Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2018-110-RC1, 2018

Annales Geophysicae Discussions

Interactive comment on "Wave activity in front of high-β Earth bow shocks" *by* Anatoli A. Petrukovich et al.

Anonymous Referee #1

Received and published: 28 October 2018

General comments

The main goal of the paper is to study the properties of the Earth's bow shock during intervals of high plasma beta (>10). The authors initially employed a semi-automated search routine to identify intervals of high beta when spacecraft were within 5 Earth radii of the model bow shock. In the end, 22 suitable bow shock crossings were identified for study. After reading the manuscript, I am unable to recommend the paper for publication due to many issues which I will present in detail below. I do feel that the reviewers have the concept and the data to make an interesting study, however the current form is too poorly executed with numerous issues and needs significantly more analysis. Since the paper does not reach any meaningful or clear conclusion, then I the current iteration cannot justify publication. It is my recommendation that the authors

extensively improve the paper and re-submit when it has been sufficiently revised.

Specific comments

The title suggests that waves upstream of the bow shock are studied, however in each case, waves downstream, or in the shock transition are investigated.

After reading it, I am unable to identify any clear conclusion or result from the analysis. Several shock crossings are presented, and in each one waves are shown. The authors: 1) do not identify the wave-mode, 2) determine the role of the waves in dictating the shock structure, 3) determine what the relationship between high beta and the waves are, 4) compare with shocks of lower beta, 5) consider the role of the waves in dissipating energy at the shock front.

The authors claim on line P7 L4-5 that "the observed front structure is very different in comparison with that expected for a supercritical shock". The shock profile appears to me to be very similar to a quasi-parallel shock and nothing remarkable. Since the geometry is 46degrees based on a model, then based on the error of a model shock normal, the quasi-parallel/perpendicular geometry cannot be confirmed. However, based on the profile, it appears to be a quasi-parallel shock structure, and thus a prolonged and turbulent upstream-downstream transition is expected.

The authors identified 22 suitable bow shock crossings, however these 22 only occur on 7 days. For example, 12/22 take place on 3-4/01/2008. This should me mentioned in the paper.

In the conclusions, there are many issues, for example: 1. It's known what caused high beta solar wind, one would only have to perform some rudimentary analysis of the OMNI data. This is not necessarily a result. 2. It is stated "Our shock analysis was limited to quazi-perpendicular cases", this is not true, many of the shocks are around the 45degree threshold and look like quasi-parallel shocks (even more so considering possible error of the model normal). Some shocks in the supplementary table are

<45degrees. The authors seem to only consider the quasi-perpendicular shock geometry, they need to also include quasi-parallel. 3. It is said that differences are observed compared to lower beta shocks, but this is barely discussed and verified. The authors should select shocks with similar geometry but with lower plasma beta to compare. In fact, on the first line of the discussion it's said that their observations are similar to already reported structure of high-beta shocks. So, it's unclear what the new results are from this study.

Why are these particular shocks selected for study and what is their significance? There are other shocks which are more clearly defined as quasi-perpendicular in the supplementary table. For example, the 2 Jan 2008 has a geometry of 83degrees. This would likely have a more clearly defined ramp structure and would be easier to determine the shock properties. Please justify the event selection.

I find it unfortunate that 22 shocks are identified but no effort is made to use all these data. How do the few selected shocks compare to the other ones identified? Are the shock profiles and structure comparable? Are similar waves seen for all of them? Are the waves associated with the geometry or Mach number?

The geometry of the shocks found is from 39degrees to 84degrees. Thus, these shocks will have very different structure as they cover the quasi-parallel to quasi-perpendicular regimes. What effects reported are from the high-beta upstream condition, and what are simply from the geometry.

In the final sentence it is said MMS is needed to conclude the wave-mode analysis, why not Cluster; this has been done before. The main issue with wave-modes is to separate temporal and spatial variations. On larger scales (larger than ramp width) then Cluster is a better dataset. Is there no shocks in which the wave-telescope or phase-shift analysis can be performed? Also, if MMS is required then why not use MMS? Maybe there were not enough events?

P7 L18: I am unsure how reliable is the calculation of the wavelength is based on a

delay is so small at 0.13seconds, the waveforms look almost instantaneous? I am not convinced that one can separate temporal and spatial variations on this scale for this spacecraft separation. Also, what is the angle between the spacecraft separation and the direction which was used to determine the delay. If it is perpendicular, then this would also significantly increase the error.

Hodograms: it is difficult to get any useful information from Figure 10. In fact, there is no clear polarisation. Did the authors try to compute this over one or two wave cycles? This might be more meaningful. The min-int hodogram should also be plotted.

More detailed explanation should be given to the observations. Can the authors discuss the parallel heating upstream of the shock in Figure 7.

In summary: the paper requires too much work and revision for revisions. To give the authors enough time, then I suggest the paper is rejected this time but I would encourage them to re-submit when it has been sufficiently revised.

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