

Interactive comment on “Evaluation of the ground surface Enthalpy balance from bedrock shallow borehole temperatures (Livingston Island, Maritime Antarctic)” by M. Ramos and G. Vieira

T. Zhang (Editor)

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The authors intend to study changes in surface enthalpy using borehole data. First of all, I strongly recommend that the authors should seriously consider all comments by two reviewers in their revised version. I fully agree with two reviewers' comments and suggestions. Your reply in discussions is unsatisfactory.

As both reviewers raised the issue that the authors intend to deal with soil freezing and thawing but there is no soil water phase change. Strictly speaking, there is no soil freezing and thawing if there is no soil water phase change. Therefore, this issue may not be applicable to most of permafrost studies. I have to say that the authors

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are dealing with heat transfer problem in a semi-infinite region without phase change. This is a classic soil heat transfer problem in non-permafrost regions with a sinusoidal upper boundary condition to consider diurnal or seasonal temperature variations. The solution can be found in many books on boundary layer climate and/or boundary layer meteorology. The only difference in this study is the mean annual air temperature is lower, below the freezing point. The authors do not mention about this well-known classic problem in the current version. I strongly recommend that the authors apply this classic solution to their problem and compare with their current approach. Most of all, the authors should clearly state that why their new method is superior to the classic solution and what is new from their current approach.

The authors intend to estimate surface enthalpy changes over seasons. In other words, the authors intend to estimate how much heat into the soils during summer months and how much heat out of the soils during winter months. Russians have done a lot of this kind of work in 1960s and 1970s, named "heat turnover";. The authors seem they do not know about this work. I recommend that the authors refer the earlier works.

The authors mention that there is no "Zero Curtain Effect" as demonstrated in Figure 4. There is no soil water phase change, certainly, there is no zero curtain effect. I am also very confused by the "cooling" and "warming" showed in Figure 4. The authors describe that "cooling" starts right after soil temperature below the freezing and warming starts right after soil temperature above the freezing point. I wonder what the authors would define the period between "cooling" and "warming" periods and/or between "warming" and "cooling" periods.

The authors need to answer all these questions before the decision can be made for possible publications in The Cryosphere.

Interactive comment on The Cryosphere Discuss., 2, 153, 2008.

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