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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.

Thank you for your positive evaluation. We address your concerns in the following.

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.

We agree. As in Le clec'h et al. (2019), the basal drag coefficient in Antarctica shows a very high spatial variability. This coefficient can vary from ~1 to 10^5 Pa yr m^{-1}. However, in practice a value above 10^3 Pa yr m^{-1} produce very limited sliding velocities. Also, the absolute value of the basal drag coefficient has none or a limited impact in the interior of the ice sheet, where the SSA velocity is small anyway, but is very important in the coastal regions, where the ice streams are located.

Our approach is very simple as we applied a uniform perturbation. It allows for an artificial speed-up of the ice streams but it is not suited to investigate realistic changes that could occur in the future. For example, for a realistic ice sheet, it can be envisioned that a grounded point switches from a state where it slowly flows to an ice stream state. In this case, in the model, it means that the basal drag coefficient switches from a value greater than 1000 to lower than 100 Pa yr m^{-1}. To test such phenomenon in the model we could apply a random noise in the basal drag coefficient with much larger perturbation than the one we used here.

The problem is that we only have one map for the basal drag coefficients, being the one obtained after the initialisation procedure. Ideally we should have tested alternative maps. However, if such alternative maps not resulting from our inversion were used, it would have resulted in unwanted drift in the control simulation. The use of these maps would be difficult to justify.

We added a number of new simulations in order to get an idea of what range of values for the uniform perturbation is acceptable. We now perform new ctrl_proj simulations in which the basal drag coefficient (and the enhancement factor) is perturbed in the same way as the NorESM1-M projection shown in the initial version of the manuscript. We computed the volume drift of these perturbed ctrl_proj experiments and compared it to the volume drift in the standard ctrl_proj experiment. We consider that a 0.15% difference between the standard ctrl_proj experiment and the perturbed one is acceptable. We chose 0.15% of the volume difference since it corresponds to 10%

of the change in volume simulated in 2100 using NorESM1-M under RCP8.5 (medium oceanic sensitivity meanAnt).

This discussion has been moved from the discussion section to the results section. It has been largely rephrased and extended as well.

Minor points:

P3L9. the abbreviation SSA should be inserted as SIA.

Added.

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.

We simply rephrased to:
"For temperate regions, we assume a linear basal friction (Weertman, 1957):"

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.

We have added a map as Fig. 1.

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.

The RMSE of simulated ice thickness was given in P6 L22-23 of the original manuscript. It is about 120 metres and it is the 5th lowest in the ISMIP6 ensemble (21 models). As a result, compared to the other participating models, only the velocity error is large for GRISLI.

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

Not sure what you meant. The spatial correlation between two 2D variables is a scalar right? We computed a Pearson correlation of 0.24 between the two variables shown in Fig 1a and b. We have added this value in the main manuscript. This relatively low value can be explained by the noisy signal of the ice thickness difference between the end of the historical experiment and the observations.
To visualise this correlation we could plot the thickness difference at the end of the *ctrl_proj* as a function of the thickness difference at the end of the historical experiment *hist*. We show this figure in this response (Fig. R1). We are unsure if this will bring additional value? If yes, we would be happy to add such a figure in the paper.

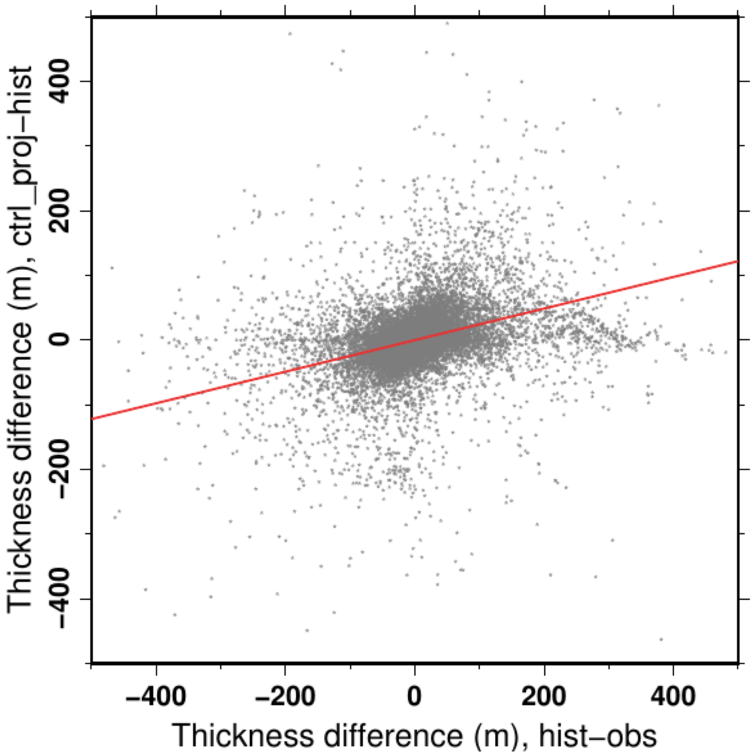

Figure R1: Ice thickness difference at the end of the control experiment ctrl_proj with respect to the end of the historical experiment as a function of the ice thickness difference at the end of the historical experiment with respect to observations (Frettwell et al., 2013). The red line represents the linear regression with a correlation value at 0.24.

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.

Modified for:
"In addition, except under the HadGEM2-ES forcing, the Antarctic contribution to global sea level rise is always smaller than under the control experiment under constant present-day forcing. This suggests that the climate forcing computed from the GCMs in the future leads to a larger integrated total mass balance compared to our reference present-day mass balance. In fact, most GCMs simulate an increase in precipitation in Antarctica related to the projected warming."

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.

We have changed the colour scale. The range -1 to 1 m/yr is white. This is now specified in Fig. 3 (former Fig. 2).

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).

We are not sure what you want us to do here. To express the volume shown in (a) in sea-level equivalent instead of in km^3 (or Gt)? We prefer not to do so as it might appear confusing for the reader to express in cm SLE a volume change that is not contributing to sea level rise. What we could do instead is to express all the volume changes in Gt instead of using the sea-level equivalent. However, we think that most people are interested in the sea-level equivalent so we prefer to use this unit. In order to facilitate the comparison of the two panels, we have added the conversion factor (1 mm SLE = 372 Gt) in the figure captions when applicable.

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.

We have changed the colour scale. The range from -0.1 to 0.1 m is white. This is now specified in the caption.