Review on “Glacier dynamics over the last quarter of a century at Jakobshavn Isbrae” by I.S. Muresan *et al.*

The paper presents an application of the PISM ice flow model to the region of Jakobshavn Isbrae. The paper discuss the results of a forward run from 1990 to 2014 that gives the best match to the observations. They identify 2 distinct flow accelerations in 1998 and 2003. A long discussion is devoted to the fact that the model does not reproduce the velocity peak in 2012. This is attributed to the exceptional long melt season.

The velocity peak in 2012 (and in 2013) has been previously attributed to the retreat of the calving front to an area of deeper bedrock (Joughin *et al.*, 2014). In his interactive comment I. Joughin already replied that there is no such over-deepening in the bedrock topography used in the paper. I agree with this comment and the paper fails to demonstrate that there is a causality relationship between the anomalous melt season and the model not reproducing the observations.

As stated in the paper “the details of the processes triggering and controlling thinning and retreat remain elusive” (p4867; lines 26-27). And “investigate the processes driving the dynamic evolution and the seasonal velocity variation of JI” (p4868, lines 6-7) is an important motivation of the paper, but there is very little discussion on the processes; the model physic and the forcings are too briefly discussed, so that it is difficult to estimate the robustness of the results and how much tuning has been required to match the observations and how sensitive is the model.

Looking at Figure 2 the model does a relatively good job in reproducing the retreat of the front from 1994 to 2014, so I would be much more interested by a detailed discussion on the model physic, the forcing required to obtain this result, and the sensitivity to the parameterisations. The observed seasonality of the glacier front position is at the order of few kilometres which is approximately the grid size in the model (2km). There is a strong sub-annual signal in the model that seems also related to the calving events (but the relation between calving (or grounding line movement?) and variations in the velocity is not really discussed in the paper). So I think discussing the seasonality in the model, especially as I understand there is only a seasonality in the surface mass balance model and not in the ocean forcing, is going too far in the interpretation of the results.

In conclusion I suggest to rewrite the paper to discuss the robustness of the retreat of the front and of the timing of the retreat to the different parameterisations of the model and to the forcing. A sensitivity to the grid size should also be added, especially if the authors want to discuss the sub-annual variability.

Detailed comments:

- Sect 2.1. p4869, line 9 :The grid for the enthalpy is not following the ice thickness? I don't understand the “4000m above the bed”.
- Sect 2.1. p4869, line 3, and p4870 lines 6 and 7: “adjusted to simulate 1990 metrics”, what does that mean?
- Sect 2.1.2 :“Along the ice shelf front we apply a physically based calving parameterisation and an ice thickness condition”; This is a key point of the simulations presented here and what is really applied should be discussed in more details.
- Sect 2.1.3. With the appellation “ocean model component” we understand that there is some kind of dynamic ocean model. But it seems that what is applied here is a parameterisation of the heat flux which at the end depend only the the shelf thickness? So there is no external forcing (and no seasonal forcing) from this side? Again this is a crucial point for the simulations and this is too briefly described. In Sect 3.1 give the values for the melt rates as I think this is an important point in the forcing of the retreat.

- Sect 3.2 Seasonal variations in velocities. Seasonal variations are not really described (and as already said I think that this would be sur-interpreting the results). There is more discussions here in the inter-annual variations. Again I think that the discussion should focus on these inter-annual variations and on the retreat of the front and the sensitivity of the model to the parameterisations.
- Figure 3 bottom. Are the observed thicknesses really in absolute value or have they been adjusted to match the model thickness at the first year? I will be very surprised that after a transient spin-up there is a so good match between observed and model thicknesses.
- In general the figures are poor quality. Revise your colour-scales.