Annales Geophysicae Discussions

Ann. Geophys. Discuss., author comment AC1
https://doi.org/10.5194/angeo-2021-35-AC1, 2021

Reply on RC1

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Author 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-AC1, 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.

RESPONSE: We do not believe there to be any inconsistency in describing the simulated system as both “small” (relative to di) and “Earth-like” (relative to the magnetopause and bow shock standoff distances). However, we will change our language to use “ion-scale” instead of “small” in order to emphasize that our system size is much closer to di than Earth’s would be, even though the rest of the system parameters are representative of Earth.

We note also that there is no ambiguity here with respect to Mercury. Mercury lives in a different magnetospheric parameter space (different dipole strength, different solar wind parameters). However, since both this ion-scale Earth and Mercury share a similar scale size with respect to the ion inertial length, these magnetospheres have important similarities in how the Hall effect impacts plasma behavior and observables. These similarities may shed light on MESSENGER observations.

Yes, these are close to the normalization values we used. We have added the normalizations to the problem setup in order to facilitate discussion and comparison. We will change to use physical units where convenient and appropriate (e.g.

“minutes” instead of t_0, R_E instead of L_0).

A minor correction: since di depends on the square root of the density, the ions in your example would actually be closer to 70^2 amu. Our selected normalization parameters leads to ions weighing 3942 amu.

>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.

RESPONSE: Yes, “ideal MHD” is a bad typo and our mistake. Thank you for catching that. We have clarified that the MHD portion of MHD-EPIC is **Hall** MHD. We have also clarified that it is unclear how the embedding and boundaries of the kinetic regions within Hall MHD affects the local-global feedback dynamics. It may end up not significantly mattering, but we will not know for sure until we are able to compare global magnetospheres from full kinetic, Hall MHD, and coupled kinetic-HMHD simulations.

>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.

RESPONSE: This criticism was meant to apply to our work as well as previous work by other groups, and leads into the following paragraph (L70) which argues that such global models need enormous computing power. GPUs are one possible way to provide this power.

We have modified the introduction to emphasize that *all* current work, including this paper, fails to meet this 20 cell/di limit. We have also clarified that we mean 5 cells/*solar wind* di; the simulation is able to obtain 10-20 cells/*tail* di (see new di figure as per minor comment below).

>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.

RESPONSE: We wanted to emphasize to the reader that, at minimum, the Hall effect can produce tail asymmetry. We note that Chen+2019, in the last paragraph of Section 4 states:

<<<“In order to demonstrate the importance of including physics beyond Hall-MHD, we compare the MHD-EPIC simulations with pure Hall-MHD simulations. Figure 14 shows the evolution of plasma jets and Bz for Hall-B simulation. This simulation does not show any significant dawn-dusk asymmetry and the results are quite different from those of the MHD-EPIC-B run.”>>>

Additionally, Chen+2019, in the conclusion, state:

<<<“There are not any significant dawn-dusk asymmetries of the reconnection products in the Hall-B simulation. In general, Hall-MHD simulations do not appear to match observations very well in terms of dawn-dusk asymmetries of magnetic reconnection. MHD-EPIC simulations contain more physics than the pure Hall-MHD simulations due to the kinetic treatment of both electrons and ions by the PIC code.” >>>

Unless we are misunderstanding, these statements by Chen+ seem to imply that kinetic effects are required for dawn-dusk asymmetries, and that Hall MHD is not sufficient. Our results demonstrate that it is possible for Hall MHD to produce *significant* tail asymmetries. However, we acknowledge that we did not simulate the same system as Chen+2019 nor use the same code, so there may be additional nuances that are not being considered. We will be more explicit in stating that our simulations, along with Chen+2019 and Dong+2019, support the idea that tail asymmetry is an universal consequence of the Hall effect in ion-scale magnetospheres (and not specific to magnetospheres that are physically small, like Mercury). We are not superseding any previous studies, but are adding to them.

We also have modified our discussion to make it clear that, although the ion-scale Earth and Mercury simulations have similarities, there may be important differences due to the different physical scale lengths (i.e. bow shock and magnetopause standoff distances). Further studies are needed to understand completely the nuances of how all these length scales interact.

>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?

RESPONSE: For this simulation, we used 8 NVIDIA K20x GPUs. From internal testing of the magnetosphere code, one K20X GPU is comparable to the equivalent of about 70-80 CPU cores. This comparison was made using the same `mpiexec –n 8` command for both GPU and CPU codes: we took the benchmark run time for 8 GPUs and compared it to 8 CPU nodes, resulting in a speedup of about 80x. However, since we did not use an optimized CPU code (e.g. with OpenMP to use multithreading/cores on the CPU nodes), we interpret these results to mean that 1 K20X is roughly equal to 80 CPU cores (not nodes, which may have up to 16 cores each) for this application.

We note that there are several caveats with this particular timing: 1) the CPU code was not optimized; 2) the K20x GPUs used here are now obsolete (newer V100s are about 1.5-2x faster) ; 3) we do not use AMR or other similarly intensive algorithms which use load balancing to redistribute grids/calculations across processors (which would decrease the overall speedup due to GPU memory loading/offloading).

The ideal MHD portion of the simulation took about 2 days to simulate 240 t_0, the Hall MHD portion took about 10-11 days to simulate 57 t_0. The 8 K20x GPUs were able to do about 3.2 steps/second for ideal MHD, and 2.9 steps/second for Hall MHD.

>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.

RESPONSE: We have changed B_g to B_0, and changed B_0 to other names where appropriate.

>Also, splitting the magnetic field is not Powell's idea. Tanaka 1994, JCP 111, 381 is a better reference.

RESPONSE: We have added the Tanaka reference. Thank you for pointing this out.

>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.

RESPONSE: Since this is a explicit Hall MHD code which uses the Courant condition, we follow Huba 2003 and Toth+2008 in using the whistler wave speed to estimate the maximum wavespeed. We have added this detail to the manuscript.

The steady state timesteps became about 20-25x smaller after switching over to Hall MHD.

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

RESPONSE: We acknowledge that this is not typically done. As we explained, we had difficulties with density depletion near the inner boundary and experimented with several different combinations of floating/fixing variables at the boundary. The one that worked best to produce a stable evolution without density depletion is presented in the paper.

We have made it more clear that this boundary prescription was arrived at via experimentation, and was driven by numerical considerations rather than physical considerations.

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

RESPONSE: We have added in this information. In normalized lengths, the domain size was $-35.6 < X < 122$, $-86.4 < Y,Z < 86.4$.

>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.

RESPONSE: As long as one side of the current sheet sample has $B_x > 0$ and the other side has $B_x < 0$, the best-fit offset will always be less than the best-fit B_0 (which we have renamed to B_a). Indeed, checking the fitting data shows that the offset is less than B_a for each valid fit ($L>0$, $chi\_sq < 0.01$).

>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.

RESPONSE: We will seek clarification on this policy.

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

RESPONSE: The plots have been remade to add colorbars, and the normalization values are added to the captions. Other changes have been made in accordance with Referee 2's comments.

>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?

RESPONSE: For the real Earth, di = 101km = 1/70 R_E in the solar wind with the given parameters in the paper. In the tail, due to a lower density, Earth's di is closer to 1/15

R_E. We are using these numbers for comparison with our simulations. We have rewritten the discussion to clarify these points.

>Typos and minor corrections:

>Line 34: creates *an* electric field

>Line 71: the the

>Line 110: *a* user-set

RESPONSE: Thank you for pointing these out. These typos have been fixed.

>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.

RESPONSE: We have removed 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.

RESPONSE: We have modified the discussion accordingly.

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

RESPONSE: Removed.

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

RESPONSE: We have now clarified this.

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

RESPONSE: We have added physical units wherever appropriate.

>Figure 5 caption: cyan line -> dashed cyan lines

RESPONSE: The caption has been changed.