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 is under way at present time. Typo errors and identified duplicated sentences are under correction.

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 is 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 at the end of the concerned 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 are 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 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.