The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-23-AC1, 2017

The Cryosphere Discussions
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EGU

Interactive comment on "Geothermal heat flux and basal melt rate in the DomeC region inferred from radar reflectivity and thermal modelling" *by* Olivier Passalacqua et al.

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Received and published: 19 June 2017

The paper presents a new method for estimating the geothermal heat flux in regions of slow flowing ice. The authors apply the method to the Dome C region. The study makes use of a combination of radar data to infer wet/dry conditions, the one-dimensional heat equation and inverse methods. The authors first construct a time-dependent, one-dimensional heat model including vertical advection of ice. The model is forced with past temperature and accumulation rates reconstructed from a deep ice core. The geothermal heat flux is initially assessed at ten spots, where the bed is known to change from wet to dry conditions from radar measurements. The geothermal heat flux

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Discussion paper

is estimated by calculating a critical ice thickness necessary for basal melt, and then applying an inverse method to get the most likely geothermal heat flux. These values are interpolated to the entire region. The heat flux field is then used to calculate melt rates and the authors then arrive at a parameterisation for the melt rate that depend on geothermal heat flux, ice thickness and ice-flow parameter p. This parameterisation is used to calculate the melt rate over time in the region.

MAIN CONCERNS Overall, the scientific method is sound and it is a nice combination of radar observa- tions, simple ice-flow assumptions and inverse methods. The use of rational assump- tions such as negligible variation of the geothermal heat flux on small spatial scales is a good example of how a complicated and under-measured parameter may be simpli- fied. However, I found the structure of the manuscript rather confusing to a point where it detracts from the scientific content. I have listed some of my main points of concern below but overall the manuscript would greatly benefit from a critical revision by the authors with regards to structure, presentation and grammar.

We would like to thank the reviewer for the fruitful comments made on our work. We agree that the structure of certain sections needed to be modified to make the article more clear-cut. In particular, the introduction section now benefits from a better explained 'oldest ice' context, and section 2 (method) and 6 (discussion) were reordered. We also agree with almost all the minor comments and tried to correct the text accordingly.

The present version of the paper has been corrected by a native speaker for english wording.

When needed, the old sentences are colored in green with corresponding line, the new ones in red.

1. Introduction: It is never explicitly mentioned what "old-ice" is. I assume it refers to the on-going international effort of locating ice that is more than 1.2mio years old but the manuscript does not state this nor is the reader told why this is important. Instead

the introduction jumps between general statements about geothermal heat and radar data processing, and specific descriptions of a dataset from the region. It is only at the end of p. 3 that the reader is told what the aim of the study is. I suggest splitting the introduction into three sections: i) A general introduction to why "old ice" is important etc. including the general effect of geothermal heat flux and ice thickness on basal melting, ii) an overview of past studies of radar data processing and what have been achieved so far with this technique, and iii) an introduction to the study region and the specific dataset that this study uses. The authors are of course free to use a different structure but I strongly recommend rewriting the introduction in one way or another. Finally, a figure with a context map would be very helpful. For example, the introduction mentions studies from Thwaites Glacier, West Antarctica, but the Dome C region is in the central part of East Antarctica. A map could prevent confusion as to why the geothermal heat flux values differ significantly between the two sites.

We followed the above suggestions, in particular to explain how this work is involved in the more general frame of the oldest-ice research. We also added a configuration map. The introduction is now clearly separated in 3 subsections :

1.1) The oldest ice research

Why oldest ice, and how basal melting can be avoided

1.2) GF assessment methods unders ice sheets

Concerning Thwaites Glacier, we wanted to point out the method and its accuracy, not the particular value of the GF.

1.3) Exploitation of available RES dataset around Dome C

Here we explain how we want to use available RES data, and we set the goal of the paper : L. 107 : "this work aims at constraining sites known to be frozen today and that are very likely to have been frozen in the last 800 ka as well."

2. Heat model: This section consists of several short subsections that not always follow

each other in a logical order. For example, the one-dimensionality of the heat equation is presented first as a model assumption in section 2.1.1., then expanded on in section 2.2 and the reader is presented with the equations in 2.3 and 2.4, while the values of the parameters in the equations are mentioned in sections 2.5 and 2.6. It would greatly improve the readability of this section if the heat model is described first in its entirety, then the velocity model and then the assumptions about geothermal heat flux and water circulation.

We reordered the text as suggested for the "Heat model" section.

2.1 Geometry and coordinate system

2.2 Heat equation

2.2.1 Ice thermal properties

2.2.2 Basal boundary conditions

2.2.3 Boundary condition at the surface

2.3 Velocity model

2.4 Proceeding assumptions

2.4.1 Correspondance between wet and thawing areas

2.4.2 GF spatial variability

3. Basal melt rate emulator: What is the advantage of the "emulator" (I assume this is the same as a parameterisation)? The model is run for the whole domain over the period of 800kyr so why is the parameterisation needed? Can't the model be applied directly to the different scenarios? Is it too computationally expensive?

We understand "emulator" as the process we use to empirically replace the whole heat model, to save computation time. We added this new sentence to make it more clear :

L. 344 : "As the computation for a given set of parameter lasts several minutes, com-

puting the basal melt rate for each point of our domain would be far too expensive. Here, the result of the whole forward model is mimicked by an emulator that depends on the input parameters H, Φ_g and p"

Giving an emulator will also be useful to help anyone who wants to compute realistic basal melt rate with a future refined bed for example, and starting from the map of GF.

4. Discussion: Again, I find the order of the sections confusing. The model as- sumptions and sensitivity tests are followed by a comparison to other studies and then a discussion of the geothermal heat pattern followed by a section titled "In- terpretation" (interpretation of what?). I suggest having a separate section with comparison between this study and previous studies, then the discussion section that could start with the overall interpretation and then the discussion of sensitivity tests etc. in the context of the interpretation.

Following your suggestions, we reordered the section :

6.1 Consistency with published data and measurements

6.2 Model assessment

6.2.1 Method validity

6.2.2 Sensitivity to parameters

6.2.3 Spatial variation of the GF field

6.3 Lessons drawn for the oldest-ice research

6.3.1 Interpretation of the wet/dry pattern at the ice base

6.3.2 Old-ice targets

MINOR COMMENTS There are several small typos e.g. "southest", "explicitely", "conlude", "flown" instead of "flowed", "extend" instead of "extent", "additionnal" that need to fixed. In addition, I have the following comments Line 5-7: there is a word missing

The new sentences are the folowing :

"But, since basal conditions depend on heat transfer forced by climate but lagged by the thick ice, the basal ice may currently be frozen whereas in the past it was generally melting. For that reason, the risk of bias between present and past conditions has to be evaluated."=

Line 19: which ice core?

The EPICA Dome C ice core (EDC) is now explicitely mentionned.

Line 56: What are internal layers? Presumably radar layers but this need to specified and explained why they can be used.

L 56 : "basal melt rates have been estimated by fitting the vertical strains with dated internal layers"

Radar layers are used to constrain the vertical advection of ice, which is one of the component of the energy budget. The sentence is now :

L.80 : "basal melt rates for the region north of Dome C have been estimated by fitting the vertical strain rates with dated radar layers, to constrain the vertical advection of ice, and its energy budget"

Line 60: How can the method of Carter et al. be used in this study without using the internal layers?

L60 : "Without using internal layers, which are not available everywhere, we will follow a similar approach to these two last studies"

Our formulation was ambiguous, we do not use the method of Carter et al. The sentence is now :

L.85 : "As dated layers are not available everywhere, we will follow a radar-based approach like that of Schroeder et al. (2014), but adapted to the specific pattern of

radar echoes under Dome C"

Line 74: "amplitude difference" – is that the same as the difference in returned signal strength?

We can also talk of radar echo strenght but the calculation of reflectivity done in the paper of Zirizzotti et al (2012) was based on the difference between the amplitude (strenght or power) of the radar wave reflected by the surface and the amplitude of the radar wave reflected from the ice/rock (or water) interface in dBm. We prefer to keep "amplitude difference" for consistency with the 2012 paper.

Line 105: the heat balanace is assumed to be only vertically dependent.

New sentence L134 :"Thus, the heat balance is assumed to be only vertically dependent."

Line 140: Need a few more details here. What are these tabulated parameters? What is the uncertainty in the method? How well do the results compare to those of a 3D model?

L139 : "At each timestep, explicit expressions for the thickness and the bedrock elevation are solved, that depend on the accumulation rate and six tabulated parameters."

The parameters account for the sensitivity of the ice thickness, surface, height, etc. to the climate forcing.We do not go into deeper details here, since the model is fully explained in Parrenin et al 2007, and since the influence of the accumulation and temperature reconstruction, used as forcing, does not deeply change the results. However, we added a few explanations :

L.143 : "... six tabulated parameters, that account for the sensitivity of physical quantities (in particular bedrock and surface height and ice thickness) to the climate forcing."

Line 143: Missing a word? Coordinate?

Yes, "coordinate was missing : L.148 : "The heat balance of ice only depends on the

vertical coordinate"

Line 160: what is the physical meaning of the parameter p? Is higher values equivalent to more/less basal sliding? What is appropriate for a dome region.

We completed the paragraph with additionnal explanations concerning the construction of synthetic profiles :

L.217 : "Far from divides, and for an isotropic ice, this parameter depends on the non-linearity of the ice rheology and the vertical temperature gradient at the base (Lliboutry, 1979):

p=n-1+Q/RT2*dT/dz

where n is the exponent of the Glen's flow law, Q = 60 kJ mol^{-1} is an activation energy, R = 8.314 J mol^{-1}K^{-1} the gas constant, and Tb the basal temperature. Following Eq.(11), the values of p should range within 7 to 9 on the East Antarctic plateau. But in practice we will use p close to divides in a larger value range, as a parameter able to account for realistic vertical velocity profiles. For exemple, dome profiles are expected to correspond to low p values due to Raymond arches (Raymond, 1983), whereas basal sliding will make the profile more linear and increase the value of p."

Line 174: The density of the firn layer from Dome C? or from somewhere else?

L.164 : "The density profile of the Dome C firn layer is taken from Parrenin et al. (2007)"

Line 180: This paragraph seems to contain some information that is irrelevant.

We wanted to be precise on our choice concerning the expression of the pressure melting point. But we understand these explanations may make the reader lose his train of thoughts, so now we just say what is our expression :

L.168 : "For thawing glacier ice, the melting temperature T m linearly depends on the ice pressure P and the partial pressure of air dissolved in the ice P 0 , which is expressed as (Ritz, 1992): ..."

Lines 191-193: These sentences are very confusing. What is too low for what?

L. 192 : "The melting temperature computed with B = 0.098 K.Pa -1 would be 0.8 K too low, whereas, it is found to be 270.96 K with Eq.(9)"

The sentence is reworded, just saying that the equation is close to the observation :

L173 : "The temperature profile can be extrapolated to the bedrock (similar to Dahl-Jensen et al. (2003) at North GRIP) to 271.04 K, where Eq. (5) finds 270.96 K."

Line 209: Odd to use the value 1/6.04 instead of 1.656.

We prefer to keep it like this for consistency with literature (Lorius and Merlivat, 1975).

Line 261: Reference to Monte Carlo method missing (e.g. Tarantola, 2005 "Inverse Problem Theory and Methods for Model Parameter Estimation"). Also, from the description of the inversion it does not sound like a Monte Carlo approach but rather like a search of the parameter space. Is it a random parameter space exploration? And how is this done?

The parameter space is not explored in a uniform way, because each parameter couple (H, p) is given random values along a gaussian joint-distribution. So we think "Monte-Carlo" is the appropriate word for what we did. The gaussian distribution for H and p are described in the rest of the section.

Line 274: This is the first mention of potential drill sites. Why C6 and H1?

Now a specific sentence introduces the denomination of the points in the introcution section and figure 1 is referred.

L.121 : "For convenience, the domain is referenced with letter-figure couples, corresponding to the grid of the Italian survey (Fig. 1). In particular, two promising old-ice candidates are located at C6 and H1."

Line 285: Where is E4 and E6? Which figure is referred to here?

The previous sentence (L 121) now gives the needed information as well.

Line 311-317: There should be a reference to Fig. 7 somewhere in this paragraph. Generally, this paragraph is not very clear. E.g. in line 315: How do you assign values to some of the variables? Equations (15) and (16) do not help the reader nor do the several almost identical symbols for different GHF.

This paragraph is taken off, since it was unnecessarily complicated with respect to what we want to show. Now we simply present the past basal melt rate calculated with the central values of GF and p, which is the main message of the paper.

Line 323: Point N8 in Fig. 1. Reference to figure 1 added L. 371

Line 361-373: This is another paragraph that is not clear. For example, what is meant by "much of the map is quite well assigned"? or "well described". Does this mean that the model agrees with the observations? Line 389: ". . . realistic transport of cold..." Cold snow?

We agree that this paragraph was not precise enough, and it was reworded to make it more clear-cut.

L.414 : "Superimposed on the observed data, the model output shows that large-scale patterns of wet-dry areas are respected, especially on steep bed slopes (candidate B, C, D, and to a lesser extent candidate A). On these bed reliefs, certain points however show a discrepancy between model and observation, but the gap to the critical thickness is often close to 0 m (D3, D5, D8, M3), meaning that a small change in GF forcing, or a better description of the ice thickness, would better assign these particular points. The 1 km-resolution of the Bedmap 2 bedrock dataset (Fretwell and coauthors, 2013) smoothed along-track subkilometric features detected by our RES survey.

The steeper the bed, the sharper the limit between melting and non-melting areas. In the central, flatter, part of the domain, the position of this limit is more blurred. As we could not assess the GF with our method there, it was interpolated. Despite this lack of

constraints, several small-scale features are well mimicked (dry areas at I9, G-H8, wet areas at G9 and L7). Other regions were not attributed in agreement with observations (G6-7-8, H-I8), meaning that the GF is overestimated, probably up to 3mW m-2 , which is consistent with the uncertainties given by our method (inversion and interpolation)."

Line 391-385: How does this affect the conclusions?

L 391 : "Some of the given confidence intervals are quite low (E4, E6, L7 and N8), and this is a consequence of the tiny altitude difference measured on Fig. 1 between the highest wet points and thelowest dry ones for a given spot. Since the correlation between ice thickness and reflectivity was weak, the confidence intervals at E4 and E6 are probably underestimated, and some local effects may not have been accounted for in this study (e.g. small relief and GHF variabilities)."

As the GF values found for these point are not strongly discordant with the closer points, no particular warning should be placed. But we had to mention these points because we know the observation constraint is probably not very good there (now this pargraph is at L.467)

Line 414-419: Is the accumulation rate influencing the results? That question is raised by not answered in this paragraph.

L. 487 : "The sensitivity of the GHF on the surface accumulation is less than a few tenths of mW m -2 so that, accounting for its spatial variations would not radically modify our results."

Line 447: Is it truly Occam's razor or just a lack of good quality data inhibiting model validiation?

L. 447 : "This application of the parcimony principle of Occam's razor supports the validity of our 1D approach, and means that the main physical mechanisms have been accounted for."

We agree with your remark. We think that our quite simple model works quite well

on bed reliefs, where the results show patterns similar to the observations. On flatter areas, the quality of the method is more difficult to assess, given the quality of the bed DEM, and the compatibility with our assumptions (no upstream flow of basal water). So we reworded the paragraph thiws way :

L. 494 : "This means that the main physical mechanisms have been taken into account, at least where it was possible to evaluate the critical thickness on significant topographic features. Where the GF is interpolated, in flatter areas, the lack of constrains prevents us from really assessing the validity of the method, meaning that our method needs a sufficient hilly bedrock to be applied."

Line 482: What clue? Please clarify.

L. 537 : "Given that this hint of a low GF value is the result of only one observation,..."

Line 495: The amplitude analysis was not performed in this study but from the sentence it sounds like it was. L. 550 : "a previous amplitude analysis"

FIGURES: None of the map have an indication of scale. Presumably the axis are in metres but it is not stated anywhere. Figs. 1 and 9 are very busy and could be split up into several maps. Additionally, The combination of magenta/orange and red/blue in Fig. 9 makes it difficult to read.

The projection and coordinate system are specified in the caption of figure 1. Colours of figure 9 were changed.

We could not split the figures 1 and 9, because the information they contain is much richer when superimposed the ones on the others. We hope their large size should be enough for the clarity.

Fig. 2 is a very nice schematic of the assumptions on this study.