Response reviewer 1

Firstly, we would like to thank the reviewer for useful comments and feedback. Below you will find each comment along with our responses in red text.

This simple and short paper deals with an important, and yet neglected (after 22 years of Cluster) topic, on whether magnetic holes are convected with the solar wind plasma or if they have an intrinsic velocity in the plasma frame. To do this the authors use the well-known timing-method by Harvey, which has proven itself well over the duration of the Cluster mission, on the magnetic field data. The solar wind velocity is taken from the CIS-CODIF/HIA instruments. The combination of the timing velocity and the solar wind velocity projected in the timing normal delivers then the hole-velocity in the plasma frame. Also, the Alfvén velocity is determined for each event, in order to check the possible creation of the holes. It is found that the magnetic holes are basically convected with the plasma flow and have only a small, sub-Alfvénic velocity in the plasma frame, where the error in the velocity is mainly determined by the error in the plasma instrument. The paper is well-written, and the results are clear. However, there are a few minor points that should be checked:

- Page 2: Here the FGM is described and the text says that the full resolution of the instrument is 0.04 s, which would be roughly 25 Hz. However, the normal-mode of the Cluster FGM is 22 Hz. Then later on page 7 it is said that the error in the time is taken to be two data points, claimed to be ~0.09 s, but that does not agree with the 0.04 s on page 3. Therefore, it is better to write the sampling frequency, mentioning the ~0.09 s would then be okay. Yes, this is a mistake from our part, and will be corrected in the manuscript.

- Page 3: "standard deviation" I think the authors have mixed up "low" and "high". To use the std to filter out the "noise" the threshold has to be high enough (put infinity, no signal comes through), but is should be low enough to let the real signal (the MH) through. – Yes, we have mixed up the two, and will change it in the paper.

- Figure 1: I think panel g is unnecessary, the location can be put into the caption. We agree with this, and will remove the last panel, and instead put location as text in the figure.

- Page 4, line 90: (Horbury et al., 2004) LaTeX \citep should be \citet – We will rephrase the text in the paper to "(..) and Horbury et al. (2004) used it for Cluster observations of mirror modes.

- Page 4, line 93: "However, suppose …", I think is would be better to write "However, we suppose that …". We suggest changing it to "Suppose, however, …"

- Page 4, line 96: "The plane perpendicular …" I do not understand this sentence. Timing analysis assumes that the structure is a plane wave moving over the tetrahedron, and one gets the normal to this plane. How this is related to "minimum magnetic field strength" is unclear to me. Do the authors mix up here maybe minimum variance of the magnetic field? What is meant by this, is that for a cylindrical geometry, the plane that is relevant is a plane perpendicular to the flow velocity, and defined by the magnetic field minimum along each flow line. This methodology was also used by Horbury et al. (2004) when performing timing analysis on mirror mode structures.

- Page 4, Eq. 6: There is no upper limit for the summation, and there is an empty Eq. 8 The upper limit should be N, and the empty equation 8 has been removed.

- Page 5: (Wang et al., 2020) LaTeX \citep should be \citet. We will rephrase the text to "(..) was used by Wang et al. (2020)"

- Page 6, line 129: The authors use an angle of 50° for the split between linear and rotating magnetic holes and base that on Karlsson et al. (2021). However, looking at that paper I seem to find that there the distribution of rotation angles shows a (inverted) knee at roughly 30° (their figure 4 and equations 6 and 7). What is the reason for choosing this angle differently? Clearly, the distribution shown in Fig. 2c indicates a boundary at 50, but how should that be combined with earlier determined boundaries? We believe that for our data set the boundary at 50 degree corresponds to a distinctive change in the distribution. In previous studies, several different definitions have been used, so there is no standard definition to relate to. Also, the data are available in Appendix A, and can be regrouped by other boundaries, if desired.

- Page 5, line 143: "the magnetic field direction", I would add "background". We agree and will add it to the text.

- Page 6, line 150: "to the high error" I would write "to the assumed high error". Yes, we agree to this and will change it.

- Page 7, line 189: "a factor of 2", this is more that a factor of 3 (25/7.4) Yes, that is right, and we will change it in the text.

- Table A1: This table give a lot of information that can be used. I would ask for an additional 2 columns, one with the Q-value of the tetrahedron (see Robert et al., 1998, in "Analysis Methods for Multi-Spacecraft Data", Paschmann & Daly) and the angle of n_timing and V_sw. The former will show an indication of how well the timing analysis is done and the latter will (maybe) show that current sheets (strong rotational holes) have a normal more tilted wrt. the solar wind direction. A good suggestion, we will add these values to the table.

Horbury, T. S., et al. "Motion and orientation of magnetic field dips and peaks in the terrestrial magnetosheath." *Journal of Geophysical Research: Space Physics* 109.A9 (2004).