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Interactive comment on "Semi-automated calibration method for modelling of mountain permafrost evolution in Switzerland" *by* A. Marmy et al.

Anonymous Referee #1

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In the manuscript "Semi-automated calibration method for modelling of mountain permafrost evolution", Marmy et al. present simulations of the future ground thermal regime at six instrumented sites in the Swiss Alps. In its scope and effort, this work is virtually unparalleled and deserves publication in The Cryosphere. However, I am not at all convinced that this work can re-define the state-of-the-art for such studies, and I recommend major revisions before publication. From the material presented, it does not become clear to me that the method can increase the confidence in future predictions compared to much simpler methods.

Major Comment:

In a certain way, the authors treat the COUP model as a “back box” for which the calibration procedure produces an optimal set of parameters. However, at least in some cases, these parameter sets are not really physical realizations, i.e. some parameters would most likely not be confirmed if independent measurements (e.g. of surface or ground thermal properties) were available. In previous studies dedicated to future projections of the ground thermal state, the authors have chosen a sufficiently simple model (e.g. based only on heat conduction), estimated the parameters according to field knowledge/physical constraints and then compared the results to measurements e.g. in boreholes for validation. While the match with measured data is in general not as good as in this study, it will generate the right results for the right reasons, or at least the limitations will become more obvious. In particular for future simulations, which cannot be validated, the “black box calibration” approach chosen for this study has the potential to produce artifacts in the future simulation. The authors should therefore make the link between the fitted parameters and observable/observed processes much clearer (wherever this link exists). If a parameter set is clearly unphysical (see below for more specific comments), I suggest not to show the future simulations since artifacts are highly likely.

Minor Points:

-p.4788, l. 27: Explain what is meant by “GCM-RCM chain” and “ENSEMBLES data set”.

-p. 4789, l. 2: I don’t think that “Langer et al. (2013)” is not a good reference in this context. It would be much more appropriate in l. 20ff.

-p. 4789, l. 20 ff: The classification in 1D-2D- and 3D models does not follow strict and logical criteria, or at least it does not become clear which variable or process are 1D, 2D or 3D. To me, the mentioned 1D and 3D approaches have a lot in common, since they explicitly account for energy exchange processes in a more or less physically-based way. The mentioned 2D approaches, however, are more (semi-)empirical schemes which are aimed at estimating averages of the target variables of the 1D/3D schemes in a simplified way (except Hartikainen et al., which stands out in that it focuses on much longer timescales than the other studies). In addition, there is the class of spatially distributed 1D-models, sometimes referred to as 2.5D. Examples are the later mentioned Westermann et al. (2013), but also Jafarov et al. (2012) and Zhang et al. (2012).

p. 4792, l. 1: the statement “potential scenarios of possible. . .” contains some redundancy. “scenarios of. . .” is enough, in my opinion.

p.4795, l. 19: Wicky (2015) refers to a master thesis (in German) which has not undergone the normal review process. While this is generally problematic, the statement seems to be sufficiently backed up by another reference. I would therefore leave it up to the authors to decide whether to remove this reference or not.

p. 4798, l. 10, p. 4800, l. 14: it is not directly clear to me why wind speeds play only a minor role in the modeling. From my experience, there are some cases (e.g. high global radiation, but cold air temperatures), where wind speeds have a pronounced effect on active layer thickness and ground temperatures using similar model approaches. It is quite possible that this is not the case for the investigated sites, but it should become clear whether this was checked, and to what extent the role is “minor”.

p. 4799, l. 2: What is meant by “virtually all”?

p. 4800, l. 8ff: This is obviously a huge limitation for MBP, and the effect on the results is not clear at all. If there is a strong bias in the global radiation forcing the model, the optimization procedure would tend to correct this by adjusting parameters in a potentially unphysical way, making the simulated future ground temperatures more or less useless. How well can the model estimate global radiation based on latitude/air temperature? Has this been checked for the other sites, where global radiation was available? p. 4801, l. 16: at what depth is the lower boundary? Is it below the depth of zero annual amplitude? In this case, the amplitude should be negligibly small.

Does this treatment take the geothermal gradient into account at all, i.e. that ground temperatures become warmer at depth? In this case, how can ground temperatures at depths of up to 80m be modeled?? How is the lower boundary condition for the future runs?

p. 4801: I don't understand how Eqs. 1 and 2 are related. There are four fluxes, qh, qv, qh and qin, are they related somehow? In Eq. 2, the units don't match, the first term has the unit K/m, not J/m2d.

p. 4802, l. 7ff: This treatment seems to be mainly focused on reproducing spatial averages of the ground thermal regime. Is this appropriate for reproducing temperatures close to the surface, which would either "see" snow or now snow, not patchy snow conditions?

p. 4802, l. 11: What is Eq. 9?

p. 4802, l. 25: I can't believe that this procedure will create anything realistic for the "deep" ground temperatures. At a depth of almost 80 m, like in Stockhorn, the ground temperatures should still be influenced (if not completely determined) by times before 2000, and even 1981. The modeled ground temperatures at depth would then only reflect unphysical steady-state conditions for the applied forcing data. If this is the case, the deep ground temperatures should be completely removed from the optimization routine and analysis, and the limitations on the future simulations resulting hereof should be mentioned.

p. 4803, l. 26ff: This is unproblematic if it is done for periods when observations are available. It is extremely problematic as a basis for future simulations, as it is done here. It is not clear at all, if the parameter set is also an optimal one for the future forcing, or if it creates a fake model reality. See also major comment.

p. 4804. L. 4: How about the parameters that do have on-site measured values. Is the uncertainty/spatial variability/changes over time taken into account? This could play a major role for parameters to which the model is highly sensitive.

p. 4804: In the fitting routine, only ground temperatures are used to determine the model performance. This could be problematic as the freezing point of water is not exceptional in this procedure. It is the same if the model is wrong by 0.2 degree C at +10 degrees or at -0.1degree C. For the latter, the differences in ground ice and thus the energy content of the ground would be substantial, with pronounced impacts on the future simulations. This would in particular influence deeper ground temperatures. The effect could potentially be moderated by using the energy content of the ground (e.g. as in Jafarov et al., 2012, calculated with the freeze curve also assumed in the model) instead of temperature. The authors should at least comment and discuss this limitation. Note that this comment does not refer to using additional measured parameters, such as water contents.

p. 4805, l. 7: see comment above on initialization/deep ground temperatures.

p. 4808, l. 5: I suggest leaving the deep temperatures out, see above.

p. 4809, l. 14ff: Which criteria is the statement on equifinality based upon?

p. 4817, l. 20: So what's the value of the simulations in these cases then? The authors clearly state that the model cannot represent site-specific conditions and the associated ground thermal regime, so simulations of the future ground thermal regime could feature a strong bias. Wouldn't it be better to clearly state that the procedure is inappropriate for such conditions, and that it is not meaningful to conduct future simulations with the scheme in this case?

p. 4813, l. 17ff: It would be nice if the authors could directly provide some statements on how these two points affect the outcome of this study.

p. 4815, l. 28: remove "provide again. . ."?

p. 4816, l. 8: How is the partitioning between transpiration and evaporation controlled in COUP? Are there really plants on Stockhorn, and is it realistic to assume that the latent heat flux is strongly controlled by transpiration rather than evaporation? If no, this would be a good example where the effects of incomplete or even flawed model physics, input data and/or other biased model parameters is compensated by tuning the model in an unphysical way. In my opinion, this does not result in a model that can describe reality in a better way (although it can describe the training data sets).

p. 4817, l. 23+25: I would interpret these two statements in the way that the results are not meaningful for this case. The optimization procedure yielded unphysical model parameters to compensate for the incomplete model physics, and then the model is run in this configuration with the future forcing, not knowing anything about the effect of the biased configuration under the warmer future conditions.

p. 4820, Conclusions: I expect a clear statement from the authors if the considerable efforts involved in this method can bring a performance gain over much simpler methods (e.g. the comparatively primitive approach to simulate future scenarios for borehole temperatures in Etzelmüller et al. 2011, Hipp et al. 2012, or the spatial modeling of Jafarov et al., 2012).

References

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Interactive comment on The Cryosphere Discuss., 9, 4787, 2015.