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The Cryosphere Discussions

# *Interactive comment on* "Environmental controls on the thermal structure of alpine glaciers" *by* N. J. Wilson and G. E. Flowers

**Anonymous Referee #1**

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Review :

"Environmental controls on the thermal structure of alpine glaciers" from Wilson and Flowers, submitted to The Cryosphere

General comments

This paper deals with the analysis of thermal structure of alpine glaciers using numerical modeling and synthetic glacier geometries. The objectives of this study is to analyze the contributions of heat sources which influence the thermal structure, to analyze the sensitivity of thermal regimes to variables such as air temperature, aquifer thickness, degree-day factor, ELA, englacial water content, advection, and to analyze the responses of thermal structure to climate changes. The authors use a two-dimensional



mechanically-coupled thermal model. In order to avoid to include the snow/ice temperature and water content in the model, they use an enthalpy-based formulation proposed by Aschwaden and Blatter (2009). The main conclusions of this paper are the following: . the meltwater entrapment is the primary source of heat for small polythermal glaciers dominated by temperate ice. . for these glaciers, the thinning of firn and the retreat of accumulation zone are able to cut off the supply of latent heat and to enlarge the cold ice zone. . it means that, warming climate conditions can lead to increase or decrease the glacier temperature depending on the competing effects of elevated meltwater production, reduced accumulation zone and thinning firn. It means also that the surface mass balance sensitivity plays a main role for the evolution of thermal structure. . The rheological softening of ice due englacial water content has a limited effect on geometry and thermal structure for polythermal glaciers with a small volume of temperate ice, but could have a substantial effect for other polythermal glaciers.

These conclusions deserve a publication in The Cryosphere. However, some points are not clear and need to be improved before publication.

. The authors performed a parameter sensitivity from parameters shown in Table 2 but they did not discuss all the parameters (see specific comments below). . In Conclusions and Abstract, they pointed out the strong mass balance impact on the thermal structure but the sensitivity to balance gradient and bmax are not studied in this paper. . the authors show that the aquifer thickness and the snow water content play primary roles to thermal structure but it seems that these parameters are poorly constrained. The text is poorly referenced about his point. The authors do not provide details about these parameters (see specific comments) and about their uncertainties. In the conclusions, the authors highlight the need for a better understanding of the formulation of the flow-law coefficient but I believe that the uncertainties related to aquifer are more crucial. Given these uncertainties and the strong assumption relative to the parameterization of aquifer thickness, I believe that the authors should be more cautious about the conclusions. . the authors do not mention anything about the sensitivity to basal mo-

tion. In section 2.1.4 ,the authors mention that they neglect the sliding. However,from Figure 2, it is obvious that REFT model contains a large part of temperate ice. It is not discussed anywhere in the paper. How the sliding affects the thermal structure ? What is the influence of sliding increase on the thermal structure ?

This paper is generally well organized and well written. I believe that this analysis from synthetic glaciers is very useful to understand the different contributions which influence the thermal structure and how the thermal structure of a polythermal glacier will adjust in the future with climate change. This paper is suitable for The Cryosphere. I will recommend the paper to be published in The Cryosphere after the required revisions

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.

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

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.

p. 3785, Equations 4 and 5: in Equation 4, thermal conductivity is k; in Equation 5, the authors use keff and the difference should be explained. I assume that keff is used for k in Equation 4 but it is not very clear.

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

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

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.

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 ?

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.

p. 3793, l.26 to p.3794, l.5: the authors provide an annual balance function without any discussion about the uncertainties on bmax or balance gradient. bmax 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).

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

p. 3796, l. 16- 24: It is not clear how this factor Cu 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 Cu. 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 Cu. (see also p 3802, l.23)

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 au-

thors should provide justifications or recognize the weakness of this parameterization.

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

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

p. 3801, l. 14-28: the role of haq is very important. However, haq 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 ?

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%.

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 bmax 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.

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.

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.

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.

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.

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.

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.

References: revise the reference Aschwanden and others (2012)

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

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).

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.

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

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

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

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

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 ?

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