Authors answer : The authors thanks both referees for the analysis of the document and encouraging remarks. We have followed all suggestions, and corrections. At present time, the document has been under deep proofreading ; the text has been improved in order to be enlightened at some locations where necessary. We thank the Editor for his help and we hope that the revised manuscript will be suitable for publication after these corrections.

Interactive comment on **"Evidence of the Nonstationarity of the Terrestrial Bow Shock from Multi-Spacecraft Observations: Methodology, Results and Quantitative Comparison with PIC Simulations"** ***by*** **Christian Mazelle and Bertrand Lembege**

Anonymous Referee #1

Received and published: 30 October 2020

The manuscript describes an in-depth, solid analysis of the Terrestrial supercritical, quasi-perpendicular (Qperp) shock substructures (foot, ramp and overshoot) from in- situ, multi-spacecraft magnetic field and plasma measurements. The analysis is based on a new, detailed methodology which is applied to 96 Earth shock crossings by the Cluster spacecraft. The study shows that the ramp thickness is at least of the order of a few electron inertial lengths, but also, that the depth of the foot region is highly variable with maximum values in agreement with previous theoretical studies. Finally, these results are discussed in the context of previous works and compared with advanced, PIC simulations. In particular, the latter show that the ramp depth is not appreciably sensitive to the shock reformation phase, while, as expected, the foot depth varies dramatically on a similar timescale. This is the first time a clear methodology to identify and measure the extent of these regions is put together in a coherent and meticulous fashion. This will surely be gladly received in the shock community who will hopefully use this work as a reference. But additionally, the paper presents new, relevant results on the size of the Qperp substructures at the Earth. For those reasons, I find the manuscript suitable for publication in Annales Geophysicae.

We thank the referee these nice comments.

There are only a few, minor shortcomings that should be addressed.

First, the text is sometimes hard to follow and this is not desirable in a manuscript that could also be used as a tutorial. The authors may find useful doing a more careful proofreading. Finally, the manuscript displays a fair amount of typos, cross-out words and a few confusing sentences that should be addressed.

Yes , at present, the whole document has been under deep proofreading, Corrections are applied on typo errors and on some incorrect sentences which either have been duplicated or are sources of confusion.

Line 14: Most statistics clearly evidence that the ramp (please reconsider the use of the verb evidence)

Line 21: 'A comparison with..'

Lines 20-25: List of results a, b, c or i. ii and iii

Line 28: Confusing, please rephrase.

Line 191: ’as close as possible’

Line 195: ’or it is not satisfactory’

Line 197: finest? => shortest? Smallest? Thinnest?

Line 300: n0 is a vector (bold)

Line 635: Newbury

Line 900: Paschmann

Line 989: nonstationarity

Thanks. All these typos have been corrected.

We also need to mention that we found necessary to add a small paragraph in Section 5. It refers to a work exactly in the relevant topic but published very recently (on September 20, 2020). We were of course not aware of this study at the date of our original submission (on July 23) but if we consider the time left before the final publication of our paper, it may look unfortunate to miss this reference.

We reproduce below the small paragraph we propose to add at the end of Section 5 (after appropriate shortening of other parts):

Yang et al. (2020)

Very recently, Yang et al. (2020) claimed to have identified shock front self reformation with the help of high-resolution Magnetospheric MultiScale (MMS) satellite data. This study using measured ion phase space together with B profiles clearly shows the importance of dissipation effects carried by the reflected ions. However, the comparison with the results of the present study proves to be quite difficult due to the lack of precise information. While the MMS data show clearly that the crossed shock is nonstationary, a clear evidence of the self-reformation is still questionable for the following reasons:

(a) the whole study is based on one shock crossing only and eventually restricted to a comparison between two satellites since three of them show very similar profiles and appeared to be in a plane nearly parallel to the shock front. No statistical results are shown or even summarized which could have stressed the ion vortex formation over different steps of its formation.

(b) authors mentioned to have used the timing technique (Schwartz, 1998) to determine the normal and the velocity of the shock front from multi-spacecraft measurements. But, no information is given on the application of the procedure itself (in each spacecraft frame), and on the estimate of associated errors. The use of typical magnetic field peaks around the overshoot is mentioned but without precising where in the different time series. The use of the overshoot is quite odd since it is not precise enough (contrary to the middle of the ramp) as a reference point. It contains superimposed fluctuations and is partially polluted by reflected ions. Moreover, important information equivalent to 'reference satellite' and 'reference time' (as proposed in our Sec. 2.2) is missing. In addition, no information is given on the identification of the ramp itself and on the conversion from the time series to distance profile along the shock normal.

(c) The analysis mentions a shock ramp less than 0.3 c/ω_{pi}, which is not precise enough, seems high and in contrast with the fact that M_A is relatively high (M_A= 10.8). For such value one could expect a much narrower ramp width (see statistics in our Fig. 8d). In addition, one ignores (i) how this ramp width has been measured and (ii) the precise values of the ramp width during the shock crossing by each satellite.

(d) The emerging large scale fluctuation announced as a new ramp for only one satellite may be questionable. The new front is not 'mature' enough during the shock crossing and the precise location of the 'new ramp' within these fluctuations is not clearly identified. One can wonder whether it could be the signature of front rippling or/and multi-crossing due to the back and forth motion of the shock front, which would need a further analysis.

Authors answers : The authors thank the referee for his/her constructive remarks and have answered to the different suggestions which reveal to be quite helpful to improve the manuscript. Details of the different improvements are indicated below.

Comment on **"Evidence of the Nonstationarity of the Terrestrial Bow Shock from Multi-Spacecraft Observations: Methodology, Results and Quantitative Comparison with PIC Simulations"** ***by*** **Christian Mazelle and Bertrand Lembege**

Anonymous Referee #2

Received and published: 19 November 2020

The article by C. Mazelle and B. Lembege focuses on the analysis of the terrestrial bow- shock, specifically focusing on its non-stationarity through data analysis of 96 shock crossings and their substructure (ramp, foot), PIC simulations and comparison with past observations and relevant publications.

The analysis and discussion of the results (whether from data analysis or the particle simulations) is very comprehensive, certainly the authors leave no stone unturned. The paper can be treated both as a review and as an original research manuscript. I agree with the review from Referee #1 that this would be a significant contribution for re- searchers who focus on the physics of shocks. The limitations of data analysis are also nicely highlighted, certainly a caution for researchers who investigate detailed shock structures at other planetary bodies and under different upstream solar wind regimes through single spacecraft measurements. Given the length of the manuscript, I didn't find typos/language errors as critical - there was enough information to back-up information lost in some confusing sentences, but I of course agree that extra proofreading could benefit the quality of the manuscript.

Yes, a deep proofreading has been made. Typo errors and identified duplicated sentences have been corrected.

What I see as a potential problem is what, at the same time, is described as a unique aspect of this article, ie that the manuscript contains both review and original research elements. The reader has too process a wealth of new information (data analysis simulation results & methodology, as well as extensive review elements of past works). The article in most parts seems too verbose and its very tiring to read. Its very easy to miss key points reading through, I had to go through certain sections multiple times to absorb critical or necessary information. I leave to the authors' discretion whether to review which parts of their manuscript can be shortened - but I definitely urge them to consider this to improve the manuscript's readability and for it to reach to a larger audience. Bulleted lists describing conclusions or methodology steps can also help a bit with organising the text, as an alternative to long paragraphs. I especially think that the section where results are compared with separate investigations in detail can reduced considerably. I will not object if the authors decide to maintain the lengthy text, however - this is only a recommendation with little or no impact on the scientific quality of the work.

Yes, we agree with the referee on the fact that the manuscript is too verbose. The actual proofreading was performed to make the document more concise. Moreover, we also realized that some sections of the text appear too long as a whole (e.g. the technical part) which might be

difficult for the reader to 'digest'. In order to find a compromise between the detailed description of the methodology (tutorial part never made in previous works to the knowledge of the authors) and making the reading more attractive, we are improving the structure of this whole part by inserting very short heads of paragraphs (half line), in order to define precisely some segments easy to identify by the reader. Then, these short heads will be gathered in a synthetic synoptic (located in the corresponding section) that the reader can refer at any time during his/her reading of the document. This will avoid the reader to move backward/forward through the whole document.

Finally, I think that the quality of Figures is quite variable. Few figures have good contrast and sharp lines, most are quite blurred and difficult to read. Not sure if this is an issue with PDF conversion of the manuscript, but better Figure quality could benefit readability (especially figures 5, 6, 11, 12).

Yes, the referee is right. These figures have been under total reshape from the original data in order to get the best possible quality.

Overall, I recommend publication of the manuscript in An. Geoph., but would urge the authors to first consider the presentation issues highlighted above.

Thanks. As mentioned above we have been working on the presentation issues.

We also need to mention that we found necessary to add a small paragraph in Section 5. It refers to a work exactly in the relevant topic but published very recently (on September 20, 2020). We were of course not aware of this study at the date of our original submission (on July 23) but if we consider the time left before the final publication of our paper, it may look unfortunate to miss this reference.

We have already reproduced above the small paragraph we propose to add at the end of Section 5 (after appropriate shortening of other parts).