The paper introduces a time-dependent three-dimensional magnetopause model, named the POS (Position-Oscillation-Surface wave) model, based on quasi-elastodynamic theory, addressing limitations of existing time-independent models. It emphasizes the dynamic behavior of the magnetopause, which includes oscillations and surface wave-like structures in response to solar wind variations. The model was validated against 38,887 observed magnetopause crossing events, achieving a root-mean-square error of 0.768 Earth radii, marking an 18.7% improvement over five widely used models. Notably, the POS model showed enhanced accuracy under disturbed solar wind conditions and in specific magnetopause regions. The study highlights the need for new strategies to balance dynamic representation and computational feasibility in magnetopause modeling. The paper is well written, and I have few concerns to be addressed before the paper will be acceptable for publication.

Lines 61 – 69: I would suggest the authors to include the results presented by Collado-Vega et al., (2023) in this discussion. They compared the magnetopause predictions obtained by different MHD models, showing the discrepancies for the standoff positions.
https://doi.org/10.1029/2022SW003212

Lines 147 – 150: Are the final dataset (38,887) single magnetopause crossings? When all five THEMIS/Cluster satellite cross the magnetopause in a short time interval, it is counted as one or multiple crossings? What is the maximum time interval between two consecutive MCEs to be considered redundant? This information is not clear in the text.

Line 153: Please clarify the 300 s condition (why 300?).

Line 153 – 155: The one-hour average velocity is based on which assumption? Is it reasonable to use the same 1h average for all solar cycle phase? How different/better is this methodology from the time shift provided by OMNI service? How much better is Wind data compared to ACE or other available solar wind monitors?

Line 222: There is no **n** (normal) indicated in the equation 2. Please, check.

Line 255: typo "... three-dimensional structure (Formisano et al., 1979; ...".

Line 292: "two terms on the right side of the equation ...". Suggest labelling the equation.

Line 314 – 317: Have the authors analyzed the results for the flank regions independently? Is there any asymmetry in the results?

Lines 363 – 382: Figure and the following discussion. Are the authors projecting the THEMIS position at the subsolar region? In this case would not be more interesting to calculate the magnetopause model results at the real THEMIS location? On my understanding, when the authors analyze the projection of the spacecraft position at the subsolar point, they are assuming the magnetopause is "shrinking" and could be neglecting any wave motion along the surface caused by a pressure pulse. Please, make a comment on that point.

Lines 422 – 434: Figure 6 and the following discussion. The authors claim that the POS model's predictions are more effective than C02 model. However, the results on Figure 6b are considering $\theta = 0$ and the spacecraft locations are not. Also, in comparison with the satellite positions both models show very close results (by visual analysis) on figure 6C, the POS is closer on figure 6d and C02 is closer in figure 6e. I suggest the authors to point out these discrepancies.

Discussion and Conclusion

How does the POS model respond to transient events formed at the bow shock or in the magnetosheath when they reach the magnetosphere? See Sibeck et al., (2022) https://doi.org/10.1029/2022JA030704 and Silveira & Sibeck (2023) https://doi.org/10.1029/2023JA031362 .

Is POS model capable to catch return magnetic fluxes from nightside to dayside due to nightside magnetic reconnection? See Silveira et al., (2024) **https://doi.org/10.1029/2023JA032166**.