The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-241-RC2, 2018

The Cryosphere Discussions
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EGU

Interactive comment on "Basal drag of Fleming Glacier, Antarctica, Part A: sensitivity of inversion to temperature and bedrock uncertainty" *by* Chen Zhao et al.

Anonymous Referee #2

Received and published: 7 March 2018

General comments

This paper from Zhao and colleagues evaluates the sensitivity of the inversion of the basal friction coefficient of Fleming glacier, Antarctica, to (i) initial (i.e., before the inversion) temperature, (ii) different bed topographies and (iii) ice front boundary conditions. The simulations are performed with a control inverse method (MacAyeal, 1993) implemented in the Elmer/Ice ice sheet model and uses the full Stokes version of the ElmerIce model.

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The novelty here is the use of a three-cycle spin-up (initially proposed in Gladstone et al, 2014, but for one cycle) scheme to avoid the influence of initial temperature field on the final inversion results.

The paper is quite long compared to what it could be. There is a substantial number of repetitions, which should be avoided when possible. The figures are not very clear, some differences pointed out by the authors between experiments being barely visible, thus I was not always sure by how much the three cycle methods improved the inversions results. In many places in the text I was often doubtful about the assertions. Moreover, I am not an English native speaker, but I am sure that the English could be improved. Related comments are written down below.

I have a concern with the Bedmap2 data. Since this is not written in the paper, I would like to be sure that the authors removed the difference between the Geoid and Ellipsoid height, as they did for the other DEM used, which led to have 15m of sea level. If no mistake was made with the Bedmap2 data, could you please adapt your figures to a sea level at 0, which is more common.

I question the last experiment that consists in applying different sea level at the ice front in order to deal with the uncertainties linked to the potential presence of ice mélange, the proximity of icebergs that could push back the ice stream... First, this case need to be documented with literature, or, it needs to be strongly argued. Neither the former nor the latter is done here. Another thing is that the authors have an uncertainty on the position of the ice front, I think a better experiment would be to assess the sensitivity of the results to the position of the ice front, even though I don't think that changing it by 15 km (the uncertainty) would significantly change the results.

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I had issues understanding how you chose your experiments. For example, why choosing -20°C as an initial temperature pre-inversion ? Is this number related to anything real, such as a yearly average temperature ? In the paper from Schaffer 2012 that you cite, their cold and warm scenario were linked to observations, which is what you should do here, or at least explain how you chose those temperatures.

The authors need to be consistent with the terms basal drag, basal friction coefficient, basal sliding coefficient, basal shear stress. They keep mixing up those terms all over the text to mostly talking about the basal friction coefficient.

Finally, I would recommend this paper to be merged with its companion paper, also in The Cryosphere Discussion, which deals with simulating the evolution of Fleming Glacier from 2008 to 2015. All those sensitivity analysis (the first two for me) that were done in this inversion are to me verifications that you start with a sufficiently good initial state. This is not my choice of course but the one of the editor.

In all cases, this paper needs substantial rewriting before publication.

Specific comments

l20: I don't think you have done a sufficient number of experiment to say so, at least to say it this way. Would you explore other glaciers with the same conclusion, this assertion would be more justified.

l22: Is it true ? Looking at your fig7 I see Vb/Vs=1 over a substantial area in the ice stream part ? Means that vertical deformation here is not significant...

l24: You have done some sensitivity test, but I am not sure that those tests specifically

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show the importance of what you say. I go back into this below.

l28: Here you put the glaciers of the AP and the WA ice sheet in the same category. The way those two parts of Antarctica are losing mass is fundamentally different and you should mention those differences.

l31: this sentence (mostly the same as in l14) is the kind you would find in an abstract but neither in the introduction nor in the main text.

l33: Is this always the case ? Fast flowing outlet outlet glaciers can have a small slope and be driven by basal sliding mostly, such as for the Siple coast glaciers... Could you rephrase.

l35: This way, all those processes appear to have equal impacts onto the dynamics whatever the situation...Could you rephrase. And remove strongly.

l37: Same remark as above. What is disturbing is that you seem to put all those things in the same order in influence whatever the situation.

l40: Again, this kind of sentences should be in the abstract not here, at least to me.

l42: What you infer primarily with inverse methods is basal friction (or sliding) coefficient (sometimes ice rheology). Could you rephrase.

l44: In topography, do you put basal and surface topography ? I don't think so. Maybe use the term geometry or thickness and surface topography, because we need the thickness and one of the two surfaces... Please rephrase.

l47: Why especially for small scale glacier ? We have major challenges for modelling temperature in the bigger glaciers as well. I understand you want to guide the reader to you specific case, but this comment is misleading.

l48: I feel like your analysis mostly relies on those two publications dealing with the same glacier. from that you generalise things that should not be.

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l49: What type of inverse methods, did they use many ? Rephrase please.

l49: A lot of things here are not correct or need to be rephrased. 1) the results of Schafer2012 have a dependence to mesh resolution (you should read section 4.3). 2) this is not as simple as that for bed topography and velocity uncertainties. You should be less approximative in your assertions.

l51: This sentence is not clear, rephrase please.

l52: And I don't think you are doing this generalisation in your paper. This is clearly overstating to me.

l54: Do you test this to all the inversion methods. please rephrase.

l56: What robust means here ? You will have tested on one single friction law, and almost the simplest one. You should rephrase.

l60: Maybe here you could add some figures, what are the velocities, the size, some more details about the glacier...

l65: You invert the basal friction (or sliding) coefficient. You need to be consistent over the text.

l66: What you invert is the basal friction coefficient. Rephrase please.

l80: Just a question here to be sure because you don't mention it after. Did you make sure you accounted for the Geoid-Ellipsoide difference for Bedmap2, which reference is the Geoid ?

l82: This is rather strange and unusual to use sea level of 15m. It would be much clearer to take the geoid as the reference.

l86: Since you mentioned the Wordie ice shelf in the previous section, you should replace "This"

l87: shear stress $->$ friction coefficient

l95: Could you break down this sentence in two parts, otherwise this is hard to read.

l100: To calculate the Hmc, did you use ElmerIce ? I think it needs to be mentioned since this would not be an official feature in Elmer.

l103: This is not really true for bedzc since Sbm has a resolution of 1000m. How did you interpolate Sbm from 1000m to 500m ?

l107: could you mention the fact that they are both part of the same basin.

l112: shear stress $->$ friction coefficient

l124: basal drag $->$ basal friction coefficient

l134: Here you need to mention the difference that you have between your reconstructed ice front and the grounding line of Rignot2011a.

l144: My personal viewpoint is that the mesh resolution influence should always be checked beforehand... This is not such a strenuous task to do this.

l149: The temperature is fixed to what dataset ?

l152: You describe the BC and then you switch into something different, which should be more in the discussion section, not here. This way of writing just affect the reading in a bad way. Please consider not doing this in the text.

l159: Temporarily : what does it mean ?

l169: Ah here you talk about temperature data. It should be written in the same place as above.

l178: Ok, Why 0.2 ? Did you check other values ?

l186: drag $->$ sliding

l187: As there are many types of cost functions in the literature, you should define yours.

l193: Here I think you should cite Gillet2012 as it seems that you do exactly the same thing for the cost function

l200: You should add a figure showing the improvement made with E=2.5. I would also be very pleased to see the L-curve, for instance in a supplementary.

l207: Actuality: I am not sure we can use this word here, change please

l210: If you say so, you need to show that Greenland glaciers and the domain of your study can be similar to each other. Or you need to rephrase your sentence...

l215: you mention Gong2016 (this is 2017 actually) for the spin up scheme or for Elmerice. For the latter, better to cite Gagliardini2013

l219: There is a step here that is not common, surface relaxation with C at its initial chosen value. What is done usually is the inversion, then the relaxation over about 15 years. I wonder the effect of the surface relaxation using a C that is far from reality...

l220: Basal sliding

l225: Means you don't account for the modification of surface with relaxation at the beginning of the last two cycles ?

l228: Basal friction

l229: To your inverse method, not all of them

l243: Don't say linear but rather Control

l246 to l265: I don't really understand the relevancy of this scenario. To me you should rather study the influence of the position of your ice front, since this is what you are not sure about with your hypothesis assuming ice front = grounding line.

l267: Results and discussion

l270: what do you call robustness here ? Replace drag by sliding. Rephrase please.

l273: There are only 3 TEMP experiments, be more clear

l275: Here what we need to have is a metric like the RMS, otherwise this is only a maximum value that is not representative of the rest of the data.

l277: I don't understand what you say here ?

l279: This is quite difficult to evaluate the differences between the different experiments in your maps. I would recommend to the relative differences with a reference experiment.

l281: Figures should be ordered differently, such vertically Control, Temp1, Temp2, Temp3, this is otherwise very difficult to follow.

l283: Looking at Schaffer2012, it does not seem to me that the dependence of their model to temperature scenario is smaller than yours... You do need to quantify your differences, because this is really not clear.

l286: They showed a non influence onto the modelled surface velocity, not the friction coefficient, or I misread their paper... Their Fig8 shows the differences in terms of basal friction coefficient, but this slightly affect surface velocity as the inverse model tends to minimize the differences.

l289 to l291: Already said, please avoid repetitions. You are in the result and discussion, thus adding other unnecessary stuff is only distracting the reader.

l283: I think this is normal to have different results if you choose a sort of outlier in your initial state, like -20 degrees everywhere for the initial state. I don't think you discussed this as a comparison with the final result ? Is -20 in the range of this final result ?

l291: Drag $-$ $>$ friction coefficient

l295: Could you quantify your sticky spots ?

l296: "therefore..." remove this as this was already written

l300: You should say Control instead of linear scenario

l306 to l308: Third time I see this in the paper, remove repetitions please.

l306: The low impact is on modelled surface velocity. There is an impact on basal friction coefficient (or basal drag as they say)

l313: No need to say "inside the yellow contour" in the text

l318: "shows that internal deformation": you should vertical deformation here.

l319: I don't agree with this assertion. Vb=Vs in the fastest flowing areas. In between those you have an area with Vb much lower than Vs, but this matches the places where driving stress is much higher. So this is the driven stress that may drive the vertical deformation, not only the ice internal temperature... You need to rephrase.

l330 to l332: not necessary because already mentioned

l334: Remove mentions to colors and rather explain with the physical parameters

l340: I don't understand

l345 to 347: Why mentioning the MISI in a paper that only deals with inversions, there is no point to me.

l347: basal friction

l350: What is behind "it" ? The link with previous sentences is not quite clear. Rephrase please.

l355: Ok great, you calculated RMSEs. However, 1) you should have done it before (see previous comment above) and 2) please give us numbers.

l357: I guess this is justified by your RMSE. I think you should discuss more this result, as it suggest that using data taken over a short time range improve the results compared to Bedmap2, which is taken over larger time scales than a year... If I am not wrong.

l368 to l371: remove this as already written in the methods section

l371: You did not really have investigated the sensitivity to uncertainty to me. You only have tested two datasets, one being more accurate than the other by the way. The Mass conservation based inversion for bedrock is quite an efficient method to infer the bedrock (see Morlighem2014 NG)

l382: This is the kind of things you need to check really. You may have the answer in the paper by Mouginot 2012 in the Journal Remote Sensing. It seems to be a combination between 2007 to 2009 data.

l387: I really question the relevancy of this experiment. Why doing so as it seems to me that more relevant experiment would be to adjust the ice front position, where you have your uncertainty, and check the sensitivity of inversion results. This latter experiment would not change mush the results to me, because over 1.5 km of ice shelf, you don't have much buttressing, but it would be more relevant than what you propose to me.

l421: I don't understand, in what context ?

l429: This is still about this experiment. To test such an amplitude in the influence of sea levels in inversion results, you need to cite literature about what buttressing could be added from ice mélange (see Krug2014 by the way)...

Figure 4: add relaxation time here

Figure 5 caption: Temp4 doesn't exist

Figures in general: All the differences that you comment are not always visible. These are to me really tight differences so if you want to argue on this to underline the improvement that are brought by your 4 cycle spin up scheme, you should care more about the figures. Use relative differences between the Control and the other experiments.

Figures in general: please, for the readability order vertically your subplots like: Control,

temp1, temp2, temp3

Figure 7: Here is certainly a way to remove those zigzags discontinuity, I know Paraview is not user friendly for some stuff, but I don't think this is acceptable for a peer reviewed paper.