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Interactive comment on "A glacial systems model configured for large ensemble analysis of Antarctic deglaciation" *by* R. Briggs et al.

Anonymous Referee #1

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General comments

This article describes a glacial system model that was developed for data-analysis of past Antarctic ice sheet evolution with an ensemble approach. The paper essentially describes all the parametrizations that lead to 31 ensemble parameters. All components of the ice sheet model are considered (basal drag, climate forcing, calving, sub ice shelves melting, . . .) and I appreciate the fact that, in each case, physical processes are central to the study. This approach is original and represents a huge amount of work, however, the paper is very difficult to read and very often the validity of parametrisation is difficult to assess just because it is not illustrated enough. Presently, this article is written for the few modellers who will want to use these parametrisations in their own simiulations. I think this work deserves a larger audience and to do so must

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be substantially improved in its presentation. Beside editing comments, the two major points are to systematically illustrate the dependency of variables with parameters and to add maps of reconstructed characteristics. In some cases, it can be for the baseline run (nn2679), in other case, minimum and maximum maps are more relevant.

The names of parameters make the reading difficult. I understand very well that these names may be used in the code but more readable or even pronounceable (try zcrhslid) names would help. Morover, I doubt that many people are able to remember the names of 31 ensemble parameters, more than 50 other variables, and 6 metrics. In the text, please recall a short definition as often as necessary to make the reading fluent.

It would be useful to have a typographic way (name, font, Bold ...) to make the difference between the 31 ensemble parameters and the characteristics that are derived from these parameters (Slk, Se, ...). In particular, when these characteristics are maps, please mention it (Se(X) for instance). It is done in the climate forcing part, but not in the basal drag one. Same comment in table 2

To better illustrate the dependency of variables with parameters, an example would be a figure with fstd as a function of Se for various roughness. This is only an example, the same type of plot should be given for most parameterisations.

A map of basal drag at present time should be given because it would help comparison with other works. This can be done for the run that gives the smallest misfit with present topography, (or for the baseline run). For the same run, please give a map of velocity compared with observations.

A table with the metrics would be useful. There are not that many otypes of observation over the ice sheet and I regret that the observed present velocities are not used as an additional metric.

The parametrisation of climate forcing is very clever. To illustrate the time evolution would it be possible to have the envelope of the climate record in ice core locations (for

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instance Dome C, Dome Fuji...) so it could be compared with ice records ?

The part concerning sub ice shelf melting, with 3 different processes is the best approach one could have as long as no real coupling with the ocean is possible over these long time scales.

At the end a sensitivity study is presented, and here also it could be better illustrated. For instance a plot of the envelope of the grounded volume evolution, with the baseline run. As a conclusion, the scientific content is interesting and useful but the presentation must be strongly improved.

Detailled comments

p. 1535- line 24. The phase space is larger but the dispersion between reconstructions might be reduced if the complexity comes from adding a physical process

p. 1540. equation 1. The exponent of the flow law could have been a parameter as well . As Cuffey and Paterson mention, it could depend whether basal drag is governed by sticky spots and sediments properties. Can the authors justify their choice ?

p. 1542 line 12. Considering the non local aspect of both the SSA equation (even in its hybrid form) and mass conservation equation, the impact of crh on ice thickness is non local so the observation-model misfit might not be related to local crh. Can you comment on this point ?

p. 1542. equation 4. All these details are very useful if someone wants to reproduce the experiment or use this parameterization but it seems a bit “magical”. Why for instance, zcrhsed/zcrhslid is at the power Se and not multiplicated by Se.

p. 1544. line 7 This model participated in the MISMIP3D. Could you please state how it compared with the other models.

p. 1549 equation 12. Please gives the dimension (1.5 ???)

p. 1552. equation 21. the case Hcrit2 < H < 3000 and Ts < -3°C is missing

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p. 1553, lines 12-16. unclear.

p. 1563 line 21. Surface temperature defined at 391 ka. For which values of the surface temperature parametrisation ?

p. 1565. lines 1, Is the grounded ice volume the full one or the one above flotation ? In the first case, it is not fully consistent to convert in sealevel equivalent (ex. Line 21).

Interactive comment on The Cryosphere Discuss., 7, 1533, 2013.