Reply to Referee 1

We thank the referee for taking the time and effort to evaluate our paper. Some of the points that the referee makes clearly need to be addressed. However, we do not agree that this is "the same paper as Volwerk et al. (2016)". This investigation has been performed on the full VEX data set, i.e. ~9 years, instead of 2 Venus years ~225 days (one during solar min and one during solar max), with newer and more stringent criteria than before. This was done to have the same analysis for Venus as was done for Mars in Paper 1, so in the end both planets can be directly compared. We do not think that is would be a good idea to change this paper directly into a comparison paper of Venus and Mars as that would lead to a very long paper. We are a bit surprised about the comments on some of the figures, that they are similar to those in Volwerk et al. (2016). It would be surprising if they were not, however with the much greater amount of data it was possible to decrease the grid size from 0.25 to 0.1 Venus radii. This shows much more details, which in the overall view might resemble the course figures, but which do give new and better information about the mirror mode structures around Venus. We do agree that some points could be presented better, which we will do in the revised version of this paper. Our replies to the comments of the referee are below.

The major flaw of the introduction is that it totally ignores on mentioning the work that has been already performed by the main author on this subject. That gives a somewhat misleading impression that the work presented in this paper is new and original, instead of putting it in context of earlier work. Comparison of earlier work only appears late in the paper (from line 265.)

The referee is correct, that the older paper should have been mentioned in the introduction, with the explanation why there is now a new, and more extended study of the MMs around Venus. This will be added to introduction.

lines 32, 40, 55, 58, (and also 311, 336, 352): The authors talk about 'temperature asymmetry', while in reality they should be discussing temperature anisotropy.

Indeed, there seems to be sudden change from "anisotropy" to "asymmetry". This is naturally incorrect, it should be "anisotropy".

line 35: a version of the magnetic mirror (MM) mode for multiple species is given (Equation 1). How is this relevant for this paper? What are the species to be used apart from protons? Cold electrons? Oxygen ions?

line 43: Here another form of an instability criterion is used, while discussing ion cyclotron waves. Why is the criterion given is this form? It is actually the same criterion as Eq. 1 for a single species, which is easily verified by a few lines of algebra. What is the relevance of giving this equation?

The general expression for the mirror mode instability criterion is given in the paper. However, it should be explained in the text that in the case that looked at in this paper only the protons are important. This then leads to another form of the instability criterion if some basic math is done, as shown in Eq. (3). This form of the instability criterion is given as it is used in recent papers.

Results:

As mentioned, most results in this paper are simply confirmations of earlier results. Specifically:

We disagree here with the referee, the much longer time that has now been investigated, i.e. the whole VEX mission instead of only 2 Venus years, shows that at least three of the conclusions in the Volwerk et al. (2016) paper, cannot be kept, because of statistics on a larger data set.

Figure 2: Here even the same example as in [Volwerk et al., 2008a, 2008b, 2016] is used. Some new data would be in place here.

True, the examples are the same, in order to show the difference in the more elaborate identification method that is being used now. This could have been shown with only one example, and new data should have been shown. One figure will be taken out and another figure, of MMs measured near the magnetic pile-up boundary will be included in the paper.

Figures 6-8: These are very similar to Figure 4 of [Volwerk et al., 2008b], and Figure 3 of [Volwerk et al., 2016].

They are similar but not the same, as here the full data set of the Venus Express mission is used and the grid size on which the statistics has been done has been reduced from 0.25 Rv to 0.1 Rv.

Figure 11: Simply a reproduction of [Volwerk et al; 2016] (as the authors mention themselves.)

True, this figure has been added as help to the reader. It needs not be in the paper, per se.

Figure 12: Very similar to Figure 4 of [Volwerk et al; 2016].

Naturally, this figure will look similar to the one in Volwerk et al. (2016). However, because of the larger dataset, it is now clear that there is not such a big spread between solar minimum and maximum, the asterisks and triangles basically lay on top of each other. Also, now there are three well defined slopes for the distribution of the number of events per depth of the events.

Figure 13: There is not much additional information as compared to Figures 5 and 6 of [Volwerk et al., 2016].

The referee must have a different view on the figures than we have. The perceived decrease (sol min) and increase (sol max) of the depth behind the bow shock is no longer apparent, nor that there is a difference between both solar activities.

line 248-249: The conclusions here are the same as in the earlier work.

True, but it needs to be stated here anyway that the much larger dataset statistically shows the same behaviour as in the older paper.

General issues. These are just some examples of the paper not having been carefully prepared. I stopped taking notes at some point.

line 46: ‘B strength’. Too informal.

Will be changed in “slow changes in the magnitude of B”

line 47: ‘conservation of first adiabatic invariant’. Grammar.

“the” will be added

line 47: ‘The first process…’ This sentence discusses on of the processes responsible for creating a temperature anisotropy. Then the discussion ends abruptly with no further discussion of the other processes that are mentioned. Is is unclear what the authors want to say.

There was a small paragraph missing here.

The first process will occur mainly in the solar wind interaction with the planetary exosphere (with the exception of Jupiter’s magnetosphere and the Galilean moons, where the Jovian corotating magnetic field and magnetospheric plasma is taking the role of the solar wind) in the low-beta plasma case and generation of ion cyclotron waves will take place (Delva et al., 2008, 2009, 2011, 2015; Schmid et al., 2021). After crossing the quasi-perpendicular bow shock the anisotropy is increased, as is the plasma-beta and the MM instability will take over. The second process will occur mainly near the magnetic pile-up boundary where the magnetic field gets compressed and slowly increases in strength whilst getting closer to Venus.

line 72: ‘the dats’ ?

Typo: the data

line 86: ‘the data B’ What is meant by this?

The magnetic field data, B, are low-pass …

line 97: ‘to which the reader is referred to’ Grammar.

The second “to” is deleted.

line 101: RV not defined.

Omission: added “(Venus Radius, 1 R_V = 6051.8 km)”

line 114: ‘refsec:detection’ ?

LaTeX “\” missing.

line 137: What is the CSW method?

The “CSW method” abbreviation should have been mentioned in section 2.2. The two subsections 2.2.1 and 2.2.2 together build the CSW method, which is explained in more detail in Paper 1.

line 265: ‘Comparison with Volwerk et al.’ Not correct reference format.

The year has been added to both sections 4.1 and 4.2

line 278: ‘The probability for both solar conditions is the same for solar minimum and maximum’ Unintelligible sentence.

This is indeed doubled and should read “The probability is the same for solar minimum and maximum”