Referee report for revised paper "A multi fluid model of the magnetopause", Manuzzo et. al

December 29, 2019

The authors have addressed my comments from the first round of review comments and the manuscript has improved in legibility considerably. Especially in relation to my overarching comment that the connection between the analytic model and discussion of simulation results seemed to be disconnected from one another, the authors have made considerable modifications to the sections motivating their simulation approach and discussing the importance of the obtained results.

The addition of the sentence in line 272, where the long term stability of the unperturbed configuration in the numerical code is explicitly highlighted, further helped to underline the versatility of the chosen approach.

I still have some minor comments remaining for the manuscript before I can recommend it for publication:

- While the introduction mentions kinetic simulations as a potential source for magnetopause equilibrium data, the scope of referenced literature is quite narrow. The recent advent of global kinetic and hybrid-kinetic simulation models such as Chen et al. (2018) (`https://doi.org/10.1002/2017JA024186`) Karimabadi et al. (2014) (`https://doi.org/10.1063/1.4882875`) or Palmroth et al. (2018) (`https://doi.org/10.1007/s41115-018-0003-2`) provides an additional avenue to analyse and provide magnetosphere equilibria. Addition of a short discussion of their properties in relation to the manuscript at hand would be welcome.

- In the updated formulation of the multi-fluid equations (eq. 1a - 1g), some equations appear to use α as a species index, while others use β. The distinction between the two (if there is any) does not become clear from the text. Please clarify or homogenize the terminology used in these equations further.

- The model described in section 3 apparently works excellently for the described example case of a tangential magnetopause situation near GSE z=0, as its mathematical formulation is based on this assumption. I am still missing some discussion about the boundaries of applicability of the model for magnetopause locations further away from z=0, or further removed from the dayside reconnection. A short critical discussion of conditions that would make the presented model less reliable would help the reader appreciate its value in the domain of applicability.

Some notes about the referenced literature:

- Neither a URL nor DOI has been listed for the paper by Alvarez Laguna. Is it this one? `https://lirias.kuleuven.be/retrieve/531033`
- Please give an ISBN reference for the book by Canuto
- The papers by Dargent et al., Dungey at al. and Shumlak et al. are giving duplicate DOIs
- The papers be Lee et al. and Lele et al. are referenced using both DOI and URL, which is redundant.
- A DOI should be added for the paper of Modolo et al.

Altogether I believe the manuscript is presenting an important new result and can be accepted for publication once these minor comments are addressed.