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Interactive comment on "Low frequency magnetic variations at high-β Earth bow shocks" *by* Anatoli A. Petrukovich et al.

Anonymous Referee #2

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The paper is about magnetic fluctuations in the Earth's high-beta bow shock. The authors combine data from the Cluster spacecraft and the OMNI database. The topic of high-beta shocks is highly interesting because of their application to astrophysical shocks.

The language in the paper is mostly fluent and clear. The data analysis is detailed and largely precise. However, the presentation of the data is at times lacking and the analysis is sometimes incomplete.

The conclusions mentioned in the paper are that the shock ramp are smeared to 10s of seconds due to the magnetic fluctuations and a somewhat quantitative description of the wave properties of the fluctuations.

I am inclined to consider these conclusions not sufficient to advance our understanding of high-beta shocks and how the fluctuations are different from those in other shocks. The authors should strive to at least qualitatively describe how the high-beta shock fluctuations differ from "normal" shocks and attempt to measure the spatial extent of the fluctuations. The authors should also more carefully try to validate the OMNI values of beta with the Cluster measurements.

Detailed comments followed below:

Introduction, last paragraph:

Page 2, Line 30: The authors should specify how the Geotail, THEMIS, and Interball data were used in this study. This is as far as I can see the last mention of these missions.

This paragraph appears out of place. Such a detailed description of the data used is probably better in the next section.

Page 3, Line $\sim$3: Motivate why Cluster data was not used directly to calculate beta. Specifically why CIS-HIA/CODIF was not used for density/temperature of the solar wind.

Page 3, Line 5: The authors should clarify what the constant values in OMNI are and how that might affect the validity of the reported beta value.

Page 3, Line 24: The figure referred to is Fig 4.

Page 3, Line 25: "The scatter is indeed large." Include a discussion about why and what it means for the results of the study. Also specify which data were used when both were available. ACE or WIND?

Figure 6: - Panel b: Plots of magnitudes should contain the value 0. - Panel c: The authors should explain why the temperature appears to fall over the shock when the primary role of a shock is to heat the plasma. Also explain why the parallel temperature

is larger than perpendicular in the shock foot. - Panel e: Something seems to be wrong with the ion energy here (even more clear in the supplement). The solar wind speed: 250 km/s -> proton energy: 330 eV -> should be on line between 10^2 and 10^3 which is ∼320 eV. But the solar wind is even below 10^2 eV. This must be wrong.

Page 7, Line ∼5: I think there is a missed opportunity to determine the shock speed here. It could put a number on shock extent in space, and not just time. There are several ways to calculate shock speed with one spacecraft, see the ISSI book "Analysis Methods for Multi-Spacecraft Data" chapter 10. Also, Cluster 2 encounters the same shock ∼1min later. This could be used in a timing analysis.

Page 10, Last paragraph: I think the statement "minimum variance (nominal propagation) direction is well defined" is a bit strong considering the eigenvalue ratio is as big as 0.5.

Page 10, Last paragraph: "The time shift between the magnetic measurements along the maximum variance component is 0.22" It seems that the two time series (Fig 10b) are quite different. The authors should discuss how this might affect the certainty of the timing.

Figure 13: Legends for C3, C4 are missing.

Page 19, Line 13: "The ramp is often used to determine the shock motion with multi-point measurements, but in our case it is impossible". This is not true. With only C3 and C4 it is impossible but in at least one case there is another Cluster satellite that could potentially be used.

Page 19, Line 27: Thus observed variations are strongly different from that in low-beta supercritical events ..." This appears to me to be a strong conclusion. Why is this not mentioned in "Conclusions"?

Page 19, Line 29: "Observed polarization is also not consistent with the earlier suggested alfven mode" also appears to be a strong conclusion. Also it is: "Alfvén".

Page 20, Line 21: "Magnetic field and ion density jumps are smeared to a couple tens of seconds" This is a very imprecise statement. It is completely possible for even low-beta quasi-perp shocks to be so slow that the ramp is smeared to minutes. An attempt to measure the shock ramp in kilometers instead of seconds would be desirable.

Figure S1: - Caption says C4. C4 does not have a functioning CIS-HIA. It should say C3? - Panel b: The OMNI density appears to not match the CIS-HIA density. The authors should try to validate the density with some other instrument, like CIS-CODIF or WHISPER. - Same comment about ion energy in spectrograms.

Figure S3: Same comments as Fig S1.

There are also some minor language errors that I have not listed. Watch out for missing or superfluous "the" as well as the difference between "its" and "it's".

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