

Reviewer report on paper "ICME impact at Earth with low and typical Mach number plasma characteristics" by Antti Lakka et al

The authors invested efforts to improve the manuscript by addressing the referee's comments. They reformulated motivation, added the GUMICS validation as one of two main goals, added couple more reference parameters (PCI and ϵ) for validation purposes, computed the deviations between predictions and (now in Table 3-6) and extended the discussion (paper expanded a lot and now the text is on 17 pages as compared to previous 12 pages).

Still I have to reiterate two previous conclusions. One is that the paper does not expose any new result related to ICME effects on magnetospheric dynamics (one of two goals of the paper).

Another one is related to the additional statement (which appeared in both abstract and Conclusion section), that "GUMICS-4 results are in a good agreement with the reference values."

I can not accept this statement, because the materials presented rather show the opposite things.

(1) In fact, statistical evaluation gives good marks (deviation of the order of 5%) only for standoff distance parameter (which, by its physics, is mostly dictated by SW flow pressure, has rather small relative variations, and all MHD models agreeing rather well with predictions in the large range of conditions, as was also showed in Gordeev et al. SW 2015 paper). This is not much surprising and quite expected, even the simple Chapman-Ferraro model with pressure balance provides quite successful predictions of the subsolar nose distance variations.

(2) The validation success critically depends on the choice of 'reference parameters', intended to show the realistic values of key global characteristics of the system to be compared with model predictions. I was surprized by your choice of PCI index as a reference to cross-polar cap potential drop (CPCP), which is actually an indirect uncalibrated proxy (based on PC index value) with unknown (and hardly good) accuracy. Why not using the potential model by Weimer et al (JGR 2005), based on direct E-field integration results from thousands of direct DE2 polar cap crossings occurred under various SW conditions and parametrized by SW parameters? Or some other data-based representative proxy?? Anyway they will be more representative compared to your magnetogram-based proxy.

In the same way I hardly can take seriously the ϵ -parameter as a realistic reference to actual energy consumption, even although it is used as such in a number of studies.

As you also mention in the text, that these indices are very indirect proxy, but still continue to use them in quantitative comparisons. I believe the material in Table 3 you complete and corresponding discussions is irrelevant. Anyway it also shows rather bad agreement (relative difference of 30 to 70% for average values).

(3) Using B-field measurements in different parts of the system is a good (although difficult) choice.

By some reasons you compare only difference of B-magnitudes (magnitude of vector difference will be larger), but even with this choice you systematically infer very large deviations (40 to 80% in Table 4, 35 to 60% in Table 5, using different averaging rules), indicating that GUMICS predicted significantly smaller magnetic fields everywhere in the system than those which Geotail and Cluster actually measured. For me this would be an indication of a kind of disaster in the code performance.

This can not be explained by a manifestation of local mismatch between predictions and observations (which can sometimes affect the temporal variations or similar), because the effect is a system-wide one. The origin of mismatch is NOT discussed and it is not investigated in the paper.

SUMMARY: Big mismatch in predicted/observed B-field and also between CPCP values seem to indicate serious problems in the GUMICS performance. (Good consistence for magnetopause stand-off distance is of little consolation because it is well predicted by all models, starting from Chapman-Ferraro model.) I would rather reformulate your statement as "GUMICS-4 results are in a serious disagreement with the reference values."

In this situation it seems that there are no sufficient reasons to suggest the publication of this paper

-----end review