Annales Geophysicae Discussions

Ann. Geophys. Discuss., author comment AC1
https://doi.org/10.5194/angeo-2022-2-AC1, 2022

Reply on RC2

Krzysztof Stasiewicz and Zbigniew Kłos

Author 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-AC1, 2022

Reviewer (R)
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?

Authors (A)
We thank the Reviewer for carefully reading our paper and providing many comments. Unfortunately, some negative comments and the conclusion focus on imponderables or refer to some misconceptions in shock physics. We hope that we have now clarified these issues in our reply and in the revision of the manuscript.

(R1) 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.

(A1) The Reviewer claims that the observed wavy motion of the shock front in Figure 1 is not a wave. The data clearly show that shock oscillates with frequency of ~1 mHz. Periodic oscillations are generally regarded as waves. However, to avoid confusion, we have changed the sentence in question to:

"The decomposition suggests that the oscillatory behaviour of the shock and wave steepening process are related to the large-scale □1 mHz oscillation seen at the bottom of Fig. 1, which causes the spacecraft to exit and re-enter the shock. The oscillation triggers cascades of compressional waves extending to 1 Hz and above."

(R2) 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.

(A2) Yes, this is a correct statement, part of our conclusions supported by test-particle simulations. We are not familiar with any other plasma process which could create coherent secondary beam by scattering the solar wind beam on magnetic field turbulence in shocks with $\delta B/B \sim 1$. Furthermore, the secondary beam is not "gyrating" as the Reviewer claims, but displaced perpendicularly from the ExB drift direction. Our model of SRA (stochastic resonant acceleration) provides for the first time physically justified explanation of this phenomenon.

(R3) 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.

(A3) Protons accelerated to some 5 keV by the ExB (or SRA) mechanism described in our paper would have gyroradius of 600 km in the magnetic field of 10 nT. The gyroradius of these ions exceeds by factor 6 the thickness of the shock ramp, so they could remove excess heat from the shock needed by the process described by Kennel (1987) and based on resistive MHD equations. Many brilliant physicists working on shocks some 30 years ago had no access to multipoint measurements of high resolution now available, and were unaware of the stochastic heating mechanism that operates in shocks due to large amplitude waves generated by plasma instabilities. The stochastic heating does not need "anomalous resistivity" and is clearly outside the scope of fluid models.

(R4) 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.

(A4) We show indeed with test-particle simulations that secondary beams in the perpendicular plane can be created by SRA mechanism. The secondary beam can appear in any direction, related to the phase velocity of waves. Observations are consistent with our model. A dedicated simulation of the whole particle distribution will be the subject of a separate paper.

(R5) 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).

(A5) Shock reflected, or rather magnetic mirror reflected ions streaming along the magnetic field are indeed observed, mainly in quasi-parallel shocks. Interacting with the solar wind beam they produce counter-streaming ion-ion resonant right-handed instability responsible for the creation of quasi-parallel shocks, which is addressed in separate paper (Stasiewicz & Klos, submitted to MNRAS).
Different situation is observed in quasi-perpendicular shocks discussed in this paper. Distribution functions measured by MMS and shown in Figure 2a,b,c are inconsistent with the concept of reflection which should produce reflected ion beam in panel 2c (parallel direction) and possibly in panel 2a (ExB direction). Instead, the secondary beam is in panel 2b (E direction), which can be explained by the SRA mechanism discussed

extensively in our paper. Ions heated and accelerated by the ExB or SRA mechanism would have large gyroradius ~600 km (see A3 above) and should be seen in front of the shock in kinetic simulations. These accelerated ions could appear also in front of the shock in simulations cited in R5. However, we are not in a position to compare results of these simulations with the measured distributions functions shown in Figures 2 and 3. Such comparison should be made and publicised by involved authors.

(R6) 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).

(A6) We explain: the electric field measured by a satellite acts on all particles measured at the concurrent position. If this electric field corresponds to the primary beam marked with magenta circle in Figure 3C1,D1,E1, then the displaced secondary beam does not obey ExB motion, despite the fact of being in the same electric field as the primary beam. To our knowledge, this discrepancy can be explained only by the SRA mechanism as demonstrated in our paper.

(R8) 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.

(A8) We hope that our answers A1-A11 clarify the physics of the quasi-perpendicular shock based on careful interpretation of MMS data, and in particular the role of large amplitude electrostatic waves for particle acceleration.

(R9) 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".

(A9) The timing analysis (Schwartz, 1998) was made by finding the time lags Δt between two signals within the ramp using the least squares method. Strong wave activity in the magnetic signal sampled at 64 Hz introduces indeed some uncertainty into the results. We have used multiresolution wavelet decomposition to remove high frequencies which produce jitter. Wavelet decomposition was chosen instead of low-pass filtering to avoid introducing phase distortions. The least squares values were minimised for signals at frequencies f=0-2 Hz, which were used to determine the shock velocity. The neighbouring frequency level f< 4 Hz gave a velocity difference ~2 km/s, which we assumed corresponds to the error of the analysis.

(R10) 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).

(A10) Observations of lower hybrid waves in the dayside magnetosphere have been reported by many authors (Bale et al.,2002, Vaivads et al.,2004, Walker et al.,2008, Norgren et al.,2012). We have discussed identification of LHD and other wave modes in previous articles, so we feel it is not necessary to duplicate it again in this paper. The measured waves have large amplitudes 10-100 mV/m in the frequency range 1-1000 Hz, so the linear dispersion equations in broadband, fully developed turbulence are not valid. There are also other wave modes, like whistlers in this frequency range. Separation of wave modes in strong turbulence is difficult, if at all possible. These waves are observed in regions of strong density gradients with spatial gradient scales L ~ 50-200 km, as demonstrated in earlier papers. Since the density gradients destabilise first LHD waves in the frequency range f_cp-f_lh, the observed waves in this frequency range have been labeled as 'LHD' waves. These waves would accelerate and heat ions irrespectively on the labels 'LHD' or 'f_cp-f_lh' which we may put on them.

(R11) 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.

(A11) Yes, we agree with the Reviewer and we have added a sentence that the values are likely overestimated.

In summary, we have carefully addressed all points raised by the Reviewer and amended the text in the revision, where appropriate. We hope that the points raised have now been clarified.