Referee comments

Scalar-potential mapping of the steady-state magnetosheath model

Round 2

General comments

The changes made by the authors improved the impression of the manuscript. Specifically, more focus is put on achieving a reasonable grid in the flank region rather than the ability to generalize the method, which is in line with the outcome of the study.

The added figures improved the clarity, especially Fig. 1-3. The updated section headers are also appreciated as they highlight the difference between previous works and the present study.

To represent a significant scientific advancement, the manuscript would need to be extended by benchmarking with real spacecraft data and comparing to the performance of previous methods (e.g. Soucek and Escoubet (2012)). Having said that, I will continue with more specific comments regarding the content of the current manuscript.

Still concerns regarding the generality of the mapping procedure and the presentation of the method.

Specific comments

The generalizability of the mapping procedure remains a bit unclear. The phrase “arbitrary shape” has been changed to “non-parabolic shape”, but does it also need to be axisymmetric? The discussion reads:

line 326:
“Our method has the possibility to be extended to three-dimensional, non-axisymmetric modeling by the use of magnetopause normal mapping. It is possible to obtain the steady-state magnetosheath potential in a more general sense without referring to the KF94 solution. [...] Various numerical solvers are known for solving the Laplace equation such as the Jacobi method, the Gauss-Seidel method, and the successive over-relaxation (SOR) method. These Laplace solvers are numerically more expensive than the mapping method, but the computation in 3-D is feasible with the contemporary computational resources.”

Here, it seems like the Laplace equation needs to be numerically solved for a 3D non-axisymmetric magnetosheath. But I thought your method was to use the analytic expressions from the KF solution and map them onto a magnetosheath with new boundaries. Is this not possible in the non-axisymmetric case? If so, this is quite a crude restriction which should be noted (perhaps in the introduction and/or around line 155).

Staying on the topic of the generality of the method, the following sentence is a bit strange:

line 170:
"We use a specific exponent for the Shue model (with an alpha exponent of 0.5) in an effort to show that the analytic model is 'simple'. The solar wind conditions for which this exponent is applicable is not often encountered"

This is a direct response to a previous referee comment. The impression is that you are only showing that the model is simple in a special case which is rarely encountered. With this result, you cannot claim that the general method is 'simple'. Thus, this sentence weakens your argument that the method is simple and/or computationally inexpensive. To improve credibility, would it be possible to give the results with a general alpha exponent?

The methods section still seems unnecessarily lengthy (compared to the scientific contribution of the study), since the same set of equations are repeated twice with only some changes in the notation. However, if the authors after thorough consideration regard all details as necessary, it can be included as-is.

I have a number of suggestions regarding the figures which might give them a more solid impression:

The figure titles are inconsistent – for example, in Fig. 11 the titles describe which functions are plotted and in Fig. 2 the titles refer to the grid and mapping method. The point of this study is that Fig. 11 (left panel) is different from Fig. 2 (right panel), so the 'structure' of the figures should be similar and Fig. 2 should be clearly referred to when discussing Fig. 11.

Instead of referring to the figure panels as left/right, why not introduce subfigures (e.g. Fig 2a)? I also suggest to add colorbars so that absolute numbers can be compared between the results of the different methods. In addition, the captions could probably be more informative.

It would be nice to have figures that should be compared with eachother side by side (e.g. Fig. 2 (left panel) vs Fig. 2 (right panel) vs Fig. 11 (left panel)), but I understand that this might not be reconcileable with the order in which they are referred to in the text. However, as stated above, there can be more references to the figures (e.g. Fig. 2 vs Fig. 11) when making comparisons.

Maybe combine Fig 1 and 3 to facilitate the comparison (keep all plots but make 2x2 subfigures).

On line 123, the reader might ask: You say that Soucek et al (2012) were able to avoid the problem, so why is your orthogonality needed?

Technical corrections

Text

- u and v are introduced on line 50 but defined/explained on line 72-73. Consider defining them where they are introduced.
- The stream function should be mentioned closer to Eq (9).
- Line 108-110 and line 116-118 are almost the same sentence, a bit repetitive.
- line 323-324: references are in the wrong format (parentheses).

Equations

- Eq (39): Parentheses in the denominator that should not be there.
- Eq (41): Are $e_{mp,x}$ and $e_{mp,y}$ the x and y components of $e_{mp}^{(k)}$? If so, they should have the superscript (k).