level cognitive functions, or with a dose–response relationship between prism strength and the magnitude of visuospatial aftereffects ([McIntosh et al., 2019](#)).

Third, we found the same pattern of strong sensorimotor aftereffects without visuospatial aftereffects in a preliminary group of 62 participants adapted to 15° prisms (Section 1.2, [Fig. 2](#)), and in an additional group of 29 participants adapted to 10° prisms ([Fig. 7a–b](#)). This result for the 10° prism group might be relatively unsurprising in the context of a possible dose–response relationship with prism strength ([McIntosh](#)

et al., 2019), but as part of the broader picture of data in this study, it further undermines our confidence in visuospatial aftereffects.

4.2. The (lack of) influence of baseline bias

Some previous claims of visuospatial aftereffects have been motivated by sub-group analyses, where it appears that leftward prisms induce a rightward shift mostly or only in participants who have a pre-existing leftward visuospatial bias (Goedert et al., 2010; Herlihey et al., 2012; Schintu et al., 2017). It has even been suggested that prism adaptation induces opposite visuospatial aftereffects contingent on the baseline bias: a rightward shift in participants with a leftward baseline bias and a leftward shift in participants with a rightward baseline bias (Schintu et al., 2017). Unfortunately, these studies have all shared a methodological confound, that the measure of baseline bias used to sub-divide participants is also subtracted from post-prism performance to calculate the visuospatial shift. This design is prone to a regression-to-the-mean artefact, which could create a spurious negative correlation between baseline bias and visuospatial shift (Campbell & Kenny, 1999). Assuming only that there is some random error in the measurement of landmark bias, then any chance factor that pushes a participant's bias further leftward in the pre-test, will simultaneously make them more likely to be classified with a left baseline bias and to show a rightward shift when the pre-test performance is subtracted from the post-prism performance (and vice-versa if chance factors push pre-test performance rightward). To properly examine the influence of baseline bias on visuospatial aftereffects, it must be measured independently of the post-prism shift.

For this reason, we included an initial separate baseline block of the landmark task. We used this block to divide participants into left and right baseline bias groups, and observed no obvious differences between these groups in the post-prism shift (Fig. 6a). But when we deliberately made our analysis prone to regression artefacts by calculating the shift in landmark performance between baseline and pre-test blocks, we saw that participants with a left baseline bias shifted rightwards, while those with a right baseline bias shifted leftwards (Fig. 6b). This pattern of regression to the mean cannot be mistaken for a contingent effect of prism adaptation, because the calculated shift (pre-test minus baseline) precedes the prism treatment, and the effects are equally clear in the sham control group. We suggest that a similar regression artefact is responsible for baseline-contingent prism effects in the earlier literature (Goedert et al., 2010; Herlihey et al., 2012; Schintu et al., 2017). We would note that the high reliability of our landmark task (Cronbach's alpha .90) implies relatively little measurement error, so our regression artefacts were fairly subtle. If a less reliable landmark assessment were used (e.g., fewer trials per block), the pattern would be expected to be amplified.

4.3. Meta-analysis reconsidered

This Registered Report was informed by a meta-analysis of prior studies on the visuospatial aftereffects of leftward prism

adaptation (McIntosh et al., 2019). That meta-analysis concluded the visuospatial aftereffects for the landmark task are “real and robust” ($P271$). The strongest evidence for this conclusion was an apparent dose–response relationship, such that the size of the visuospatial aftereffect was statistically moderated by prism strength and duration of exposure. This pattern of moderation suggested that some failures to observe visuospatial aftereffects could be due to insufficient prism strength or exposure duration, but that adapting participants to 15° prisms with more than 250 pointing movements would ensure robust effects. This Registered Report was a stringent, high-powered test of that prediction, and we were unable to elicit the expected effects, so where does this leave the conclusions of the meta-analysis?

Meta-analysis is a useful tool, but a meta-analytic estimate cannot easily transcend the quality of the literature included. If a literature is biased, the meta-analysis will inherit the bias. As discussed in Section 1.3, one method for assessing whether biases are present is to examine the funnel-plot for asymmetry, particularly for signs that smaller studies produce more positive effects. This could imply that larger effect sizes are over-represented amongst smaller studies, so perhaps some smaller studies without positive effects have gone unreported. There was no strong evidence of asymmetry for prior literature using the landmark task, even when updated to include the novel experiment reported in Section 1.2 (see funnel plot in Fig. 1e).¹² However, the asymmetry would be stronger if the effect sizes from the 15° ($n = 102$) and 10° ($n = 29$) groups reported in our Results were now to be included.¹³ The apparent dose–response relationship would also be weakened, because the original pattern was driven mainly by earlier small-scale studies, which were the only prior studies to use strong (15°) prisms and long exposures (black triangle symbols in lower right of Fig. 1a). Additionally, the decline effect in Fig. 3a would become stronger if we included the new evidence (ρ would change from $-.67$ to $-.72$). These considerations all increase the impression that potential biases in the literature may make the original meta-analysis a misleading representation of the true effect.

If the prior literature is potentially biased, then there would be limited value in adding our new data to the pool and updating the meta-analysis. Rather, we should regard these Registered Report data as the most verifiably unbiased, and by far the largest-scale evidence available. On this basis, we do not rule out any possibility that leftward prisms induce rightward shifts in landmark judgements, but we should be more sceptical about this claim, and we suggest that any further studies should also follow the Registered Reports route. We are not motivated to perform these studies ourselves, not least because a similarly high-powered test for the central estimated effect size from the present study ($d = .27$) would require more than 300 participants per group.

¹² As noted earlier, this was not true for the meta-analysis of manual line bisection studies, where a significant asymmetry remained even after moderation (McIntosh et al., 2019).

¹³ Egger's test of asymmetry, for the unmoderated random-effects model, would be significant at $z = 2.11$, $p = .03$.

4.4. Conclusions

We do not reject the null hypothesis that leftward prism adaptation does not shift landmark judgements to the right in neurotypical participants. More broadly, we conclude that there is no compelling evidence to support the existence of visuospatial aftereffects of prisms on the landmark task. If they do exist, they are almost certainly much smaller than a meta-analysis of prior literature has suggested (McIntosh et al., 2019), and small enough that they may be of limited theoretical or practical importance.

We chose the landmark task as the perceptual task for which high-level aftereffects were first claimed (Colent et al., 2000), and which has the largest evidence base. Having specifically studied the landmark task, we cannot readily generalise our conclusions to the various attentional (e.g., Bultitude, List, & Aimola Davies, 2013; Bultitude & Woods, 2010; Loftus, Vijayakumar, & Nicholls, 2009), representational (e.g., Loftus et al., 2008), non-visual (e.g., Girardi et al., 2004) (Michel et al., 2019), or other tasks for which ‘high-level’ aftereffects of prism adaptation have also been claimed (see Michel, 2016, for an overview). Nor can we speak to the evidence for changes in activation patterns within attentional networks following prism adaptation (Clarke & Crottaz-Herbette, 2016; Crottaz-Herbette, Fornari, & Clarke, 2014, 2017). But if we now have greater cause for scepticism about the robustness (or reality) of visuospatial aftereffects for the landmark task, it may motivate a re-appraisal of similar claims for other tasks and measures.

Finally, the applied significance of a potential link between low-level sensorimotor plasticity and higher-level spatial cognition is that it bolsters the rationale for using prism adaptation to treat spatial neglect following stroke. Nearly a quarter of a century on from the first report of these therapeutic effects (Rossetti et al., 1998), the underlying mechanisms are still uncertain (Rossetti, Kitazawa, & Nijboer, 2019). Moreover, the dramatic changes seen in some patients have failed to translate into functional benefits in randomised controlled trials (Longley et al., 2021). The present null findings cast further doubt on a causal link between low-level sensorimotor plasticity and higher-level spatial cognition, and weaken one part of the rationale for applying this treatment to neglect.

CRedit author statement

Robert D McIntosh: conceptualisation; methodology; software; formal analysis; resources;

Data curation; writing—original draft; writing—review & editing; visualisation; supervision;

Project administration; funding acquisition.

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Hannah Jones: investigation; data curation; writing—review & editing.

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Melissa Thye: investigation; data curation; writing—review & editing.

Janet H Bultitude: methodology; resources; writing—review & editing; supervision.

Open materials and data

The study in this article earned Open Data, Open Materials and Preregistered badges for transparent practices. Materials and data for the study are available at <https://osf.io/k7b28/>.

Data availability

Compiled and source LabVIEW code for the experimental tasks, and full raw and processed data and analysis R code are archived at <https://osf.io/k7b28/>

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Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.cortex.2022.11.003>.

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