

Review of “Revisiting Mirror Modes in the Plasma Environment of Comet 67P/Churyumov-Gerasimenko” by Fallau, Goetz, Simon Wedlund, Volwerk and Moeslinger

Referee #1

This is an interesting paper but it is unconvincing that it is correctly identifying solitary mirror mode waves. If one looks at Figure 1 and the identification of the mirror modes, the readership will ask what are all the other waves which have not been identified as mirror modes? They look pretty much the same as the events that you have identified. Is it just by chance that sometimes the angular changes fit their conditions that the authors have set for mirror modes and sometimes they do not? I realize that the authors are claiming that these other waves are evolutionary structures of mirror modes, but there has not been any past observations/evidence of this. This is just pure speculation. Maybe this is an entirely different wave mode, or possible this is a coupling of mirror modes with another wave mode? Or could there be two instabilities occurring, say a mirror mode instability and an ion cyclotron wave instability? The authors should discuss this in some detail. Give balance to the paper so that the readership will not be misled.

– We thank the reviewer for their input and have made changes to address the comments. The changes are highlighted in the attached file. There seems to be some misunderstanding of the main points of the paper, so we have reformulated to hopefully avoid any confusion.

The core of the method that we use is well known and has been used many times before to identify mirror modes. Of course the choice of parameters influences the method outcome, so we have either chosen values that other publications have used (for compatibility) or explained why an adjustment was necessary. This results in a catalog of mirror mode wave candidates that is the basis of a statistical study. This is an established method of investigation. We have discussed in the text why some events that could be mirror modes are not identified. Nowhere in the draft do we intend to claim that we present evidence of mirror modes developing out of/into magnetic holes. We merely point out their similarity and that there is a theory that they are related. We have reformulated the text to make this clearer to the reader. No ion cyclotron waves have been reported in the cometary environment of comet 67P and the aim of this paper is not to search for them.

In light of this I suggest that the authors put in the word “possible” in the title before “Mirror Modes” and in line 3 of the abstract. Future work on this by other people might be able to resolve what is creating these perplexing waves.

– The method that was used in this paper has been used many times to identify mirror mode structures. Mirror modes have been reported at comets. While the interpretation of the generation of the waves is certainly difficult, it is not warranted to discount the characterisation of the identified events as only “possible” mirror mode waves. In Sect. 2

we emphasize which waves other than mirror modes could be detected by the original B-field-only detection scheme and present several additions and ways of mitigating this (B-n anticorrelation, wave-like morphology): all of those observational additions, paired with theoretical and modelling results, are strongly compatible with the actual presence of mirror modes. We therefore opt to retain the title as it is, but changed the abstract's 3rd line to "mirror mode-like" to emphasize that, because the ion/electron temperature information was impossible to derive with accuracy, the origin of the waves we see is difficult to ascertain, as already stated at the end of Sect. 2.

In reading through the paper, it appears as if this is a reporting of the authors' efforts in research and not a streamlined scientific paper. In the abstract, sentence line 8 -9, you mention a magnetic field only method of detecting mirror modes and later in the body of the text, you discard it for a magnetic field and plasma density method.

– The magnetic-field only method was used as the first step of the multi-step identification process. It was not discarded, but added to. We have clarified this in the text.

It would be best if you delete this sentence in the abstract and in the body of the text and get to the main point of the results. In the sentence on lines 9-10, I think you should correctly state that the 565 events were detected by the standard technique of identifying mirror mode events (anticorrelated magnetic fields and plasma densities)? This is not a new idea and should be the starting point of your discussions.

– Previous studies just used the magnetic field only method to identify mirror modes in planetary or cometary environments. One of the realizations of this study is that this method is not enough to correctly identify mirror modes at comet 67P as stated in the text. As we detail the selection method, we also reduce the number of events, for repeatability and traceability we have added the number of events at each step.

Abstract, sentence lines 12-13, you mention that you have detected only one mirror mode train. This is interesting and should be expanded upon in the body of the text. It should be noted that for mirror modes in planetary magnetosheaths, the waves are always in trains, not single events separated by other types of waves. It should also be noted that previously observed mirror modes at comets were trains, but of very short duration. Some discussion of this needs to be added. References to mirror modes at comets are: GRL, 14, 644, 1987 doi:10.1016/S0237-1177(97)004730; JGR 98, 20955, 1993; NPG, 6, 229, 1999 doi:10.5194/npg-6-229-1999. These references should be added to the paper. Previous reports of mirror modes in planetary sheaths and at comets (and elsewhere) have all reported trains, not single events as shown here. This should be discussed in the body of the text.

– The lack of mirror mode trains is indeed interesting. For the abstract we have retained the text as is, but we have added a more detailed discussion in the main body of the text. The requested references were also added in the text.

Detailed Comments

Introduction line 27. I suggest the deletion of the Ip reference and the replacement by Wu and Davidson. Ip was just a copy of the original. They did nothing new.

– Done

Lines 27-28, references should be given for the different wave modes mentioned. For magnetosonic waves, the original discovery was GRL, 13, 3, 259, 1986. The most recent work on this is the Ostaszewski et al article that you quote. Both should be mentioned here.

–We have added the suggested reference (Tsurutani & Smith) as well as Tsurutani et al 1987 . We also included mention and references to lower hybrid waves, ion cyclotron waves, and singing comet waves.

The term “Alfven wave” should be deleted here. It is realized that theoretically Alfven waves are the low frequency analog to ion cyclotron waves, but detailed studies of Alfven waves indicate that they are not left hand polarized, but are arc polarized spherical waves. Please see JGRSP 123, <https://doi.org/10.1002/2017JA024203> for details. Nothing like these waves were detected at comets, thus the suggested deletion. The readership will be confused.

–Done

For ion cyclotron waves, a good reference is JGR, 98, 20921, 1993. You might wish to add other wave modes that have been detected at comets (and references).<https://doi.org/10.1029/93JA02583>.

– Done

Line 32. I suggest the deletion of the term “magnetic holes”. There are many people that do not believe that magnetic holes are mirror modes. Please note the references in Tsurutani et al., 2011 which indicates many references to mechanisms for magnetic hole generation which are not temperature or pressure anisotropy generated. If you discuss magnetic holes here or elsewhere, these references should be quoted as alternative explanations.

–We have deleted the mention of magnetic holes in this context and discuss the relation between magnetic holes and mirror mode waves in more detail later on.

There has been a problem with the analysis of “linear magnetic holes” as well. It is the same problem as discussed with your waves. Singular events were selected out of a string of events that did not fit linear holes. What are the rest of the events? Are they due to some other mechanism? The idea that they are the evolution of mirror modes is not convincing. I don’t believe anyone has shown a case where mirror modes have only partially evolved. Again this is pure conjecture that these are due to the evolution of mirror mode waves. Others have suggested mechanisms for magnetic hole formation. These should be quoted and discussed.

–We have added the references and discuss this in more detail.

Line 33. The original theoretical paper and observational paper on mirror modes and proton anisotropies should be quoted here instead of what you have. They are Phys Fluids, 12, 2642, 1969 and JGR, 87, A8, 6060, 1982.

–Done

Line 49. Remove “Alfven wave” here. See previous comment.

–Done

Line 51. Remove “Alfven” in front of ion cyclotron waves. See above comment. A couple of references on ion cyclotron waves should be added here: Remya et al. JGR, 118, 785, 2013 doi:10.1002/jgra.50091 and Remya et al. ApJ, 793:6, 2014 doi:10.1088/0004-637X/793/1/6. They are follow- ons to the Gary 1992 paper and give further information for the instability and the waves.

–We have added the publications by Remya et al. Ion cyclotron waves are still mentioned as the low beta mode.

Line 51, sentence beginning with “Moreover...”. This sentence should be reworded. This was a result from theory but have never been verified experimentally. And a “minute” amount of heavy ions would not have much of an effect. Almost nothing depending on the “minuteness”.

–We have rephrased the sentence.

Line 52. Remove “Alfven”

–Done

Line 60. A reference to field-line draping should be added here. I recommend the original paper: JGR 68, 5111, 1963.

–Added

Line 71. Does the “two different sizes of mirror mode waves” still hold after you found that the magnetic field only technique produced errors? If not, then this sentence should be removed.

– The magnetic field only technique does not produce errors, as stated later on in the manuscript, the dataset has been improved since the original study which results in different identifications of mirror mode candidates in this study. Therefore this paper cannot make any statements about the size of the mirror modes found in the original study. As this is simply a statement of what was found previously, we do not see a need to remove it.

Lines 80-89. Are all of your possible mirror modes pressure balance structures or can you not say? This should be discussed in the paper. On line 89 besides the Hasegawa (1969) reference, you should add Price et al. 1986 and Tsurutani et al. 1982.

–In this section we summarize the Volwerk 2016 study, the paragraphs are not related to this study. As our events are constrained to intervals where high resolution plasma density is available, we only include events that are pressure balanced structures.

Lines 90-96, linear magnetic holes. A comment concerning this was mentioned before. Although this conjecture is possible, for balance some statement indicating that they are possibly not mirror mode structures should be made. The many possible mechanisms for their generation should be quoted.

–See answer above.

Lines 96-99. This conclusion is too strong. These structures may not be mirror modes at all and may be generated by processes not involving anisotropic protons. A statement should be added stating this.

– With the additions to the discussions on the origins of mirror modes and bearing in mind that this section simply refers to the results of other publications, we think this wording is warranted. It is not a conclusion at all, simply a summary of what others have found.

Lines 144-146. It seems unnecessary to describe your selection process as a two step procedure. Why don't you cut to the chase and state your final selection criteria?

–The selection method is a multi-step procedure and we describe it as such.

Table 1. Your criteria for detection of your events are clear. But later in the paper you mention that some of the selected events do not have anticorrelations between B and N. This seems contradictory. Can you please clean this up? Throw out events that don't have clear anticorrelations?

–Our final event list does not contain any events without a clear anticorrelation.

Lines 171-176. Is all this detail necessary to tell the readership of AG? I think it is important to tell the readership that the “magnetic field only technique” does not work for identifying possible mirror mode waves at your specific comet, but I think this could be done in one sentence. When you mention different wave types earlier in the paper, you should add “steepened waves” (plus reference) and “singing comet waves” (and references).

– We have added references as requested. The detail in the description is necessary to inform the reader who may want to do a study similar to this one, that the magnetic field only method misidentifies a well known wave mode as a mirror mode because it disregards the pressure balance nature of the waves. This is why we inform the reader of the problem and the solution (add anti-correlation)

Line 181. Telling the reader that the Volwerk et al. (2016a) results are now known to be incorrect will be useful to the readership, but perhaps state this more succinctly?

–We have reformulated this section.

Line 185. Why don't you start the true data analysis section with the discussion of the Wedlund et al. (2022) method which works? Why discuss methods which don't work? And by the way this is not a new method but was discussed by Hasegawa 1969; Tsurutani et al. 1982 and Price et al. 1986. Plus of course the anisotropic protons and pressure balance, both topics which you have not discussed/shown. This latter point should be mentioned prominently. Since you were not able to do these types of analyses, your method only partially identifies mirror mode structures.

–We are of course aware that the usage of the magnetic field only method and the anti-correlation is not new. The text does not claim this. However, some additions were made to accommodate for the complexity of the cometary environment (peak identification method). We have made an attempt to reformulate this description slightly to better convey the points.

Line 205. What does the presence of magnetic holes have to do with the generation of mirror modes? Please explain or delete. Also note previous comments that many people do not believe that magnetic holes are the remnants of mirror modes. So this point should also be made if you wish to revise this sentence.

– This section is indeed misplaced. We simply meant to point out that temperature data is not available so we could not do any checks of the plasma beta or the anisotropy. We have therefore removed this paragraph and added a sentence on the unavailability of the plasma temperature earlier in the section.

Figure 1/lines 223-226. Please shade the intervals that you believe are mirror modes for the readership. I don't see where your criteria are being met.

–We have added shading to Figure 1 and 2.

Lines 227-233. Since these “events” do not meet your criteria, why don't you delete the vertical lines and delete these sentences?

–I think there was a misunderstanding here. All of the four intervals meet all criteria. Only the wave structures between them do not. We have shaded the four events and reformulated the text to make this clearer.

Lines 277-280. This is a highly biased statement. Another possibility is that magnetic holes have nothing to do with mirror modes. As previously mentioned there have been many proposed mechanisms for magnetic holes, which you should include in your revision. Please revise the discussion so it is more even handed for the readership of AG.

–We have reformulated this. We simply meant to point out that it has been shown before that plasma structures formed upstream in the solar wind or cometosheath can convect into the inner coma. As they do so, they are modified. This example holds whether or not magnetic holes and mirror modes are related.

Figure 3. I think this is perhaps the most interesting figure of the paper. It will help others who wish to tackle this wave problem to realize that this may have nothing to do with mirror mode generation.

–We are pleased that the referee finds this figure interesting, but we do not understand what the remainder of the comment is referring to.

Lines 328-342 discussion. The authors should mention that the previous studies of the Saturnian magnetosheath have shown no evolution of mirror modes into magnetic holes. Thus the comment by Plaschke et al 2018 is conjecture, not observations.

–As we think is clear in this paragraph, we do not talk about magnetic holes at Saturn. Later, we make a tentative comparison of a mirror mode deformation on its path downstream with the solar wind flow with the evolutionary morphology of a MH crossing the cometary interaction region as described by Plaschke+ 2018 (see their Fig. 5, which is a simplified sketch and example of possible deformation of a large-scale magnetic hole). We added the reference to Fig. 5 of Plaschke to help the reader understand what specific aspect we are referring to here. As discussed by Plaschke+ 2018, expected differences between a Jupiter/Saturn/Earth-like environment and cometary/Mercury-like environments has to do with scales of mirror mode-like structures as compared to the size of the interaction region (typically, the bow shock).