In this document we give responses to comments from both referees. We start with referee #1 and then answer referee #2.

Author response to referee #1

The authors thank Anonymous referee #1 for their comments on the manuscript. We will take the comments into account when revising the manuscript. In this document we provide responses to each of the referee's comments (formatted as italics in indented paragraphs). At the end of our answers, we indicate in blue where the changes have been made in the revised version.

> *1) The authors added the analysis of VDF bringing interesting results but the discussion of them is insufficient. There are several features that should be clarified because they can influence the interpretation of results:*
>
> - *The authors conclude that only distribution in the panel b2 exhibits two peaks. However, even a very brief visual analysis reveals two populations in a1, a2 and c1 panels.*
> - *The distribution of Vx in a1 and c1 peaks at the jet velocity, whereas the distribution in b1 peaks at the background magnetosheath velocity. It implies that THD is outside the jet proper even at the maximum of the dynamic pressure. A possible explanation could be the compression of the ambient magnetosheath plasma that leads to increase of the dynamic pressure.*
> - *The rest of the ion population with a low (magnetosheath) velocity can be identified also in a1 and c1 panels.*
> - *Distribution of Vz has two peaks in b2 but similar two populations are visible in a2.*
> - *The last three points suggest that the plasma inside the jet is a mixture of original magnetosheath population with the incoming jet population.*

We agree with the referee that the figure and its implications should be discussed in more detail. We also can not rule out that THD is observing the ambient magnetosheath rather than the jet. In the following responses, we will go into more detail about the jet velocity determination and its impact on the results. In the revised version the discussion of Fig. 2 is updated in lines 100-111.

> *2) Since VDF inside the jet is non-Maxwellian and consists of two populations with very different velocities, the velocity determined as the first-order moment strongly depends on their relative abundance. This abundance would vary in space and time and it can affect the analysis of velocity direction that is discussed in Figures 3 and 4 and used in determination of the jet dimensions.*

We agree with the referee that the VDFs may vary temporally and spatially. Therefore, we manually inspected the 1D VDFs at each time step within the jets and selected the jet velocity in the following way: If we observe two populations in the 1D VDFs (two peaks or a strong deviation from the ideal Maxwellian curve), we choose the highest absolute velocity in the x-direction and for the y- and z-directions the coldest population (peak with steeper slope and smaller width) [similar to Raptis et al., 2022].

When comparing the manually selected velocities with our previous approach of using the velocity at which the 1D VDFs peak, we find almost no difference for THA and THE. This means that we have already successfully selected the velocity of the jet plasma population. For THD, we observe more deviations. However, this has no influence on the analysis of the flow pattern as we only use THA and THE measurements. However, it should have an impact on the dynamic pressure profile as the measured dynamic pressure is affected at THD, as the referee notes in the next comment.
In the revised version we describe the velocity determination in lines 112-118. We updated Fig. 3, 4, 5 and 6 and the figures in the appendices as velocities at THD have changed marginally.

3) The size analysis in Figure 6 would take into account the fact that the dynamic pressure enhancement observed by THD is caused by an increase of the density whereas the velocity increase is the major contributor to the dynamic pressure observed by other two spacecraft.

We agree with the referee that the density increase at THD can also be explained by the ambient magnetosheath plasma being compressed by the jet and thus implying that THD is not observing the jet. Therefore, we fitted the Gaussian profile to both the THA and THE measurements only and to the measurements for all three spacecraft with updated THD measurements (manually selected as described above). The qualitative behavior and our conclusion remain the same. This is shown as an example for three profiles in Fig. 1 for the fit to all three spacecraft and in Fig. 2 for the fit to THA and THE only (note that we chose a different time step than in the current manuscript after $t_{\max}$ as the Gaussian profile was not applicable for this case).

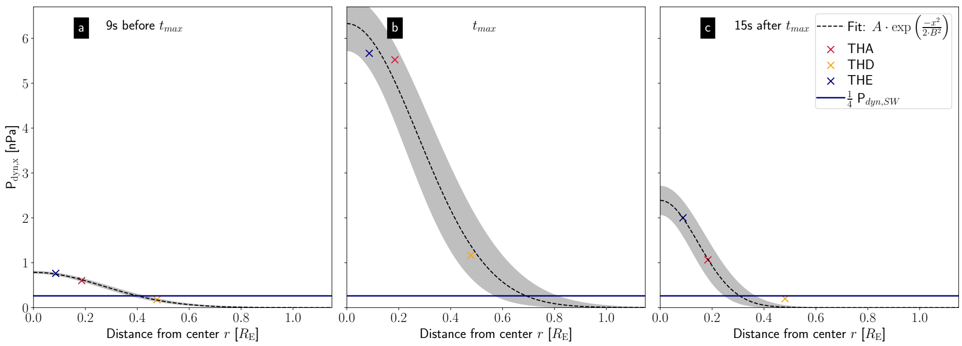

Figure 1: Dynamic pressure $P_{\mathrm{dyn,x}}$ derived from velocities from the 1D VDFs in the spacecraft system versus the distance from the center r at THA, THD and THE (crosses in red, orange and blue, respectively) at 9 s before $t_{\max}$ (a), at $t_{\max}$ (b) and at 15 s after $t_{\max}$ (c). The black, dashed line represent a fit with a Gaussian distribution to the data points of all three spacecraft and the gray area visualizes when we subtract/add one standard deviation σ from the optimal fit parameters. The blue horizontal line depicts a quarter of the solar wind dynamic pressure.

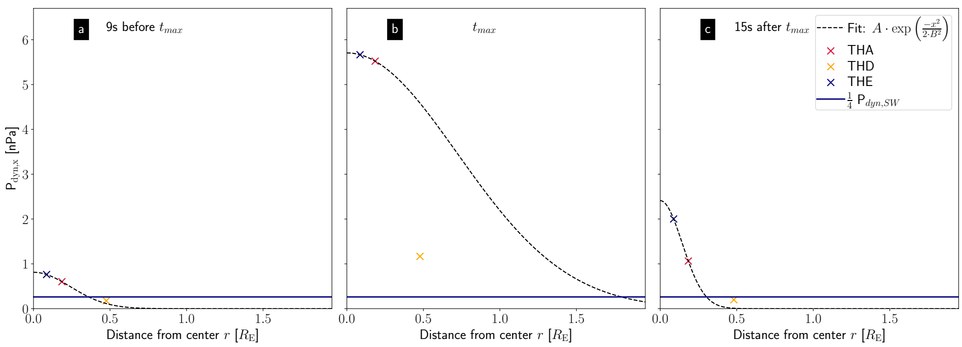

Figure 2: Dynamic pressure $P_{\mathrm{dyn,x}}$ derived from velocities from the 1D VDFs in the spacecraft system versus the distance from the center r at THA, THD and THE (crosses in red, orange and blue, respectively) at 9 s before $t_{\max}$ (a), at $t_{\max}$ (b) and at 15 s after $t_{\max}$ (c). The black, dashed line represent a fit with a Gaussian distribution to the data points of THA and THE. The blue horizontal line depicts a quarter of the solar wind dynamic pressure.

We can see that the Gaussian profiles 9s before and 15s after $t_{\max}$ are quite similar. The profile at $t_{\max}$ differs more in both figures, but the conclusion of higher perpendicular size and higher dynamic pressure is visible in both. In the revised manuscript, we will continue to fit the Gaussian profile to all three spacecraft measurements because the qualitative results are the same and we can provide estimates for the uncertainty of the fit, which is not possible with only two data points. However, we will discuss the possiblity of THD not observing the jet but the ambient magnetosheath. We will update Figure 6 and Figure B1 in Appendix B in the revised version. We will also explain the manual verification by selecting the velocity of the jet population as described above.

In the revised version we add the parts in lines 108-110 and 218-224.

> *The abstract would contain major results, it is not a proper space for discussion or description of the methods.*

We agree and will change the abstract accordingly.
In the revised version the updated abstract is in lines 1-11.

> *Line 43 – maybe it would probably be appropriate to also mention the paper by Nemecek et al. (2023) which illustrates the importance of jets for magnetopause processes.*

We agree and will add this part to the revised version.
In the revised version this is done in lines 42-44.

> *Figure 1 – there are no dashed vertical lines*

We will enhance the line width and separation to make the dashed lines better visible.
In the revised version Figure 1 is updated.

> *Line 39 - ..."jets can trigger and suppress reconnection", this connection it seems contradictory, it needs to be explained*

We believe this is a leftover from the first referee report as in the actual version of the manuscript this sentence is in line 45-46 and has the following explanation: "Hietala et al. [2018] showed that jets can trigger and suppress reconnection at the magnetopause, as they can modify the magnetic field in the magnetosheath and thus alter the shear angle at the magnetopause."
If the referee is not satisfied with the explanation, please let us know.
In the revised version this can be found in lines 44-46.

> *Line 183 – the brackets are probably at a bad location*

We agree with the referee and will split and rewrite this sentence to make it easier to read.
In the revised version this is done in lines 212-214.

> *Lines 101 and farther – It is not clear which velocities were used in the calculations – standard onboard moments or velocities determined by authors using analysis of 1D distributions. If the second is true, the method of determination of velocity components would be briefly discussed.*

In lines 99-100, we explain that we will use the velocity at which the 1D VDFs peak for the analysis. We will give a more detailed explanation in the revised manuscript and alter/remove the reference to Fig. 1 where we show moment data to avoid confusing the reader.
In the revised version the determination of the velocities from the 1D VDFs is discussed in lines 112-118. In lines 119-121 we are now referring to velocities from 1D VDFs.

Author response to referee #2

The authors thank Anonymous referee #2 for their comments on the manuscript. We will take the comments into account when revising the manuscript. In this document we provide responses to each of the referee's comments (formatted as italics in indented paragraphs). At the end of our answers, we indicate in blue where the changes have been made in the revised version.

> *1. The dynamic pressure in the central part of the jet is higher at tmax (6 nPa) and decreases towards the front and rear. So, from the 3 cases presented in your reply (Figure 9) (b) and (c) have a decreased dynamic pressure towards the front and rear. The case (a), as also stated in the reply, is unrealistic, and I fully agree. We already know that it is unrealistic from previous studies.*
> *Therefore, this conclusion is bounded to happen and is driven by the methodology as written in*

158-159 and 210-21. Is that correct?

The methodology only leads to a monotonic decrease of P_{dyn} with greater distance from the central axis r in the direction perpendicular to the jet propagation (since this monotonic decrease is inherent to the Gaussian fit). The lower dynamic pressure at the front and rear (along the propagation direction) are actual results of the measurements and the only realistic explanation if the time series exhibit a peak in the center. If the time series exhibit additional peaks in the front and rear part, the central dynamic pressure would increase again, as we will show in the revised version for our event.
In the revised version we explained our assumptions in more detail in lines 176-181.

Similarly, lines 176-177, seems to always be true for single-peak dynamic pressure jets? Or if you pick times that a monotonic relationship applies? (i.e., skipping the second peak in dynamic pressure shown in ~16:04:30 on THE?
If that's the case, maybe rather than stating a tautology, it can be more interesting to comment on some more details? For example, the decay of dynamic pressure before, after and at tmax seems to drop under slightly different rate with respect to distance. Could this be due to evolution in these small-time scales for the jet? Could it be something else? One could comment on the slope of these curves and have some discussion or ideas for future work.
If I misunderstood again your argument, it would be very useful to include some hypothetical timeseries of a jet that you would have to be observed by a spacecraft that would give you a different conclusion than the one you present.

We assume a monotonic decrease from the central axis towards the edge (in the direction perpendicular to the jet propagation). We make no assumptions for the dynamic pressure along the central axis (in the direction parallel to the jet propagation). We can also apply the method to multi-peak jets, and the referee rightly points out that there is a second peak after $t_{\max}$ (12 s afterwards). This peak is visible, for example, in Appendix Figure B1 (a1). Here you can see that P_0 rises 12 s after $t_{\max}$. Thus, even more peaks in the dynamic pressure measurements lead to a more complex 3D shape of the jet.

We will add the second peak to the discussion. On the other hand, the referee gives a good suggestion for potential future works.
In the revised version we updated Fig.6 and included the time line of P_0 and ΔR. We discuss the shape in more detail in lines 193-224. We also discussed potential future works in lines 267-270.

It should be noted that the jet discussed is a rare event. With relatively low velocities and very high densities at the order of 100 [1/cc]. Especially, these high values on the number density are describing a not so average jet. This happens due to the upstream density being very high during that interval. While I don't see any immediate effect on the results, it should potentially be mentioned and discussed very briefly in the manuscript. Jets can have different conditions under different solar wind drivers, and therefore potentially different shapes.

We agree that this is a rare event. In lines 233-234 of the current manuscript, we have already written that this jet event belongs to only a fraction of the jets that show clear signs of the flow pattern. The density of the solar wind upstream is at the upper end of the distribution with 11-12 [1/cc] [see Fig.1 Ma et al., 2020]. Looking at the density in the magnetosheath, we find that the density in the ambient plasma is about four times the upstream density and an 8-12 times higher density is observed in the jet (increase of 100-200% from the magnetosheath to the jet). This is relatively high, but should not affect our conclusion as we used the Plaschke criterion, which uses the solar wind dynamic pressure as a threshold. The velocities are not so low given the fact that we are closer to the magnetopause (X=11R_{E}) and still observe velocities above half the solar wind values (350 km/s) around the jet core.

Other events may differ quantitatively, but we see no reason why it should not work for them. Therefore, the method should also be applicable to "weaker" jets.

In the revised version, we will discuss the jet and its properties in more detail and emphasize that the shape we obtain for this case should not be taken as a general shape. We will put even more emphasis on the methodology than on the explicit result for this jet.
In the revised version we added parts to the description of the jet in lines 92-99 and expanded the discussion in the Summary and Conclusion section in lines 278-282.

I am glad that the authors clarified the "evasive maneuver" part of their work and on previous *studies. It appears, both myself and Referee #1 were confused by the terminology. The jet appears to have a variable velocity, but also variable magnetic field. Could this evasive maneuver primarily the jet being field aligned and following the magnetic field variation? At least THA/THE shows a correlation between the components of B and v.*

We agree that there is some correlation between the components of B and V. Compared to the ambient magnetosheath, the correlation within the jet structure is higher. However, the correlation coefficients for the individual components within the jet interval are not high for THA/THE except for the GSE-Z component (THA: $x = 0.50, y = 0.56, z = 0.80$ and THE: $x = 0.65, y = -0.34, z = 0.92$). In comparison, the correlation coefficients in the ambient plasma are between -0.13 and 0.55.

In our opinion, this is in agreement with Plaschke et al. [2020, 2017] and their description of a tendency for the alignment of B and V in jets. They argue that the reason for this may be the high speed at which jets move through the slower ambient magnetosheath plasma, thereby straightening the magnetic field in their path. And also here we would argue that the plasma flow affects the magnetic field as the plasma beta $\beta(= p_{\mathrm{therm}}/p_{\mathrm{mag}})$ is always above 1 within in the jet interval and even the ambient magnetosheath (median values of β in jet interval for THA and THE are 8.22 and 11.84, respectively; median values in ambient magnetosheath for THA and THE are 15.96 and 37.14, respectively). The mean values are even higher. This hints that the magnetic field is not strong enough to significantly alter the plasma flow.
The discussion about the possible correlation of V and B is treated in lines 95-99.

Lines 231-232: That's something relatively obvious, and the community is (hopefully) aware. Always good to remind, though. Maybe this is also a good point to highlight that to compare observations with simulation this needs to be considered for all mesoscale transients and localized phenomena in every plasma environment.

That is a great comment and we will add it in the revised version.
In the revised version this is done in lines 271-276.

Line 110: "variate not too much" is a bit weird, perhaps rephrase to "do not variate significantly" or something along these lines.

We agree and will change the sentence in the revised version.
In the revised version this is done in line 128.

Lines 237-238: It would be nice to be consistent and reference the mission articles for all the missions mentioned there.

We agree and will add the corresponding references.
In the revised version this is done in lines 285-287.

List of relevant changes

- Changed Abstract
- Added more discussion to Figure 2
- Changed the velocity determination according to Raptis et al. [2022]
- Changed Figures 3,4 and 5 due to new velocities
- Added 2 panels and more discussion to Figure 6
- Added discussion in Summary and Conclusion
- Changed Figures in Appendix A and Appendix B due to new velocities

References

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Ma, X., Nykyri, K., Dimmock, A., and Chu, C. (2020). Statistical study of solar wind, magnetosheath, and magnetotail plasma and field properties: 12+ years of themis observations and mhd simulations. *Journal of Geophysical Research: Space Physics*, 125(10):e2020JA028209.

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