To Referee #1,

We appreciate the very detailed and carefully thought out response by the anonymous reviewer. We think that a number of valid points have been brought up, specifically regarding clarification of how field measurements published in the literature have been incorporated into the model and the assumptions inherent in Experiment 3.

We have incorporated the majority of the suggestions into our manuscript. The one major point where we disagree regards section 3.2.2, discussed below. The two objections mentioned in the review are that assumptions regarding aquifer thickness sensitivity are hard to validate, and that the conclusions are not significant. While we somewhat agree with the first objection, we note that it is only ever used in order to plot the light dashed lines over the parameter space, and serves as a visual aid for a single set of hypothetical future trajectories. To the second objection, we think that these results are useful in developing predictions about how thermal regimes may evolve across a wide range of environmental conditions (more than can be represented by independently varying parameters in the REFT/REFC models). The conclusions that we draw are that the relative importance of air temperature, aquifer thickness, and ELA (AAR) vary across the parameter space, such that various glaciers may respond differently.

Responses to specific comments:

> P 3783, line 10: "Models of polythermal ice masses often neglect...": I suppose that the authors refer to "ice-flow models" but they should mention it clearly.

– This is correct, and we have now been explicit about this.

> p. 3783, line 16: which kind of implications?

– We are concerned primarily about the implications regarding heat conservation (that may allow temperate ice to contain more heat than represented in temperature-based models), and secondarily about ice flow enhancement. The text has been clarified.

"Accounting for the water content of temperate ice therefore changes the simulated thermal structure. The implications for glaciers with large volumes of temperate ice have not yet been thoroughly investigated. Using a temperature-based model for polythermal glaciers violates energy conservation and ignores the potential effect of water content on ice flow enhancement."

> p. 3784, l. 9-11: the topic 3) seems not clear to me. I have the feeling that it is not very different from topic 2). Could the authors merge the topics 2) and 3) or could they formulate them better? In addition, in Section 2.3 (P. 3793, l. 14-18), the authors mention 3 experiments in order to reply to 3 topics. The authors should clarify this point.

– Because point (3) is an interpretation of (2), we agree that it is reasonable to merge them. As the referee hints, this has the added benefit of establishing parallelism with the three experiments described.

> p. 3785, Equations 4 and 5: in Equation 4, thermal conductivity is$k$; in Equation 5, the authors use k_{eff} and the difference should be explained. I assume thatkeff is used for k in Equation 4 but it is not very clear.

– It is correct that they are the same at shallow depths. Following the advice of referee #2, we have combined equations 4 and 5 into one and discarded the unnecessary names k and k_{eff}.

> p. 3787, l. 4: the authors should explain why they do not consider the basal ablation.

– A quick calculation with the reference geothermal flux of $60\,\text{mW}\,\text{m}^{-2}$ yields a basal ablation rate of $6\,\text{mm}\,\text{a}^{-1}$, which is small enough that we ignore it. We don't expect basal ablation to be large in most settings with polythermal glaciers compared to the surface ablation, however see Alexander et al. (2011) for a counter example (in the glacier considered there, intense rainfall events are required to lead to high predicted basal melting). Very high sliding rates or geothermal fluxes might make basal ablation relevant in isolated cases.

p. 3787, Eq.10: the authors should mention that Q_m is calculated in the whole aquifer thickness.

– Yes, the calculation is per-unit height in the vertical dimension. We have clarified this.

p. 3788, l. 10-21: The runoff fraction is an important point of the model which has a strong impact on the results. Unfortunately it is poorly referenced. Moreover, the only references are relative to Greenland and not to alpine glaciers.

– We acknowledge that r is poorly constrained by limited and imperfect data, and have modified our manuscript to emphasize this. One possible additional reference is that of Rabus and Echelmeyer (1998). They estimate ranges of internal accumulation for McCall Glacier that seem to imply r values that vary widely and exhibit high annual variability. At the time of measurement, McCall Glacier did not consistently have an accumulation area, so it is not clear how well these results should be applied to a simplified and more classical glacier with an accumulation area.

> "Rabus and Echelmeyer (1998) give estimates of internal accumulation on McCall Glacier that imply a wide range in melt run-off can occur from year-to-year, although this may be exaggerated on in this case because of the mercurial accumulation zone conditions on McCall Glacier. The run-off fraction is therefore poorly constrained, so with a reference value of $r = 0.5$, we alter the run-off fraction between 0.2–0.8 in order to evaluate a range of contributions to water entrapment."

p.3789, l.1-8 : The near-surface aquifer thickness is poorly constrained. In the model, the near-surface aquifer thickness is invariant in space and equal to 3 m in the accumulation zone. The authors selected a test range of 0.5-6.0 m. Are the results very different with larger values of aquifer thickness or with a different spatial pattern?

– The range of values that we chose spans the range of sensitivity of the overall thermal structure (see Figure 5b, with an asymptote at $H_{\text{aq}} > 4\,\text{m}$). Regarding spatial patterns, see the response below.

p. 3789, l. 20-24 : water content in snow : the authors should provide more information about the variability. They give a range of values in table 2 without any explanation or reference.

– The density, and therefore the allowable water content in dry snow varies, and may depend on temperature and accumulation rates. The range that we choose spans the range of interesting model behaviour. We have added note of this variability to 2.1.2, and added the results to Figure 5. We find that above a small threshold that is likely to be exceeded in realistic settings, snow water capacity ceases to be a limiting factor in our model.

> "To quantify model sensitivity to ω_{aq}, we test over a range of 1–15%. Plausible physical reasons for this variation include variations in accumulation rates and temperature-dependent densification rates."

p. 3793, l.26 to p.3794, l.5: the authors provide an annual balance function without any discussion about the uncertainties on $\dot{b}_{\text{max}}$ or balance gradient. $\dot{b}_textmax$ and balance gradient are supposed fixed and not considered in the model sensitivity tests (Table 2) (Degree-day factor and ELA are considered). It is surprising, given that a main conclusion of this paper is related to the mass balance sensitivity which "plays an important role in determining how the englacial thermal regimes of alpine glaciers will adjust in the future" (see Abstract).

– We try to separate the balance gradient and the balance-temperature sensitivity ("balance sensitivity") into two separate issues, from which we focus on the latter. The conclusion that balance sensitivity is important is drawn from the results of Experiment 3, which tests numerous balance sensitivities.

Detailed exploration of the mass balance parameterization itself is beyond our intended scope, yet we agree that a simple test of balance gradients may be warranted in order to provide a sense of how important the simplifications used are to other results. Furthermore, we see why the referee may have expected to see this in Experiment 2.

Accordingly, we have added an additional figure that is similar to that for independent parameter sensitivity, but to describe balance sensitivity. We choose not to add balance sensitivity to Fig. 5 because the experimental set-up is different enough that to do so would be misleading. A description and interpretation of the results has been added to Section 3.2.1.

> "In a final pair of single-parameter sensitivity tests, we investigate the effect of adjusting the vertical mass balance gradient ($\dot{b}_z$) and the maximum balance threshold ($\dot{b}_{\max}$). In these experiments, the differences in glacier geometry are large enough that we report the results from tests with a freely-evolving ice surface (Fig. fig:balancesensitivity). When the balance gradient ($\dot{b}_z$) is small, mass turnover within the glacier is low. The corresponding lower advection rates cause temperate ice to be largely constrained to the upper glacier, and the area of the modelled flow-band that is cold is large (Fig. fig:balancesensitivity a). As the balance gradient rises, the cold ice area drops slightly and the temperate ice volume rises steeply. The mass balance threshold ($\dot{b}_{\max}$) affects the thermal structure largely by restricting glacier accumulation. At low values, the glacier is thinner and flows more slowly, which causes the temperate area of the flowband to be small relative to the REFT control model. As the balance threshold rises, the cold ice area stays nearly constant (Fig. fig:balancesensitivity b), but the temperate ice area rises."

p. 3796, l.13 : add "(Eq. 14)" to help the reader.

– This addition has been made

p. 3796, l. 16- 24: It is not clear how this factor C_u is included in the model. In Equation 3? The authors should mention it. Moreover, I do not understand why the authors introduce a new variable C_u. Changing the surface mass balances should also change the ice flow velocities and would have a similar effect. It is not clear for me to which changes is associated C_u. (see also p 3802, l.23)

– Yes, u in Equation 3 is multiplied by C_u — we have clarified this. We introduce this experiment in order to isolate the effect of increasing advection alone. Different advection rates than are represented by the reference model are possible, for example, due to difference values of A_0 in the flow-law parameterization (due to impurities, anisotropy, etc.). We have attempted to address changes in the balance gradient separately (see comment above).

p. 3797, l .10-12 (Eq 21): the authors assume that the near-surface aquifer thickness is related linearly to net balance. Given that this assumption has a very large impact on the sensitivity study, the authors should justify it. Is it realistic to parameterize the aquifer thickness in this way? Does it correspond to data found in literature? The authors should provide justifications or recognize the weakness of this parameterization.

– As accumulation rates decline, there should be a response in the near surface density profile, which we hypothesize directly alters the near surface aquifer thickness. The form of this relationship is complicated by additional processes such as water percolation and refreezing, and we are not aware of field results that explain how the aquifer thickness should respond over time in polythermal glacier settings. Therefore, we make the assumption embodied in Equation 21. An alternative assumption is that the aquifer thickness is unresponsive to net balance. I have re-performed Experiment 3 making this assumption. As one might expect, the effect of rising temperature is weaker when the aquifer thickness is not assumed to decline, but the trajectories experienced by REFT in the posited climate scenarios are not substantially different. The trajectories experienced by REFC are worth mentioning in this case – it becomes more likely that the temperate ice fraction increases as the glacier retreats. The following discussion has been added:

> "We recognize that this simple parameterization is an important assumption, but it qualitatively captures the behaviour that we hypothesize. Namely, we expect that as temperatures rise the

near-surface aquifer will grow thinner and this rate of change will to a first approximation be proportional to the change in accumulation rates. The percolation pathways within the aquifer also involve ice lenses formed by seasonal refreezing of meltwater (Jansson et al., 2003). We do not consider lateral transport within the near-surface aquifer, but we speculate that the presence of ice lenses will have a limiting effect on aquifer thickness that brings the aquifer sensitivity into closer agreement with the annual balance sensitivity (e.g. Eq. 21)."

as well as the result:

> "In order to test the impact of the assumption in Eq. (21), we perform the experiment above with the alternative assumption that the aquifer thickness h_{aq} is constant. The resulting thermal evolution for REFT is similar to above, however the temperate ice loss is milder. In this case, the effect of the rising equilibrium line (z_{ELA}) is sufficient to cause a significant reduction in temperate ice extent. For the REFC model, the results are slightly different; a number of balance scenarios result in rising temperate ice fractions as more meltwater is captured in the near-surface aquifer. Uncertainty therefore exists because the future evolution of thermal structure depends on the behaviour of the near-surface aquifer. Although $\partial h_{\mathrm{aq}}/\partial T$ is poorly-known, we consider the relationship in Eq. (21) to be more realistic than assuming no change."

p. 3798, l. 2: the authors should mention the timestep.

– We have added a note discussing timestep in section 2.2. The timestep is permitted to change adaptively in order to maintain convergence. The maximum timestep is chosen on an ad hoc basis, but is never greater than 60 days to ensure that the simple seasonality is represented well enough for an accurate solution. In prognostic experiments, we found it better to limit it to 36.5 days.

p. 3800, l. 1 : the section is 3.1.1 but section 3.1.2 does not exist.

– The section heading has been removed.

p. 3801, l. 14-28: the role of h_{aq} is very important. However, h_{aq} is poorly known and probably vary spatially a lot in the accumation zone. I believe this point is important and should be highlighted by the authors. Moreover, the authors should explain why the temperate ice fraction do not increase with value larger than 3 m ?

– Spatial variability in h_{aq} is discussed in section 2.1.2 (p. 3789). In short, we agree that spatial variability is almost certainly large, but lacking data or theory we choose to simplify with a single characteristic value.

> "It is reasonable to expect that the aquifer thickness varies spatially, perhaps being thicker at high elevations resulting in a tapered shape. Alternatively, colder temperatures at higher elevations may cause faster refreezing and decrease the thickness of the permeable layer. In light of uncertainties in how to best represent variable near-surface aquifer thickness, we make the minimal assumption that the near-surface aquifer thickness is invariant in space. We choose $h_{\mathrm{aq}} = 3$ m as a reference value, and test over the range 0.5–6.0 m. If this assumption is violated, areas where the aquifer is thicker would tend to preserve more liquid water through the winter, while areas where it is thinner would preserve less. This might either reinforce or oppose the gradient in water entrapment implied by melt volumes that decrease with altitude."

The temperate ice fraction does not increase after a critical threshold in h_{aq} (here, ∼4–5 m) because once water is preserved through the winter at the base of the aquifer, there are other limiting factors. One is the amount of water that can be contained within the glacier ice (ω_{eng}) because quantities above this are assumed to "drain". Another is the amount of water that is generated through melting, limited in the model by f_{dd} and air temperature.

p. 3802, l 1-2: the authors should add in Figure 5 a graph with the sensitivity to snow water content although they mention that the thermal structure is insensitive to water content >5%.

– We have now done this; see comment above.

p. 3802 section 3.2.1: Section 3.2.1 concerns “parameter sensitivity” but all the parameters shown in Table 2 (“Environmental parameters varied in model sensitivity tests) are not discussed. The runoff fraction sensitivity is not discussed. Again, I believe that the authors should add in Table 2 the balance gradient and $\dot{b}_{\max}$ and discussed them in the sensitivity analysis. Given that meltwater entrapment plays a primary role in the thermal structure, I believe the authors cannot avoid them in the discussion.

– Run-off fraction has now been included. Discussion is limited because it is not mathematically independent from f_{dd}, and so our simple model cannot distinguish between increases in r or decreases in f_{dd}. Balance parameters have been added; see response to previous comment.

p. 3802, l. 8-12: the discussion about the aquifer geometry remains very qualitative and from this paragraph, it is difficult to know if the assumption made by the authors (constant aquifer thickness in the accumulation zone) has a strong impact or not.

– We discuss the aquifer thickness in greater depth on p.3788 l.24–29 and p.3789 l.1–7. There are a large number of possible aquifer shapes, and lacking either data or a conceptual model for small glaciers, we do not feel comfortable making more than the simplest of assumptions. The results from Experiment 2 provide a hint as to how variable aquifer thicknesses would affect the glacier-wide thermal structure, and we have added a qualitative description to the description of h_{aq} (Sec 2.1.2).

p. 3802, l. 23 to p 3803 l.4: from these sentences, I understand that the rate of heat and ice advection is changed without changing the glacier geometry. However, the topic of this test is not clear and I have the feeling that the conclusions are not significant.

– Changing environmental conditions (e.g. climate warming) may have multiple effects on the thermal regime of polythermal glaciers. These effects are sometimes opposed to one another, and it is not obvious what the ultimate result will be. The point of this experiment is to compare synthetic glaciers across a wide range of conditions and to show how a steady-state regime might change as these conditions co-vary. These results show how the direction of thermal change varies across the parameter space, and gives useful results about which environments are likely to contain glaciers that will grow warmer, and which will grow colder.

p. 3803, l. 10 to p. 3804, l.16: this section 3.2.2 is not clear and does not provide significant conclusions. The discussion is based on assumptions which are poorly constrained. For instance, the assumption according to which the near surface aquifer is equivalent to the annual net balance is not justified. I believe that this section is very speculative and does not provide significant conclusions. I believe that this section 3.2.2 and Figure 6 should be removed.

– Although it is useful to consider the variables in isolation as in (3.2.1) precisely because the assumptions needed to couple them are difficult, making actual predictions relevant to real glaciers (see comment above) might require a more realistic view. The assumption regarding the near-surface aquifer thickness–net balance equivalence (Eq 21) is only relevant to the dashed lines, which are meant as a visual guide. The actual parameter space shown below does not rely on this assumption (see comment in h_{aq} sensitivity above).

p. 3804, l. 27 to p. 3805, l. 9: “strain heating represents the primary source of englacial heat”. Does this conclusion come from the analysis performed in Experiment 2 ? I would expect that this conclusion comes from Experiment 1. Similarly, the following sentences do not belong to parameter sensitivity.

– This conclusion comes from a process of elimination based on results from the previous sensitivity tests. It is an interpretation of the thermal structures derived during the sensitivity tests. This subsection was intended to address goal (3) (now merged with goal (2)). We have merged what we feel are the important elements from this section into the sensitivity results.

p. 3805, l. 13-14: "with Eq. (20)" the authors should add "and Eq.(21)". Again given the impact of this assumption (Eq 21), the authors should justify it or should be very cautious with the results.

– We have added Eq. (21). We have attempted to justify Eq. (21) (see comments above), but have added a test with h_{aq} held fixed in order to evaluate the impact of our assumption.

p. 3805, l.18 to p. 3806 l.13: How do the results depend on db/dz ? Again, I believe that the sensitivity to db/dz should be discussed.

– See comments above

References: revise the reference Aschwanden and others (2012)

– We think that the problem was the missing doi, which has been included.

Table 1: i in order to be consistent with the text.

– Thanks for catching this

Table 3: line no strain heating : -0.55 K : it seems not consistent with the text (-1.8 K according to line 21, p.3798).

– This is a mix-up between average and maximum differences, which was perhaps unclear in the text. The maximum differences are not actually very useful and make the text harder to read, so they have been removed. The table has also been fixed (see referee #2) to match the figure shown.

Figures 3a, 3b and Figures 7a-7e: I am not convinced that enthalpy values are very useful and relevant in these figures. I believe that the authors should report, in the graph, the temperature values for the cold part and the water content for the temperate part with 2 different color scales.

– This is a good idea, and we have modified the relevant figures to incorporate it.

Figure 4: The authors should mention that the X axis extends from the middle of the glacier to the snout.

– Now mentioned.

Figure 4b: the results with REFC model are not shown. Any reason ?

– The REFC results were originally not shown in (b) because they are very similar between A and A_e. The results have been added so that the reader can see this.

Figure 8: the authors should explain the meaning of the thin lines (10%, 20%....)

– We attempt to explain this in the caption and section 2.4.3. In light of the following comment, we have not added to the caption.

Figure 8: the caption is probably too long and a part of the caption should be included in Section 3.3

– We have moved the justification for truncating the lines into the main text.

Figure 8: I do not understand why the authors write "For these reasons, the lines are terminated when glacier length falls below 3 km". Is it related to Figure 8?

– Yes, it explains why some of the lines are shorter than others in Fig. 8. We write this because the model does not do well with short glaciers. When glaciers are short, two things happen. (1) the glacier is restricted to the steepest portion of the bedrock surface, and the assumption that the SIA is based on is violated the most here. (2) Our horizontal grid is not set-up to scale with glacier length, so when the glacier becomes short, it is represented over fewer horizontal grid nodes. The error that results starts to be large relative to the glacier length. We hope that by shortening the caption (see comment above), this reasoning becomes clearer.

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References

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