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Interactive comment on "ICME impact at Earth with low and typical Mach number plasma characteristics" *by* Antti Lakka et al.

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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 as-

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Discussion paper

pects (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??

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.

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.

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?????).

Some specific comments are given below. ==
Abstract: it mostly explains what you did, only 1 line (of 11 lines) tells what you obtained (your results).

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?

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

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.

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.

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).

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.

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.. ——————————————————————————-end review

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