Reply to comments of Reviewer #3

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 #3

General comments:

A system for computing the volumetric energy balance of the rock glacier from measurements, including radiative and turbulent energy transfer within blocky debris, and change in heat storage is presented here. This is an interesting step forwards in developing modeling approaches suitable for permafrost bodies with coarse surface material. The energy balance results are compared to those from a permafrost model (COUP) that does not explicitly account for these processes, but introduces and heat sink/source term to encapsulate these excluded processes, and in contrast to the measured energy balance does account for freeze/thaw in the medium. Thus the results of the two approaches are not directly comparable, but both offer useful tools to develop our understanding of the system.
A comprehensive, and well-written introduction to the topic is given, and the whole paper is well written and presented, and I recommend it for publication once the following points have been addressed:

(1) The aim is stated to compare two approaches that are not really directly comparable. I think this might be better stated by the following chain of arguments: (a) existing energy balance formulations do not account for the complex surface of block materials, (b) that is addressed here by developing a volumetric energy balance, (c) existing models do not account for all the energy exchange processes (d) a method to account for these by adding a sink/source component is examined here, (e) the results of both methods and relative strengths/weaknesses of the approaches with respect to different applications are discussed. The reason I suggest this is that your paper is focused on improving both measured energy balance and modeling approaches at the same time, and on the first reading I was a bit unclear about this duality.

This clear and very helpful suggestion to improve the introduction will be integrated in the revised version of the manuscript.

(2) I would like to see some assessment of the errors associated with the parameters
and correction factors included in the volumetric energy balance included (reduction
factors, geometrical corrections etc.)

See general comments, point 3.

(3) I can see it is difficult to make direct comparison between the results and relative deviations, due to the different structures of the model to the measurements. So I understand you have presented the energy balance with seasonality of the model sink/source layer, but is there a different emergent seasonality in the energy balance measurements, or does that conform well to the seasonality defined by the COUP results? I'd also like you to try and add a bit more explicit detail on what processes you think are causing deviations in the different seasons. This might have to be partly evidence based speculation, but would be a useful addition for non-expert readers in terms of determining the relative strengths and weaknesses of the two approaches in different seasons or environments.

The seasonality is based on the COUP model sink/source layer activity. We agree with the reviewer that a different and more detailed seasonality might be more appropriate for the measured energy balance (see e.g. Westermann et al., 2009, Langer et al., 2011).
More explicit detail on the processes causing the deviations, as suggested by the reviewer, will be given in a revised version of the manuscript.

Specific comments:

P142/L6: (sp) discontinuous

P145/L21: (sp) comparison

P147/L9: what is the timestep of this calculation? 30 minute? Daily?

The time step of input data is 60 minutes and the calculation is based on 1440 iterations per day. We will add a corresponding sentence to a revised version of the manuscript.

P149/L13: (sp) Therefore

P150/L4: is there any field data upon which the assumed snow density is based?

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 shownn 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.

P150/L17: is it possible to add a comment on how the exclusion of these processes could be expected to affect the results? E.g. relative over/under estimation in freeze or thaw times. Then you could return to that more explicitly in the discussion section of seasonal energy balance differences with the model data?

Based on the high porosity due to the large voids between the blocks and the resulting low retention capacity, we assume that changes due to water and ice content are likely to be negligible. As the reviewer suggests, this will be addressed in the discussion section of a revised version of the manuscript.

P150/L22: is the 3.55m temperature actually 0C during this period in the measurements? Why use a fixed value instead of measured temperatures?

This is an assumption based on the concept that the lower boundary of this layer represents the permafrost table where the thawing process is supposed to keep the temperature at 0°C during the summer period.

P152/L18: density assumed to be 40% in this case, but in section 2.3.4 ground heat flux was reduced by a factor of 1/3 to account for air filled voids – does that not imply that the ground heat flux reduction is assuming a porosity of 30%?

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%.

P153/L17: What is the timestep of the model versus the measurements?

Both, measurements and model output have 1 hour intervals. The model was run with 1440 iterations per day.

P154/L1: detail here that the layer is 1m thick, and its location with respect to the surface. Also perhaps add some information on why this layer placement was chosen.

Information about layer placement, thickness and location will be given in the respective paragraph of the revised version of the manuscript.

P154/L10: Which 2 depths was it optimized to? Can you explain the optimization procedure more explicitly – minimized RMSD on a daily basis, or it is something else?

The depths of optimization were 5.5 m and 11.5 m as shown in Figure 5. The procedure was an iterative adjustment of the parameters in the model and optimization aimed at minimal RMSD on a daily basis.

P154/L19&20: 'Figure X shows...'

P155/L22: This is really interesting that you need the additional sink/source component to create permafrost conditions.

P158/L22: Was the additional 5° geometrical correction optimized through any procedure? What does it look like with no additional factor (i.e. 10° slope) or a larger additional factor? You mention that large errors could be associated with this unknown term so it might be nice to quantify the impact of these errors on the net radiation and total energy balance.

The additional 5° were based on a rough assumption. In a revised version of the manuscript the authors use 10° slope angle and a correction factor of 0.9 taken from a U.S. patent 7,305,983 B1. 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 the subject. See also the respective comment to a similar question of reviewer #1.
The impact of a possible error to net radiation and the total energy balance will be addressed in the discussion section of a revised version of the manuscript.

P157/L1: did you screen the met data for snowcover on the upper sensor? In data sets I have looked at it is usually possible to identify these cases when the lower sensor registers higher radiation than the upper sensor, and these can then be 'corrected' on the basis of an assumed fresh snow albedo.

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

P157/L5: I am not clear how the low wind speed would lead to discrepancies, as the measured low wind speed is an input to the model. Perhaps I have missed something here? Is it associated with the comment on P161/L7 which refers to a low wind speed sensible heat flux enhancement factor within the COUP model?

Yes. We will clarify this point in a revised version of the manuscript.

P158/L20: Is it possible to just briefly mention what this work was?

It is based on unpublished work by S. Schneider. The authors will consider to delete the respective paragraph in the revised version of the manuscript.
Turbulent fluxes would then be consistent in all seasons and would add to the uncertainty to the deviation term.

P159/sect 3.1: Can you add a comment on the potential role of lateral transfers which are not included in either approach as far as I can understand?

A comment on the role of potential lateral transfers will be added to the revised version of the manuscript.

P160/sect 3.2: Did you experiment with different sizes and placement of the sink/source layer – if so what was the impact of that and why did you chose this structure in the end?

We did not experiment with different sizes and placements of the sink/source layer. The position was chosen to be beneath the surface and the thickness was chosen large enough to approximate the natural situation (40% porosity in the active layer) and thin enough not to cause numerical problems.
We assume that given the simple physical processes caused by this layer (i.e. extraction/addition of heat) changes of the size and the position (within reasonable boundaries) would be minimal.

P162/L10: (sp) from

Fig 1: very useful figure to understand the differences in approach.

References:

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