

Interactive comment on “Crustal heat production and estimate of terrestrial heat flow in central East Antarctica, with implications for thermal input to the East Antarctic ice sheet” by John W. Goode

Anonymous Referee #1

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This article written by J. Goode is an attempt to improve our knowledge on the geothermal flux under the Antarctic Ice Sheet, and more precisely on the Byrd and Nimrod catchments. This topic is an important issue in our community, regarding the stability of the Antarctic Ice Sheet from a thermal and mechanical point of view in the future. Even if the conclusions do not change the previous estimations of the geothermal flux, the sample-based method used here constitutes a crucial cross-checking from field work. While our knowledge is mainly based on modelling results and a few analysis of temperature profiles in boreholes, this work takes advantage of glacial rock clasts gathered at the catchment output and taken as samples of the whole catchment. This original approach is valuable considering the difficulty to access the bedrock under the

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ice sheet. Furthermore, it allows the author to have an interesting interpretation of the heat flow distribution from a geological point of view.

The article is pleasant to read, and its structure makes the text easy to follow. Scientific context, methods, assumptions and discussion are well stated. The figures are precise and complete. The uncertainties are well evaluated and their origin discussed.

General comments

Hereafter are a few remarks that could help improve the manuscript. The main concern I have is related to how the sampling process is explained.

Samples

Your results and interpretation are based on the processing of 18 samples. Of course, the processing step is heavy and it is difficult to have more samples. However, the interpretation of the distribution is based on these 18 samples only, and a different sampling could lead to significantly different interpretation. For example, only one sample correspond to a heat flow higher than 80 mw/m², and appears like an outlier, but this could appear quite different with a larger sampling. I suggest that a small paragraph discuss this point, even if it is impossible to have more numerous samples.

More serious, I do not understand how the samples have been chosen, and why the different sampling sites are not more equally distributed. This is a problem since your conclusions are very sample-dependent. Furthermore, Table 1 is not so clear, since the acronyms LWA, LWB, MSA, TNA are never explained. I understand it refers to the sites, but I cannot figure out which one for LWB. Why Lonewolf Nunataks would be present so many times, whereas AGA and MRA disappeared ? Did you sampled some rocks at AGA and MRA, and discarded them in a second step ? I think §2 needs to be completed, so that your selection criteria are clearly stated.

Interpretation

Your sampling concerns the Byrd catchment mainly, which, I think, stands across Vic-

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toria Land, Adélie Land and Wilkes Land. But your interpretation concerns “the Wilkes Land region” (P.7 L.10), which is a much larger region than your source area. This is a bit confusing. Your conclusion say that an extrapolation of CAHFP under Antarctica is potentially erroneous, whereas your samples concerns a very small part of the Wilkes Land. For example, an extrapolation could be justified close to the coast of the Wilkes Land, or even at Dome C. I suggest the end of §4.3 is changed for a more careful formulation, conclusion and abstract as well.

Exploiting the ice flow ?

I was first very pleased to see that you mixed geology and ice flow aspects. However, after reading the whole paper, I felt like disappointed that you cannot fully exploit the geographical aspect given by the ice flow. The Transantarctic mountains make the problem much complex, but having a comprehensive ice-flow interpretation could be an interesting scientific perspective... to add in conclusion ? In the meantime, could you at least look if there is any gradient between MSA and LWA for example, in heat flow or chemical composition ? Even if it does not give any additional information, you could just mention that you looked at it.

Minor revisions

P2, L12 : Is the work of Fisher et al (2015) not already a direct subglacial measurement of geothermal flux ? So I would say “..., and very few from direct subglacial measurements”. The same in the abstract.

P2, L24 : Since the heat budget of basal ice depends both on ice thickness and geothermal flux, there is no direct pure correlation between ice thickness and basal age, I suggest “relatively thick, slow-moving...”.

P3, L19 : the samples have travelled for hundred of thousands of years, and the ice sheet has not always had the present shape. In particular during glacial areas, do you have any idea of the shape of the catchment given by continental model outputs ? (if

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possible)

P4, L11 : how are the samples selected, and how many for each site ? Why are some sites over-represented in the number of samples ? How is the choice of the sample number affect the conclusion ?

P6, L21 : Could you briefly explain why it is necessary to make use of the two methods, since the results are similar ? I guess that, in other circumstances, they could significantly differ ?

P7, L8 : “Compared to global examples”. To make it more clear, could you briefly specify, or add references ?

P7, L19 : I think q and H are not defined, maybe change for a specification in words : “the function linking heat flow and heat production”.

P8, L23 : A suggestion concerning Δhr : could you consider the variability of the bed topography on the source area as an estimator for the crust uncertainty, so that your uncertainty has a better specified origin ? I think you should be close to your present value of 1500 m and will not change your conclusions. If you disagree with this approach, could you better justify this value or add a reference ?

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