Author's response to comments from William Colgan, Referee #2, posted on 09 Nov 2023

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Dear William Colgan,

we value your time investment to review our submission and sending very helpful comments on our manuscript. Find below a detailed list of your comments and our response along with references to the manuscript describing changes we made.

Reviewer comment:

Term Convention – The V_z term in equation 2 is referred to as "outflow velocity". I think this would more probably be called the "basal vertical ice velocity", or even most conventions would probably refer to this as "basal mass balance", and denote it more analogous to the B-dot surface mass balance term. Later, it seems that the "V_z" in Eq 2 is being denoted "UZ" in Section 2.3 and beyond. It seems UZ(UZ_0) is applied within the caldera, but it is not clear if there is a basal mass balance applied outside the Gaussian representation of the caldera. The reader could use some clarity on this term, both regarding the notation and the written description.

Authors response:

We think the reviewer refers to equation (1), as equation (2) in the manuscript describes a standard surface evolution equation for glaciers. Indeed there has been some confusion in the initial manuscript on what the term v_h (initial manuscript notation) should be. Based on the comments of reviewer #1 and the comment here, we have rewritten equation (1) where now the variable v_z,bh is called "a basal, vertical ice flow velocity" (as suggested above) and the new version of equation (1) clearly defines how this velocity is calculated from a given basal heat flux. We refrain from calling the variable a basal mass balance, as we want to convey the notion that there is basal ice outflow (in the model). Regarding the question " *if there is a basal mass balance applied outside the Gaussian representation of the caldera [i.e. heat source]"*, the paragraph right below equation (4) clearly states that equation (4), the Gaussian representation of the heat source, is only applied within a given radius R and set to zero outside. So no additional basal outflow velocities are used.

Reviewer comment:

Heat Flow Units – The peak "outflow velocity" (or peak basal mass balance) of the simulations are given in m/yr, and then area-integrated heat flow in W. It would be quite helpful to have the UZ_0 also given in W/m2, which is the more conventional units with discussing heat flow. This would allow the heat flows being reported here to be more directly compared with extreme values in the International

Heat Flow Database, for example. At first glance, basal melt on the order of 1000 m/yr seems phenomenally high, perhaps even unrealistically high.

<u>Authors response:</u>
This is a very good idea. Even though Table 1 includes everything needed to calculate the average heat flux (bar{q_h}), we have added a column that includes average heat flux values for the whole subglacial heat source area. We think this measure is more informing than reporting the peak heat flux based on (UZ_0), which is applicable for a very limited spatial extent.
However it is noteworthy that we 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 then heat fluxes created by vertical, conductive heat flow through the Earth (which are mostly listed in the International Heat Flow Database).

<u>Reviewer comment:</u>
Simulation Type – I would be interested to see the heat flow inferred by a steady-state simulation (i.e. maintaining a supraglacial caldera depression over centuries). It can be difficult to entirely attribute simulated changes in ice geometry to specific processes over a 1-year transient simulation, as I guess there would be some underlying transient drift or model relaxation. I see mention of a "heat sources off" simulation (L234), which may be akin to a relaxation simulation, but this simulation suggests the depression only in-fills by 15 m. I am therefore wondering how 15 m of ice dynamic infill requires 100s of m/yr of basal melt to maintain the depression. Or simply put, why is 15 m/yr of infill not just balanced by 15 m/yr of basal melt?

<u>Authors response:</u>
The statement in L246 (L235 initial manuscript) refers to the glacier surface elevation change in the center of the surface depression. This is not an intuitive measure for the overall mass flux into the surface depression as it is only an easily observable consequence of a large scale horizontal mass movement into the surface depression. This can be seen in Figure 2, where the surface lowering around the depression (blue colors) has a significantly larger spatial extent in comparison to the red circle (subglacial heat source extent). All the mass moving from the blue areas in Fig. 2 towards the red circle have to be compensated by basal ice outflow within the red circle to reproduce the observed surface depression change, hence the significantly larger values of basal outflow in comparison to the surface lowering. Simply put, it is the difference in "area of influence" between the surface and the base of the glacier which creates the difference in vertical movement magnitude.
A centuries long study of the surface depressions would indeed be interesting, however is out of the scope of this study, partly due to its high computational cost.

<u>Reviewer comment:</u>
Subglacial Water Storage – The assumption that basal melt flows away immediately, and there is no change in subglacial water storage during the simulation year seems quite important, as changes in basal water storage can directly influence the surface modelling target. The authors write "In contrast to observations from a GNSS station, operated at K6 in the summers of 2016 and 2017, revealing seasonal water storage and drainage under the simulated cauldron, we assume continuous and instant water drainage underneath the glacier." It would seem useful to show such a GPS vertical displacement record and provide more description of the water storage signal (i.e. magnitude and temporal variability).

Authors response:
Indeed the assumption that “possibly created meltwater at the base of the glacier is drained instantly” is a simplification made in this study and is in contrast to observed GNSS station data which hints to temporal variations in subglacial water storage. However the focus of this study is such a simplified view on basal processes as explained in lines 214-218 (new manuscript version, lines 202-206 initial version) to be able to apply the numerical modelling. A separate study is in the making which will analyze several cauldrons on Mýrdalsjökull with respect to potential subglacial water storage and collected GNSS data.
For this contribution we choose to omit GNSS data records as they are not part of the study design nor the modelling capabilities and focus on the overall surface geometry changes of K6 as discussed in the paper.

We would like to thank you for taking the time to review our manuscript and all the valuable comments you gave.

Kind regards,
Alexander Jarosch on behalf of the authors.