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Interactive comment on "A multi-fluid model of the magnetopause" *by* Roberto Manuzzo et al.

Anonymous Referee #2

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1 General comments:

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 can not 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 steadyly growing instability results, including a linear and an 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.

2 Specific comments:

- 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.
- 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?
- 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?
- Line 140: Likewise, is this a global constant, or a spatially varying one? Please clarify.
- 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 $\text{div})(\vec{B}) = 0$?

- 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"?
- Line 239: Please explain the coordinate system. If this is a 2D code, why are there x,y and z coordinates?
- Equation 9: The choice of epsilon is confusing here. Make sure to give it more distinction to the epsilons used before.
- Equation 9: what are the quantities i and j, mentioned as $i \neq j$ in this equation set?
- 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.
- Figure 4 and 5c should reference each other, or might even be overplotted in the same axis.
- Figure 6: If the numerical values are normalized to N_{MSh}, why isn't this reflected in the colorbar unit label?

3 Conclusion:

The presented manuscript provides a novel and interesting magnetosphere model. However, it's current presentation is not entirely convincing and will require some revision.

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