

Interactive comment on “A system of conservative regridding for ice/atmosphere coupling in a GCM” by E. Fischer et al.

Anonymous Referee #1

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A system of conservative regridding for ice/atmosphere coupling in a GCM

E.Fischer, S.Nowicki, M.Kelley and G.A.Schmidt

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This is a clear and carefully written paper that contains precisely what the title promises. As such, I see no bar to it being published in GMD. Of course, I have some comments and opinions on how I think it might be improved.

1) As noted above, I thought the paper generally very clear for what could be an involved and complicated topic. The flip-side of this is that I also found it a little long-winded, even repetitious in places (e.g line 4, pg 6498 repeated from line 11, pg 6499; the 10% change in grid size in SeaRISE on pg 6507; line 4, pg6498 gets repeated)

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and found myself looking for things that could be cut to sharpen the focus. Perhaps the early parts of section 2, section 5, figure 1 and the repeated theme of the grid exchange schematic in figs 2, 14, 24 and 25 could be condensed or omitted?

2) I currently work at the very coupling interfaces that the authors propose to remap, so I would be in the target audience to which they wish to appeal with their scheme/library. From what I read here, I have two major issues that leave me unconvinced that I should take them up.

a) I don't think they really made the case that the practically/physically intuitive sort of remapping between the elevation grid and the ice grid that is done in e.g. CESM (horizontal bilinear for all elevations, then vertical interpolations between elevations, followed by some kind of post-interpolation gridbox correction) is actually so bad. Sure, that post-interpolation correction isn't very elegant, but it does the job in a practical sense. The list in 6.2 covers some potential theoretical issues that result from bilinear interpolation, but I don't think the examples in section 10 compare this 0-order case in a concrete way with the more sophisticated scheme on offer here - perhaps the authors could work up another example, or some simple numbers on the level of distortion attributable to the ad-hoc post interpolation correction.

b) The practical restrictions of fully implementing the scheme into models that have already been written without this sort of thing in mind (introduced in section 8) seem to be onerous. The mathematical perfection of the transformation appear to be rudely brought to ground by the prospect of a non-local RM, which would require the basic atmosphere->land surface coupling to be significantly tinkered with. As the authors helpfully list, the RM transformation is only properly local for one choice of model setup (Z interpolation to the "exchange" grid (G) of an L0 icesheet), and even then the overall coupling loses its perfect shine in the dispersive transform from G to the icesheet itself.

Since there is no way that the atmosphere-land-surface coupling in my model will be rewritten to accommodate the needs of the icesheet, I'm personally left with the choice

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between my currently implemented, non-ideal coupling with a slightly rough correction for overall conservation, and taking on this new scheme, which will also have its flaws given the restrictions of the models I'm working with. The icesheet, indeed the entire Earth System model it sits within is not perfectly conservative of energy or water. Given an otherwise perfect model system, or a large reservoir of time to try a variety of different coupling options, I'd give this a go, sure. As things stand, I'm less sure developers like me will take the time to give it a whirl. Maybe if more of a case were made for a), above, I might be forced to reconsider.

This is not, of course, an argument for not publishing the paper, or even a criticism of the proposed method, more a caveat about the potential impact of the offered library.

3) I'm afraid I think that calling the package GLINT2 is a bad idea. It's not, after all, the new version of the current Glint library, part of Glimmer-CISM which is (I think, still) approaching the release of its own version 2. Having this appropriate the Glint name based on the fact that it replicates some of Glint's functionality is just confusing.

4) A few language/style issues that could be clearer or made more general: - there are some colloquialisms (e.g. "gotchas" pg 6526, "dump" pg 6511) that may be unclear to non-native speakers - some places take an ice-modeller-biassed viewpoint of things that might be expressed more carefully - e.g "modelers care more about the details of E-G than E-A" (line 25, page 6513) - I know plenty of climate modellers who care not a jot about E-G! Also, line 8, pg 6498

Specifics

line 7, pg 6499: "the ice sheet model is ideally 15m thick" - presumably "ice surface" is meant, in the nomenclature of the paper. Surely the "ideal" thickness very much depends on one's specific setup?

line 24, pg 6494: the AR4 quote is of course now outdated in some respects, and AR5 doesn't have the same restriction/caveats on the sea-level numbers.

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section 2.5: I do not know the specifics of the GISS/PISM setup, but water fluxes might potentially also be returned in general? Additionally, ice-sheet/shelf-ocean coupling is clearly beyond the scope of this paper, but some note might be made somewhere of the requirements for the ice-sheet to exchange information with other parts of the Earth system too.

line 5, pg 6499: out of interest, why couple monthly if you've designed your ice surface layer to fully insulate the icesheet from seasonal effects?

line 14, pg 6503: the number of elevations actually chosen doesn't appear to have been justified at all.

line 20, pg 6518: "[...] the GCM will have multiply by [...]"

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