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Interactive comment on "Reducing uncertainties in projections of Antarctic ice mass loss" *by* G. Durand and F. Pattyn

Anonymous Referee #2

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By combining and comparing output from ice dynamics models with a large range of complexity, the authors argue that future inter comparison projects, which use model ensemble means to improve predictions of ice sheet mass loss, should only contain MISMIP (and its successors) approved models. They show that results from a simple model with a biased (i.e. wrong) description of the grounding line dynamics are not significantly different from results previously obtained in the SeaRISE inter comparison, which mostly employed models that did not comply with MISMIP requirements. They further show that the spread in predictions of future mass loss of Pine Island Glacier is largely reduced when only MISMIP approved models are included in the ensemble.

I agree with the authors that recent advances in model verification (incl. MISMIP and MISMIP3d) have unambiguously shown the importance of a correct description of the

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grounding line dynamics. Only when this dynamics is described correctly can we as a community attempt to proceed towards model validation and reliable predictions. In this light, this is an important paper, which provides newly acquired evidence that only models which contain the correct physics, should be used in ensemble predictions. Other models might produce equally compelling results, though for the wrong reasons. Although such conclusions have been reached in less clear terms before, this work sheds a new (qualitative and quantitative) light on the matter, and publication of this work is recommended.

The authors provide two important test cases, a pan-Antarctic and a basin scale case. Only for the basin scale case, a comprehensive comparison is provided between MISMIP and non-MISMIP models. It would also be interesting to see how MISMIP approved models, such as [Cornford et al., 2015, doi:10.5194/tcd-9-1887-2015] perform in the pan-Antarctic case. As such results have been obtained in the aforementioned reference, it is presumable not a major effort to include those. Do you expect similar conclusions to emerge as in the basin scale case, i.e., are the mean mass loss and the spread reduced?

In the abstract, and later in the main body of the manuscript, there seems to be a contradiction. On the one hand, it is stated that the "representation of the grounding line dynamics is essential to infer future Antarctic mass change" and yet, "the biased model (with wrong GL dynamics) can hardly be discriminated from the ensemble based on its estimation of volume change". Does this mean that GL dynamics is important, but models without GL dynamics can produce similar predictions in mass loss (but for the wrong reasons)?

Although differences in described physics seem to dominate the spread in Figure 4, even for models with the correct first-order physics (dark shaded area), inter-model differences are large compared to differences within a single model for e.g. different melt rate parameterization. This was a surprise to me. Should this be a reason for concern, as ultimately we want to obtain a range of predictions based on uncertainties in physical parameters such as mass balance, sliding etc., rather than model design? Does this mean that only full-Stokes models should be used, as they describe the full physics?

A minor comment about the Figures: It is difficult to see the shading of blue in Figure 1. Similarly the dashed black and red lines are difficult to see in Figure 4.

2 small typos: abstract l11: biais -> bias p2632, l10: a -> at

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Interactive comment on The Cryosphere Discuss., 9, 2625, 2015.