

## Reply to comments of Reviewer #1

We thank all three reviewers for their thorough and constructive reviews. We would like to respond in a first paragraph to some fundamental points that were addressed in most of the reviews:

1. We agree with reviewer #1 that an additional parameter is likely to increase the modeled thermal regime substantially. However, the parameter is kept constant over the respective 4 month periods during all the calculations and it is integrated into a fully coupled heat and mass transfer model with freezing and thawing. Our experience shows in general that the improvement of a model performance by simply adding more parameters does not automatically lead to better model results.
2. As reviewers #2 and #3 had no major objections concerning the usefulness of the modeling approach, the authors will keep the structure of the manuscript including the modeling part. Regarding the discussion of the benefit of the two approaches, the similarity of the order of magnitude of the calculated energy balance components within the active layer and the model parametrization (i.e. the heat source/sink) will be addressed more clearly in the revised version of the manuscript.
3. We agree with reviewer #1 and #3 that an assessment of the errors associated with all the parameters would be helpful to the reader, but to do this with justified ranges for both approaches would be beyond the scope of the here presented manuscript (see e.g. Gubler et al., 2013, who addressed in a pure modeling study only this topic). Nevertheless, we pointed out in several paragraphs of the manuscript that the uncertainties of our approach are (probably) very large and that our results should be interpreted qualitatively. We will clarify this in the revised version at the respective places in the manuscript.
4. Grammar and typographical corrections as well as changed expressions as suggested by the referees will be used in the revised version of the manuscript.
5. Units will be given for all variables used in the revised version of the manuscript. Also, symbols will be used in a consistent way throughout the entire manuscript.

## Anonymous Referee #1

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.

We appreciate the critical review of our manuscript. We apologize for some errors in the calculation of the energy balance components and in the formulation of the energy balance equation. We corrected all these errors in the revised version of the manuscript.

However, we disagree with the reviewers view regarding the usefulness of the modeling approach which we presented. We used the COUP model in the physical based coupled heat and mass transfer mode with freezing and thawing, and not only as a 'conductive heat transfer model' as stated by the reviewer.

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

$$Q_{\text{storage}} = Q_r + Q_h + Q_{le} + Q_{g,pf}, \quad (1)$$

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

$$Q_{\text{storage},0-0.55m} = Q_r + Q_h + Q_{le} + Q_{g,0.55m} + Q_{r,0.55m}, \quad (2)$$

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.

The storage term in the equation  $Q_{\text{storage}} = Q_r + Q_h + Q_{le} + Q_{g,pf}$  corresponds to the term  $Q_{g,al}$  used by the authors. We agree that this term should appear with a minus sign in the energy balance equation. We will correct this in the revised version of the manuscript. However, we are still convinced that we used the correct signs with respect to the energy fluxes, as shown in Figure 1 and Tables 2 – 4. Thus, we also see no restriction in calculating a deviation term by summing up the energy balance components. We agree that this term is difficult to interpret and it will likely contain various components such as measurement and parametrization errors as well as unmeasured processes. This will be discussed in more detail in the discussion section of the revised manuscript.

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.

As the reviewer correctly points out, we used the bulk method with only measurements at one level and the quantities at the surface as second level. The roughness length used is 0.07 m for snow covered conditions and 0.18 m for snow free conditions, found by Stocker-Mittaz (2002) for the study site.

We used absolute humidity in the calculation of the latent heat flux, the parameter description in the initially submitted manuscript was misleading.

We agree with the reviewer that assuming a saturated surface will lead to errors. As measurement conditions at the heterogeneous surface of the rock glacier with moist conditions in the depressions and dry conditions at the top are difficult we consider that assuming saturated conditions is a reasonable

approximation. Eddy covariance measurements, which are not yet available at the study site, would certainly improve the calculations.  
This will be clarified in the respective paragraphs in the methods and the discussion section of the revised manuscript.

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 distinction between active layer and permafrost here?

Active layer and permafrost cannot be clearly distinguished here, so the resulting net melt over the entire year is composed of both active layer melt and melt at the permafrost table. See also discussion on p. 159, l.15-23.

- 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.

We thank the reviewer for this comment. Signs in the respective equation will be changed to plus signs.

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?

10° of the 15° degrees are explained by the slope angle. The additional 5° were a rough assumption on the reduction of incoming radiation by the surface geometry, i.e. blocks of up to several meters in diameter. In a revised version of the manuscript, we will calculate the incoming shortwave radiation based on a slope angle of 10° and correct this value by a geometrical factor of 0.9. This factor is taken from a U.S. patent 7,305,983 B1, which is giving insolation information on inclined roofs. This information is gained by calculating the insolation depending on roof orientation and inclination of buildings in a GIS. The reduction found by these authors range from ~95% to ~50%. We use a value of 0.9 which represents a roof inclination of ~35° to ~45° depending on orientation of the roof. We agree that this is a rather rough approximation for the reduction factor and that it would be necessary to model the real surface geometry in GIS. We will use this approach in further work on the subject.

In the initially submitted manuscript the reduction factor has been applied to net radiation, which is a clear mistake. We thank the referee for finding this error. In the revised version the authors will correct this and apply the reduction of slope angle and surface geometry only to the incoming shortwave radiation.

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

Units will be given for all variables used in a revised version of the manuscript.

- 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  $z_2$ ?

We used "log mean height" with  $z_1 = 0$  m and  $z_2 = 2$  m.

p. 148, l. 20: specific heat capacity

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

Units will be given for all variables used in a revised version of the manuscript.

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

The absolute humidity is calculated assuming saturated conditions and saturation vapor pressure at the surface using air temperature.

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

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

The authors will give the unit of temperature [K] in a revised version of the manuscript.

p. 150, l. 4: Why  $300\text{kg}/\text{m}^3$ ? 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  $400\text{kg}/\text{m}^3$ ? That would increase the melt fluxes by 25% !

Snow density estimation above permafrost is complicated, because of low ground temperatures which lead to a different snow densification pattern in spring than it would be expected for non-permafrost soils. In a work of Keller (1994) it was shown that even less dense snow may be found above the ground. Thus the authors argue that the value chosen is a good approximation for the average density over the entire snow covered period. The uncertainty will be addressed in the revised version of the manuscript.

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

We thank the reviewer for pointing out this typesetting error. The calculations results as shown in Tables 2-4 and Figures have been obtained by dividing by the respective time intervals.

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

"Specific latent heat of fusion of water" will be used instead.

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

We thank the reviewer pointing out this error as this was clearly a mistake in the calculation of the ground heat flux. This was corrected in the revised version of the manuscript by replacing the factor from 1/3 by 0.6 to account for a porosity of 40%.

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.

This is an assumption based on the concept that the lower boundary of this layer represents the permafrost table, where thawing processes are supposed to keep the temperature at  $0^\circ\text{C}$  during the summer period, whereas the 3.55 m temperature is supposedly measured by a termistor in the borehole which is situated within the ice core.

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 infinite parallel plates at temperatures  $T_1$  and  $T_2$  (at arbitrary distance from each other for vacuum) is

$$Q_{\text{net}} = \epsilon_{\text{eff}} \sigma (T_1^4 - T_2^4), \quad (3)$$

with  $\epsilon_{\text{eff}}$  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  $\epsilon_{\text{eff}}$ , 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.

The equation (11) given by the authors is the general form of the Stefan-Boltzmann law. We agree that this is misleading in the context and we will use the equation as suggested by the reviewer, as this was actually used in the calculations.

We agree with the reviewer that the explanation given by the authors is misleading. The reduction of radiative heat flux between the blocks by a factor of three was chosen because of the temperature gradient within separate blocks, i.e. the block has a different temperature at its surface than what is measured by the thermistor within the block. Given a linear temperature gradient, parallel plates and a porosity of 40%, a reduction by a factor of 1/3 results. We agree that this is a very rough assumption, as the surface temperatures of the individual blocks was not measured and the geometry of the involved surfaces is not directly comparable to infinite parallel plates. Further work has to be done in the future to account for the complex 3D case as found in nature. We will address this point thoroughly in the discussion of the revised version of the manuscript.

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 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.

For the calculation of the snow heat flux only snow thermal conductivity is used. We agree with the reviewer that by using 0.55 m and assuming a flat surface this approach would lead to errors. But considering the microtopography at the rock glacier, we consider adding 0.55 m in the calculation of the temperature gradient is reasonable.

p. 152, l. 9:  $\rho_s$  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?

See also response to p. 150, l. 6. Changes due to melt and freeze processes during the respective periods are considered by the energy balance component  $Q_{m,ai}$  in equation 2.

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

The source/sink layer is 1 m thick and is placed 0.2 m below the surface. This position is chosen to be beneath the surface and the thickness is chosen large enough to approximate the natural situation (40% porosity in the active layer) and thin enough not to cause numerical problems. This will be stated in the revised version of the manuscript.

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

This sentence is misleading and will be deleted in a revised version of the manuscript.

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?

Summer radiation has been checked and corrected regarding this aspect. Winter radiation may be erroneous due to the respective effects.

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

See comment to p. 151 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 explicitly assumed when calculating the temperature gradient, Eq. 14. If conditions are indeed isothermal, then the 0.55 should be removed from Eq. 14.

See comment to p. 152, l. 1

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!

The respective reference and paragraph will be deleted in a revised version of the manuscript. The unknown turbulent heat flux will thus add to the deviation uncertainty. This will be addressed in a revised version of the discussion.

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 symbols in the caption! Again, summing up the contributions to obtain "dev" is wrong (see Major comments).

See point 5 of the general comments above and response to major comment 1.

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.

Reduced color saturation has been used for years with incomplete data. This will be stated in the caption of the respective Figure in a revised version of the manuscript.

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

#### References:

Gubler, S., Endrizzi, S., Gruber, S., and Purves, R. S.: Sensitivities and uncertainties of modeled ground temperatures in mountain environments, *Geosci. Model Dev. Discuss.*, 6, 791-840, doi:10.5194/gmdd-6-791-2013, 2013.

Keller, Felix Urs: Interaktionen zwischen Schnee und Permafrost. Diss. Diss. Naturwiss. ETH Zürich, Nr. 10356, 1993. Ref.: D. Vischer; Korref.: W. Haeberli; Korref.: H. Gubler, 1994.

Stocker-Mittaz, C.: Permafrost distribution modeling based on energy balance data, Ph.D. thesis, Univ. of Zurich, Zurich, Switzerland, 2002.