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Comment on angeo-2021-35

Gabor Toth (Referee)

Referee comment on "Magnetotail reconnection asymmetries in an ion-scale, Earth-like magnetosphere" by Christopher M. Bard and John C. Dorelli, Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2021-35-RC1, 2021

The paper describes Hall MHD simulations with an increased inertial length and analyses dawn-dusk asymmetries and temporal variations in the solution.

Major issues:

I find the paper's title, abstract and language misleading. While the authors carefully avoid to claim that the modeled system represents Earth, everything implies that to be the intention, including the distance units shown as R_E, comparison of magnetospause stand-off distance etc.

It would be much better to say that these are Hall MHD simulations of Earth with a drastically (about factor of 70) increased Hall effect, or ions with mass 70amu instead of 1amu. Instead of writing the results in normalized units, why not write them out in physical units? With some effort, I managed to figure out that the authors most likely used the following normalization:

L_0 = 6378km, B_0=10nT, rho_0=5amu/cc, v_0=97.5 km/s, t_0=65s

This means that the simulations represent the following setup:
the incoming solar wind velocity is ~400 km/s,
the solar wind density is 5 amu/cc
and the IMF strength is 10nT, pointing mostly southward.
The inner boundary density is set to 20amu/cc.

These are perfectly fine numbers for Earth (but not for Mercury), nothing unusual about them. The only unusual value is the ion inertial length, which is 1 Re instead of 1/70 Re. That's OK too as long as it is clearly described. No need to talk about mini-magnetosphere and provide results in normalized (and incomprehensible) units.
For example the "substorm" frequency is given as 5-10 t_0 (line 187), but with the above constants it actually means 5-10 minutes, which is much easier to interpret.

Another major issue is the unnecessarily sharp and sometimes incorrect contrasting with previous work. The manuscript incorrectly claims that MHD-EPIC simulations use ideal MHD coupled with PIC (line 48), when it is clearly stated in the cited papers that the PIC regions are coupled with Hall MHD. Even if Hall MHD was not used in the full domain, it is hard to argue that Hall MHD matters away from current sheets (line 50) in the real systems of Mercury and Earth.

While it is stated that a resolution of 20 grid cells per inertial length is needed to get fast reconnection, the manuscript presents simulations that only achieves 5 cells per d_i (line 154), so the criticism of previous work (for example lines 50, 55, 60 etc) with respect to insufficient resolutions seems unfair.

The paper emphasizes repeatedly that Chen et al 2019 claims that dawn-dusk asymmetry requires electron physics (lines 28 and 245), but in reality that paper compares Hall MHD and MHD-EPIC simulations and finds that both show some asymmetries, but they are not the same. Given the hugely amplified ion-inertial length in this manuscript and the similar grid resolution of only about 5 cells per d_i, it is unclear why these results would be more applicable to Mercury than the results by Chen et al 2019, or why the general conclusions found here are better than the conclusions drawn specifically for Mercury by Chen et al 2019.

Minor issues:

Line 67: while GPUs can help, it is not at all clear how much. How many GPU-s were used? How many CPU-s would achieve similar performance? How long does the simulation take?

Line 88: the usual notation is B_0, not B_g. While B_0 is used elsewhere, probably that's the one the authors should rename.
Also, splitting the magnetic field is not Powell's idea.

Tanaka 1994, JCP 111, 381 is a better reference.

Line 128: Hall MHD has the whistler waves. This needs to be addressed. How do the authors handle it? What is the whistler wave speed compared to the fast magnetosonic wave speed? How do they ensure numerical stability? These issues are extensively discussed in Toth et al 2006. The authors should explain how those are addressed by their code.

Line 146: fixing the tangential component of B_1 (not B) is somewhat unusual.

Line 156: the size of the computational domain should be given (in R_E).

Equation 9: the offset should not exceed B_0 (to be renamed to B_g). Is this checked by the fitting script? Should be mentioned.

Line 264: I don't know what ANGEO policies are, but "code availability" is not the same as "algorithm described in detail", and in fact, it is not described in detail.

Several figures have no scales: figures 2, 4, 5, 6, 8 and 9. There should be color bars with physical units.

The authors should add an extra figure with the density and inertial length distribution. The inertial length in the solar wind seems to be about 1/12 Re with the above parameters (rho=5amu/cc). Where does the 1/70 come from?

Typos and minor corrections:

Line 34: creates *an* electric field

Line 71: the the

Line 110: *a* user-set

Line 111: the calculation of the current density is different from Toth et al 2008. That difference actually matters for an implicit solver, and it probably does not matter at all for an explicit solver. It would be best to delete this sentence.

Line 122: the non-conservative form does not lead to a loss of accuracy. It leads to incorrect jump conditions across shocks. Since the bow shock of Earth is not magnetically dominated, the error is relatively small. Anywhere else, equation 7 is just as accurate as equation 3.

Line 135: 1/60 and 1/70 are not that different. Delete one of the fractions.

Line 141: B_g=3000 B_0 = 30,000 nT is only true on the magnetic equator at r=1 R_E.

Line 165...: use physical units, not t_0

Figure 5 caption: cyan line -> dashed cyan lines