

Response Referee Comment 1 (RC1): Paper EGUSPHERE-2023-2570

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April 12, 2024

Dear Anonymous Referee #1,

Thank you for your time reading and commenting our manuscript “Proton Plasma Asymmetries between the Convective-Electric-Field Hemispheres of Venus’ Dayside Magnetosheath” for publication in *Annales Geophysicae*. Your feedback provided valuable avenues to revise and improve our manuscript. Below we list your comments in italic text, followed by our responses in normal text.

The paper is highly technical and not the easiest to read, probably requiring that the reader read several of the preceding work. This isn’t a red card, this is how science works, but I want to flag that it makes this particular paper quite hard going. This review is written from the point of view of someone familiar with Venus Express and IMA, but not with the previous research in this specific field. Apologies for any misunderstandings, this paper is quite dense.

Based on this and other feedback, we will shorten/exclude parts of Section 4, which will provide space to expand Section 2 and elaborate on the technical background without making the paper longer.

***Q 1.1** The introduction of paper would benefit greatly from a cartoon or simple explanatory figure describing the overall geometry of the Venusian magnetosphere, the different hemispheres, the actual regions being studied. While I am sure this is second nature to the authors, the paper requires a developed 3D mental picture of the induced magnetosphere of Venus and I am concerned will be quite difficult to follow for any reader who does not already have this in their mind.*

Reply: A diagram illustrating the near-Venus environment (regions, boundaries, etc.) and the electromagnetic fields defining the VSE reference frame will be added in the introduction.

***Q 1.2** This is a highly technical paper. The brief description of the novel methodology used to calculate the data products used is quite terse and the paper would stand alone a lot better if it could hold the readers hand through the process. All I got from it was that some form of gaussian distribution is being assumed and some sort of curve fitting is going on? It is not very clear. If so, how have the authors ensured that the field of view obscurations of IMA have not skewed results?*

Reply: We will expand our summary of the methodologies developed in previous papers. Regarding FOV blockage, the use of Gaussian fits instead of moment integrals helps ‘fill out’ the proton VDF whenever its sampling was not perfect (to an extent). This study uses a subset of all IMA measurements which were filtered through a variety of quality metrics to better ensure that poor measurements are excluded from the statistical analysis. The previous papers address this and similar concerns so we will make sure to mention them in our summary of methodology.

***Q 1.3** Regarding Fig 1, I appreciate that the authors have clarified that the ‘error’ bars are actually first and third quartiles with the central square presumably as the median of the distribution. From this, as a first time reader, what I take away is that all the data are mostly the same for all parameters across all VSE latitudes. No asymmetries appear present in the data. The authors claim an asymmetry in velocity, and I do indeed see how it seems to trend to higher velocities at high negative latitudes.*

“The plasma speed appears lower closer to the central parallel”. What is the central parallel? If this is

VSE 00, can you please throw the reader a lifeline and say this? Am I supposed to be looking at both the top and bottom panels, or am I just supposed to be looking at the bottom panels (normalized) with the top as reference? If so, why?

If this is what is being claimed, I have a few concerns with the claim of a trend.

All data points appear to overlap within ‘errors’. How can a trend be statistically claimed? I have exactly the same concern for Fig 6 of Rojas Mata et al. (2023) which this paper relies upon.

In the raw data (panel b) the apparent trend comes mostly from the bottom few data points, beginning at but, as the authors themselves say, “Bins 75° or farther contain much fewer scans (< 25) so these data may have lower statistical reliability.” If I cover these up, then the ‘trend’ is far less clear to my eye.

In the normalized data (panel g), again, covering up the bottom 3 data points where the statistics are dubious, I am also unconvinced that a trend exists. Please perform a rigorous and appropriate statistical test on the binned data to prove that the trends are real and not just a random pattern from poorer sampling that has been biased by some unknown reason. If the authors have doubt about the statistical veracity of data at 75 degrees and greater, then they could consider cutting off their analysis at some threshold latitude that was better supported by the data and where the statistical sampling is significant enough.

The claim “This indicates that the convective electric field has little influence on average magnetosheath properties, especially compared to the bow shock geometry.” Is similarly very strong. An alternative conclusion could be that there’s simply too much noise in the data to extract any meaningful trends. I recommend further statistical analysis to demonstrate statistically that the distributions are the same, and give the certainty to this.

The conclusion that “Both density and speed are slightly higher in the -E hemisphere, whereas the magnetic-field strength is slightly higher in the +E hemisphere” does not appear to be supported by the data. Density appears constant in both hemispheres (Fig 1a,g). Likewise with Magnetic field strength (Fig c,i, especially if one ignores the data points at high latitudes which the authors have flagged as having poor statistics). I thus cannot agree with this conclusion at present.

Reply: We agree that trends with latitude are weak (or non-existent) and the sentence in lines 98-99 is meant to reflect this; we will clarify this point. However, the statements concerning asymmetries are based on Figure 2, not Figure 1, since in line 99 we move on from Figure 1 to discuss the results of the computed asymmetries. The comments about what the data looks like in Figure 1acgi are not applicable to conclusions about asymmetries.

These comments however have provided good opportunity to review how we present our results in the context of the previous asymmetry studies. We will thus reorder the paragraph in lines 97-111 to state more directly that our analysis does not reveal as conclusive asymmetries as previous work has. The reviewer has understandable concerns that this could be due to noise or poor statistics, however the fact that the same data set analyzed with the same methodology revealed clear asymmetries between the quasi-perpendicular and quasi-parallel magnetosheaths (Figures 7 and 8 in Rojas Mata et al. 2023) suggests otherwise. We mention in lines 115-116 that we sought trends in the two hemispheres with respect to upstream O+ Larmor radius and found nothing significant. We also tried this with other parameters (density, speed, Alfvén Mach number, etc.) yet none of the scalar parameters showed significant trends. If characterizing the plasma by which E hemisphere it is in does not reveal significant trends, then this is not a significant controlling parameter (at least by itself) of the magnetosheath plasma state. We have also excluded data in the three highest-latitude bins on both sides when calculating overall asymmetries and found that it has no discernible effect on the result. We will mention this in the manuscript as it is related to the suggestion about cutting off data at a certain latitude.

Q 1.4 *The conclusion that “The y and z components of the bulk velocity and their asymmetries exhibit trends with the upstream O+ Larmor radius.” Is in apparent contradiction to the earlier acknowledgment that “IMA’s limited FOV combined with the spacecraft’s orientation sometimes leads to measurements with*

poor constraints on v_z . Reviewing the measurements to correct v_z is beyond the scope of this work". If the data is suspect, can the authors please explain why conclusions are being drawn upon it?

Reply: Our wording in the manuscript potentially comes off too harsh and arises more concern than it should. It is true that usually the v_z measurements are less well constrained compared to the v_x and v_y measurements, but not to the point of serious concern. The majority of v_z values have the sign we would expect them to have in each hemisphere as reflected by the quartiles marked in Figure 3. This expectation may also be too restrictive and having some positive (negative) v_z measurements in the -E (+E) hemisphere is not unrealistic. Romanelli et al. (2020) only discuss the means of v_z and no other study we have found includes measurements of v_z , so unfortunately we cannot compare to related work for a sanity check on these measurement distributions (this is why in lines 180-183 we advocate for an analogous study at Mars).

We could exclude or flip the sign of the v_z measurements with the ‘wrong’ sign, however this seems like an unjustifiable (or maybe even dishonest) way of manipulating the data. We did check that doing either of these things yields qualitatively similar trends with O+ Larmor radius, however we prefer to report and work with the measurement distributions shown in Figure 3. We will rephrase our critique of the impact of IMA’s FOV and spacecraft orientation so that it does not generate unnecessary concern in the reader.

Q 1.5 The caption of Fig 1 should take the opportunity to clarify once again that these are magnetosheath measurements.

Reply: We will include this reminder.

Q 1.6 How have the authors ensured that data were selected only from the magnetosheath?

Reply: The IMA scans were labeled as being in the solar wind or the magnetosheath by manually determining the location of dayside bow-shock crossing for each orbit. We will make this clear when expanding on methodology as mentioned in Q 1.2.