The Cryosphere Discuss., 2, S88–S90, 2008
www.the-cryosphere-discuss.net/2/S88/2008/

The Cryosphere
Discussions

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

Anonymous Referee #1

Received and published: 20 May 2008

Review of M. Ramos and G. Vieira, 'Evaluation of the ground surface Enthalpy balance from bedrock shallow temperatures (Livingston Island, Maritime Antarctic', The Cryosphere Discussions, 2, 153-184, 2008.

The authors propose a novel method for inferring 'valuable data for studies on permafrost and periglacial processes', namely, calculating enthalpy change in the shallow subsurface during seasonal warming and cooling. The authors outline their methodology in the Analysis section, and display their calculations in Results and Discussion. However, when I reached the end of the paper, I did not read anything that concluded the work. I feel that the work presented in this manuscript is not mature enough for publication. In the Conclusions, the authors also state that it is important that during freeze and thaw transitions the ground has an isothermal, 0 degree C profile. This restriction may render many permafrost areas unsuitable for this sort of enthalpy analysis.

I won't get into detail about specific changes but I have a few major concerns:

1) The flow of the paper seems fine overall, but the English needs to be improved. Throughout the manuscript are awkward sentence structures, typographical and grammatical errors, (e.g., 'looses' instead of 'loses'). Some key permafrost stability references like the works of Sushama et al. 2006 and 2007 and Lawrence et al, are missing from the review.

2) The thermal diffusivity is calculated from analysis of the subsurface temperature gradient, and the rest of the subsurface properties (density, thermal conductivity and specific heat) taken from tabulated data. It seems to me that most of these properties could be easily measured from a sample of the bedrock, as it is described as being largely homogeneous.

3) Assuming that the propagation of the zero-degree isotherm is linear is almost certainly incorrect. The solution for isotherm propagation due to a surface step-change in temperature was first solved by Stefan in 1891 and is a square root function Z(t) ˜ sqrt(t).

It seems to me that the authors are using a purely conductive approach to estimate the energy balance at the ground surface. They appear to have assumed that latent heat effects are not important due to the lack of moisture at the site of study. However, I remain puzzled by such attempt to infer any information from this model regarding anything related to permafrost. Permafrost modelling requires by definition the effects of phase changes. As the authors note in page 3, line 7-8, there is no zero curtain effect visible in the ground temperature dataset in question (Figure 4). If the authors were to plot the thermal orbits of these data (i.e. plot air temperature vs soil temperatures) they will see that the regularity of the figures of interception reveal little non-conductive effects due to moisture content, thus how can such conditions and a model of this situation shed light on permafrost? This apparent inconsistency appears again in line p 161 line 20 when referring to the active layer.

I am also puzzled by the commentary in page 3 lines 22-25 stating that the granite thermal properties (thermal depth I suppose) can serve as a proxy for estimating active layer depth to 5 m. Diffusivity of quartzite is estimated to be 1.23 x10 -06 m2/s, this is larger than the typical 1.0 x10 -06 m2/s used in many problems of heat conduction in borehole and climatic signal propagation, but is it enough to double the depth of seasonal thaw? Are the authors bundling into this statement other undeclared site-specific conditions? If so, they should explain further.

The authors should also comment on the long-term effects of surface temperature changes that occurred before the beginning of the period of observation. Such changes -in a scale of decades or even centuries, have recorded their transient signatures deeper than 2.4 m and their effects, perhaps small for the three or four years considered here, should be discussed. The objective, I presume, of calculating the energy balance at the ground surface (as proposed here) is that eventually it can be used as an indicator for larger scale energy imbalance in order to complement subsurface heat content and its evolution as determined from boreholes temperature profiles in other regions. I assume also that an important motivation of this work is the search for a robust metric of planetary energy imbalance as proposed by Hansen et al. a few years ago.

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