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Interactive Comment

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

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We thank the reviewer for the helpful comments, which have improved the manuscript. Below are our answers to the reviewer's comments.

`Unnecessary repetition: (e.g line 4, pg 6498 repeated from line 11, pg 6499`

Line 4 pg 6498 has been removed.

`the 10pg 6507; line 4, pg6498 gets repeated`

Line 4, pg6498 has been removed.

`Perhaps the early parts of section 2... could be condensed or` omitted?

In response to both reviewers, Section 2.1 has been rewritten to better justify the use of an ice surface model between the ice flow and atmosphere models.

`Perhaps the early parts of section 5... could be condensed or omitted?`

In response to both reviewers, Section 5 has been rewritten to better separate three issues: (a) How to correct for errors, (b) How to eliminate projection error, (c) How to minimize geometric error — and the difference between projection and geometric error.

`Perhaps figure 1... could be omitted`

We have received positive feedback from others on Fig. 1. It is helpful to have a simple reference cartoon showing the physical configuration of the different models.

`Perhaps the repeated theme of the grid exchange schematic in figs 2, 14, 24 and 25 could be condensed or omitted?`

Reviewer2 made these suggestions as well. Our response is below.

`(Reviewer 2): Fig. 24 can be omitted or merged with Fig. 14.`

Fig. 14 describes the five grids used in the coupling problem. It is the "basic map" of regridding transformations, and should serve as the primary guide on "how to regrid from A to B."

Fig. 24 shows how to regrid ice surface model state from one elevation grid to another, when ice extent or elevation changes. The symbol E is used for the old elevation grid, and F is used for the new elevation grid. This diagram is derived from Fig. 14 in two ways: (1) The elevation grid is duplicated (E and F) because we're regridding from

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one elevation grid to another, and (2) Portions of the diagram not central to elevation regridding are omitted for clarity.

The caption for Fig. 24 now reads:

> When ice extent, ice topography or elevation points change, the basis functions for the elevation grid E change along with it. Ice surface model state, which exists on E, must be regridded to the new set of basis functions.
>
> Shown is a grid system that can serve as a map for this regridding: E is the old elevation grid, F is the new one and G is the interpolation grid (same for old and new). Ice surface model state may be regridded from E to F by first regridding $E \rightarrow G$, then $G \rightarrow F$. Note that this diagram is a simplified version of Fig. 12 in which two different elevation grids have been acounted for.

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(Reviewer 2): Fig 25 can be omitted, refer to Fig. 2 instead
at line 19 page 6529.
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Fig. 2 shows how a two-way coupled GCM operates, assuming it has the five required transformations at its disposal. Fig. 25 shows the data GLINT2 needs to produce these transformations. The caption of Fig. 25 has been updated to make this clear. Fig. 2 caption has been edited to better explain the meaning graphics used.

Fig 2 caption:

> Data flow for the coupling between atmosphere, ice surface and dynamic ice flow models. Outputs (blue arrows) from each model (boxes) are fed as inputs into the next. Since the three models run on different grids, regridding operations (ovals) are required at each step. Fig. 24 shows the inputs required to compute these regridding operations.

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Fig 25 caption:

> The GLINT2 workflow used to compute the regridding operations required by the fully coupled system (Fig. 2). GLINT2 produces regridding operations based on a variety of factors: atmosphere and ice grid geometry, ice topography and extent, and levels chosen for the elevation grid. GLINT2 must recompute the operations when any of these factors changes.

```
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.
```

This paper develops a theoretical framework for the two-way ice/GCM coupling problem, and places existing work in that framework. This allows us to analyze existing work in ways that were not previously possible. In applying the theory, the paper provides a set of "recipes" for regridding, along with choices the user can make – and it

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analyzies the consequences that would follow from those choices.

Post-interpolation gridbox correction described above is required in the CESM case because the transformations for $E \rightarrow A$ and $E \rightarrow I$ are inconsistent with each other. This paper gives us the tools to recognize that non-conservation is caused by that inconsistency, rather than properties of any single transformation in the system.

With that in mind, this comment seems to address two orthogonal issues: (a) the advantages/disadvantages of using consistent transformations for $E \rightarrow A$ and $E \rightarrow I$, and (b) the consequences of bilinear interpolation for $E \rightarrow I$.

Consistent Transformations

The suggestion above is to compare the pro/cons of using a consistent set of five regridding transformations, versus an inconsistent set and then applying post-hoc corrections.

This paper is the first (that the authors are aware of) to develop a theoretical understanding of the ice/GCM coupling problem – including a precise definition of the vector space implied by elevation classes, and the basis functions used in that vector space. The fact that it is possible to create a set of five consistent transformations without post-hoc corrections is interesting and novel. If nothing else, it provides a "theoretically pure" baseline against which other approaches might be compared.

The authors developed this theory in order to support conservative tight two-way coupling for GISS ModelE. Such coupling requires a set of 5 transformations, whereas past 1-way coupling efforts have required only 2 transformations. No one has proposed a full 5 inconsistent-but-mass-corrected transformations that could be used in a conservative setting, leaving this paper as the only practical proposal for use in GISS ModelE.

It would require significant research to extend existing post-hoc correction schemes to

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5 transformations, and then compare them with the results of this paper. Such an effort is outside the scope of this paper.

Bilinear Interolation

In this paper, it is assumed that the user provides an $E \rightarrow I$ transformation – and then the other four required transformations are derived, based on that. In principle, $E \rightarrow I$ can be anything, bilinear interpolation for example. We discussd bilinear interpolation for $E \rightarrow I$ within the context of the consequences that result in the other four transformations.

We began work on this paper assuming that any choice of $E \rightarrow I$ is as good as any other. But over time, we realized that bilinear interpolation introduces serious problems, when one considers the five required transformations together:

1. Most significantly, the use of bilinear interpolation can easily cause implausible negative precipitation fields from the transformation $A \rightarrow E$ (Sect. 12, Fig. 23). Alternatively, one can choose to not conserve mass when regridding precipitation from A to E, or use some other post-hoc correction scheme. We believe this is reason enough to avoid bilinear interpolation.

2. Bilinear interpolation introduces unnecessary, unphysical numerical diffusion into the GCM.

3. Bilinear interpolation introduces significant non-locality in the RM matrix, adding unwanted complexity to the GCM.

For all these reasons, we recommend avoiding interpolation in the horizontal. Z Interpoltion is a preferred alternative. Of course, this produces SMB fields with discontinuities at atmosphere cell boundaries, which could potentially cause problems for the

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ice flow model. We have considered some possible ways to conservatively smooth a field after it has been produced by $E \rightarrow I$. However, we would like to test these schemes in a real coupled situation with a real ice model. If the ice model doesn't care about discontinuities at atmosphere cell boundaries, maybe we don't either. Post-hoc conservative smoothing is therefore a topic of future research.

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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)..

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 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.
```

This paper develops a theory that allows us to make good choices for conservative regridding strategies while being mindful of practical implementation issues. Good theories point the way to good engineering solutions, but they frequently also point out fundamental limits. This paper does both. We believe that anyone seeking conservative coupling with high fidelity to the physics will run into issues similar to the ones we have described: that the issues we uncovered are fundamental to the problem, not just

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to our solution to the problem.

Our first practical efforts at two-way coupling will be restricted to a local RM matrix because that is what our GCM (GISS ModelE) currently allows. We chose to describe the entire theory, even portions of it that we might never be able to use in our modeling efforts. In the future, this paper can help give us an understanding of what we might gain by upgrading ModelE to use a non-local RM — and could also help provide the justification to do, based on a programming cost/benefit analysis.

```
and even then the overall coupling loses its perfect shine in
the dispersive transform from G to the icesheet itself.
```

Numerical dispersion is to be avoided and minimized, but it hard to eliminate entirely. In the paper, we have considered different ways to arrange the set of tranformations, and have recommended a set of choices that (a) results in minimal numerical dispersion, and (b) encounters that dispersion only once each coupling timestep, rather than once each atmosphere timestep. This is not perfect: some amount of numerical dispersion is inevitable when regridding between grids that do not line up exactly. But it is very good, and orders of magnitude better than other approaches one might use.

```
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.
```

Different GCMs are built with different conservation requirements. In its copuling, GISS ModelE requires conservation to machine precision.

Although other GCMs might have looser conservation requriments, issues uncovered

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in this paper might still be relevant. For example, the SMB field can be significantly different, depending on one's choice of $E \rightarrow I$, which could cause biases in the long-term evolution of the ice sheet. Numerical dispersion can also cause problems, whether or not conservation is required. For these reasons, the authors believe that this paper adds an important contribution to the conversation, even for GCM authors who do not require strict conservation or who are not involved in tight two-way coupling.

`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.`

Thank you. The offered library implements the parts of the theory that we have needed to use so far with GISS ModelE.

`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.`

The GLINT2 library has the same goals as the original GLINT — to couple n GCMs with m ice models using $n + m$ programming effort, rather than $n \times m$. The original creators of GLINT have been made aware of GLINT2 multiple times over the past year, and have not asked for a name change. We would be happy to change the name in the future, if that is desired.

`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`

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“gotchas” changed to “surprises”. “dump” changed to “leave”

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- some places take a 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!
```

This sentence has been removed.

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Also, line 8, pg 6498
```

This text was rewritten based on other comments.

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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?
```

This text was rewritten/eliminated based on other comments.

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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.
```

At the time the original manuscript was prepared AR5 was not available to read or cite. As the AR5 report is now available, we have omitted reference to AR4 and instead updated the introduction to reflect the findings of the IPCC AR5. We note in particular that "The representation of glaciers and ice sheets within AOGCMs is not yet at a stage where projections of their changing mass are routinely available. Additional process-based models use output from AOGCMs to evaluate the consequences of projected climate change on these ice masses." Chapter 13, Sea Level Change, p 1145.

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section 2.5:  I do not know the specifics of the GISS/PISM
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setup, but water fluxes might potentially also be returned in
general?
```

Text has been changed to make it clear that we are only talking about fluxes that go to the ice sheet:

> On each atmosphere time step, relevant flux outputs from the ice surface model are accumulated as $\bar{\boldsymbol{F}}^E$ for future coupling with the ice flow model. They are named $\bar{\boldsymbol{F}}^E$ because these fluxes are in general equal and opposite to fluxes sent to the atmosphere.

```
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.
```

"GCM" has been replaced with "atmosphere model" in the abstract, to emphasize that this paper covers only coupling with the atmosphere. The following text has been added near the begining of Sect. 1 (Introduction):

> A full understanding of the long-term evolution of an ice sheet within a coupled climate system requires coupling with the ocean as well as atmosphere. Surface runoff, ocean cavity circulation and salinity gradient effects are all important. In this paper, we focus only on coupling with the atmosphere.

```
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?
```


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“one month” has been changed to “one month or any other time period.” We have not run the full simulation yet, and do not know what coupling timestep would be best. Monthly coupling might still turn out to be useful, depending on the importance of the effects of snow/ice being added/removed from the ice sheet.

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

We intentionally oversampled. Text has been changed to: “In our tests, we have used 40 points at 100 m spacing, which is probably more than sufficient for coupled ice sheet simulations. We have not done a careful study of the optimal number of elevation points.”

`line 20, pg 6518: "[...] the GCM will have multiply by [...]"`
Changed to “the GCM will have to multiply by”

Interactive comment on Geosci. Model Dev. Discuss., 6, 6493, 2013.