The Cryosphere Discuss., 9, C1024–C1030, 2015
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The Cryosphere Discussions
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Interactive comment on "Quantifying the resolution level where the GRACE satellites can separate Greenland's glacial mass balance from surface mass balance" *by* J. A. Bonin and D. P. Chambers

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Received and published: 12 June 2015

Thank you for your review. As I understand it, you had several general concerns regarding the overall point of the paper and whether it is worthy of publication in The Cryosphere. While that decision is, obviously, up to the editor, I would point out that many of the people reading this journal are precisely those people who might wonder if GRACE could be used in this way to learn something about the mass variability of large glaciers. Put another way, if this method had worked, an additional section would have been added giving the mass variability of the glaciers, as opposed to the broad-

scale “SMB” which most GRACE papers using similar techniques solve for. Had that been the case, I suspect most editors would not argue against this paper being entered into this journal. That being so, I would argue that it is as important to tell the same audience that such a technique DOESN’T work, as it would be to demonstrate that it DID work.

You also note that it “should not be surprising” that the small glacier basins I chose cannot be resolved satisfactorily. I do not think this is at all obvious. Many people (including ourselves and Schrama and Wouters) have successfully used inversion methods to solve for basins as small as our basin #7. Similar mascon methods have been created to at least 2-degree resolution. That being the case, I would argue that it was reasonable to test if these tiny basins could be solved for. I, also, am not shocked that GRACE errors are too large – but nor would I have been shocked if it the method had worked. How can people know what does work and what doesn’t, if the failed cases AND the successful ones aren’t published?

As to your larger comments:

(a) I apologize for the typo at the start of Section 4.1. You are correct; that should have been refering to the SMB, not the GMB. That has been fixed. In addition, I have done a major rewrite to the entire simulation description and a large section of the analysis, in an attempt to make things more clear.

(b) I did, of course, double-check the code before accepting this surprising result. When I checked, I found that the inverted results, when smoothed into spherical harmonics, really DO match the input simulation well. That is, the math in the least squares process works: it produces what appears to be an optimum fit to the data given it. As for a physical meaning behind why the errors increase with maximum degree, the math doesn’t care. This is one of the problems with least squares in general: that the output, while mathematically optimal, may not be the “best” answer in terms of what makes physical sense. I don’t like that, and in fact, such problems are a large part of the

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reason I wanted to run a simulation to test this technique. Right now, people are using similar methods to this one and getting answers which look okay, but which may really be just as inaccurate as this, just because the least squares math is blind.

So I think it's important to both compute the error and admit that it does not make physical sense. I suppose I could fudge some excuse "explaining" it away, or else dodge and avoid the topic at all. But doing the first would both be dishonest and the second unhelpful to others using this technique. Besides, perhaps this way, someone else will be able to figure out why it happens, if I admit the oddity. I will leave it up to the editor to decide whether a physical explanation must be had for every detail of every error in order to publish a paper on a technique. What I do know is that the mathematics support the statistics I listed.

(c) Yes, I realize that the same dynamic processes that I'm focusing on at these three glaciers exist at a lot of other places in Greenland. However, I am pragmatic enough to realize that I can't possibly use GRACE in this way to solve for relatively small dynamical effects over many, many small regions. It is, however, reasonable to ask if those dynamical effects can be seen in the few places where the signal is very large – hopefully larger than the errors. I realize that all other dynamical signal will be inaccurately lumped in to the "SMB" basins. However, that is the standard technique right now. Surely it is better to try and separate out the difference in a few places, even if we can't do it everywhere, than not even attempt to separate out the physical causes anywhere.

I have explained some of this rationale, as well as adding a better description of what we are doing, in the following new introductory paragraph (due to another author's suggestion, the cumulative dynamic glacial mass balance is called "DMB".): "We expand this technique to include regions designed to contain the mass signal of the largest of Greenland's glaciers: Kangerdlugssuaq, Helheim, and Jakobshavn. These glacial regions experience two different physical processes atop each other: the localized DMB signal and the broader-scale SMB signal. Unlike most places in Greenland, the DMB

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signals in Kangerdlugssuaq, Helheim, and Jakobshavn glaciers are expected to be larger than the local SMB signal. That fact allows us to potentially separate the dynamical effects from the SMB effects in these regions, by making a pair of assumptions. First, since SMB is correlated over fairly large regions, we assume that the SMB signal across each of the large glaciers is similar to the SMB just outside the glacier. Second, we assume that any local signal which is not defined by the broader SMB signal is caused by glacial dynamics. The latter is a reasonable assumption in the case of these three glaciers, due to the relatively large size of the expected DMB signal compared to discrepancies in local SMB relative to nearby SMB. This allows us to use two overlapping basins to separate the two independent signals: first, a large SMB basin, similar to those used in previous studies, and second, a small basin covering only the area just around the glacier. The smaller basin is designed to trap the localized signal, which we know to be largely caused by the DMB, while the larger basin will trap the underlying larger-scale signal, which we know to be largely caused by the SMB."

As to your specific comments:

1.) "Glacial mass balance" is not the right term to denote mass changes due to changingice flow dynamics. Why should SMB be "non-glacial"??

I have altered this term to the more accurate "cumulative dynamic mass balance".

2.) A more intricate point about the SMB-versus-dynamic separation: While the authors oppose dynamic signals to atmospherically forced signals (p. 1316, line 19), dynamic signals are partly triggered by SMB (and hence by the atmosphere)through complex feedbacks. [e.g., Murray et al., doi:10.1029/2009JF001522, Zwallyet al.,10.1126/science.1072708]

Yes, we agree that there are complicated correlations between the two. However, as the two types of signals are often separated in the literature, we have gone with a simplified description, mostly as a memory aid for those less familiar with the terms. (In reality, this method doesn't separate the physical reasons for the signals anyhow,

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just their strength and spatial extent.)

3.) Line 2 of the abstract: "precipitation based SMB" sounds odd. Both precipitation and ablation determine SMB.

Agreed. We have removed this phrase.

4.) Line page 1318, line 13: "roughly based off the island's drainage": not clear to me what this means.

We have altered the line to be more explicit: "We use 13 SMB basins covering Greenland (Fig. 2). These are relatively large-scale drainage basins of the area, with the coastal regions separated from the interior."

5.) Line 25. RACMO2 is not an ice model.

We have corrected this to "regional climate model".

6.) page 1324 line 8: You removed the JPL_ECCO ocean model? This sounds like you didn't account for the fact that an ocean model has been already removed during GRACE processing?

Sorry. I am so used to adding back the monthly-averaged OMCT ocean model, that I forgot to mention it here. We did add it back, then chose to remove a different ocean model, which we believe is more accurate in some regions, as well as a hydrology model, in order to focus on the ice signal. I have altered the text at the start of section 3.3 to say as much: "The GRACE data we use is the standard RL05 solutions from the Center for Space Research (CSR), with the AOD1B ocean dealiasing monthly averages added back."

7.) page 1325, line 15 computed -> compared? page 1326, line 10: areas -> errors?

Sorry, yes. Because of the general alterations to this section, the first phrase is no longer around. The second has been corrected.

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8.) page 1327, line 12: "certainly a plausible achievement...": Make clear here that you know about the n_max=96 solutions by CSR. There are, by the way, n_max=120 solutions by ITSG (TU Graz). As long as you disregard errors (as you do in this section) you may think about solutions at any resolution, anyway. You should incorporate a bit more GRACE insights here.

We have altered the wording on this section. And yes, I agree that it's interesting to think about how even higher-resolutions would affect things. That is why the 120 and even 180 simulations were run.

9.) page 1328, line 11: "quadrature summed" sounds odd to me.

This is a statistics term for taking the square root of the sum of squares (ie: the standard method of combining two or more error estimates). Often you run across the phrase "combined in quadrature" instead, so I altered the phrase to that in the paper, in an attempt to be clearer. I am not sure what else to call it, otherwise.

10.) Line 15 "SNR increases everywhere". Incorrect, as far as the figure shows for basin 2.

I have added this technicality into the document.

10.) page 1329, line 19-21: The argumentation is not convincing. Large north-south elongated SMB basins are as well prone to absorb stripes.

That is true, but all of the Greenland SMB basins I use are significantly wider than the glacier basins (see Fig 2). However, to be more precise, I have added the term "wider" into the phrase in question.

11.) page 1330, line 10: The sentence about the 90 x 90 resolution has limited sense, given the existence of such solutions.

Good point. I have altered the line to say simply: "A 90x90 spatial resolution is achievable for GRACE or for a future satellite gravity mission."

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12.) Fig. 13) looks like the signal consists of two grid cells, contrary to the explanation in the main text.

There is no figure 13. I'm sorry, but I don't know what you refer to. If you mean Fig 1, the text reads: "The glacial basins are each dominated by a single 1°x1° grid cell, with 1-3 non-zero neighboring cells whose weights are defined by modeled ice loss rates." This shows the basin with one central cell and one smaller neighboring cell.

13.) Fig. 10b: wrong ordinate axis label

Oops. Thanks.

I appreciate your time,

Jennifer Bonin

Interactive comment on The Cryosphere Discuss., 9, 1315, 2015.