Editor comments:

This is nearly there; just a couple of final minor points to fix. I will run it past the reviewers one final time to check they are happy, but I'll ask them to just focus on these final points to speed things along. Please make sure you signpost any changes clearly for them. One additional thing: can you please edit the Stage 1 Methods section to now be in the past tense. This was a style choice made by the team a while back, I'm sorry that I forgot to mention it before.

**We have edited the Methods section to be in past tense, as requested.**

On the issue of the data cleaning point: include the incorrect trial removal in the text, and add a footnote to the effect that this was in the analysis plan but accidentally omitted from the Stage 1 text. This will just help keep everything transparent, as the Stage 1 approved version remains available as well.

**Thank you, we have edited the text and footnote accordingly.**

Reviewer 1:

Unfortunately, there are still a few issues I'd like to have resolved before I can recommend publication of this paper:

1. Dr. Lleras' comments on strong guidance by color reminded me of my earlier comment that it is important for the present study to report the exact color values and prompted me to compare these values with those reported by Buetti et al. I had not done this before, because I had assumed that - in response to the respective comment in my very first review of this paper - you had made sure that the colors are the same. I apologize for this oversight! However, I encountered a series of problems: (a) color values are not reported in the present manuscript, but only in a supplement; (b) this supplement is not linked in the present version of the manuscript (this and a few other supplements seem to have been dropped), (c) the supplement to an earlier version reports xyY instead of the L\*a\*b values reported in Buetti et al. I strongly encourage you to report L\*a\*b values in the text and also add a statement whether these are the same as the colors in Buetti et al. If, in order not to change the Stage 1 version, you don't want to do this in the methods section (where the reader would usually expect it), you could do it in the section where you discuss potential differences compared to Buetti (either acknowledge a difference in colors or exclude this as an explanation). Also, can you verify that colors have not unintentionally changed from the pilot to the main experiment (e.g., due to employing other hardware, changes in display settings)? From my perspective even slight (unnoticeable) changes in color can have huge effects, because of large changes in salience.

**Apologies, these data should still have been available on GitHub, but they must have disappeared from the submission system somehow (we thought that unchanged documents would be ‘carried over’, but this was clearly not the case). Both the pilot and main experiment were carried out on the same computer, with the same display settings.**

**We have now included the L\*ab values in the main text, as requested (see P27). We discuss the changes in colours used in the paragraph beginning L686 - most of the colours we used were very intentionally different from those used in the original manuscript, after discussions during the initial review process with reviewer 2. We also found that our reanalysis methods changed the best supported combination method when using Buetti et al (2019) data. However, we take the point that it may be that the preferred combination method does vary with colour, and we have now added this idea to the main text (L690-692).**

2. It was difficult to judge whether and how well you've implemented my advice to improve the ease of reading, because your response was quite unspecific - can you point me to a few places where you improved the writing of the manuscript? But I can also understand if you decide not to follow this advice.

**Apologies, we should have made our edits clearer. In the main, we were attempting to ensure that there was always a clear statement about how the results link to our different hypotheses, which hopefully should make the process of reading and understanding the results more straightforward.**

**L473, added “in accordance with our registered hypothesis”: trying to make it clearer here how our findings relate to the predictions.**

**L481: similar to above, but now for section 5.2.**

**L515: similar to above, but clarifying for section 5.4.1.**

**L533: similar to above, but clarifying for section 5.4.2.**

**L588: similar to above, but making conclusions for the target eccentricity section more explicit.**

**L597: adding in more detail here to explain the issues with the collinear contrast method more thoroughly.**

**We have also extensively edited the supplementary materials to include more explanatory text.**

**We hope this helps, but we are of course happy to make further edits for clarity if you have specific suggestions.**

3. Regarding my previous Point 2, I think you must mention in the paper that you removed incorrect trials even though you had overlooked this detail in the Stage 1 report. Hiding this fact now that the oversight was pointed out seems unethical, thus definitely overruling any principle of Registered Reports. I'd suggest to keep the wording from the first version of the Stage 2 report and add a footnote pointing out this omission in the Stage 1 report (comparable to a formal correction notice). Just mentioning the removal in a footnote (which would be another alternative) would also kind of hide this analysis detail. A similar footnote explaining what you explained to me in response to my respective comment is needed for Fig. 2, I feel. Maybe there is already a standard solution for such situations and the editor can advise accordingly. If not, consider this comment a suggestion on how to handle this issue in general (which did certainly not occur for the last time here) and feel free to pass it on to others.

**See above - we have followed the Editor’s advice on this point.**

4. As a very minor point, note that citing Rangelov et al. (2017) again would further support "that negative search slopes do occur in some situations".

**Thank you, we have now included this citation.**

Reviewer #2: I want to thank the authors for a very thorough and informative round of revisions. I think everything is in order in the current manuscript, except for one minor inaccuracy from new text (below) that should be corrected. I specially want to thank the authors for doing the new analyses trying to address the negative slope problem. Those results were indeed very interesting. I think they will be valuable to readers. Personally, it was nice to see the collinear model make sensible predictions again. Congratulations on an excellent project.

Alejandro Lleras

The incorrect new text is on line 704. The authors write: "However, the distractors could change from trial to trial in all other blocks, unlike in Buetti et al. (2019). In their experiments, it is possible participants used strategies such as shifting the target representation away from the distractors, or generally using relational strategies (Navalpakkam and Itti, 2007; Becker, 2010; Yu et al., 2023), which would be more challenging in our experimental set up." This is incorrect.

In our paper, distractors were intermixed, not blocked. Every experiment had three different types of distractors, and all three possible colors and three possible shapes appeared in each experiment (so it is not the case, say, that one experiment had only red distractors, of various shapes, or anything of the sort). So, no, distractors were not blocked and there was no shifting target representation strategy possible, as suggested by the authors. As to whether tuning of some kind was possible, if it was possible in those experiments, it should have been possible here as well, since the same intermixing process was used, and generally, the same sort of color distribution for stimuli. The bigger difference, interestingly, is that in this experiment, participants were exposed to more different kinds of distractors (9), than in any of our experiments, where they only experienced three. I don't know if it is worth noting that anywhere in the manuscript, actually, but I do know that researchers like Joy Geng to study quite a bit how the ecology of distractors experienced during an experiment impacts performance (that is, the number of different distractors, the type of those distractors, etc...). So, to some readers, it might be interesting to think about that difference.

Anyway, editing this line of text is important as we don't want readers to believe our subjects were only seeing one kind of distractor per block. That was not the case. Thank you.

**Apologies, we have removed this sentence and agree completely with the point about the total number of distractors, which we have now incorporated into this paragraph as well (L707-709).**