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Interactive comment on "On the alignment of velocity and magnetic fields within magnetosheath jets" *by* Ferdinand Plaschke et al.

Anonymous Referee #1

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The manuscript presents statistical results of how the angle between the local magnetic field and velocity vectors varies during magnetosheath jet events. This work has been motivated by recent simulations and case study observations. Given the myriad of impacts on Earth's magnetosphere that magnetosheath jets can have, understanding their propagation from their bow shock origin to the magnetopause is important and has largely been an open question in this particular topic within the solar wind - magnetosphere dynamical coupling. The methodology and results are clear and well presented and the results quantitatively align with previous theoretical interpretations of previous work, lending the statistical weight to these. I recommend publication subject to the authors addressing a number of minor issues.

General Comments:

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Discussion paper

It was not clear to the reviewer whether the angles used (phi) were limited to be the acute angles (0-90 degrees) between the vectors. The figure limits throughout suggest this may be the case. However, while perhaps unlikely, it might be possible under certain configurations that a jet could bend the magnetic field lines back on themselves significantly resulting in angular deflections greater than 90 degrees which this analysis would not capture. This would result in the wrong angle being measured in the deflected regions. The authors should check that no greater than 90 degree deflections within the jet from pre/post occur in the dataset. If they do, the authors will need to re-do the analysis using the full angle between the vectors. They may wish to counteract the effects of the sign of Bx, which would lead to two separate populations in the data corresponding to either side of the heliospheric current sheet, by the average pre/post interval sign of Bx into account when calculating the angles for each event.

In several cases, the authors quote median values as well as standard deviations. However, commonly a standard deviation is a difference from the mean value rather than the median. Medians are appropriate here as the mean is likely to be affected by outliers. So to would even a standard deviation about the median be affected. Quoting the lower and upper quartiles would be more appropriate throughout.

The abstract did not make it clear that there is a significant trend in deflection angles with jet speed. This is a key result of the paper and should be made more prominent in the abstract.

Specific Comments:

Page 1 Line 2 - The authors should also briefly comment on other scenarios such as that proposed by Karlsson et al. [2018, Ann. Geophys., https://doi.org/10.5194/angeo-36-655-2018] concerning SLAMS transmission through bow shock ripples which have recently been shown in Vlasiator simulations [Palmroth et al. [2018, Ann Geophys, https://doi.org/10.5194/angeo-36-1171-2018].

Page 4 Line 14 - Do the authors have an estimate on the number of independent jets

observed, taking into account those that were observed by the same spacecraft?

Page 4 Line 23 - It is unclear why twice the solar wind density is used as a comparison measure when the bow shock typically compresses the solar wind density by a factor 4. If the authors mean the enhancement in density was greater than twice the solar wind density they should so state and make this clearer in how this result is depicted in Figure 2.

Page 6 Line 17 - While the pre/post interval angle angle does decrease with cone angle (a result of the different draping patterns), it does appear that there is a very slight difference in the depth of the median deflections with cone angle. The authors should estimate these depths (the effect size) and the significance of any differences with cone angle more thoroughly.

Page 8 Line 1-2 - Can the authors comment more on expected draping angles at MMS's location for jet events or at least cite previous statistical studies into IMF draping near the magnetopause?

Page 8 Line 9 - The jet identification method does not necessarily mean a deflection towards the Sun-Earth line. Given the criteria, it could possibly have been the case that the y and z components of V similarly increased as Vx does which would not result in a deflection.

Page 8 Line 20 - The authors should perform simple estimates of expectations given the picture in Figure 1 i.e. from a purely geometric point of view, ignoring any resistive forces, how much deflection would be expected for the set of observed jets and draping angles purely by the jet's flow locally advecting the field lines. To what extent could e.g. magentic tension forces slow the jet's motion thereby reducing the deflection etc. This would bring into context the results and interpretation more clearly.

Page 9 Line 9 - The authors should also mention another statistical method which might be adopted - computing individual deflection depths based on the (average of) pre/post

jet intervals and estimating the distribution of deflection angles from this, rather than distributions of absolute angles. I am not advocating this be done for this manuscript, merely suggested as future work.

Technical Corrections:

Page 4 Line 12 - The statement "We require Vx to be negative within jet intervals and surpass half of its value at t0 within both pre- and post-jet intervals" is a little confusing and I would suggest the authors instead of discussing a negative number surpassing a threshold in the pre/post interval, instead talk about the absolute value dropping below said threshold.

Page 5 Line 4-5 - Please make it clear that values at each individual time are used, showing the same symbols as in the figures i.e. B(t) V(t). This will help contrast later with the other angles used.

Page 6 Line 6 - Please make clear that the magnetic field here is still taken at each individual time B(t).

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