Report #1

Author general comment

We would like to thank the reviewer for reviewing manuscript "ICME impact at Earth with low and typical Mach number plasma characteristics" and thus helping to improve it. We considered carefully every comment made by the reviewer and prepared responses accordingly. Please find our responses to the comments below.

General comments

Reviewer report on paper "ICME impact at Earth with low and typical Mach number plasma characteristics" by Antti Lakka et al. The authors present their analysis of global MHD (GUMICS model) simulations of solar wind-magnetosphere-ionosphere system during two interplanetary cloud events. They compute the magnetopause standoff distance and magnetic fields along the trajectories of magnetospheric spacecraft (and compare them to empirical models and spacecraft observations), estimate approximately the amount of energy transferred into the magnetosphere, and analyse the potential drop applied in the ionosphere (CPCP) which characterizes the intensity of global convection.

The main problem for me with this paper is that it actually tries to adress two related problems, physical effects (ICME impact differences, e.g. saturation) and technical aspects (validation of GUMICS computation results). Second aspect is crucial because, if the computed values are wrong and do not characterize the reality, they can not be used to study the physics in the magnetospheric system. Unfortunately in the paper only the first problem is formally claimed as a paper goal: all Introduction, the paper title and most of the abstract are about the properties of ICME. As concerns the results - the only ICME-related conclusion (last line in the Abstract) is that 'CPCP saturation is affected by the upstream conditions, with strong dependence on the Alfvén Mach number'. In such formulation this is actually well-known from many previous studies, including simulations. So - no new results??

We thank the reviewer for this comment. It forced us to rethink what we want to say in the paper and obtained new results. The leading thought of the paper is to 1) consider two different ICME events and observe if they produce different effects on the magnetospheric physics by considering several parameters and 2) assess how GUMICS-4 reproduces those events by providing an uncertainty estimate with every parameter. We e.g. show that the accuracy of GUMICS-4 results is dependent on the magnetospheric region under inspection. We have now improved the exposure of the technical aspects starting from abstract and introduction (see pages 1 and 3).

My impression is that throughout the paper the authors are under a strong pressure of technical aspects because the GUMICS validation results are not very optimistic: the B-field comparisons demonstrate big differences between the predictions and observations (whose origin is not identified); the computed CPCP values are much lower than usual ones; their values differ significantly between two simulation runs at standard and doubled resolution, and there is no confidence that the high-resolution run reached the optimum (CPCP values are still low to my view). No clear conclusions about validation success were done in the discussion/conclusion sections.

We agree with the reviewer in the sense that GUMICS-4 produces different results when compared with e.g. in-situ satellite observations or measured polar cap potential. It is not even a surprise, since there have been many studies before reporting how well GUMICS-4 (or any other global MHD code) captures the magnetospheric dynamics. The problem is partly due to MHD physics not being sufficient, but also partly because the compared quantities may not represent the same quantities at all. For instance, a corresponding observation for global MHD CPCP is hard to find. Some studies have used PCN index, which doesn't really represent a global CPCP value. Others have used potentials deduced from ionospheric radars, but still do not capture the entire polar cap area. Given these difficulties in the validation, our best approach is to use well-known references, validate simulation results, report the shortcomings of our validation, and assess how well we succeeded. This is just what we do in our paper and we hope that it is now easier to see especially in the discussion and conclusion sections of the revised manuscript.

A big general problem with GMHD simulations is that, for the same solar wind inputs, different GMHD models (their runs at comparable resolution) provide very different answers for essential output parameters (incl.global parameters) –see Gordeev et al. (Space Weather 2015, 2017). For some parameters like the MP standoff distance, the deviations between models were not large. For some other parameters like CPCP and total field-aligned current, their values differ greatly between models, with GUMICS showed too low values of both global variables. This problem is not distinctly articulated in the paper, although a common need of truly global and accurate simulation models justifies paying attention to the technical (validation) aspects as well.

Different GMHD models have different strengths. Deviations in e.g. CPCP values are caused by differences in how the models handle excessive amount of electric current through the polar cap, which causes some models to underestimate, others to overestimate CPCP. Using GMHD model requires knowledge of the general features of the model performance and understanding their strengths and limitations. Sheding light to this issue is one of the key targets of this paper. From our paper point of view, comparing the time evolution of e.g. CPCP between GUMICS-4 and reference parameter is important. This aspect is articulated better in the revised manuscript.

In view of these problems, I believe, the paper in the existing form can not be recommended to the publication. However, I believe, the authors still have potentially interesting material in hands and can possibly find a proper balance between two (physical and technical) aspects to reorganize the manuscript, to clearly formulate and answer the main questions to be addressed, and to expose the new results as a response to the formulated goals (not necessarily being all positive?????).

Specific comments

----

p.3 - l.3 to7: Paper goal is not actually explained, you only tell that you do simulations during two ICME events and compute magnetopause, but not – which problem are you focusing on in that paper? What drives your choice of computed characteristics (MP distance, energy input, magnetic fields, . . ., how it helps to reach the goal?

We agree with the reviewer. Those parameters are used because they are strongly affected by (especially strong) ICME events. The goal of this paper is to see how the parameters are affected by ICMEs with different strength AND how accurate GUMICS-4 results are in those (ICME) conditions. To achieve our goal, we use those parameters and compare

simulation results with known references and compute uncertainty estimate. The end of the introduction section hopefully highlights these issues better now. Please see page 3.

Figs.1,2: The energetic particle fluxes are not used in the study??? Why don’t use logarithmic scale for MA, otherwise the values in the most interesting small MA region are not readable from the plot

We thank the reviewer for this comment. We adopted logarithmic scale for MA since it really makes figs 1,2 a lot better. However, even if energetic particle fluxes are not directly used in the study, showing them along with solar wind data provides additional information in a sense that it verifies magnetic cloud onset time especially for the 2012 event; gradual decrease of proton flux is observed at the same time with solar wind density decrease. On the other hand, absence of such flux decrease in 2014 shows that the event truly is moderate compared with the 2012 event.

p.5, comparison of magnetic fields in the magnetosphere. First, it would be natural to place comparison of simulated/observed fields at the end of this paragraph where you show the results, otherwise (as it is now) the discussion of comparisons (now placed in sect.4.3) stays couple pages later from the corresponding figure, very hard to read.

We think that the actual results are better to be found in the same section (Analysis) together with global dynamics results. Otherwise we would have to choose which results we are reporting in section 3 already (just measured Bmag or GUMICS-4 Bmag as well, what about the relative differences shown in the revised figures 4 and 5?)

p.6-26:” Total energy through the dayside magnetopause is computed by evaluating the Poynting flux in the vicinity of the (Shue) magnetopause, and its component parallel to the magnetopause surface normal.” Your method to compute the energy flow is not sufficiently introduced and analysed, although there are big questions. The Shue magnetopause stays at some distance from simulated MP, in the region with large spatial gradients of flow and other parameters; also, the shapes of computed and Shue magnetopauses can be different. That means some portions of Shue MP can be in the magnetosheath (with tailward energy flow), some in the magnetosphere(with sunward Poynting flux near the dayside MP). How can you justify your computations? One way to quickly look on that is to compute energy flows throughout Shue MPs displaced, say by dX = +/-0.2 (or 0.5)Re. Anyway, the uncertainty of such computations should be somehow estimated.

We provide detailed explanation of the used method in the revised manuscript. It is true that the shape of the Shue magnetopause probably differs from the actual magnetopause. Previously, Palmroth et. al. (doi:10.1029/2002JA009446) computed the shape of actual magnetopause from GUMICS-4 results and compared energy transfer to the epsilon parameter. They also showed that the energy perpendicular to the boundary did not change with small displacement of the boundary thus demostrating the robustness of the method to calculate the incoming energy. Instead, we consider the Shue magnetopause surface by displacing its nose 30% Sunward. We use 30% since it is maximum relative difference in magnetopause position between GUMICS-4 and the Shue model. This prevents underestimation of the size of the magnetosphere. The method used here gives values for energy of the same order of magnitude compared to study by Palmroth (mentioned above). Thus, we have good confidence in the methodology. See pages 8-9.

pp.6-7: When validating MP and CPCP it would be reasonable to compare with empirical values for those conditions. I would also recommend to compare your results with Gordeev et al.(2015, doi:10.1002/2015SW001307) validation effort, where the empirical data have been used for testing (e.g., their Fig.9).

We agree. In order to be consistent when using references, we have compared MP, energy transfer and CPCP to known references. For MP the reference is the Shue model, for energy tranfer it's the epsilon parameter, and for the CPCP it is PCI (Ridley, Polar cap index comparisons with AMIE cross polar cap potential, electric field, and polar cap area, (2004)) deduced from PNC index. All of these have been used in previous studies and are easy to plot alongside GUMICS-4 results. Comparisons to PCI and epsilon were missing from the previous manuscript version, but are added in the revised version (see figures 6 and 7 and section 4.1). Moreover, we provide a framework to our study by comparing our results to work by Gordeev (see Discussion section).

Section 4.3.Local dynamics. I think, a so big difference of magnitudes between GUMICS predictions and actual observations in Figs.4,5, a two-fold differences (or more) in many regions, is a kind of bad news for GUMICS validation. However- no analyses is provided – what was wrong in simulated field in these regions? How much the total pressure is wrong? Which components are most affected, etc?? Why don't you show the traces of high-resolution run results on Figs.4,5, are there differences between two runs? I don't see any conclusions from these comparisons, it may not be a good idea to show such bad agreement without explanations.

We agree. The discussion of the results in this section is now improved. We show that accuracy of GUMICS-4 is dependent on which part of the magnetosphere is considered. The absolute value of Bmag in GUMICS-4 agrees better when Bmag is high (S/C is close to the Earth).

An interesting aspect: if the saturation works under total FAC being an order of magnitude smaller than real , it may show that magnetospheric mechanisms (e.g. the FAC influence on the dayside magnetospheric magnetic field as suggested by G.Siscoe et al) do not contribute to the saturation effect. This can be a useful side result in case if your high resolution is not yet sufficient to increase CPCP and total FAC toward realistic values..

Thank you for this comment. We considered it carefully in the text.
