

Response to reviewer 1 of Jenner et al., Plasma density gradients at the edge of polar ionospheric holes: The absence of phase scintillation

We thank reviewer 1 for their detailed and careful review of this paper. We note that reviewer 1 stated that this paper is clear, well written and worthy of publication after minor revisions. We thank reviewer 1 for this comment.

Reviewer 1 has raised a number of important points. All of these have been addressed below, and we believe that these substantially enhance the manuscript.

Comment 1: The meaning and definition of phase scintillation at high-latitudes

The reviewer commented that this was missing from the introduction. We had omitted this in the interests of concision, but the review is absolutely right that it needs to be included. A brief but comprehensive summary is now given in lines 117-130 using many of the references suggested by the reviewer.

Comment 2: Title of the paper

We agree that the suggested modification gives a better title for this paper, and have amended the title.

Comment 3: Would phase scintillation be observed at lower time windows?

This is an excellent suggestion, and one worthy of investigation. The effect of the length of the sampling window on phase scintillation indices would be a fascinating next step, and one which shall certainly be investigated in future experiments. A comment to this effect has been added in lines 425-429. As only the scintillation indices (and not the raw 50 Hz data) have been archived it is not possible to investigate within this study, and we have used the 60 second window which is commonly used within the community.

Comment 4: Abstract should refer to GNSS receivers, rather than GNSS satellites

Agreed. This change has been made (line 20). Similar changes have been made to lines 263 and 442.

Comment 5: Figure 5 is cited before figures 2, 3 and 4.

Thank you for pointing this out. Line 182 has been re-phrased accordingly.

Comment 6: ACE data is available time-shifted to the bow shock

Indeed it is. We have re-produced figures 1 & 6 and modified the text accordingly.

Comment 7: EISCAT data (figures 3, 8 and 10): Choice of colour scale and scaling of velocity data

The reviewer suggested changing the colour scale on these plots from red-green-blue to red-white-blue. We have retained the red-green-blue colour scale, as this is commonly used for EISCAT data and so enables an easy comparison to other data sets in the published literature.

The reviewer also commented that, for Fig. 3 (bottom panel) “The adopted color code is not suitable to catch positive/negative variations.” This is true, but the choice was deliberate. The same scale was used for the velocity information in figures 3, 8 and 10. This was so that the difference between the larger velocities in Fig. 10 for the radar observing at low elevation and the smaller (negligible) velocities

at high elevation (figures 3 and 8) could be clearly seen. However, we did not make this clear in the paper, and thank the reviewer for their comment. We have amended the text in lines 205-208, 274-275, 307-308 and 324-329 accordingly.

Comment 8: Citation of chosen threshold for SigmaPhi

A discussion of different thresholds (along with suitable citations) has been added to lines 279-282.