

Reply to William Harrison (open online review)

The authors present a series of experiments in which they test the detectability of distorted letters in the periphery. Four target letters are presented at four isolated locations (north, south, east and west of fixation), and are presented alone (unflanked) or closely flanked by four distracting letters. Distortion detection thresholds depended on the type of distortion added to the letters, with band pass distorted letters having a tuning function that shifted to higher frequencies in the presence of flankers. The authors conclude that distortion detection is prone to crowding.

The manuscript is well written, the experiments are expertly conducted, the results are clear and the theoretical contribution is incremental. I have only one major concern that requires only rewording, and minor points of clarification.

Major

Line 171 – although the flankers are set to be within “Bouma’s law”, I would be surprised if the letter could not be more crowded for some if not all observers. There is no discussion about this point but it’s important. In fact, I don’t think spatial interference per se was quantified in any of the experiments here, but instead what was quantified was the influence of flankers present vs absent. The authors present only a partial snapshot of crowding, with no discussion about the bigger picture of the changes in performance across areas of space which typically define crowding.

The reviewer is correct that we make no attempt to quantify spatial interference in this crowding task, and we agree that such a characterisation would be interesting. However, in the interests of brevity (given that we have added some additional analyses to the paper in response to the other reviews), we have chosen not to include a new experiment here. We instead now discuss this issue in the revised discussion (lines 453–460).

Minor

Introduction: I think the theoretical motivation could be strengthened for this choice of manipulation. It’s not immediately clear if we stand to gain a greater understanding than just a quantification of crowding with a slightly different task and stimuli.

In line with these comments and the other reviewers, we have rewritten parts of the introduction and discussion to make the theoretical motivation for these experiments more clear. We hope the reviewer agrees.

Paragraph starting line 145 – The pre-generation numbers are not clear to me. There appears to be no random element of the RF distortion, and so the “repeats” produce the same stimuli (same for the undistorted stimuli). If this is correct, I suggest a note is added acknowledging this to save the reader from thinking there are somehow more unique stimulus combinations than actually created.

The phase of RF distortion is randomly determined on each trial, creating unique images over repetitions. We have revised this sentence to make it clear that these were indeed unique.

RF distortion – it took me several reads, and a long time looking at Figure 1, to get my head around how you computed the distortion. I think my confusion came from the fact that the text suggests that the pixels are shifted by a sinusoid extending from the centre of the image (ie. along radial axes), whereas the sinusoid actually wraps around polar angles, but causes pixels to shift along the radial axes. Both alternatives could be sensible – radial sinusoids would create a ripple effect emanating from the centre of the image – and so the ambiguous wording needs clarification. E.g. lines 80 – 83 are written as though there will be a radial ripple effect. Also, lines 134-135 read: “...created by modulating the distance of each pixel from the centre of the padded image sinusoidally...” I know the directly following equation clarifies the coordinate system, but I

found the aforementioned sentences confusing.

We thank the reviewer for pointing out these sources of confusion; we have clarified these distinctions in the revised manuscript.

Clarify line 156 – it sounds like you were asking observers to report the distorted letter identity, when they must have been reporting its location.

Done.

Line 54 – metamorphopsia is raised in the introduction. Are the results relevant to this disorder? A sentence or more may be warranted in the discussion.

To the extent that our results are generalisable, the finding that crowding impairs distortion detection offers an explanation for why patients with metamorphopsia often fail to notice their distortions until probed with artificially-regular stimuli: crowding. We now include a paragraph in the discussion pointing out this possibility (lines 484–495).

Line 84 – Although it may be unlikely, observers may use different mechanisms to identify each type of distortion – a single model may not suffice.

We have amended the sentence to note this possibility.

There is a bracket missing in the filter equation.

Fixed.

Lines 392 – 295 – It may be worth noting more explicitly that the general point from our paper is that quantifying whether perceptual reports appear to follow target-flanker averages or substitutions does not necessarily distinguish mechanisms. Empirically, it's clear that both types of errors occur (and probably occur in these experiments too). It's less clear what theoretical distinction there should be.

We have modified this sentence to make this point more explicit.

Finally, the authors could consider using the term crowding more precisely. There seems to be no consensus on whether the word crowding refers to an effect or a cause or something else, but here the authors seem to switch between meanings. For example, in the introduction, crowding is described as the loss of discriminability (line 39), whereas in the discussion, it's described as a cause of the loss of discriminability (line 326).

Thanks for pointing out our lax language use here. We define crowding as an effect, not a cause (i.e. the former). We have amended our language throughout the paper to clearly delineate “crowding” from “the mechanisms that cause crowding”.