**Climate
of the Past**

Discussions

EGU

Interactive comment on "Ground surface temperature reconstruction for the last 500 years obtained from permafrost temperatures observed in the Stelvio Share borehole, Italian Alps" *by* Mauro Guglielmin et al.

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While the manuscript contains interesting data worth while publishing, I'm highly skeptical with regard to the particular approach taken by the authors. It leaves open several important questions, cannot be regarded as really novel, and is not well described. Therefore I think the article could only be published in CP after very thorough (major) revisions.

Major comments:

P2, L43-55: These paragraphs read as f the existence of permafrost is essential to the reconstruction of past surface temperatures from borehole temperature profiles, which is not correct. Furthermore the early work of Lachenbruch Marshall did not take into account the generic permafrost-related processes as freezing/thawing. This is related to the question of the existence of significant porosity (not even mentioned in the text). If water/ice-filled porosity is very small, the "dynamic" effects of permafrost are of course negligible. But then, the title may be a misnomer, and permafrost should be omitted there ("subsurface temperatures" instead of "permafrost temperatures").

P2, L53-53: I disagree with the sentence regarding the importance of topographic effects. Even on a fully symmetric mountain these effects will be present. Moreover, the differences in insolation will produce an asymmetric regime. A N-S slice thus would have been more relevant in Fig. 2, as this would be more characteristic with respect to the surface temperatures, and also would show more asymmetry. In order to be published, there should be a quantitiative assessment of the topographic effects with respect to the 1-D model used for inversion.

P3, L83: Please comment on these literature values.

P4, 3.2 Lab data: In table 2 there are three facies with quite different properties. How did you use this in your inversion? Note that a correct layered solution is given in Bodri & Cermak 1995.

P4ff, 3.3 Theory: While the authors try do give a description of their approach, there are many claims or assumptions which need clarification. As the theory section is already rather long, it might be useful to put the details into an appendix, and concentrate on the essentials in the main text.

- Any assumptions on the physical limitations of the model should be mentioned in the text, e.g. porosity, latent heat release, properties regarded constant.

- Why and how detrend? Is this detrending unique? (L132ff). This trend surely does not only realut from regional heat flow density, bur includes all earlier events, which often leads to an approximately linear signal at these shallow depths (amongst others, Safanda & Rajver 2001, Rath et al. 2012). I would also not call it a detrending - it is a different nontrivial inverse problem for background heat flux. This is also related to the choice of the length of the temperature history to be reconstructed and the relevance of the τ_∞ resulting from the inversion.

- Time lapse used in this inversion(L152-154)?

- This sentence is not comprehensible (L156-157).

- Why is Tikhonov better than TSVD? A comparison figure would help. (L 164ff).

- Which method was used to choose alpha for this study (L169ff)?

- Smoothing regularizations have been used many times in the past (L175) - see Shen et al. 1992, Bodri & Cermak 2007, also the references given by Referee #1.

- This sentence is incomprehensible (L178-179).

- **R** is not square. Which solver is used? Boundary conditions in **R**? Is the condition of L180 fulfilled with your construction of **L** and choice of **R**? For **R** to be a discrete approximation to a differential operator a factor of $(\Delta t)^{-2}$ is required (for constant Δt in L187.

General:

An assessment of uncertainty is missing, which is absolutely necessary particularly in the case of an ill posed problem. This is even more important when using a simple 1-D model which neglects so many effects which may bias the results. While not improving with respect to the mentioned physical assumptions, already Monte Carlo studies

and sensitivity considerations would help in this respect. Any result and interpretation based on a single inversion can not be considered as reliable and is prone to bias. The lack of a critical evaluation, which is essential when reconstructing ground surface temperature histories, makes it difficult to judge the value of the results obtained.

Minor items:

- Use dots, not commas for the decimal in the tables.

- Caption figure 3 (most important result of the study) should be informative.

- Facies d in Figure 4 not in table 2.

- Marking facies (Figure 4) and paleoclimatic evens (Figure 9) bot wit A, B, C, D may be confusing. why not use the a,b,c consistent with table 2?

- rielaborated? (caption Figure 9)

References

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C5