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Interactive comment on "Geothermal heat flow in Antarctica: current and future directions" *by* Alex Burton-Johnson et al.

Alex Burton-Johnson et al.

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Dear Anonymous Reviewer, Thank you for taking the time to provide such a helpful and thorough review. You provided many pertinent comments, and their implementation has greatly improved our manuscript. We have addressed all of your points, and list them below alongside your review. All the best,

Dr Alex Burton-JohnsonâĂČ

Anonymous Referee #2

Received and published: 20 April 2020

Review of Burton-Johnson et al. Geothermal heat flow in Antarctica: current and future directions

Burton-Johnsen et al. review the geological and glaciologic methods for inferring the geothermal flux of Antarctica. This is an important question for a variety of glaciological applications and the understanding of Antarctic geology. The paper in well written and informative. Probably the best description of its utility is that I've already sent it to 3 of my students to help them understand the different methods used to infer geothermal flux.

What I've found most interesting about the paper is the description of the geologic methods. As a glaciologist, these techniques have been difficult to understand and evaluate in the primary papers and I found this paper helpful, but given my specialty, I am also unable to critically review the geology-based content. It might be worth ensuring that a geologist/geophysicist provides assessment as well.

My only major concern of this manuscript centers on the table of compiled geothermal flux estimates. There has been no significant evaluation of the estimates provided, despite a column entitled "DataQuality". Some sites having multiple different estimates, such as WAIS Divide, with no explanation or evaluation of the difference. To be truly useful, this table needs to be better curated with commentary on why the measurements should or should not be accepted. Because of the huge uncertainties in many (if not all) of the methods, many authors have unwisely justified their own results with erroneous or preliminary interpretations of others. It would be a great service to help remove the confusion about the quality of the different measurements. I realize this is a considerable exercise and understanding the details of each and every measurement is likely beyond what is reasonable for the authors, but even a cursory classification of the confidence in each measurement is hugely helpful. And discrimination/unification of multiple estimates for the same sites seems like a reasonable request.

Minor Comment: The authors are very optimistic about long-wavelength microwave emissivity, which I do not believe is yet warranted. In reviewing Macelloni et al, the authors acknowledge that there was little actual verification, so it is not clear that this technique has added useful information. While a discussion of this technique is useful, it needs a fuller description of the limitations and how difficult they will be to overcome. The sentence on L922-924 reads like a direct funding plea and should be avoided.

Reply to main comments above:

a) "a cursory classification of the confidence in each measurement is hugely helpful. And discrimination/unification of multiple estimates for the same sites seems like a reasonable request."

- A qualitative classification of each estimate in the supplementary table has now been added along with a table of the qualifiers for each classification. These qualifiers are based on literature in the manuscript, so although likely controversial in some instances, are not arbitrarily determined (e.g. 90m as a qualifier for bedrock boreholes is based on the observed depth of surface temperature effects in the DVDP boreholes; Decker, 1974; Decker et al., 1975; Pruss et al., 1974).

b) "The authors are very optimistic about long-wavelength microwave emissivity, which I do not believe is yet warranted."

- Discussion of the caveats of this technique have been expanded.

Replies to specific comments

Specific comments: L9: I know I'm tilting at windmills here, but "geothermal heat" is redundant. Just "geothermal" is enough.

- Whilst "heat" is implicit in "geothermal", "geothermal heat flow" is the full standard terminology used in the literature, and we inherit that convention.

L13: provide at least one sentence on what you found by reviewing methods and compiling estimates before jumping into future directions

- Text added.

L15: Be specific about how the EAIS is the most sensitive to geothermal flux. The EAIS is not uniformly the most sensitive. For instance, the flux of ice in the Ross Ice Streams is incredibly sensitive to geothermal flux and basal water which is not true of the vast majority of EAIS.

- With the removal of the specific comment on microwave emissivity, the EAIS is no longer mentioned.

L16: long-wavelength microwave emissivity has not been sufficiently demonstrated to be useful to warrant a specific bullet point

- Point removed.

L25-27: This seems like an overstatement. I would argue the ice-bedrock friction which controls the basal sliding rate is far less constrained and much more important.

- Emphasis reduced.

L30-33: Provide references

- References added.

L52: change "lower" to "smaller" that size and position are not confused

- Changed.

L76: I don't think the equation came through in the correction form, or else something else is wrong. There is no integral and lamda is never defined. This whole section is therefore very confusing.

- Correct, most of the equation seems to have got lost – Quite embarrassing! This is fixed now.

L85: Geothermal flux is actually not that important to the ice temperature. The accumulation rate and surface temperature are much more important. Once the geothermal flux is sufficient to cause basal melting, the temperature profile is only minorly impacted even for large variations in geotherm flux.

- Sentence reworded. The scale and sensitivity of the ice sheet temperature to GHF variation is discussed in the following paragraphs of this section.

L99: flip -7 and -13 to be consistent with text and ordering

- Corrected.

L99: Reword this sentence because an increase from -13 to -7 would not change the basal melt rate since it would still be below freezing. So be more specific about the threshold behavior of variations in geothermal flux.

- -7°C is the mean temperature (already stated). That the basal melt rate increases is shown by the increase 16% to more than 50% of the basal area exceeding the PMP.

L106: The PMP effect is incredibly small. The PMP decreases the basal temperature by âĹij2C from the thin ice at the coast to the thick ice in the interior wherease the surface temperature decreases by âĹij30C. This just isn't a big effect.

- We agree with the reviewer that the effect of pmp on basal temperature is small. However we are discussing here the sensitivity of basal conditions to GHF. Both references cited show that the sensitivity is larger in the interior of East Antarctica and the reason is that the basal temperature is closer to the PMP. In areas where basal temperature is near PMP, small changes in GHF affects the extension of basal melting and has a stronger effect on the ice sheet. We have rewritten the sentence for clarity.

L121: change "are" to "can be" because some ice streams, like the Ross ice streams, with very low driving stress don't produce a lot of melt and are appear to be freezing at the bed (Joughin et al., 2004, Melting and freezing beneath the Ross ice streams, Antarctica).

- Text changed.

L122-125: This is a subtle concept, so please articulate what is happening in more detail.

- Text added. As with all points in this summary paper, the reader is forwarded to the correct reference.

L137-138: Frozen beds are not a prerequisite for deep coring operations. Dome C, Dome Fuji, NGRIP and NEEM are examples of drilling to temperate beds. Potential melting may necessitate clean access or a buffer, but the language should be clear.

- We agree with the reviewer our comment is focussed on 'Oldest Ice challenge' and not true for every ice core location. We have written the sentence.

L243: Fig 3: This seems like a really strange example to choose. First, there is nothing in the figure which shows what the geothermal flux is. Second, where is the bed? Third, this appears to be a high accumulation rate and shallow ice thickness site, where the temperature profile is dominated by advection, and the confidence in the geothermal flux is low. I'd recommend using either Law Dome (Dahl-Jensen et al., 1999) or Siple Dome (Engelhardt, 2004) as better examples.

- Replaced with the Dahl-Jensen (1999) example from Law Dome.

Paragraph starting at L247: Maybe I'm just confused, but where is advection in this? This seems appropriate for a material that doesn't flow. If I'm wrong and advection is being considered, please articulate how it fits into the circular frequency.

- As stated at the start of this section, the method is applicable where the ice sheet is stationary (frozen to the bed) and thermally equilibrated; hence horizontal advection is minimal. For clarity, this point is now repeated at the start of this section.

L267: These three references are all abstracts. I don't think this method will actually work because there is too much memory of past temperature, too much uncertainty in the vertical velocity profile, and too much uncertainty in the thermal conductivity of ice. (also, this will really only provide an lower limit since increasing the geothermal flux will cause basal melting at some point and then the temperature profile because relative insensitive to variations in geothermal flux)

- The debate over an estimation of a simplified model 'working' or 'not working' is futile. There is no way around the uncertainties that the reviewer is mentioning and we will only say here that in any study they should be propagated into the estimation uncertainty. In any case, we agree with the reviewer that the estimations rely on a simplified models and this need to be clearer in the paper. We have rewritten the sentence.

I don't feel qualified to comment on section 4.

L661: I don't like the forward vs. inverse model distinction since many of the "forward" methods are based on inverse methods.

- Good point; reworded.

L665: change "ground" to "ice"

- Reworded.

L711: Section 5.2. This makes a compelling case for inferring minimum geothermal flux. I'd suggest adding to this section the reverse case: that is, the maximum geothermal flux can be estimated if the ice is known to be frozen to the bed. Raymond Arches are compelling evidence of a frozen bed, which Fudge et al. (2019) used to estimate maximum geothermal flux for two Siple Coast ice rises. Together, the minimum and maximum inferences can be more useful than either alone.

- Text added.

L728: Describe how far from South Pole. There's a lot of discussion of the South Pole lake, which is âĹij100km away, so the work of Jorden et al. is actually pretty far

removed from South Pole.

- Distance added.

L736: I think the authors are overly optimistic about this method. In reviewing Macelloni et al., it seems like they were able to infer little besides the ice being warmer with depth. The authors acknowledge that there was little actual verification. While the section should be included, it needs a fuller description of the limitations and how difficult they will be to overcome.

- The limitations of the method and the challenges in developing the approach are now expanded in the section on current challenges, Section 7.3 (also responding to comment "L915-924" below).

L767: I think you should add a section on using englacial attenuation to determine ice sheet temperature. While published literature has mostly focused on depth-age average values (MacGregor et al., 2015, doi:10.1002/2014JF003418; Schroeder et al., 2016, doi: 10.1017/jog.2016.100), there has been considerable progress with obtaining depth profiles, particularly as multiple radar systems can be cross-compared.

- The studies on temperature-dependent attenuation by Carter et al (2009) and Schroeder et al (2016) are already discussed in Section 5.1.

L852: This sentence underestimates what is known about Dome C geothermal flux. The geothermal flux is actually quite well constrained by glaciological modeling (see Parennin et al., 2007). The de Mendoze retracted paper is an incredibly strange reference that is completely outside of the glaciological and ice core communities. I think this is very misleading.

- Sentence removed.

L857: Figure 16: would a log scale make more sense since the 30mW/m2 cutoff seems too small - Scale expanded to 60 mW/m2

L861-874: Thank you for this paragraph. It is wonderful to read an insightful critique of the Cure depth technique. I think you are too diplomatic when you write “without being critical of the model itself”.

- Thank you.

L915-924: I think this section is too optimistic. The last sentence in particular is inappropriate.

- The last sentence has been removed and the limitations of the method expanded (also addressing point “L736”)

L960: “sliding” not “slide”

- Changed.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-59/tc-2020-59-AC3-supplement.pdf

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