Review of Wedlund+, Statistical distribution of mirror mode-like structures in the magnetosheaths of unmagnetized planets: 1. Mars as observed by the MAVEN spacecraft

The manuscript focuses on the identification of mirror mode like waves in the magnetosheath of Mars, using in-situ observations made by NASAs MAVEN spacecraft. Mirror mode waves are common in planetary magnetospheres and are generated by temperature anisotropies in the plasma; they thus provide insight into the physical processes active there. The manuscript provides a good summary of this background information and the motivation for the presented work.

The manuscript describes an automated detection algorithm to identify mirror mode like waves in the magnetosheath of Mars, via in-situ magnetic field measurements that are made by MAVEN. The algorithm is initially trained using supplementary ion observations, but only utilizes magnetic field observations. This enables the results to be compared to other spacecraft missions, on which magnetometers are commonly carried. This intentional design of the algorithm is a well thought out strength, which should result in new and interesting comparisons being made in the future (there is a companion manuscript to this one, investigating results at Venus, for example). This manuscript presents the first set of statistical results obtained via the application of this algorithm to the MAVEN magnetometer dataset at Mars.

The method is well described and the manuscript presents a thorough discussion of the results that includes appropriate comparison to previous related works. The authors find that mirror mode like waves are most commonly observed just behind the shock, and close to the induced magnetosphere boundary, in agreement with theory and previous works. One interesting discovery is that the occurrence rate of mirror mode like waves is lower during higher solar EUV conditions, contrary to initial expectations. The proposed explanation for this seems reasonable.

The manuscript is well written with very few typos, and the figures are of high quality. I have some minor comments and questions noted below, but these should not affect the overall conclusions drawn from this work, and I expect that this manuscript will be suitable for publication once addressed.

Minor questions:

Criterion 6 in the automated algorithm restricts event detection to the magnetosheath. The manuscript provides justification for this, and this question is more of a curiosity. Do you know how this “pre-selecting” affects your results? Clearly, you are unlikely to identify mirror mode like structures outside of the magnetosheath, but do you have an idea of how many “real” events you might be removing, that lie outside of the magnetosheath? Given the instability criterion noted in the manuscript, is the magnetosheath where you most expect mirror mode waves to be observed anyway?

Line 173: the algorithm only accepts magnetic field structures that rotate by less than 10-20 degrees. Table 1 notes that this value is 10 degrees – so is this the value actually used? In addition, will this criterion still allow fast magnetosonic waves to be included in the identification (I think criteria 2-4 remove fast magnetosonic wave modes, which are typically circularly polarized)? The manuscript notes that variations in plasma density and magnetic field strength are anti-correlated for mirror mode waves (in contrast to correlated for fast magnetosonic waves) – but this characteristics cannot be taken advantage of when using solely magnetometer measurements.

Line 180: the manuscript rejects isolated single events – do you require a certain number of oscillations to occur one after the other (a wave train), for example? I think lines 180-185 describe this process, but it's not quite clear to me how this is achieved. Can you explain this section or rephrase it?

Lines 185-190: when you calculate the standard deviation of the change in magnetic azimuth and elevation angles, do you calculate this based on the full cadence (32 Hz) magnetic field dataset, or a filtered version of this? Essentially – do you calculate the standard deviation of the underlying average background field, so that you ignore any smaller scale, higher frequency rotations / features that may also be present, on top of the lower frequency mirror mode like structures?

Line 224: In Figure 1 (and perhaps 2), there are some green lines that lie over data where the values of delta(B)/B and delta(N)/N are both positive (or both negative), which I believe means these quantities vary in phase (as opposed to anti-phase). Can you comment on this and why these events are still classified as mirror mode like? There's reference to this at line 229, but I'm curious why these events are still included, if these quantities are varying in phase?

Lines 260: This is mostly a question of curiosity: if at Earth most mirror mode like waves are elliptically polarized, why does this study focus on linearly polarized ones?

Lines 314-315: typo: the second square parentheses in each pair need reversing direction. Which side of the dates are the inclusive ones here? For example, MY32 = [31 Jan 2013 – 18 Jun 2015], MY33 = [18 Jun 2015 – 05 May 2017]. Does 18 June 2015 fall in MY32 or MY33?

(Line 350: I look forward to reading Wedlund+ 2026, once MAVEN has sampled more MYs.)

Line 393: Discussing possible reasons why the detection rates are smaller in the presented study than in Ruhunusiri + 2015. You mention that your data set covers a much longer time span than Ruhunusiri+2015. Could you do a quick chop of your dataset and plot results for just the time range covered by them? This isn't necessary, but it would be interesting to see if the results are consistent with this hypothesis.

Line 393: "trains of short events" – I'm not sure this has been explained fully, and refers back to my comment about line 180 above. In addressing that, you will probably address this.

Line 396: the manuscript mentions that when false positives are removed, the automated algorithm may also remove some number of real events. If you skip this step (and keep all data), what do the results look like – are the detection rates consistent with the earlier studies, and the differences can indeed be explained by the proposed reasons? This again is not necessary (if difficult to do for example), but would be a first step to test this hypothesis.

Line 399: Does the pre-selection of events in the magnetosheath play a role here? Did Ruhunusiri+2015 search for events throughout the entire magnetosphere, with no limits on spatial location?

Line 422: Is the 68s of structures detected per day averaged over the entire MAVEN orbit, or just when MAVEN is sampling the magnetosheath? Can you comment on how only allowing events to be counted in the magnetosheath may skew this number, based on how you normalize it "per orbit"? For example, if MAVEN spends 20 minutes of every 4.5 hour orbit sampling the magnetosheath, it can only ever detect mirror mode like events for 20 minutes per orbit at most, given the current constraint that events are only counted when MAVEN is in the magnetosheath.

Around line 425: when discussing occurrence rates as a function of MY, did MAVEN sample the spatial extent of the magnetosheath evenly and equally in each MY? For example, if MAVEN sampled just the nose of the magnetosheath in one MY, and then just the tail magnetosheath in another, you would likely obtain different occurrence rates. I imagine it might be difficult to fully address given that you can only slice and dice the data so many ways while retaining good statistics, but can you comment on this at least?

Line 553 and Figure 10: bottom panel in particular: by eye I would have placed the orange fit further to the right. It seems like the extreme values on the left (large and negative) are skewing the fit to the left. If I integrate the black area by eye, there is significantly greater area to the right of the vertical dashed orange line, than to the left. Is this consistent with the negative reported skewness, which seems to be calculated in a way such that extreme outliers can bias it heavily?

Figure 3, and all figures with statistical results: do you require a minimum number of data points to lie within each grid cell, to be displayed?

Figure 7b: Should the red and blue lines overlap at all? The definition appears to split EUV conditions greater or less than 2.77mWm^-2, so I don't believe there should be any overlap?

Figures 8, 9, 11: for the right hand panels that show % difference, with values ranging from negative through zero to positive, I suggest using a colorbar where, for example, negative numbers are darker shades of blue, zero is white, and more positive numbers are red. This would allow the reader to more easily identify the different regions in those plots.