

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

Referee #1

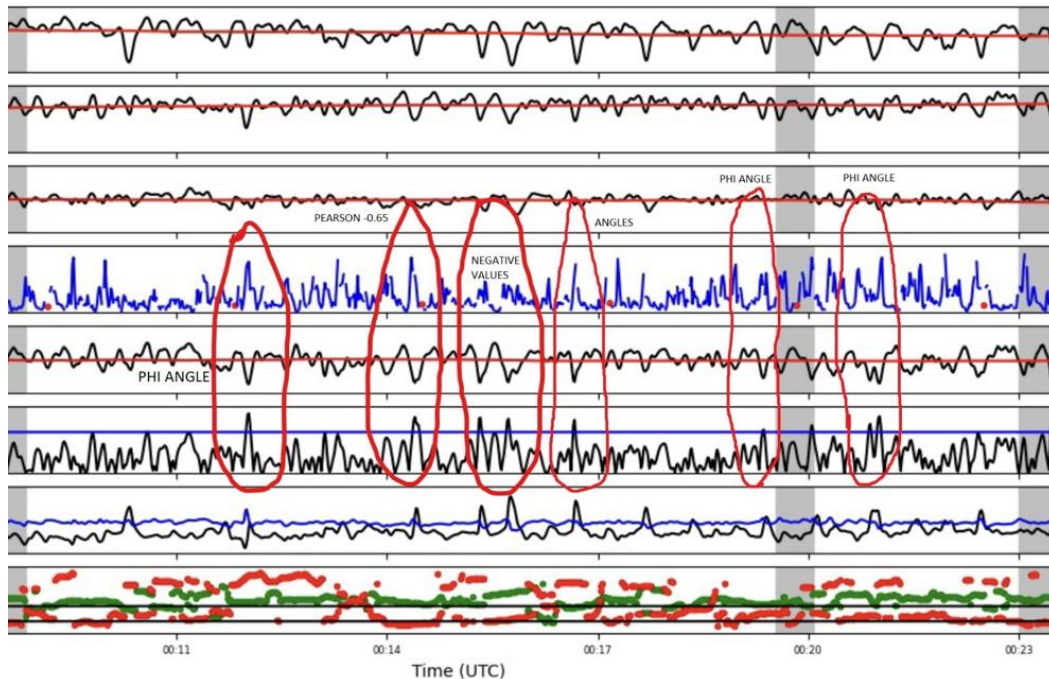
Referee second comment: I am dissatisfied with the responses of the authors in this section of the correspondence. I will give some further arguments below.

Referee first comment. 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.

Authors first response- 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 which 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.

Second referee comment. The changes that you have made are insufficient. Every paper should be a stand alone work. Just because other papers have come up with similar conclusions doesn't mean that the present work is correct. All works are possibly in error. As one example when there were many potential errors in the literature concerning magnetic holes, an editor of JGR asked for a review paper straightening out the differences between magnetic holes and mirror mode waves. The published paper is JGR, 116, A02103, 2011. Doi:10.1029/2010JA015913. It seems to me that the only difference between the previous magnetic hole issue and this paper is that you are selecting single event “mirror modes” among other structures. What I am asking for is for you to explain to the readership what you think these other structures are which are not mirror modes.

As originally suggested, please give your opinion (to the readership) your opinion of what the other waves may or may not be. I do not believe you addressed this issue at all.



Second Author comment:

We shall try to address the referees concerns one by one below:

Solitary mirror modes are not a criterion, the method identifies individual mirror modes but it is not restricted to isolated events as is shown in Figure 1. Nowhere within the method do we require that these structures are solitary, this is something that results from the method.

As we mentioned in a previous response the selection method was defined as it is, in order to retain unambiguous mirror mode events, in the paper we mentioned that the rest of the events in Figure 1 could be also possible mirror modes but because of our stringent criteria, they were not identified by the method. As stated in the Tsurutani et al 2011 paper, the $\Delta B/B$ criteria are chosen somewhat arbitrarily. Our choice of criterion is based on what other authors have successfully used to identify MMs. We speculate that if we chose a slightly less restrictive criterion the structures within this wavetrain may also be identified as mirror mode-like. Specifically in the above figure we have marked the specific reason why those events were not identified as mirror modes by the algorithm. We included in the draft the following sentence "For example, the events closer to 00:12 and 00:17 UT were not identified as a mirror mode since they did not satisfy the theta and phi angles criterion."

Referees first comment. 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.

First reply of authors- 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.

Referee second comment. These structures that you are presenting as mirror modes are not like the continuous string of mirror modes that have been observed in planetary magnetosheaths and were discussed in 2011 JGR paper mentioned previously. Yes, mirror modes have previously been reported to occur in other places such as at comets and in interplanetary space. Those events were noted to be of continuous cycles of mirror mode structures.

I repeat, the word "possible" should be added to the title. I think this is a generous amendment.

On the non-detection of mirror mode trains:

It is not because we detect only a single structure at a time that these are not part of a train of mirror modes. Detection is not reality, it is only a tool. We do not want to include too many structures at the expense of having a lot of false positives, hence the strong criteria we chose.

The referee implies that if the MMs that we found are not trains then they are not mirror modes. This is clearly against previous studies, see e.g. Pokhotelov et al 2008 and Volwerk et al 2016.

Very often, mirror mode trains are identified using the magnetic field only method by single events and then the entire interval is classified as a mirror mode train (e.g. Simon Wedlund et al 2023). Our results are similar. MM trains are seldom captured in one fell swoop by any automatic algorithms. We are actually more stringent here, and do not claim that the events in figure 1 are all part of a mirror mode train.

Other papers that only use the magnetic field only method (which is inferior to our method here) also claim the name mirror mode in the title without any modifiers. We do not see how this case would be different (Erdos 1996, Lucek 1999, Volwerk2016, Volwerk2021).

Referee first comment. 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.

Authors first reply- 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.

Referee first comment. 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.

Authors first reply. 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.

Referee second comment. I disagree with this statement. The method to identify mirror mode structures was published in JGR, 87, A8, 6060-6072, 1982. It was pressure balance structures with little or no magnetic field deviations. The magnetic field alone technique was proposed as a shorter analytical method, which you now find does not work. You are going back to the original method except not even fulfilling all of the diagnostic techniques. Please give a correct and much shortened description of your criteria. For example you could say that the original 1982 technique

was use but because you do not have pressure balance information you have forgone that information? That basically covers your final analytical method.

Second author response: We reiterate that the magnetic field only method may work well in other environments but does not work in the cometary environment.

Clearly a number of methods have been proposed to identify mirror modes. All of them have drawbacks. The method we have chosen (the entire process, not just the magnetic field only step) is based on the methods recently used at comet 67P and then refined to take into account the specifics of the cometary environment and the availability of data products. In no way do we claim that the method is perfect; indeed we do the opposite and clearly state the assumptions and limitations of the method.

Quite frankly, we do not understand the referees argument here as the method they suggest we look at is one that is also flawed, and not possible to use given that we have no temperature or plasma pressure measurements at the comet and only one spacecraft. We have included a very thorough description of the methodology so as to ensure proper scientific method through reproducibility.

Topic editor's comments

Regarding specific comments:

Referee 1 ---"As originally suggested, please give your opinion (to the readership) your opinion of what the other waves may or may not be. I do not believe you addressed this issue at all."

---I believe this to be a reasonable request since the text around line 240 (for example, but not limited to) does raise a number of questions, and probably should be placed on a more scientifically solid footing. e.g. the sentence "We suspect ... but could still be ... " does suggest things that you probably don't want it to suggest?

("Although only the four marked intervals were identified as unambiguous mirror mode structures, there are other signatures 240 in the magnetic field, especially between 00:14 and 00:17 UT, that resemble the other mirror mode events. We suspect that these do not fulfil our stringent criteria but could still be mirror modes, following similar remarks made in Simon Wedlund et al. (2022). This is the only interval in our database where such a train of mirror modes was observed.")

Referee 1 ---"I repeat, the word "possible" should be added to the title. I think this is a generous amendment."

---I am inclined to sympathise with the referee here. If the authors have decided to change the text in the abstract to "mirror mode-like" in then a similar qualifier should also be in the title? I also note that discussions regarding mirror-mode and mirror-mode-like appear in other sections and suggest different levels of ambiguity at different points. It seems reasonable to modify the title in some way that acknowledges the variety of wave modes discussed in this paper, and the detailed discussions. And I don't think this is a weakness, rather it is a strength.

Referee 1 ---"I disagree with this statement. The method to identify mirror mode structures was published in JGR, 87, A8, 6060-6072, 1982. It was pressure balance structures with little or no magnetic field deviations. The magnetic field alone technique was proposed as a shorter analytical method, which you now find does not work. You are going back to the original method except not even fulfilling all of the diagnostic techniques. Please give a correct and much shortened description of your criteria. For example you could say that the original 1982 technique was use but because you do not have pressure balance information you have forgone that information? That basically covers your final analytical method."

--- I should imagine you can find a way to accommodate this request relatively easily, one way or another?

Of course, please do offer a respectful rebuttal if you have strong views in the other direction(s).

Unfortunately, we strongly disagree with the points made by the referee. We have outlined our concerns in detail above. Some of the referees statements in this round clearly ignored additions made to the paper in the last round, so we have reiterated those.

We also have trouble understanding why the referee insists on changing the title, when many papers have been published, using less stringent methods, and made no mention of the word "possible" in the title. In the text we state clearly that, as with any method, we cannot be 100% sure that all the structures we find are mirror modes, but we do not know of any other structures that fulfill all of these criteria (Wedlund et al. 2023). This is why the criteria were chosen to be quite stringent, so as to reduce the number of false positives.

The method described by Tsurutani et al 1982 cannot be applied to the data from Rosetta at the comet. Other well-documented and discussed methods are feasible and we have chosen to use them. Does the referee imply that only the original 1982 method is correct and all publications since then have been wrong? While the referee may believe this, many other authors, including the second referee would disagree.

If we shorten the description of our method, our results would not be reproducible and we believe that this is not in line with a proper scientific paper.