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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 #2

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Comments on Evaluation of the ground surface Enthalpy balance from bedrock shallow borehole temperatures (Livingston Island, Maritime Antarctic); M. Ramos and G. Vieira The Cryosphere Discussions

General Comments This paper focuses on the ground thermal regime of a monitoring site in the Maritime Antarctic. Ground temperature data over a 4 year period from a shallow borehole in bedrock are presented. A methodology is also presented to determine the enthalpy change and to model the progression of the 0°C isotherm. The paper provides information that would be of interest to permafrost scientists and also the modelling community. For the most part the paper is fairly well written and orga-

nized. There are however a few issues and several comments and suggestions are provided that hopefully will lead to improvement of the paper. A few general comments outlining the major issues are given here with more detailed specific comments to follow.

The paper points to the need for more field sites to better characterize permafrost conditions in the Antarctic and the borehole on Livingston Island could contribute to increasing our knowledge of permafrost conditions in this region. However, this borehole is only 2.5 m deep and does not appear to penetrate the active layer and therefore provide any information on the thermal state of permafrost. The quartzite bedrock has a high thermal conductivity and deep summer thaw penetration is expected. The ground temperature observations however are not collected at a depth sufficient to directly confirm the presence of permafrost. If permafrost is present it is likely quite marginal especially given that mean annual air temperatures are fairly warm (approx -2°C). Evidence for permafrost from borehole excavations or geophysical investigations comes from higher elevations than the monitoring site (pg 158) or different geological conditions.

A second concern with the paper is that the modelling is done for a very simple situation. This is perhaps valid given the characteristics of the field site (a bedrock site). The authors however highlight the importance of the active layer in influencing polar ecology and the moisture and gas exchanges between the atmosphere and the ground. The effects of thickening of the active layer on physical, geomorphic, hydrologic and biological processes are also mentioned. Changes in active layer thickness for the field site considered here likely have little influence on these processes, especially if the analysis conducted considers that the bedrock has negligible water content and there are no phase change effects. The fact that the ground temperature passes through 0°C does not mean much in terms of these other processes if there is no moisture to freeze or thaw. This leads one to question the applicability of the results in terms of the impacts of changes in active layer thickness (in response to climate warming) on the

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various biophysical processes discussed in the introduction.

Based on these concerns and a number of other issues mentioned in the specific comments (see below), I have concluded that more than minor revisions are required to produce a paper acceptable for publication. Moderate revisions (note major recommendations are indicated on the form) are recommended.

Specific Comments pg 154, line 23; Are the authors referring to regional/spatial homogeneity?

pg 155, line 19 (and perhaps elsewhere); refer to active layer thickness or thaw depth (not active layer depth).

pg 155, line 22-25; In the discussion of factors controlling ground heat flow, the thermal properties (including conductivity) are important. Note that although moisture content is mentioned here it is not considered in the analysis that is presented.

Pg 156, line 6-9; It might be worth mentioning that part of the reason for the considerable number of field sites in the Arctic is that this is an area of economic and resource development and information on permafrost conditions is required for engineering design and landuse planning decisions.

Pg 156, line 16; Should Guglielmin et al, 2001 be 2003 (referring to paper in 7th Int. Permafrost Conf. proceedings?)

Pg 157-158, section 2.2 (Climate); Can any information be provided on snow cover for the field site or the region?

Pg 158, line 24; Can the authors offer any explanation for the flattening of the curve as 0°C is approached during spring 2003. Does this indicate that there may be appreciable moisture in the shallow ground?

Pg 159, line 9; Is the borehole filled with fluid to reduce convective effects? (is convection in the borehole likely to present problems).

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Pg 159, line 18-20; Is the presence of frozen ground based on direct observations of ice in the surficial materials?

Pg 159, line 20-23; If the moisture content is negligible (and therefore no change from ice to water through the profile) what is the utility of these techniques in delineating frozen ground?

Pg 160, line 5; If moisture content is negligible (and therefore no phase change), can we really refer to frost and thaw seasons?

Pg 160, line 8-13; Reference is made to the soil surface but is the analysis really considering soil or bedrock? Earlier references to the field site only refer to quartzite bedrock and do not provide information on overburden. Some clarification is required here.

Pg 161, line 17-27; Some clarification is required in this section. This section refers to frost and thaw seasons but there is not really anything special about 0°C when there is no moisture and no phase change? Cooling of the ground occurs as the temperature decreases (losing energy), not just when the temperature is below 0°C as stated here. Similarly, warming occurs as the temperature increases (gaining energy) not just when the temperature is above 0°C. In other words warming can occur when the temperature is below 0°C. (Note also that loses is spelled incorrectly in line 26).

Pg 165, line 20 to pg 166, line 3; Are the freezing and thawing indices presented in Table 1 based on modeling results or on the temperature measurements from the field site – some clarification is require (how do observed and modeled results compare?).

Page 166, line 17-26; Reference is made to the ratio between air and ground freezing indices and the N-factor is provided in table 1 defined as air index:ground index. This definition is the reverse of the accepted definition for N-factors, ground index:air index (see for example Lunardini 1978, Proc. of 3rd Int. Permafrost Conf). The authors

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should present N-factors that follow the accepted definition with air temperature (the forcing variable) as the denominator. This would avoid confusion in the interpretation of the values presented in Table 1. Most readers would expect to see a smaller value (less than 1) for the N-factor if snow cover is of sufficient depth to insulate the ground from the changes in air temperature. To support the discussion on the relationship between snow conditions and the length of the freezing season, the authors may wish to consider plotting air and near surface ground temperature (15 cm) on the same graph to show the lag in the increase in temperature in the spring.

Pg 167, line 5-7; At sites where there is low buffering capacity, such as bare surfaces with no vegetation or organic layer, a ground thawing index that is greater than the air thawing index is expected leading to N-factors that are greater than 1. The values for the thawing indices provided in Table 1 would indicate that there is no significant buffer layer (however as mentioned earlier the N-factors have been calculated incorrectly).

Pg 168, line 1-3; Is this lack of inter annual variability during the summer largely related to the lack of variability in moisture conditions?

Pg 169, line 23; Note that an important turbulent heat flux, evaporative heat flux, does not appear to have been considered as only a simplified situation where there is no ground moisture has been considered. Only sensible heat flux has been considered.

Pg 169, line 3; It is still unclear why these distinct periods of freezing and thawing are important if there is no phase change.

References I did not notice a citation for Ramos and Viera (2004) in the text.

Figures If colour is permissible (with no additional charge), the authors should consider utilizing colour to provide a clear presentation of information. Colour is suggested in particular for Fig. 1, 3 and 4. It is very difficult to distinguish between the lines in Figs. 3 and 4.

It is not clear why both Fig. 3 and 4 are required. Fig. 4 presents essentially the same

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information as Fig. 3 but includes additional labels to indicate freezing and thawing periods. Only Figure 4 is required. As mentioned in the specific comments, the authors could consider a figure that shows air temperature and 15 cm temperature on the same graph.

Figure 6 and 7; The authors should consider plotting temperature on the x-axis and depth on the y-axis in order to change the orientation of these graphs (note this is commonly done to present ground temperature profiles or ground temperature envelopes).

Figure 7; It is not clear why there appear to be 2 measurement points below a depth of 2.5 m. The borehole depth is only 2.5 m. Some clarification is required.

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