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Interactive Comment

Interactive comment on "The Potsdam Parallel Ice Sheet Model (PISM-PIK) – Part 1: Model description" *by* R. Winkelmann et al.

Anonymous Referee #2

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This paper introduces a 'new' continental ice sheet model (PISM-PIK). The paper is divided into two main parts. First, a description of the flow physics in particular the model used to simulate the distribution of stress and velocity within an ice mass, as well as information on a new subgrid parameterization of ice shelf advance and retreat, finite difference discretisation of the mass (thickness) balance equation and calving law. Second, the application of the model to the MISMIP inter-comparison experiments and analysis of the results.

I found the paper disappointing in two ways: first, most of the material is not particular new or novel and, second, the amount of detail of the more novel aspects is very sparse so that it is very difficult to judge the contribution. The area of grounding-line migration is clearly of great importance and warrants the great amounts of attention that it is currently receiving. The bulk of the literature in this area points towards the difficulties of producing meaningful simulations of this process (indeed the paper references some key publications in this area); progress in the area is therefore only possible if results models are clearly described and adequately tested. I do not believe that this is the case with the present draft.

Key issues that need to be addressed include the following. First, a better, clearer account of how the two stress models (SSA and SIA) are implemented and, in particular, the effects of the crude way in which the models are combined (the reference to Schoof and Hindmarsh as justifying this approach is a bit too tenuous). The present version is vague and a little confusing when details are presented; it relies on the reader knowing their way around an ice sheet model and reads very much like extracts from a larger work (where concepts, terminology etc appear without due introduction). The ad hoc addition of the two resulting velocities appears crude and very little effort is made to justify this or to assess the errors introduced. Second, the remainder of Section 2 (calving front stress boundary condition, advance/retreat parameterization, mass transport discretisation and calving law) appears to be a standard reworking of material that has been published several times elsewhere or is presented in a superficial manner. In both cases, it is hard to see what the real advance in understanding is. Finally, the presentation and analysis of the MISMIP results are, again, superficial. If the intention of the paper is to develop some level of trust in the model's predictions so that it can be used in full Antarctic simulations, then far greater attention needs to paid to explaining how grounding-line migration is simulated; it is hardly mentioned while the less relevant and fairly standard implementation of shelf boundary conditions, etc etc is given a great deal of attention.

1278.10. 'naturally emerge' what is meant by this statement? The discussion thus far is limited to models used to solve for stress/velocity, which are purely diagnostic (i.e., no time dependence). The term 'emerge' implies time dependence so make no sense in this context. Clarification required.

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1278.11. ‘membrane stresses’ define what is meant here.

1278.25 onwards. This discussion mixes dimensionality with physics and is therefore very confusing. 1-d. models could have Stokes physics and (perhaps) be computationally cheap. The key issue is dimensionality which is omitted from the second half of the paragraph.

1279-1 why do they fail with ice shelves – this is not inherent to 1-d. models but to a certain type of 1-d. model!

1279-16. More correct to talk about gravitational driving stress here rather than height and surface slope.

1279.20. Need further explanation – combining SSA and SIA is exactly the current paper does so why are the Ritz etc papers different? Clearly, some may be aware of these issues from prior knowledge but this cannot be relied on by the paper; a great deal more effort is required to make the point here.

1280.1. This paragraph reads as if it were the introduction and has little connection to the previous discussion.

1284.10. Equation has pw. (water pressure) where is its determination discussed? This is key because it determines what the relative contributions of SSA and SIA will be to horizontal velocity, and hence the location of fast flow etc etc.

1285.1 onwards. I assume that both the SSA and SIA are found over the whole domain (i.e., that the SIA is used in both fast and slow flow, as is the SSA even though the assumptions on which it is based a clearly violated). It would be worth mentioning this, at present it is not immediately obvious why this approach removes the abrupt changes mentioned on 1285.1.

1285.10. Schoof and Hindmarsh (2010) do indeed present a vertically-integrated model but it is a little more subtle the addition of velocities derived from the SSA and SIA. At least to my reading of the S&H paper, the suggestion that ‘this combination of

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velocities ... yields a good shallow approximation' is a misrepresentation of their work.

1285.10. 'superpose' do you mean add?

1285.15. I do not see how the claim can be made that the two end-member flow regimes 'naturally emerge': they have simply been added together in an ad hoc manner. Supporting this claim with a reference to a cartoon illustrating different types of flow adds nothing.

1285.20. Why is this diagnostic required? Aren't horizontal velocities being found as the sum of SSA and SIA over the whole domain irrespective of being ice stream or not? Is this used for display purposes?

1286-1. The application of stress boundary conditions on the SSA is fairly standard and I do not see how the description in 2.3 adds anything to the literature on the use of SSA.

1288-1. This section is too vague to be of any use. It is not at all clear how the partial cell methodology is implemented or how this affects model dynamics. The parameterization is a 'precondition' for the application of a 'continuous calving law': what does this mean? Is a calving used? What is 'just the right amount' of mass loss

1288-14? The idea of dealing with calving using a partial cell technique is interesting but this section needs a great deal more information before it becomes useful. Is a velocity found in these partial cells; 1288-5 suggests not but how can a mass budget then be determined?

1288-25. What staggered grid is being employed here? We have not been told how the variables are arranged on the FD grid.

1289-3. Equation 25 have left hand side something like Delta Q / Delta x. This is a first-order upwind scheme isn't it?

Interactive comment on The Cryosphere Discuss., 4, 1277, 2010.