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The Cryosphere Discussions

Interactive comment on "The GRISLI-LSCE contribution to ISMIP6, Part 2: projections of the Antarctic ice sheet evolution by the end of the 21st century" *by* Aurélien Quiquet and Christophe Dumas

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This paper presents a detail of ISMIP6 Antarctic ice experiments using a numerical ice-sheet model GRISLI. In my opinion, it is worthwhile to present detail results of an individual model to participate an intercomparison project, because the corresponding main paper usually focuses on general feature among the participants. I think this paper is fairly well written with some exception below, and can be accepted with minor revision.

There is one relatively major point in the manuscript, which is argued on the experiments shown in Figure 12. In the text the author mentioned that (P12L16): 'A uniform reduction of the basal drag coefficient by 30% leads to a 13000 km3 total volume reduction contributing to about 50 mmSLE in 2100. This means that, with our model, it is unlikely to obtain a significantly different ice volume change for slightly different basal initial conditions.' I do agree the former sentence, but I am not sure what the authors mean in the latter. Is 50 mmSLE insignificant? Or, is 30% change in the basal drag coefficient already too large to be worried about that expected contribution is much smaller than 50 mmSLE? The authors do not provide the inferred basal drag coefficient map in the manuscript. Le clec'h et al. (2019) present the basal drag coefficient map, but for GRISLI Greenland simulation. In this basal drag coefficient map, at least in Greenland ice sheet, the coefficients seem to vary more than a factor thousand. If this factor holds true also for Antarctica, 30% changes in the coefficient may be far smaller than the variation of the coefficients. I appreciate if the author extend this discussion to describe clearer from the experiment design. Moreover, there are not enough information about the sensitivity experiment for the ice enhancement factor, which should be extended.a

Minor points:

P3L9. the abbreviation SSA should be inserted as SIA.

P3L9 and Eq.(2) It is confusing to describe SSA is as a sliding law while a linear till parameterization (2) is used as sliding velocity. Better to explain clearer.

Sect 3.1 and others. There are not a few names of glaciers and the region without explanation. I know that this journal is the Cryosphere and many readers are familiar with such local names, however, I really appreciate if the author show a map of these locations for better understanding of result description.

P7L3, about RMSE of simulated velocity fields. I am interested in the relative rank of RMSE of simulated topography (thickness) by GRISLI. I suspect that the dispersion in

the simulated topography by the participants are smaller than that of the velocity, but I want to know whether GRISLI's errors are both large or only velocity is large among the participants.

P7L14, resemblance of patterns between Fig.1a and b. Why not show a figure of correlation?

P8L12 '... suggesting increased precipitation in the future'. As far as I understand the experiment protocol and the mentioned in the next sentence, changes in simulated ice sheet volume never suggests the precipitation increasing, but it originates from the boundary condition. Please rewrite this part.

Figure 2 and other velocity figures. The range of smallest velocity color (white) is not explicitly written. Or I suspect that it is from +1 m/yr to -1 m/yr, because there are three color boxes between 10 and 100 or 100 and 1000 while only 2 between 1 and 10.

Figure 6 and other evolution figures. Adding numbers of sea-level equivalent height to the ice volume axis (a) will help to compare with (b).

Figure 11b. I do not understand the rule of annotations in the color bar between 0.1 to 10 and -0.1 to +0.1.

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