Reply to Referee #1.

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.

We are grateful to the reviewer for the attentive reading and detailed comments and advices. But we cannot agree, that the paper does not reach any conclusion. We determine the time scales of the shock transition, frequency and polarization for the highest-amplitude magnetic variations and estimate their spatial scale. There is no definite conclusion on the wave mode, but this is because linearly polarized waves not allow to determine reliably the wave vector direction. Besides that, there was a large amount of hidden work for initial data selection, since these are relatively rare shocks, almost never described before. We selected all crossings, suitable for multipoint analysis, at least in principle (out of almost 20 years of Cluster observations). Yes, about other shock types a lot more is known and results are more quantitative. However, this was achieved after tens of publications and thousands of crossings studied. It is not unexpected to achieve not so high level of details in this very first publication using very rare events.

The paper was initially written in a very concise manner to avoid discussion of secondary phenomenological features. However, this may result in a somewhat misleading presentation. Now the description is substantially extended. We added more details on the sample crossings, review generality of results for the whole statistics and discuss comparison with other publications. More specific replies are below.

Corrections in the text: the manuscript is extended by almost 50% and improved. In Sec 1 and 2 changes are marked by bold. Sections 3 and 4 (data examples and discussion) are rewritten almost completely. The full analysis of Event #2 (similar to that for Event #1) is given. Description on Event #3 is significantly extended, including a new note on substantial variability of background magnetic field. Summary on statistics of events is added. In total 3 figures and 1 table are added in the main text and 2 figures in Supplement.

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.

Ok. We assumed the term "front" in a very general sense. We study the dominating high-amplitude variations immediately downstream, since their amplitudes are larger than background field, these variations are actually a part of the transition. **Now we corrected the title.**

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.

As written above, we conclude on frequency, polarization, estimate of spatial scale, general structure of shock transition. Without definition of wavelength and wave vector directions it is impossible to conclude on the wave mode and on the contribution to dissipation and structure. We specially collected all available Cluster project statistics of multipoint crossings. However, it proved to be impossible even with these data to determine reliably wave propagation direction (because this are high-amplitude linearly polarized wave, may be actually – growing localized magnetic clumps).
Text added: more explanation in the text and comparison with the previous results on low beta shocks (more details are below in a reply to the similar comment).

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.

We now add information on shock angle using other methods. All variants are not ideal. Coplanarity definition has its own errors. In two presented cases the shock normals are almost the same with all variants. In Event #3 with very low magnetic field, its direction varies and coplanarity approach cannot be applied. We also discuss some possible implications.
The transition lasts couple of minutes and has very laminar gradual change in ion moments, thus it is similar on a large scale to other oblique or quasi-perpendicular shocks. However, on a smaller scale the structure is somewhat different since there is no clear localized magnetic jump. The main increase of magnetic field is smeared and dominated by variations in all events."Typical" quasi-parallel shock (e.g. Burgess et al. and earlier references therein) has a prolonged transition up to several Earth radii long with patchy ion interaction with individual intensifications (SLAMS). Thus our events are more similar to reforming oblique shock (e.g. Lefebvre et al 2009) or quasi-perpendicular shock in the decay phase.
ext added: Results on theta-Bn with other methods. More careful definition of events as "oblique" shocks. A more detailed discussion of comparison with the quasi-parallel shock.

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.

OK. **Mentioned.** This issue does not affect validity of conclusions. In fact, it is even highlights stability of the observed profile for Event 3 (the nearest neighbor in 10 min has no “high-frequency” pattern).

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.

This in fact is not an independent result on origins of high-beta in solar wind. It is more or less clear, even intuitively, that such conditions correspond to high density and low magnetic field. The main purpose of this study is to determine implications for high-beta shocks: how often such events can be found, how transient is magnetic field. This is **very important methodical issue**, since high-beta intervals correspond to extremely small and hence variable magnetic field. Such magnetic input might have important consequence for shock structure. The decreasing role of magnetic field in higher beta shocks is actually one of important motives for this study.
Text added: More explanations on the use of our solar winds statistics result in the end of Sec 2 and in discussion

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 consider-ing possible error of the model normal). Some shocks in the supplementary table are <45degrees. The authors seem to only consider the quasi-perpendicular shock geom- etry, they need to also include quasi-parallel.

Actually in the literature, there are two approaches. With one quasi-perpendicular and quasi-parallel shocks are separated by 45 degrees. With the other, “the middle” around 45 degrees is attributed to oblique shocks. Quasi-parallel shocks are then that with angles closer to zero. In fact, the second approach is more correct. We now more carefully call our shocks as oblique and quasi-perpendicular. Oblique and quasi-perpendicular shocks are, in a sense, similar, having a well-defined transition (on a scale of minutes). All our events well fit this sort. Quasi-parallel shocks are quite different having very prolonged and patchy transition on the scale of Earth radii. Some discussion of this issue is included.
Text added: More careful definition of events as “oblique” shocks. A more detailed discussion of comparison with the quasi-parallel shock.

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.

There are thousands of crossings of oblique shocks, and many investigations are published with very diverse results. It would be difficult to justify the prompt selection of those for comparison. Some initial comparison was done in Winterhalter et al 1988 (see the next reply also). Thus we concentrated in this first study on common features of high-beta shocks. This is an essential step,

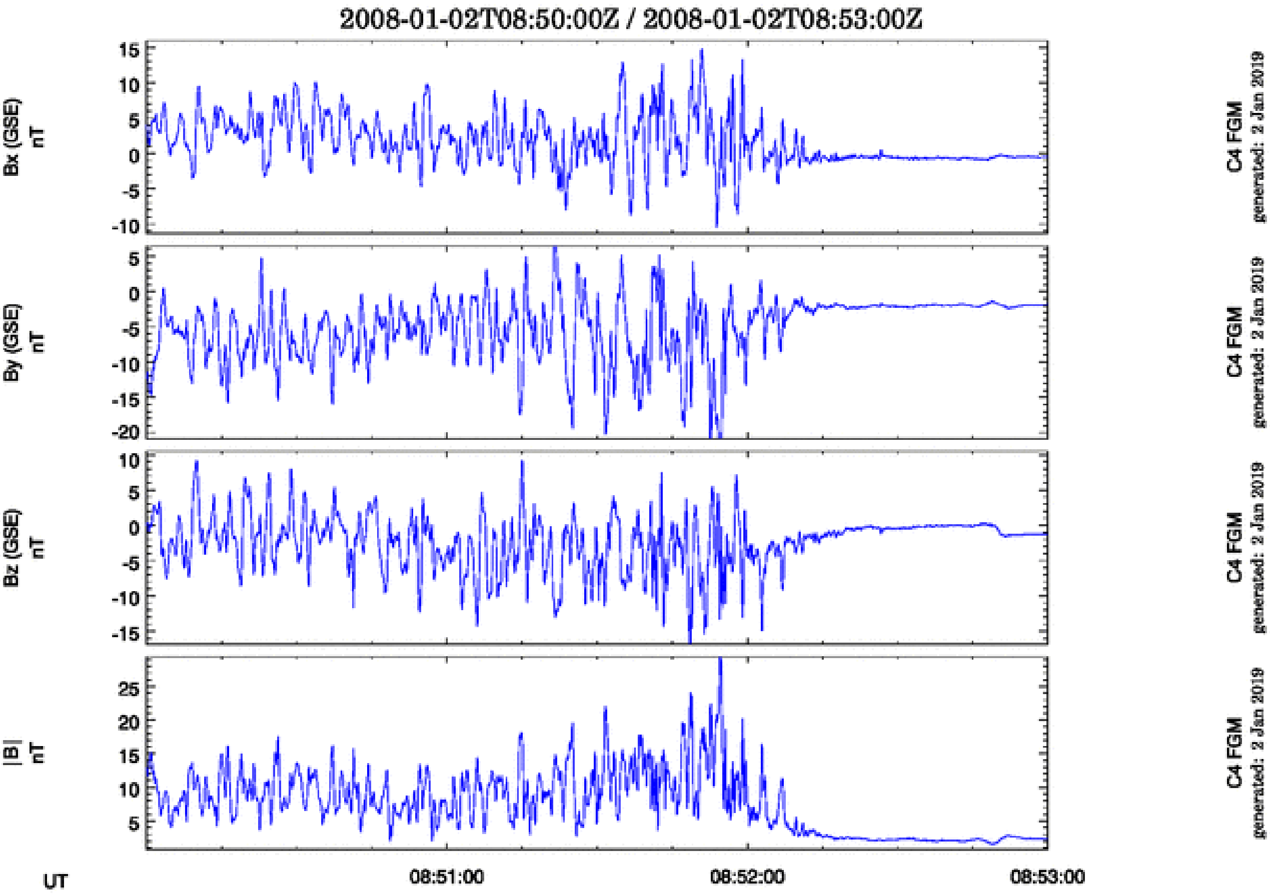
since it is important first to understand what are the primary properties to be compared. We used previous publications on oblique shocks for comparison.
Text added: Some results on waves in lower beta shocks are now cited and compared in Discussion.

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.

The detailed connection of this report with previous publications is provided in the Introduction. Earlier only very general conclusions on "very high amplitude" of variations were available in comparison with the lower-beta variants (Winterhalter et al) The first phrase in Discussion only means that our study does not contradict this previous result. In this report we determine frequency, polarization and estimate the spatial scale of these variations, as well as make some conclusions on general structure of these shocks. If this phrase is so disorienting, we reformulate it in more detail.
Text added: A more detailed statement in Sec 3.4 and Sec.4

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.

Three typical events were taken. The only full justification can be to plot all 22 events. As for the particular event with 83 degrees – it is actually not different, there is no clear ramp, magnetic field grows gradually (see the Figure below). The explanation can be, that for such low magnetic fields, their exact value and orientation might be not so important for shock properties.



Overview of shock from 2008-Jan-2

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.

Actually there is only one event with 83 degrees and one with 64 degrees. All other angles are around 45. There is no “truly” quasi-parallel shocks with the angles close to zero. All shocks have similar large Ms number. Thus it would be misleading to present any statistics (of transition length or frequency of variations) depending on the angle or the Mach number.
Text added: We now included a more detailed reference to statistics, pointing similarity of all shocks with the presented examples.

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?*

The full reasoning of data set selection is included in Sec. 2. Out of about a 100 of candidate events (from all projects) we selected 22 Cluster events with separations about couple hundred km and smaller right in the attempt to perform multipoint analysis of ramp structures around 0.1-0.5 Hz (actually all our studied variations can be in a sense attributed to the smeared ramp structure). Wave telescope can be used with four-point data, but we have only one event from 2003, when the regular tetrahedron was available. Unfortunately, this event has waves “type-2” and the separation of 300 km was too large to determine the scale. All other events are from the later years of Cluster, when only two spacecraft C3 and C4 were close sometimes. Such poor statistics is just because high-beta solar wind is rare. With the two-point data the spectral or phase shift analysis can provide the scale only if wave propagation direction is known. Exactly such analysis is performed in this study. We choose the time-domain analysis, since the main frequency of oscillations is well defined. To perform the wave-telescope analysis with no supposition on wave propagation we need (potentially!) the four-point MMS data with the regular separation about 10 km. Since MMS is new project, there is no so much statistics and one needs to wait. This analysis will be a part of future publications.
Text added: Our discussion is extended to make this point clearer.

P7 L18: I am unsure how reliable is the calculation of the wavelength is based on a C3 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.

In our case 0.13 sec corresponds to 2-3 sampling intervals. It is quite sufficient to perform reliable phase shift analysis. In fact, the stable time shift of two curves is well observed even visually in Fig. 8.
Text added: information on validity of phase shift is added.

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.

Table 1 includes also spacecraft distance along the min-var direction (10 km, compared with 30 km full distance). The range of possible scales for different propagation direction is given in the text. Of course, the wavelength can be very small in a rather unlikely case, when the spacecraft are exactly perpendicular to the wave vector.

Text added: Actually this information on separation is present in the text.

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.

All full hodograms are now included. Indeed the main conclusion is that there is no stable polarization. The given time interval was selected because of the stable phase shift between the spacecraft. Selection of even shorter one with two "nice" cycles will not result in any changes in determination of the spatial scale, because (1) phase shift is the same, (2) max var direction (important to fix propagation direction in linear polarization case) is the same.

Text added: Additional hodograms are included + more explanations.

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

A substantially longer description is now included. The increase of parallel temperature looks natural since ions tend to escape upstream along the magnetic field, so the spread of their velocity is converted in temperature. The detailed analysis of ion dynamics is left to future publications.

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.

The manuscript is significantly rewritten. Observation and discussion section is changed almost completely.