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Interactive
Comment

# *Interactive comment on* "High-resolution simulations of the surface mass balance of Greenland at the end of this century" *by* G. Krinner and N. Julien

**J. Bamber**

j.bamber@bristol.ac.uk

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**General comments**

This is not a review of the paper but I would like to make some observations on a number of fronts. Some of these may be a little coloured by my role as an editor in chief and my desire to ensure high standards for papers in TC and TCD.

The contribution of the Greenland Ice Sheet (GrIS) surface mass balance (SMB)now and in the future to SLR is an important question and certainly one that currently has a large uncertainty associated with it so the aims of the work presented here are commendable. My major concern with this work, however, is whether it really provides with any kind of reliable, quantitative estimate of the future SMB of the GrIS. There are three categorie of uncertainty in the future behaviour of the GrIS SMB: i) uncertainties in GHG emission scenarios ii) uncertainties in how the climate will respond to i) iii) uncertainties in modelling the response of the SMB to a given climate perturbation.

All three of these categories provide uncertainty to predicting the future SMB. The paper by Gregory and Huybrechts (Gregory and Huybrechts, 2006) explores aspects of i) and ii). Fig 4b of that paper indicates the spread of behaviours encompassing different GHG scenarios and structural differences between the AOGCMs. The spread by 2100 is between 0.1 and 3 mm/yr change. For the SRES A1B scenario used here the spread between AOGCMS (i.e. the influence of factor ii) above) is something like 0.5-3.2 mm/yr, which implies a factor six difference due to structural differences in AOGCM alone.

If we now consider factor iii): SMB model physics, the authors will be aware that a recently published paper in GRL (Bougamont et al., 2007) suggests that model physics has a significant ( factor 2) impact on predicted future SMB for Greenland for the same emission scenario. Our concern with the use of an empirical, heavily parameterized approach to calculating melt and runoff (such as the positive degree day method or similar) is that the parameters used have been established for an extremely narrow region of parameter space: i.e. for the present day climate. There is no reason to believe or to expect them to be suitable for a different part of parameter space, such as a future climate scenario. This issue is partly highlighted by Fig. 6, where two different models are used for the SMB producing very different estimates. at 900 m asl, the "Ohmura" model gives a net SMB of -10 cm, the "Krinner" model -120 cm and the obs are -210 cm w.e. These are **big** differences. Admittedly this is a difficult region to get right but these numbers give some indication of the uncertainty in the calculation of SMB and it is, clearly, large.

My next concern is that the key "improvement" or development of this study compared, for example, with the 23 AOGCMs used in the fourth IPCC assessment report (AR4) is a stretched grid that provides "up to 60 km resolution" over Greenland. This is certainly significantly better than the AR4 simulations over Greenland but it does not follow, de facto, that this means that this resolution is adequate for resolving the key processes controlling the SMB. Climate modelling is awash with many examples of apparent improvements to model physics and or resolution that actually produce a worse climatology or parts thereof.

All of the issues above beg the question: what is the uncertainty in the projected SMB estimates from this study? I was always taught that a result with no error bars is uninterpretable. I realise uncertainty is a difficult thing to tackle in climate modelling but for the results of this study to carry any weight, it is essential that the authors address this in some way.

For me, the most interesting and noteworthy aspect of this work was the magnitude and spatial distribution of the change in precipitation over the ice sheet. If this result is robust then it has implications for SMB, not so much because of the amount of precip. but because of the effect this precip. has on surface albedo, refreezing and, therefore, melt rates. Fig 10. is interesting and suggests that the rigorous handling of rain in the runoff model could be extremely important for the estimate of future SMB. The sensitivity of a PDD and EBM-based melt model to snowfall is very different...It would be interesting to see the seasonal pattern of increased precip.

**Specific comments**

The authors do not cite the appropriate references in many places and are using old data sets for i) surface topography and ii) accumulation (line 10 page 357). For the former, it will likely make little difference to the result although in the associated paper the authors show the importance of topographic resolution on melt estimated using a PDD model and how, at resolutions coarser than 2 km significant errors are introduced (Bamber et al., 2001). The most recent accumulation compilation, however, is a very substantial advance and improvement on the data set that they refer to (Bales et al.,



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2001).

The introduction is wandering and loose in language and precision. The opening sentence is misleading, for example. What they are probably trying to say is that "large changes in surface elevation are taking place around the margins of the GrIS". This is not what is written. A much better reference to use than the Science perspective they quote here is (Thomas et al., 2006). At the top pf page 353 they mean (Zwally et al., 2002) not Zwally 2005. Why are they comparing their SMB with the TAR (2001) (line 27, page 357)? These estimates have been superseded by many more recent and more accurate estimates. How about all the papers by Hanna and Box, for example? and so on and so on.

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