gmd-2013-165

A System of Conservative Regridding for Ice / Atmosphere Coupling in a GCM

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

Enclosed are author responses to reviewer comments. Responses to Reviewer #1 are first, followed by Reviewer #2. The following changes were made *in addition* to changes documented in the following pages:

1. Figures numbering has changed, due to consolidation of figures.
2. Section headings have been rewritten to be more consistent and readable.
3. Text added to abstract: "This paper de- velops a theoretical framework for the problem, and shows how each of these transformations may be achieved in a consistent, conservative manner. "
4. Definition of Symbols (Fig. 3) added

Geosci. Model Dev. Discuss., 6, C2728–C2739, 2014
www.geosci-model-dev-discuss.net/6/C2728/2014/

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

R. Fischer et al.

robert.fischer@nasa.gov

Received and published: 3 March 2014

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`

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

`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 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.

`(Reviewer 2): Fig 25 can be omitted, refer to Fig. 2 instead at line 19 page 6529.`

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.

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 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

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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 $E \to I$ transformation – and then the other four required transformations are derived, based on that. In principle, $E \to I$ can be anything, bilinear interpolation for example. We discussd bilinear interpolation for $E \to I$ within the context of the consequences that result in the other four transformations.

We began work on this paper assuming that any choice of $E \to I$ 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 $A \to E$ (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 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.

> 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

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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

```
- 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.

```
Also, line 8, pg 6498
```

This text was rewritten based on other comments.

```
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.

```
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.

```
section 2.5:  I do not know the specifics of the GISS/PISM
```

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

`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?`

“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.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Geosci. Model Dev. Discuss., 6, C2740–C2753, 2014
www.geosci-model-dev-discuss.net/6/C2740/2014/

Geoscientific
Model Development
Discussions
Open Access

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

R. Fischer et al.

robert.fischer@nasa.gov

Received and published: 3 March 2014

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

`I miss at several places the intuitive explanation of the formal maths. I think this paper would gain a lot if the basic concepts are explained more intuitively in addition. I have to admit that I did not convincingly understand the core of this work, and therefore my review is incomplete.`

We have carefully reread the paper and have included intuitive explanation to the formal maths. In particular, edits were applied to Sections 2.2, 3, 4, 4.2, 4.3, 4.4, 6.1, 6.3, 7, 8.1, 8.5.

`There should be made a convincing case that there is a need for the use of an intermediate ice surface model. After reading the text this is not transparent to me. Line 26 page 6498 argues vaguely about important problems. Mapping and regridding in one step may well circumvent the addition of the ice surface model. If combined with coupling at low frequencies the two-way coupling may also be established.`

We added this argument in section 2.1 ("Ice surface model").

`Line 1, page 6494: The method of elevation classes is probably not familiar to every- body`

A short definition of elevation classes was added to the abstract. It now begins (italics to show new text): “The method of elevation classes, *in which the ice surface model is run at multiple elevations within each grid cell*, has proven to be a useful way for a low-resolution general circulation model”

The paper also references a seminal paper on elevation classes (Leung Gang 1998) as well as an example of their use in GCMS with ice models (Lipscomb et al 2013).

`Line 4-7, page 6494: Past ... downscaling to the ice model. Please rewrite.`

Rewritten, the new text is:

> Past uses of elevation classes have failed to conserve mass and energy because the transformation used to regrid to the atmosphere was inconsistent with the transformation used to downscale to the ice model. This would cause problems for two-way coupling.

`Line 3, page 6495: the least to be mentioned is how this is treated in AR5 (even omit reference to AR4)`

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.

`Line 20, page 6495 almost certainly arose is a bit a bold statement rephrase to feed- backs play an role in many events (e.g. ...)`

Text changed to: “This kind of feedback probably plays a significant role in many events in the paleo record (Dansgaard–Oeschger events, Heinrich events, the Younger Dryas).”

`Line 15, page 6496: replace "this" by e.g. "the latter"`

Done

`Line 18, page 6496: What kind of surface flux fields are meant? I guess these are vertical fluxes?`

This has been rephrased to add clarity: “The key insight is that mass and energy fluxes between the atmosphere and an ice sheet vary approximately by elevation within a local region.”

`Line 27, page 6497: replace "atmosphere" by "atmosphere model".`

Done

`line 4-6, page 6498: remove, because it is repeated at line 11, page 6499.`

Removed.

`line 7, page 6499: Should be ice SURFACE model.`

Removed the entire (short) paragraph. It was no longer necessary after new text at the beginning of Section 2.1 explaining the need for the ice surface model (see comment above).

`Line 10-25, page 6500: I would suggest to replace the variable names (in Fig. 2 as well) as follows:`

A few orthogonal issues are brought up here.

Superscripts

There are two general proposals to handle superscripts: (a) Superscripts are just one letter, indicating which grid the vector belongs to, or (b) Superscripts indicate not just the grid the vector belongs to, but also the history of regriddings used to create the vector.

Notations (a) and (b) both have their merits and problems. Notation (a) is simple, but not always completely descriptive. Notation (b) is more descriptive, but can add clutter and could become unwieldy2. It can also be confusing, since $F^{A \to E}$ looks like F^A, but it is really on the E grid. The authors prefer (a) for its uncluttered simplicity, using accompanying text ot explain how vectors were computed.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Base Variable Names

In the example, each upper-case letter represents a *bundle* of fields, represented collectively by one letter. The fields passed between different phases of the coupling are different. (For example, precipitation and downwelling shortwave radiation are passed from the atmosphere to the ice surface model. SMB and upwelling longwave radiation are passed from the ice surface model to the atmosphere. Accumulated SMB and small conductive heat flux are passed from the ice surface to the ice flow models. Changes in ice sheet configuration are passed from the ice flow model to the ice surface and atmosphere models.)

Because field bundles for each step are all different, they deserve different base variable names. On the other hand, there is commonality in variables in some cases. For that reason, we have take the reviewer's suggestion to rename variables in ways that enhance this commonality. SMB and associated fluxes on the ice surface is called F; the associated variables to the atmosphere are called $-F$, because they should in general be equal opposite to F. Accumulated SMB and energy fields are called $\bar{F}$.

Changes were made to the text to match this:

> On each atmosphere time step, flux outputs from the ice surface model are accumulated as $\bar{\boldsymbol{F}}^E$, named so because these fluxes are in general equal and opposite to fluxes sent to the atmosphere. Every coupling time step – about once a month – the accumulated $\bar{\boldsymbol{F}}^E$ is regridded to the ice grid ($\bar{\boldsymbol{F}}^I$) and passed to the ice flow model.

Delta

The Greek letter Delta is usually not used as a first-class symbol, but rather as a modifier. In order to reduce confusion, we have changed Δ –> D.

`Line 2, page 6501: Ambiguous definition of` Δ^I.
`Line 3, page 6501: unclear`

Unambiguated and clarified. Text now reads:

> The ice flow model produces changes in ice surface topography and extent, as well as a small energy flux between the ice flow and ice surface models (together, we call these $\boldsymbol{D}^I$). Changes in ice topography and extent are regridded to the atmosphere grid ($\boldsymbol{D}^A$) and used to adjust the atmosphere's orography. The energy flux is regridded to the elevation grid ($\boldsymbol{D}^E$) and applied to the ice surface model.

`Line 7, page 6502: The practitioner ... grid cell. Unclear sentence.`

Clarified. Sentence now reads: "The practitioner must choose which elevations to use for each atmosphere grid cell."

`Line18, page 6502: fE -SMB Do you mean fE =SMB?`

Dash changed to colon to clarify that this is not math. Sentence now reads "Suppose we have computed a flux field $\boldsymbol{f}^E$: SMB, for example."

`Line 28 page 6502: You basically suggest that there is no ablation at all in the accumu- lation area, that does not seem to be necessarily true.`

Good point. Traditionally, the ELA is defined based on yearly averages. The energy balance model computes things at an instant in time; therefore, the "instantaneous ELA" or "freezing line" is important at any single timestep. Over the course of a coupling timestep (eg, 1 month), the freezing line will move up and down, creating a zone in which there is some ablation. If this process is carried out for a year, the "true" ELA will be somewhere inside that zone.

The text describes this situation later in the paragraph; but is somewhat of a distraction to the main point, that the SMB function vs. elevation will be constant above some equilibrium "zone." We have changed “constant” to “constant to first order” in order to help focus reader attention on the main point. The text now reads: “In the face of spatially invariant precipitation, one would expect SMB to be constant to first order above the equilibrium line altitude (ELA), and to decrease linearly with elevation below the ELA.”

`Line 20, page 6503: fundamentals IN SECT. 4 and projection issues IN SECT. 5.`

Done, changes made.

`Line 18, page 6504: In left hand side: f should be fG I presume, and probably in line 15 as well.`

Yes, fixed. (Appendex A notation, item 5 descries this convention)

`Sect. 5.1: The use of an (optimal) oblique stereographic projection or the oblique Lambert Equal Area Projection projection, as used in OBLIMAP (Reerink et al., 2010, GMD), instead of a polar projetcion, reduces this error.`

`I suggest: Line 13-16, page 6508: Even ... 2008) can be moved in between line 5 and 6 of page 6508.`

We do not expect the oblique stereographic projection to make much difference because the SeaRISE projection already minimizes errors along the 71N parallel (see “standard parallel” with respect to the Stereographic projection).

The Lambert Equal Area Projection is mentioned in the paper. It is, by definition, area-preserving. Therefore, it makes no difference in theory where the origin is placed, as long as the computation involved does not become ill-conditioned. Due to these numerical issues, we would expect an oblique LAEA projection centered over Greenland to perform no more than slightly better than a polar LAEA projection.

We have modified text to specify that an LAEA projection is centered on the north pole, when that is what we mean (Section 5.2, Fig. 5). In cases where what we are saying applies to all LAEA projections, we leave the center unspecified. In Section 5.2, we now say “onsider a typical latitude–longitude grid on the sphere with a Lambert Equal Area Projection centered at the North pole (Fig. 5).” The accompanying caption for Figure 5 now reads “This map, made using a Lambert Equal Area Projection centered on the north pole, shows a set of latitude/longitude grid cells – a kind commonly used in atmosphere models...”

Our discussion is focused on area-preserving projections in general, using the LAEA as one example. Ice modelers wishing to use an area-preserving projection might use any projection they choose. This is easy in the accompanying coupling software because it relies on the PROJ.4 library for projections. (PROJ.4 implements a wide variety of projections that people have found useful).

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 rest of Sect. 5.2 and Sect. 5.3, Fig. 5 and Fig. 6 can be omitted.`

Section 5.3 and Fig. 6 have been eliminated. Sections 5.2 and Fig 5 have not been eliminated (see discussion on comment immediately above this).

`Line 10, page 6530: IS only required`

Done

`Line 3, page 6535: transformations ARE linear`

Not all regridding transformations described in this paper are linear. Text changed to

“Many but not all of these transformations linear, and can be represented by matrices.”

Figures and Captions

`The authors should carefully read and check all their caption texts.`

We did this, and modified a number of them.

`line 1 caption Fig.3, page 6546: "bass" should be "mass".`

Done

`Fig. 4 can be merged with Fig. 12.`

Figs 4 and 12 illustrate different issues. Figure 4 visualizes the definition of an exchange grid, as defined elsewhere in (eg) ESMF literature. Figure 12 shows how the transformation $E \rightarrow A$ is derived from the transformation $E \rightarrow I$ and standard area-weighted remapping. It describes a core concept of the paper.

`Fig. 9 can be combined with Fig. 7 to one figure with two panels.`

Done. This gives a nice side-by-side comparison of Z Interpolation and Bilinear Interpolation.

`I do not understand Fig. 10 and its caption text.`

The caption has been redone:

> Traditional elevation class schemes are equivalent to running the ice surface model on an L0 grid, where grid cell outlines are created by the intersection of the atmosphere grid and elevation contours. One such grid is shown in this figure. Note that grid cells extend only as far as the ice sheet;

the grey line shows the Greenland coast.

The figure has also been redone, as a line drawing on a map. It show how the elevation grid (in this case) is created by the intersection of the atmosphere grid with elevation contours.

`Fig. 11 can be omitted or merged with Fig. 3.`

Traditional elevation class schemes are equivalent to running on an L0 grid with funny-shaped grid cells, as shown in Fig. 9. GCM modelers are comfortable with this approach because it fits in with traditional notions of fractional cell areas used in a number of GCMs. The downside of the traditional schemes is the zeroth-order accurate “interpolation” in the vertical, as shown in Fig. 10.

This paper generalizes traditional elevation class schemes, and seeks to give the “lay of the land” of what choices are possible to the modeller, and what their consequences would be. It begins by describing the generalized “elevation points” set of schemes, and then specializes them to produce the traditional elevation class scheme.

In this context, Fig. 11 allows for a direct comparision of traditional elevation classes to the elevation points scheme based on Z Interpolation — making it clear that traditional elevation class schemes should provide a significantly less accurate SMB for the same number of elevation points. Not all variations of the elevation points schemes presented in this paper will be palatable to GCM authors. However, Z Interpolation should be a relatively easy drop-in replacement to traditional elevation class schemes, and it offers a number of advantages with few or no drawbacks. Traditional elevation class interpolation is therefore not on the efficient frontier of regridding methods for any situation.

Fig. 11 therefore helps make the case for why it is worthwhile to go to through the more mathematically rigorous procedures of this paper, rather than just using a traditional elevation class scheme. The caption has been changed to better reflect the intention, it now reads:

> Interpolated SMB function within one atmosphere grid cell, when using elevation classes. The resulting piecewise constant interpolation is almost never preferable to the piecewise linear interpolation in Fig. 3. Traditional elevation class schemes are discouraged because they offer no benefit over first-order Z Interpolation.

`The color bar in Fig. 13 misses a quantity-units caption. I do not really understand this part of the story. What is gray in Fig. 13, what the black lines, what the white lines (I guess the latter are the contour lines?).`

The caption has been rewritten to account for all these questions. It now reads:

> Unitless basis functions for the elevation grid E, constructed using 20 elevation points and Z Interpolation (the exchange grid was used as the interpolation grid). The grey box represents one atmosphere grid cell on the west coast of Greenland, with the coastline shown as black lines. White lines in the atmosphere grid cell represent elevation contours corresponding to each elevation point. The basis functions corresponding to elevation points at 950 m, 1150 m and 1350 m are shown. Note that basis functions overlap and are not orthogonal. Because of the Z Interpolation, each basis function has maximum value at its corresponding elevation, but it has a non-zero support up to one elevation point away.

`There is a reference to Fig. 14 before Fig. 6 is cited.`

The early reference to Fig. 14 has been removed. It is not essential to the text where it occured, and Fig. 14 has not been explained yet at that point in the paper.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

`Fig. 15 can be combined with Fig. 16 to one figure with two panels.`

Done. Combined with Fig. 17 as well.

`There are no references in the text to Fig. 17, 21 and 22. The latter two are only mentioned in the captions of Fig. 22-23.`

See p. 6523 line 1. The reference in the text was "Figure 17" instead of "Fig. 17". The problem was similar for Fig 21 and 22. All instances of "Figure" in the text have been fixed to "Fig." All figures are referenced.

`If Fig 21-22 are kept they can be placed in one figure with two panels.`

Done

`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 point A to point B.”

Fig. 24 shows how to regrid ice surface model state from one elevation grid to another, when ice extent or elevation changes. It is derived from Fig. 14 in two ways: (1) The elevation grid is duplicated (E and F) because we’re regridding from 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,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

> 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.

`Fig 25 can be omitted, refer to Fig. 2 instead at line 19 page 6529.`

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 of the 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. 20 shows the inputs required to compute these regridding operations.

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.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper