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Interactive comment on "A two-sided approach to estimate heat transfer processes within the active layer of rock glacier Murtèl-Corvatsch" *by* M. Scherler et al.

Anonymous Referee #1

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The manuscript "A two-sided approach to estimate heat transfer processes within the active layer of rock glacier Murtel-Corvatsch" by Scherler et al. presents a long-term record of energy balance measurements at a rock glacier in the European Alps and simulations of ground temperatures using the established COUP model.

After reading through the manuscript, I am left with two main results: 1. this is the measured energy balance record of a rock glacier in the Swiss Alps (based on an impressive long-term effort of field measurements), and 2. introducing an empirical term of energy generation and consumption in a conductive heat transfer model dramatically improves the fit of modeled and measured ground temperatures (which I do not find too surprising - models generally tend to agree better with observations if

more parameters are introduced). However, the authors fail to motivate what can be learned from the combination of the two approaches in terms of new science. Their main argument is that the measured and modeled fluxes largely disagree and that the uncertainties associated with either approach are too large to determine the reason for the disagreement.

This leaves the impressive 11y-time series of energy balance data as the main aspect of the study that deserves to be published and the authors should extend this in a revised version. Furthermore, they need to remove a number of serious flaws (major and minor comments) in the energy balance calculations and a considerable amount of technical and methodological shortcomings (minor Comments) in particular in the Methods section. It seems that this part of the manuscript has been prepared with little care. In addition, I recommend to conduct a more quantitative uncertainty analysis of the indirectly derived energy balance terms, such as the ground heat flux where parameters associated with considerable uncertainty are taken into account.

Major comments:

1. The energy balance equations 1 and 2 violate the continuity equation for energy, and summing up the fluxes to compute a "deviation" from zero, as in Tables 2-4, is not meaningful. The continuity equation states that the change of the internal energy (i.e. sensible heat plus latent heat in this case) of a body over a certain time interval is equal to the sum of the energy fluxes across its boundaries (multiplied by the time interval). So, if one assumes the body to be e.g. the dark gray layer in Fig. 1a (no snow and no lateral fluxes for simplicity) and adopts the sign convention introduced by the authors, the correct energy balance equation would be

$$Q_{storage} = Q_r + Q_h + Q_{le} + Q_{g,pf} \,, \tag{1}$$

   i.e. the change of the storage is equal to the fluxes at the upper boundary plus the fluxes at the lower boundary. Everything that happens in the gray layer itself is taken care of by the storage term, so there is no need to consider a ground heat flux and radiative heat flux between the blocks. Or if the body was e.g. the layer between 0 and 0.55m, then the corresponding equation would be

$$Q_{storage,0-0.55m} = Q_r + Q_h + Q_{le} + Q_{g,0.55m} + Q_{r,0.55m} \,, \tag{2}$$

   i.e. the body would now loose/gain energy at its lower boundary through both heat conduction and radiation. For this reason, a large part of the analysis presented by the authors is flawed and must be redone.

2. The calculation of the turbulent fluxes is based on the gradient method, which usually requires measurements at two different heights above ground. The authors have only measurements at one level, and appear to use the respective

quantities at the "surface" as second level. Firstly, this requires to define a roughness length z_0, at which the wind speed is assumed to be zero. The value of this roughness length is nowhere stated, and the authors should do so and provide a reasoning for this choice. Secondly, in the calculation of the latent heat flux, the authors state that they used the "specific humidity at the ground surface". While it must be absolute humidity (see below), they fail to state how this was derived. Is there a sensor at the surface? Or did they use the saturation vapor pressure at the surface temperature determined from long-wave radiation measurements? In that case, this would correspond to a water surface, not to the rather dry surface of a rock glacier. In summer, the resulting Bowen ratio is less than unity (Table 2) which I find very surprising for such a setting. This could be explained by strongly biased humidity values at the surface. In the COUP simulations, the summer Bowen ratio (Fig. 2b) looks much more like expected.

Minor comments:

p. 142, l. 6: discontinuous

p. 142, l. 7: mention that it is the COUP model

p. 142, l. 24: in the European Alps

p. 145, l. 11: then

p. 145, l. 21: comparison

p. 147, l. 24: What's the disctinction between active layer and permafrost here?

p. 147, l. 6: Q_r used instead of Q_{rad} in Eq. 1

p. 148, l. 8: “see Eq. 3” is superfluous

p. 148, l. 9: According to the sign convention of fluxes it must be plus-signs here.

p. 148, l. 13: Therefore

p. 148, l. 14: Why would one account for shading by a “geometrical” factor, which the authors understand as simply making the slope steeper than it is. If this is an established method, they should provide a reference. And why is this additional slope angle taken as 5, and not 10, or 15? What is the effect of the rather arbitrary factor on the short-wave radiation?
Strictly speaking, the slope correction should only be applied to the direct part of the short-wave radiation, not the diffuse part. The authors should at least comment on this if measurements are not available. Has the correction also be applied to incoming long-wave radiation, which is generally assumed to be undirected?

p. 148, l. 20: units should be provided for all the employed physical variables

p. 148, l. 20: specific heat capacity

p. 148, l. 22: not sure what the authors understand as “surface roughness”, that is not a defined physical quantity in my understanding. In the second edition of Oke: Boundary Layer Climates, the variable z in the respective formula is denoted the “log mean height”, $z = (z_2 - z_1)/(ln(z_2/z_1))$. Did the authors use that one? If yes, it should

be clearly stated. In this case, what is z_1 and z_2?

p. 148, l. 20: specific heat capacity

p. 149, l. 5: it is absolute humidity (unit kg/m^3), not specific humidity (unitless). Please check and provide units for all variables!

p. 149, l. 6: How is the absolute humidity at the ground surface determined?

p. 149, l. 13: It is the Bulk Richardson number.

p. 149, l. 17: It must be absolute temperature in this case.

p. 150, l. 4: Why 300 kg/m^3? Is that based on field measurements? This may be a good value for the time-averaged snow density, but at the end of the snow season, when almost all melt occurs, I would expect a significantly higher density, maybe 400 kg/m^3? That would increase the melt fluxes by 25% !

p. 150, l. 6: The equation is wrong, one must divide by the time interval to obtain an energy flux.

p. 150, l. 7: There is no such thing as “latent heat of thawing”. It is “specific latent heat of fusion of water”.

p. 150, l. 14: Why 1/3? This seems a completely arbitrary choice, which has considerable implications for the computed ground heat fluxes.

p. 150, l. 22: Why is the 3.55m temperature fixed at zero degrees in summer? If the thaw depth is, say, 3.1m, the flux will be overestimated, if it is 3.9m, it will be underestimated with this method.

p. 151: I am of the opinion, that the used method for calculating net radiation between blocks is at least partly not applicable. Firstly, the correct equation for the net radiation flux between two inifinite parallel plates at temperatures T_1 and T_2 (at arbitrary distance from each other for vacuum) is

$$q_{net} = \epsilon_{eff}\, \sigma\, (T_1^4 - T_2^4)\,, \tag{3}$$

with ϵ_{eff} as given by the authors. However, this is for infinite parallel plates, and this is certainly not the situation in the rock glacier. There exist analytical solutions for a number of geometrical cases which all have strongly different expressions for ϵ_{eff}, but I don't think any of these come close to the real situation, a complex 3D-interplay of conductive and radiative heat transfer. The authors may try to argue that the situation of infinite parallel plates constitutes a confining case, i.e. an upper or lower bound, for the true radiative flux, but I'm not sure if and how this is possible. In any case, the radiative flux is independent of the distance between the two plates (absorption and emission in the air is negligible for such distances and temperature gradients), so I don't see a physical basis for reducing the flux by a factor of three. Again, it all depends on the actual geometry and the interplay between radiative and conductive heat transfer.

p. 152, l. 1: The method assumes steady-state conditions between the snow surface and a depth of 0.55m. Furthermore it assumes that the the thermal conductivities of

the active layer to 0.55m depth and of the snow are equal. However, for the active layer, the authors assume a conductivity of 2.5/3 W/mK, while the snow thermal conductivity is 0.56 W/mK for the snow density given in 2.3.3, so at least for snow depths considerably smaller than 0.55m, the method is biased. For the steady-state case, an effective conductivity can be determined analogous to resistors in an electrical circuit, and the authors should use this to be consistent. And they should state that steady-state conditions are a gross simplification.

p. 152, l. 9: ρ_s is used for snow density in Eq. 9. And state the used snow density.

p. 152, l. 16: To obtain a flux, one must divide by the time interval. Also, this only gives the correct change of the internal energy, if there are no melt or freeze processes of water within the layer under consideration. I don't think that this is the case in the rock glacier?

p. 153: The authors should state clearly how large the layer with heat sink/source is, and why this was chosen.

p. 155, l. 4: What is "overall heat fluxes"?

p. 157, l. 1: At least a thicker snow cover on the sensor would easily be detectable in the SW radiation sensor. Has this been checked?

p. 158, l. 5: no, see above!

p. 158, l. 12: Isothermal conditions in the active layer, i.e. also between 0.5m and

0m, would INCREASE the error, since a depth of 0.55m is explicitely assumed when calculating the temperature gradient, Eq. 14. If conditions are indeed isothermal, then the 0.55 should be removed from Eq. 14.

p. 158, l. 18ff: I don't understand any of this, and I have no will to check S. Schneider (personal communication, 2013). Please stick to proper scientific conduct!

Tables 2-4: Some of the symbols are different from the text, some are different from table to table, and some are different from the tables to Fig. 1. Please use more care, and explain the smbols in the caption! Again, summing up the contributions to obtain "dev" is wrong (see Major comments).

Fig. 2: What is the meaning of the columns with reduced color saturation in the left diagrams? Please state in the caption. And again, all symbols should be consistent.

Figs. 3/4: Again, some symbols are different from Fig. 2 and the tables.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 141, 2013.