Reply to comments of Reviewer #2

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 bebeyond 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 #2

General Comments

There are two main components to this paper. First, a detailed calculation of energy balance fluxes at a rock glacier in the Swiss Alps, based on an impressive multiyear series of micrometeorological measurements. Second, the simulation of energy fluxes and active layer temperatures at different depths using the COUP model. The main finding is that introducing a heat source/sink layer in the model to account for air flow driven heat transfer dramatically improves the fit to measured borehole temperatures. The authors also estimate freezing and thawing rates in the active layer and at the permafrost table from the residuals in the energy balance. These latter results must be treated with caution due to the simplifying assumptions applied in the methodology.
While this is an interesting study, greater clarity is needed in the explanations of the energy flux calculations. Other than the model development, it isn’t clear what new insights into active layer processes are gained since much of the analysis consists of a general discussion of uncertainties in the two approaches which are not clearly quantified. I think the authors underplay their work and findings in this respect, and could improve this aspect of the paper in a revision.

Detailed comments

Equations 4-8 give the gradient form of the aerodynamic equations. It appears the bulk aerodynamic approach is applied, however, in which case surface values of temperature and humidity are needed, but it isn’t clearly explained how these variables are measured. Both surface temperature and humidity are difficult to measure or model accurately and a quantification of the error range and its effect for both variables is really needed. There appears to be some confusion over symbols. The letter z is used for both height and ‘surface roughness’ (p 148, l22). I assume the latter is really the ‘aerodynamic roughness’, which should be defined as the standard symbol z_0, as this is required in bulk aerodynamic approach. What value of roughness was used and how was it calculated and estimated?

As the reviewer correctly points out, we used the bulk aerodynamic approach in our study. The value used for surface roughness was 0.07 m for snow covered conditions and 0.18 m for snow free conditions (Stocker-Mittaz, 2002). The letter z is used for 'log mean height', this will be detailed in the revised version of the manuscript.
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.

154, 6 The parameterization of the heat source/sink layer isn't clear. Explain what the form of the parameterization is and how it was implemented in the model. What are the 'values' that were 'adjusted experimentally'? What was the range of variation of these values and do they have any physical meaning or are they purely empirical?

Parameterization of the source/sink layer consists of a constant value [W/m^2] which is either added to, or extracted from the system. The parameter changes in 4 month periods. It is positive in summer, negative in fall/winter and zero in spring. To find the right range, the parameters were chosen based on previous studies by Stocker-Mittaz (2002) and adjusted iteratively until the modeled temperatures matched the observed temperatures.
The physical meaning is complex and might be (partly) explained by processes occurring in nature that are not represented in the model, e.g. radiative heat transfer within the active layer and turbulent fluxes in the macropores. This will be clarified in the revised version of the manuscript.

150, 1-3, this sentence doesn't make sense to me, please clarify

We agree that this sentence is unclear and we will clarify the explanation in the revised version of the manuscript:
The threshold temperature for snow melt is set to -3°C, below this temperature no snow melt is calculated even if a decrease in snow height is measured. In addition, a measurement error is expected if the snow height decreases by more than 0.2 m in a 24 h interval. In this case, no snow melt is calculated.

150, 14, the correction factor needs justifying

We thank the reviewer for this comment as this was clearly a mistake in the calculation of the ground heat flux. This will be 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%.

151, 21-22, explain why gradients might be too high

This explanation was 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. This will be clarified in a revised version of the manuscript.

152, 9, snow density was already defined in equation 9 as rho_s

Rho_s will be used in this equation as defined in equation 9.

156, 22-23, justify the use of a 15 degree slope

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 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 the inventors 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 would choose this approach in a future work on this subject.

158, 19-20 The explanation is completely opaque. Explain what was done here. How can one of the paper authors give a pers. comm. !?

The authors agree that the original explanation was misleading. The respective reference will be deleted in a revised version of the manuscript. The reference pointed to an unpublished manuscript by co-author S. Schneider.

Grammar, typos, etc
143, 1 '. . . expected from . . .'
144, 7 'anthropogenic'
145, 11 '. . . since then . . .'
145, 17 '. . . and a frozen . . .'
146, 13 and 20, data are plural
155, 11 close bracket after 'Table 3'
Figures 2-4 would be clearer if the vertical axis ranges were restricted to make the columns appear larger

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.

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