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Comment on angeo-2021-1

Anonymous Referee #2

Referee comment on "Magnetosheath plasma flow model around Mercury" by Daniel Schmid et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2021-1-RC2, 2021

The manuscript “Magnetosheath plasma flow model around Mercury” by *Schmid et al.*, describes an interesting analytical plasma flow model which goal is to provide a computationally fast way to derive properties of the solar wind plasma in the Hermean magnetosheath.

As the authors wrote, an analytical model, which would quickly provide estimations for plasma parameters in Mercury’s magnetosheath, would be useful tool to interpret observations, for example, from the forthcoming BepiColombo Mercury mission.

However, although the goal of the work is interesting and an analytical model may have potential to be used in studies where self-consistent 3D plasma models would be computationally too extensive, more details about the approach and validations of the model results are needed before the work is ready for publishing.

Please see below my remarks and suggestions of how the manuscript could be improved.

The authors describe in very detail different steps which are needed to derive the velocity field from a magnetic field model. That is very important issue, but at the moment, the manuscript looks to be a valuable technical documentation of the developed software tool, but not yet a comprehensive detailed research report.

As an example, the authors do not describe in detail the basic physical assumptions to which the used equations are based on, but they rather refer to earlies works where the adopted original approaches are presented. For example, the reader may not easily understand why a magnetic field model is used to describe the plasma flow, because the manuscript does not mention that if there are no protons sinks and sources in the magnetosheath (which is not strictly speaking true because of the planetary hydrogen corona), in a fluid model stationary case the continuity equation is

$\square \cdot (n \mathbf{U}) = 0$, [Eq. 1]

where n and **U** are the density and the velocity of protons, respectively. If we the make the analogy

$n \mathbf{U}$ <-> $\mathbf{B}$, [Eq. 2]

then a magnetic field (**B**) model would fulfil Eq. [1], because the magnetic field model has to always be divergence-free:

$\square \cdot \mathbf{B} = 0$, [Eq. 3]

as discussed, for example, in *Genot et al.*, 2011. Moreover, the relatively limited discussion of the model’s theoretical background does not provide an interested reader essential information to evaluate what consequence for the velocity field would be according to Eq. [1], if a non-constant plasma density (n) model are later used.

At the level where the manuscript is now, it does not have much new elements or inventions compared with the previous works made by *Genot et al*., 2011 and *Soucek and Escoubet*, 2012. In fact, the only new aspect looks to be that the authors have adopted models, which describe the shape of the Hermean bow shock and magnetopause. Implementing those boundaries is, or course, a mandatory 1st step to develop an analytical magnetosheath model for Mercury. However, an interested reader would appreciate more comparisons, especially quantitative comparisons, between the properties of the developed model and the properties of the works to which the used approach is based on (*Genot et al.,* 2011, and *Soucek and Escoubet*, 2012), maybe also to Spretier&Stahara’s gas dynamic model, to in situ observations and probably also, at least at some level, to self-consistent models (MHD or hybrid).

Especially, in contrast to the work of *Genot et al.*, 2011 and *Soucek and Escoubet*, 2012, the manuscript does not include clear validation of the developed model to the plasma observations made by the Messenger mission. It is, therefore, unclear how realistic, and therefore how useful, the developed model in practice is. In fact, the authors express this concern also by themselves (“… *Earth, the solar wind and magnetospheric conditions at other planets can be very different (like e.g. at Mercury)* …”), but the manuscript does not provide much evidence of the realism of the developed Hermean magnetosheath model. For example, a reader may wonder how the approach where the space region which is in the Earth magnetosheath model of *Kobel and Flückiger* (1994), has been “stretched” and “fitted” inside to the space region which describes the presented Hermean magnetosheath model, affects the properties of the stream lines and, after including assumption to the plasma density, the speed of the solar wind.

As already mentioned, a more detailed discussion about the basic properties of the developed model is needed. For example, if it is assumed that the speed is constant along a stream line, then, because a flowline at the subsolar point forms the surface of the magnetopause, the density at the magnetopause is (for a high Mach values) about 4 times of the density in the undisturbed solar wind, and the bulk velocity in that situation would be about ¼ times the speed of the solar wind. How realistic those values are? One could, for example, derive the velocity and the plasma density along some orbits of the Messenger spacecraft in order to illustrate how the properties of the plasma change in the magnetosheath and, if possible, compare predicted values with observations.

It would also be very useful to analyse what consequences the derived velocity field would have to other plasma and field parameters. An approximation that the magnetic field is along the solar wind velocity vector is more justified at Mercury than at the Earth and, therefore, derivation of the magnetic field by assuming that the IMF is along the solar wind flow might be justified as a first order approximation. Would that mean (because of Eqs. [1] and [3] above) that the strength of the magnetic field would change similarly as the particle flux? If that is the case, it would be interesting, if possible, to see the value of the total magnetic field in the magnetosheath.

To summarize, as can also be seen in the comments above, the manuscript considers interesting and important plasma physical questions. The goal of the presented work is scientifically solid. However, as already mentioned, an interested reader would expect that the manuscript contains more detailed analysis of the basis of the approach and to see validations of the results.

Although the results of such validation tests are at the moment unclear, based on the validations made of the models to which the presented model is based on, such validations have a good possibility to confirm the applicability of the developed model to provide a valuable description of the properties of the solar wind plasma in the Hermean magnetosheath.