

ANSWER TO REFEREE 2

We thank the referee for her/his appropriate and encouraging comments which helped us to improve the quality of the paper. In the following our answers. In the re-submitted version of the paper all changes are in red.

- *The paper presents a newly-developed magnetopause profile model, obtained by a novel and interesting combination of analytic theory and observation quantities. There is a clear need for models such as the presented one, since the only other sources of information about the magnetosphere have shortcomings: satellite measurements are limited to the points in which they were taken and cannot easily be generalized, while global kinetic simulations are still numerically very expensive and can thus only be run for a limited number of cases. While the structure of the paper is clear and straightforward, first constructing the theoretical model and then presenting numerical verification, there are some problems with how the two are connected: I don't understand how the simulation results in section 5 validates the equilibrium solution that is presented before. As the authors strictly focus their analysis on the reconnection instability (getting steadily growing instability results, including a linear and a nonlinear phase), the only result they seem to get is that their equilibrium solution is not in any equilibrium at all. I am missing a quantitative investigation of how the initial profiles develop over time and a discussion of how their deviation from the ideal values calculated before places caveats on their usability.*

Concerning the distinction between an unstable equilibrium and the absence of equilibrium, we have slightly rearranged this part to show more clearly that we have tested the two properties. To summarize here this discussion, in the absence of an initial perturbation everything remains steady in the simulation (except for the numerical noise which can be easily controlled being about many order of magnitude smaller) until a time larger than one thousands of characteristic dynamical times, so proving that our model equilibrium is indeed so. On the other hand, when we add an initial perturbation to our model equilibrium the reconnection instability develops.

- *Line 64 71) reference a Manuzzo et al 2019 paper, which is apparently under review and does not seem to be publically available. This makes it somewhat awkward to understand the precise nature of MMS data that is being compared against. I suggest giving a compact explanation of the method, if it is possible, so that the input data can be appraised while the referenced paper is still under review*

We agree with the Referee about the need of briefly introducing the new technique developed in Manuzzo et al 2019. This is now discussed at the beginning of Section 2 and the paper is now accepted for publication. It will be soon available online.

- *Equation 1b) Why is only $\text{sign}(q)$ being used in the equation and not q itself? What is Nabla bar? Is this an unusual unit system of Maxwell's equations?*

We agree with the Referee that an adimensional form of the starting equations, as we did, could lead to misunderstandings. For this reason the system of equations (1a)-(1g) is now re-written in dimensional form (SI units). On the other hand, for computational reasons, an adimensional form is used for the numerical code. Finally, nabla bar was a mistake, it is the standard nabla symbol (now fixed everywhere).

- *Equation 2 / line 125: Is the P_{tot} here assumed to be a constant over the entire box, or a spatially varying quantity in accordance to observations?*

In a 1D equilibrium model this quantity must be spatially constant. We have slightly rearranged this part to make it clearer.

- *Line 140: Likewise, is this a global constant, or a spatially varying one? Please clarify.*

The clarification has been added to the text.

- *Equation 8: This interpolation is described as being performed for each quantity of interest independently, and it seems to be implied that this includes the magnetic field components. However, if this is performed for each B component individually, does it maintain $\text{div}(\mathbf{B}) = 0$?*

The equation $\text{div}(\mathbf{B}) = 0$ is verified since the model is 1D with variations along n only, and with $B_n=0$. The fitting of \mathbf{B} concerns the tangential components only.

- *Line 237: I do not understand what a "spectral like resolution" in a finite difference scheme is supposed to be. Do you refer to it's accuracy as being comparable to that of spectral solvers? If so, by which measure do you consider them to be "spectral like"?*

Spectral like resolution is the name given by Lele, JCP (1992) to the possibility of build up an implicit finite difference operator (i.e. including the nearby values of the derivatives) asking not only to minimize at max the accuracy (Taylor development) but also to solve the most possible Fourier equivalent wave vectors, so a mixture between finite differences and spectral methods. In the text we now explicitly refer to Lele (1992) for the significance and technical details of compact finite differences.

- *Line 239: Please explain the coordinate system. If this is a 2D code, why are there x,y and z coordinates?*

We thank the reviewer for raising this point. Now any reference to the z direction has been deleted. For the sake of simplicity in this work we limit to a 2D geometry, but the numerical code is fully 3D. This sentence has been added to the text.

- *Equation 9: The choice of epsilon is confusing here. Make sure to give it more distinction to the epsilons used before.*

We thank the Referee for letting us fix this misprint. We have now re-defined the perturbation amplitude with the symbol a both in Eq. 9 and 10.

- *Equation 9: what are the quantities i and j , mentioned as $i \neq j$ in this equation set?*

The question is no longer relevant following the modification of the equation.

- *Figure 4 should have axes units or at least explanatory references in it's caption, as in it's current form it is not understandable without reading referenced literature.*

The axes have adimensional units (added in the caption).

- *Figure 4 and 5c should reference each other, or might even be overplotted in the same axis.*

Correction made in the text (line 308-309)

- *Figure 6: If the numerical values are normalized to NMSH, why isn't this reflected in the colorbar unit label?*

The units have been added to both figure 6 and 7.

