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Interactive comment

Interactive comment on "Implementation of RCIP scheme and its performance for 1D age computations in ice-sheet models" *by* Fuyuki Saito et al.

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The authors present a detailed examination of a novel (in ice sheets) interpolation scheme with promise for improved tracing of ice age as well as annual layer thickness reconstructions in ice sheets. This study focuses on 1D examples with scenarios (e.g. mass balance accumulation rates/vertical velocities) typical of the East Antarctic plateau, with direct relevance to ice core dating and age modelling.

The study is comprehensive, with superb attention to detail and to explaining the method and the mathematical implementation, such that this should provide a strong foundation for building on and for others that choose to adopt these methods. It is a

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Discussion paper

valuable study, as age modelling or other passive tracer advection studies (e.g., isotopes, dust layers, or other chemical horizons) have not been given much attention in ice sheet studies in recent years, and are likely due for a resurgence as radar reconstructions are giving increasing detail on 3D ice sheet structure (e.g. McGregor et al., 2015); 3D tracer modelling offers an important avenue for improving and constraining ice sheet models. The methods introduced here should be seriously considered as an alternative to more 'classical' semi-Lagrangian interpolation schemes such as upwind differencing.

MacGregor, J. A., M. A. Fahnestock, G. A. Catania, J. D. Paden, S. Prasad Gogineni, S. K. Young, S. C. Rybarski, A. N. Mabrey, B. M. Wagman, and M. Morlighem (2015a), Radiostratigraphy and age structure of the Greenland Ice Sheet, J. Geophys. Res. Earth Surf., 120, 212–241, doi:10.1002/2014JF003215.

I am attaching a copy of the manuscript with several minor points. The English needs a bit of a double check throughout, for articles, but it is extremely well written and thorough, overall. I will confess that I did not work through the mathematical derivations carefully and have no experience with the RCIP or CIP techniques, so I cannot comment specifically on the rigour and appropriateness of this aspect of the manuscript, or on the novelty of the ideas (vs. e.g., existing implementations in other contexts such as atmospheric models). It is new and relevant to ice sheet modelling.

There is a large number of figures, and it could be worthwhile to consider condensing the presentation of results a little. For instance, with new experiments/sensitivity tests after Figure 7, it could be possible to show only one result (of Figures 8 and 9, and of Figures 14 and 15; maybe elsewhere), while still discussing both experiments in the text. I am also OK with the manuscript as is. Sometimes it is nice to see everything laid out and presented, without relegating additional results to supplements.

A couple of suggestions for the authors' consideration:

The accumulation rates in the experiments are very low, typical of the East Antarctic

Plateau during the glacial period. I guess that it does not affect the performance of the different interpolation/advection models, but am curious to confirm this for the case of e.g. accumulation rates 10 times higher, more typical of Greenland. Also, combined with this, high-amplitude, millennial-scale climate oscillations that are typical of Greenland (D-O cycles). Are there specific recommendations or differences in RCIP behaviour specific to these conditions?

The model is developed specific to 1D age modelling in ice core settings (i.e. purely vertical flow, positive surface mass balance). Extension to 3D is discussed near the end, but would require consideration of positive (emergence) velocities, 3D flow fields, and (typically) much lower horizontal gradients of ice age. This first comes up on p.8, l.197, where the authors develop a formulation that assumes negative vertical velocity throughout, which will not be compatible with 3D modelling. I appreciate that the extension to 3D is for future study and we already have much to chew on with the current presentation of ideas and results, but this discussion could be extended a bit and I am curious about the author's opinion of whether the more complex RCIP type of approach is warranted for the lower horizontal gradients in 3D interpolation models.

Related to 3D models: the authors explore what would be considered as high vertical resolution in ice sheet models, from 129 to 513 vertical layers. This is much higher than many operational 3D ice sheet models that look at 3d (Stokes) solutions to the velocity field or Ice Age timescales: nz = 40 may be more typical. In the section on vertical resolution, it would be helpful to include an experiment with e.g. nz=33 to evaluation model performance at lower resolution. Does it further degrade the interpolation schemes and exaggerate the differences in modelled ice age, or do models converge as resolution declines?

I am interested in the relatively strong results of the first-order upwind scheme. The authors do discuss this, but why is this consistently better than 2nd-order upwind schemes in almost all of the model experiments? In some cases it is of comparable performance to RCIP. Would the authors recommend always using 1st-order over 2nd-order upstream advection/interpolation models, and under what conditions might 1st-order advection schemes be adequate, vs. the RCIP-corr approach? A short discussion of 'practical suggestions' for eventual application of this technique in ice sheet models would be valuable.

Many thanks for this interesting contribution - I look forward to seeing the final version advance to GMD and push the research community forward.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2020-53/gmd-2020-53-RC2-supplement.pdf

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