Author's response to all reviewer comments posted for manuscript tc-2023-101 after the first round of response/review.

Date 19.03.2024

Dear reviewers and editor,

once again we appreciate you all having taken time to review our submission and send valuable comments on our manuscript. Below we have compiled all our point-by-point replies to all reviewer comments in one document. They are identical to our individual reply comments in the online discussion but are compiled here for convenience accompanying our revised manuscript submission.

Editor comment / public justification:
In taking all reviews into account, I feel that we can accept your manuscript pending some final corrections, but I would strongly advise you to strengthen the defense of your model assumptions in order to explain why they are valid despite the assessment of Reviewer #1 - and potentially what limitations they might introduce to your conclusions.

<u>Authors response:</u>
As laid out below in the response to reviewer #1's comment on our numerical approach, the basic assumption reviewer #1 builds his/her assessment upon is based on a misunderstanding. In summary, we have never claimed to attempt to simulate short term variations in subglacial heat flux which would require a different numerical boundary approach. Our data as well as the simple fact that ice dynamics smooth out any short term heat flux variations as such influences are transferred to the glacier surface, both only allow for a long term average estimation. Our methods aim to study the influence of long term (yearly timescale) effects of averaged subglacial heat flux on ice dynamics. Simply put if large values of heat flux are sustained throughout a one year period, the ice surface depressions (our data) prevail. On the other hand, if the heat flux ceases to exit, the ice surface depressions would fill in by ice flow. Having surface evolution data available only for two snapshots in time, about a year apart, only allows us to estimate approx. yearly averages of subglacial heat flux. In such a setting our model assumptions as well as numerical methods are entirely sufficient. All this has been explained in detail in the manuscript.

Thus we do not see the necessity to defend our model approach regarding this comment from reviewer #1.

Anonymous referee #1, Report #2:

Reviewer comment:

In Table 1 of the revision, the authors add a new column, average basal heat fluxes (q_h) for the geothermal area, with the unit of W m-2, and many values of q_h are about 2000 W m-2, which are super high. I noticed in the authors' response that they expect heat flux values on the order of magnitude of highly active geysers (e.g. Old Faithful) or steaming vents of powerful geothermal areas rather than heat fluxes created by vertical, conductive heat flow through the Earth.
I assume it may be true.

Authors response:

We agree that 2000 W m^{-2} is high. Such values, even higher are however not unheard of for geothermal areas in volcanic regions. See e.g. table 9 in:

Sorey, M.L., Colvard, E.M., 1994. Measurements of heat and mass flow from thermal areas in Lassen Volcanic National Park, California, 1984–1993. U.S.G.S. Water Resour. Invest. Rep., 94–4180-A.

where the size of the areas investigated is typically on the order of ~10,000 m^2 or similar to the size as the area, which we calculate average basal heat flux over.

For further information on subglacial geothermal areas in volcanic regions we also recommend reading

Jóhannesson T, Pálmason B, Hjartarson Á, et al. Non-surface mass balance of glaciers in Iceland. Journal of Glaciology. 2020;66(258):685-697. doi:10.1017/jog.2020.37

Jóhannesson, T.; Thorsteinsson, T.; Stefánsson, A.; Gaidos, E. J. & Einarsson, B.
Circulation and thermodynamics in a subglacial geothermal lake under the Western Skaftá cauldron of the Vatnajökull ice cap, Iceland Geophysical Research Letters, American Geophysical Union (AGU), 2007, 34 https://doi.org/10.1029/2007GL030686

The former paper lists several subglacial volcanic areas in Iceland (in Table 1), with long term geothermal power on the order of hundreds even exceeding 1500 MW. The main surface expression of the geothermal activity are surface depressions, ice cauldrons, like K6, which is subject to this study. In the later paper, water temperatures at a subglacial geothermal system in Iceland are reported to reach 310 °C.

Reviewer comment:

However, such high heat flux at the cauldron from geysers or steaming vents is instantaneous or transient energy, rather than long time steady energy. The estimated basal vertical velocity (Eq. (1)) is also instantaneous or transient. Therefore, I do not think one can use it as a boundary condition for steady state simulation. I am afraid the numerical modelling method is unfortunately not suitable.

Authors response:

The mentioning of geysers or steaming vents was mostly to point out the kind of geothermal processes we are likely dealing with in contrast to vertical, conductive heat flow through the Earth. We agree that this may be slightly misleading, particularly regarding geysers, which for sure release instantaneous or transient energy. Steam vents or fumaroles can however be quite stable on an annual time scale, the time scale of this study, releasing thermal energy at high and rather fixed rate. We may have fumaroles, hot springs and steam heated mud pools beneath K6, but a significant part of the heat flux may be through soil at the basin heated by the geothermal fluid from below, see e.g.:

Fridriksson Th., Kristjánsson, B. R, Ármannsson H., Margrétardóttir, E., Ólafsdóttir, S. and Chiodini G.: 2006. CO_2 emissions and heat flow through soil, fumaroles, and steam heated mud pools at the Reykjanes geothermal area, SW Iceland. Applied Geochemistry, vol. 21, 9, p. 1551-1569,ISSN 0883-2927. https://doi.org/10.1016/j.apgeochem.2006.04.006.

It is however a misunderstanding that the estimated basal vertical velocity (Eq. 1) is instantaneous or transient. The high heat fluxes and net power numbers (Q) reported in our study as well as in e.g. Jóhannesson et al. 2020 (reference above) are exactly long term average estimates. Our method averages heat fluxes over the period of about one year and the study of Jóhannesson et al. 2020 over even longer periods. Through our indirect estimation technique, i.e. studying persistent ice surface depressions sustained by basal heat fluxes, we can only detect long term averages. This is mainly due to the nature of ice flow, which averages out instantaneous variations in basal conditions. Thus we chose our numerical methods as they are presented in the paper and we see them quite fitting for the purpose of estimating long term averages.

Anonymous referee #2, Report #1:

Reviewer comment:
The only technical suggestion is that there was some confusion by both myself and the other reviewer whether the model was transient or steady state. The authors have clarified it is steady state, but expressing Eq. 2 in terms of "ds/dt = ..." is therefore confusing (i.e. a transient formulation). Eq. 2 should perhaps more properly be "ds/dt = 0 = ..." (i.e. a steady formulation). I also still feel that "basal ice outflow velocity" is a new term being created by the authors, when the traditional "basal mass balance" might serve better. Best to be consistent with: https://unesdoc.unesco.org/ark:/48223/pf0000044615 (section 4.3) - perhaps at least insert a sentence of equivalency between new and old terms.

Authors response:
We would like to thank anonymous referee #2 once again for reviewing our manuscript. We fully agree with the terminology (even though we do not use the term "basal ice outflow velocity" anywhere in the manuscript) and have added a sentence clarifying when we speak of our simulations ("ice outflow velocity" is what the numerical model uses as a boundary condition) and what this numerical setup means in reality ("basal mass balance").

However, responding to the comment on the nature of our simulations (steady-state vs transient), it seems there is still a lingering misunderstanding. Thus we have again clarified how the model operates (lines # 62-64 in the newly submitted version). In summary:

- We do not use equation (2) at all, as stated. This equation is mentioned to demonstrate the "classical" glaciological approach to ice surface evolution.
- The ice flow model simulates steady-state velocities, in balance with the boundary conditions applied, including prescribed basal mass balance.
- Equation 3 is subsequently used (now even more explicitly stated in the manuscript) to move the ice surface forward in time.

Hence the ice-velocity computation is steady state whereas the ice surface evolution computation is transient. As equation (2) is never used in our work we do not see a reason to modify it according to the reviewer's comment as it is just a statement of a general ice surface evolution equation. We do hope that we have clarified all misunderstandings with editing the text motivated by the comment of reviewer #2.

Referee #3, Fausto Ferraccioli, Report #3:

Reviewer comment:
The paper Geothermal heat source estimations through ice flow modelling at Mýrdalsjökull, Iceland by Jarosch et al. presents an indirect measurement method, which utilizes ice flow simulations and glacier surface data, such as surface mass balance and surface depression evolution to determine heat source locations to simulation grid scales. As such it is of interest for regional studies of the Mýrdalsjökull ice cap in Iceland but also more broadly as geothermal heat sources beneath glaciers and ice caps influence local ice-dynamics and mass balance and subglacial water reservoir dynamics.

The authors have done a good job in considering all the comments of previous reviewers and given the MS clarity and the important topic I would recommend acceptance basically as is.
My only small residual recommendation is to add a reference to a more recent paper on estimating geothermal heat flux indirectly beneath one of the most important sectors of the West Antarctic Ice Sheet, highlighting also that this topic is of great interest also outside Iceland.

Specifically, in lines 10-11 The role of subglacial geothermal heat in the mass balance and dynamics of glacier and ice sheets has in recent years caught increased attention (e.g. Winsborrow et al., 2010; Smith-Johnsen et al., 2020b, a). I would recommend adding Dziadek et al., (2021).

Dziadek, R., Ferraccioli, F. & Gohl, K. High geothermal heat flow beneath Thwaites Glacier in West Antarctica inferred from aeromagnetic data. Commun Earth Environ 2, 162 (2021). https://doi.org/10.1038/s43247-021-00242-3.

Authors response:
We would specifically thank Dr. Ferraccioli for taking the time to review our manuscript and adding a third viewpoint to the review process. We have, of course added the valuable reference he suggested.

We would like to express our gratitude for all the valuable comments and the time put into this review by the reviewers and the editor.

Kind regards,
Alexander Jarosch on behalf of the authors.