The Cryosphere Discuss., 9, C1406–C1411, 2015
www.the-cryosphere-discuss.net/9/C1406/2015/

The Cryosphere Discussions

TCD

9, C1406–C1411, 2015

Interactive Comment

Interactive comment on "Improving a priori regional climate model estimates of Greenland ice sheet surface mass loss through assimilation of measured ice surface temperatures" *by* M. Navari et al.

Anonymous Referee #2

Received and published: 13 August 2015

General

The title describes the content of the manuscript well, although it should be noted that the manuscript contains evaluation of the methods only. The manuscript does not contain an application of the method using real data. It focuses entirely on the results using synthetic data.
In general the manuscript is well written, some parts needs to be clarified. I've read it with interest although I was left with one major concern.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Major comment

My primary concern is that the synthetic truths used were, albeit outliers, results from the CROCUS model driven by adjusted MAR data. Hence, this synthetic truth is within the state space of trajectories accessible by CROCUS. It is by no means granted that the real trajectory of the surface state lies within this space reachable by CROCUS. If not, one can assimilate, but it might possibly not help enough to approach the true state evolution. This is a concern for the energy balance (SEB) terms and temperature (Table 2), but posterior SEB and temperature estimates after assimilation with real satellite derived ice sheet temperature (IST) can at least be evaluated using, for example, GC-net data. However, runoff is much more dependent on hardly-to-evaluate model physics than the SEB and moreover runoff is very hard to evaluate. Hence, it will be extremely hard to assess the error and uncertainty in runoff with actual observations once real ISL is used. I expect the authors in that case to look at this paper, so the uncertainty estimates presented here matters. However, given that the synthetic true is a CROCUS state too, I don't buy the presented biases and RMSEs for runoff as a relevant number for test with true data.
Although it is not a full remedy for the problems sketched above, I request to authors to repeat the OSSE using SEB and SMB data from another RCM than MAR/CROCUS, e.g. HiRHAM or RACMO2. I know that the required high-temporal resolution data are not floating around but I guess the authors have the right connections to get these data. This assessment can then presented in the added paragraph 5.4.
I know that this addition requires a significant effort, but I believe this would improve strongly the assessment of what could be expected from this method.

Other concerns

Precipitation: If got it right, precipitation has been varied has been during the tests, but precipitation results are not discussed at all. It is not so easy to evaluate real-world precipitation but within the experiment design you can. Yes, IST has only a very weak link to precipitation but now precipitation remains a free variable to change, allowing taking very unrealistic values. Your figures should show that this deteriorating of results is not the case. After all, precipitation affects the SML through albedo and refreezing capacity. Precipitation must thus be added in Figure 3, 4 and 9, and, if you take this really seriously, discussed in a figure similar to figures 5 to 8.
At the sideline, GRACE data could be helpful to constrain regional precipitation and runoff on monthly timescales and longer when the method is applied on real IST data.

Runoff: Runoff is not a simple direct result from surface processes; snowpack processes seriously adapt runoff. The manuscript tends to be over detailed, but a description how CROCUS models runoff and which subsurface processes are modeled in CROCUS is missing at all. This should be added. For example, I got the feeling that runoff is allowed in the predefined ablation zone but excluded elsewhere. Is such a prior assumption justifiable for a method like this?

Comments related to text parts

3211 16-19 & Figure 1: Why is the border between the dry snow zone and the percolation zone no straight border? Furthermore, these zones are not mentioned later, only a difference between the ablation zone and the accumulation zone is made. So why are you introducing the percolation zone?

Paragraph 3.4: I'm missing quite a few things here:

1. Equation 2: is there no refreezing in the subsurface model? In case of yes (no

refreezing), is this not a major model shortcoming? In case of no (there is refreezing), why is it absent as heat source?

2. Add information how Q_{sh} and Q_{lh} are depending on T and U and surface properties. What kind of meteorological principles are applied?

3. How is melt generated? Is there radiation penetration implemented, in that case melt could occur on multiple depths. Otherwise, melt is modeled only for the uppermost layer, isn't it?

4. Concluding, add a brief description of the physics in the subsurface model of CROCUS relevant for runoff estimates. Grain shape evolution (which is in CROCUS) is in this context not very relevant, but the implementation of percolation, retention and refreezing is relevant because you are intending to estimate runoff.

3217 L21 - 3218 L15: I was able to follow and understand for long how the method is constructed, but the concept of multiplicative coefficient as the states to be estimated remains unclear for me given the current text. Assuming that I'm representative for the TC readers – although I'm afraid that many readers stop understanding the method at an earlier point – I ask to clarify this part. Introduce a figure or scheme or whatever you need, but make this clear.

3227 L17: The term improvement factor is misleading, result aren't up to a factor 400 times better. Given the definition it has the same dimension as the variable of interest, so improvement rate is better. If you would like to present it as factor, you could divide the prior errors by the posterior errors.

5.1 - 5.2: Although strictly spoken not a SML term, I'm missing a discussion of modeled snow/ice melt energy. In the set-up of CROCUS, melt energy is not a component of the SEB although the frozen surface is bound to the freezing temperature. Also, melt can happen at some depth. So, melt energy is not fully a SEB term too.
Having said this, melt energy is in my view a very important term to evaluate if the

SEB is correct for ablation processes. Now, runoff is evaluated only but runoff estimates includes the effect of subsurface processes on the initial melt water flux. Yes, where runoff peaks, melt and runoff are almost equal, but for most sites refreezing mitigates some of the melt. Subsurface processes in snow are still rather unknown and extremely hard to evaluate (Even in situ observations won't tell easily if your percolation/refreezing model is correct). So, if the melt energy is estimated correctly but the subsurface model is err, the runoff is wrong. Or vice versa, an incorrect subsurface model can correct wrong melt water energy into a correct runoff flux.
Therefore, add to 5.1 a discussion of the (vertically integrated (?)) melt energy is improved in the posterior estimates. Yes, I expect that these results largely coincide with the results obtained for runoff (subsurface parameters aren't varied as the variables in Eq. 5), but that's a false guaranty. The real subsurface processes are not automatically equal as modeled in CROCUS, that's why I request a repeat of the procedure using SEB and SMB data from another RCM.

Section 6: The conclusions should be extended with the results coming from the new paragraph 5.4, a brief discussion of precipitation and a discussion of the uncertainty due to the fact that for most results CROCUS has been used to obtain the synthetic truth. Yes, the paper is a successful proof of concept to improve CROCUS results with respect to synthetic CROCUS data, but not yet a proof of concept that CRUCUS results can be improved compared to real world or other arbitrary but sensible SEB and SMB data.

Textual comments

3207 L4 & L26: remove "unprecedented' because it is untrue on geological time scales.
3208 L2: You could also add Johannessen et al, Science, 310 (2005).
3208 L22: it is not "difficult, if not impossible". It's simply impossible in my view.

TCD

9, C1406–C1411, 2015

Interactive Comment

3209 11-13: Rephrase this a bit to make clearer that people haven't made use of the indirect or implicit information in remotely sensed data.
3212 L10: maybe add: ... IST, of all remote sensing products available, may contain the most information about physical processes...
3215 L5-11: In MAR CROCUS is run online for a good reason. There is a feedback between the surface state and the atmospheric conditions (primarily through albedo). Is there any check that posterior energy fluxes are realistic given this atmospheric feedback?
3223 L8: Display this point in Figure 1.
3225 L6-7: It makes no sense to repeat data that is also in a Table.

Table 1: P has likely also a time dimension. mm per day, year or second?
Table 3: Explain why the bias and RMSE in SML is much smaller than in runoff. Apparently the values of RMSE are derived for a subdomain. This should be clear from the text in 5.2 and the header of the table. If my assumption is not correct, explain this difference. (And precipitation should be added here as discussed above).
Figure 2: What is I.C.? I can't find it in the text.
Figure 4c: extend the y-axis to 350 or even further until the bars aren't clipped any more.

Interactive comment on The Cryosphere Discuss., 9, 3205, 2015.