Annales Geophysicae Discussions

Ann. Geophys. Discuss., referee comment RC2
https://doi.org/10.5194/angeo-2022-2-RC2, 2022

Comment on angeo-2022-2

Anonymous Referee #2

Referee comment on "Fine structure and motion of the bow shock and particle energisation mechanisms inferred from Magnetospheric Multiscale (MMS) observations" by Krzysztof Stasiewicz and Zbigniew Kłos, Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2022-2-RC2, 2022

The manuscript deals with observations by the MMS spacecraft from Earth's quasi-perpendicular bow shock. The paper covers a very wide range of topics like: the fine structure of the shock, oscillatory shock motion, and both ion and electron heating/energization.

The paper builds mainly on work done by the same author(s), which does not give the impression that this work is of very wide interest. Furthermore, to my assessment, much of the reasoning and many of the conclusions are poorly supported or even wrong. I therefore can't recommend this paper to be published in ANGEO and instead recommend to reject the paper in its present form. Please see more detailed comments below.

I don't think any revisions to this manuscript can sway me to recommend publication. I can see that the other reviewer has a very different opinion to my own. Perhaps a third reviewer can brought in to asses this paper?

- Lines: 86-87 "The decomposition suggests that the oscillatory behaviour of the shock and wave steepening process are related to the ☐1 mHz wave at the bottom, which triggers cascades of compressional waves extending to 1 Hz and above"

It is not clear to me what the authors mean with this statement. The 1 mHz wave in the bottom of Figure 1d is the result of the spacecraft crossing the bow shock several times and filtering the time series. The peaks are the magnetosheath intervals and the dips are the solar wind intervals. So this is not a wave at all. The higher-frequency waves appear when the spacecraft are near the shock ramp/foot.

If the authors wish to argue that a 1 mHz compressional wave modulates the position of the bow shock, they would have to identify this wave in the upstream solar wind where the measurement is not affected by observing the compressed magnetosheath.

- Lines 141-149:
The authors claim that the well-described gyrating ion beam observed in the foot of quasi-perpendicular shocks is not due to ion reflection. Instead, they claim that these are accelerated by lower hybrid waves near the shock.

The idea that supercritical collisionless shock waves reflect a portion of the incoming ions is fundamental to how energy is understood to be dissipated at shocks, see (e.g. Kennel 1987). Any alternative theory to ion reflection needs to address this central question to collisionless shock physics.

The authors show that a proton can be accelerated by waves but it is not clear to me how these calculations correspond to the observations or if they are able to quantitatively reproduce the observed ion distributions. I think the lower hybrid wave model is unlikely since shock reflected ions are also observed in hybrid simulations where these waves are not resolved (e.g. Leroy+, 1983; Lowe+, 2003; Hellinger+ 2007, Caprioli+, 2015).

I do not understand the authors' claim that the solar wind and reflected ions "are in the same electric field so they should have the same V_perp". In the solar wind frame (where the electric field vanishes), the reflected ions gyrate around the center of mass. This leads to perpendicular acceleration of the reflected ions in the shock frame (but not of the solar wind ions).

Of course, it's welcome to see new ideas that challenge old truths about the field of shock physics. But in the end, I don't think that the current manuscript does this convincingly.

- Timing analysis and shock thickness: The inter-spacecraft separation at this event was roughly 20 km. The small separation, together with the strong wave activity at the shock, can reasonably make the timing analysis uncertain.

The authors claim the uncertainty is roughly the orbital speed of the spacecraft without any explanation why.

In my opinion, this casts doubt on the statement on line 248: "Using exceptional quality, multipoint measurements of MMS we have made exact determinations of the shock ramp thickness"

- Line 115: "Lower hybrid drift waves, can be identified in the frequency range fcp-flh."

It is not clear to this reviewer how these waves are identified as lower hybrid waves. Frequency is generally not a good tool to identify waves in the fast-flowing solar wind due to the unknown doppler shift. Identifying lower-hybrid waves at shocks require careful analysis of the observed dispersion relation, see (e.g. Walker+, 2008).

Minor comment:
- The FPI-DIS instrument onboard MMS was not designed to measure the cold solar wind beam and tends to overestimate the ion temperature in the solar wind. The values of ion beta and gyroradius in the manuscript are likely overestimates.

References:
Caprioli+, 2015: http://dx.doi.org/10.1088/2041-8205/798/2/L28
Kennel, 1987: https://doi.org/10.1029/JA092iA12p13427
Leroy+, 1983: https://doi.org/10.1029/JA087iA07p05081
Lowe+ 2003: https://doi.org/10.5194/angeo-21-671-2003
Hellinger+ 2007: :https://doi.org/10.1029/2007GL030239
Walker+ 2008: https://doi.org/10.5194/angeo-26-699-2008