Review:

The authors have mostly answered correctly to the numerous questions I raised.

In particular, they address much better the issues in the particle data used from MMS (with for instance the spin tone of 3 RPM not removed which was obvious in the Figures). I fully understand that they cannot correct these parameters by themselves and have to use what is provided with the identified caveats. However it was mandatory to well explain this limitations to any reader who should have been quite 'shocked' by the strong discrepancy between electron and ion density. A better job could have been done (but this clearly beyond the scope of the present study) if correct cross-calibrated particle data could have been systematically determined (maybe with the help of the instrument team) by using the numerous missions in the solar wind around the Earth like Wind as is mentioned for instance or ACE.

After saying that, I still have some concern about the quality of the determination of the mirror mode instability criterion (Rsk) which is used in the paper. This parameter obviously depends both on the ion density and temperature which are said to be both affected by the mentioned instrumental effects. Maybe the underestimation of the density is compensated by the overestimation of the temperature in the computation of the beta parameter and the temperature anisotropy is correctly determined, but it is mandatory that the authors add a small sentence about this and keep some caution about the determination of the Rsk parameter. It should be mentioned when dealing with Table 2 and Figure 15. This will not in general change their conclusions and prevent them from publishing their analysis.

So the new version of the manuscript is now suitable for publication in Annales Geophysicae provided that my last comments and suggestions are taken into account.

Typo: line 129: 'only changes a little'.