We thank the reviewer for her/his comments and suggestions which have been incorporated in a revised version of the manuscript. In the following, we provide a point-by-point response to all comments raised (in bold font):

This paper describes the complexity of simulating permafrost temperatures and degradation in a remote region with limited data. The model described in this paper is simple and focuses its complexity on the upper boundary conditions in order to calculate ground temperatures and permafrost degradation. The new aspects of the model are in the treatment of snow and a simplified way of incorporating subsidence as a result of loss of excess ice in permafrost. A lot of time is spend in the paper describing the overall problem and available data (too much maybe?),

As the reviewer mentions, there is only limited data available for our study region, although the Lena River Delta is among the best studied permafrost regions in Siberia. While some data sets are already described in dedicated publications (Langer et al. 2012a,b; Boike et al. 2013, 2015), others have not been published in this form. This concerns in particular the different ground stratigraphies which are therefore described in due detail. We feel that it is necessary to motivate their choice as the model results are crucially influenced by them.

the complexities of measurements used and overall conditions of the field setting, minimum time is spend on the equations and justifying them.

In total 13 pages are dedicated to describing and justifying the governing model equations, including the Appendix (not including figures for illustration). For comparison, about seven pages are dedicated to describing the study area and the available data sets, and two and a half pages for the Introduction describing the overall problem. We therefore can't agree with the Reviewer's statement that maybe too much time is spent "describing the overall problem and available data", while "minimum time is spend on the equations and justifying them". Concerning the justification of the different equations, we have made improvements in the revised version: in 2.2, we have inserted a sentence "The equations follow Foken (2008), …" which refers to one of the textbooks where the set of equations is described. Eq. 9 is widely used in ESMs and snow models, but we have inserted a reference (O'Neill and Gray, 1972) in the revised version. Eqs. 10 and 11 have references already, and we have inserted a reference (Westermann et al., 2013) to Eq. 12 and the accompanying text. In Sect. 2.5, we clearly state that we follow Westermann et al. (2011). In Sect. 2.6, the first four presented equations are discretized forms of Eqs. 12, 14 and 1 which is clearly stated. For Eq. 21, we have now inserted a statement that the time dependence is based on Eqs. 12 and 14. Eq. 22 is thoroughly explained in the text. The same applies to Eqs. 23 and 24 which are even illustrated in a figure.

This is excused by the authors saying that the reader is free to use the code and can change to equations at will.

The mentioned statement is in no way connected to not justifying the set of equations. We feel that we provide the required information to judge the soundness of the model physics and to provide users with the necessary background.

However ideally a modeling paper should describe why particular equations were chosen over others and a description of the pros and cons of that equation set, supported by field measurements.

The turbulent boundary fluxes are describe in great detail in this model, but no reason is given why it is important to have in the model.

The surface energy balance with the turbulent fluxes of sensible and latent heat is a crucial part of every land-surface scheme. As such, the surface energy balance must be implemented if CryoGrid 3 wants to be an experimental platform for process parameterizations which can thereafter be implemented in ESM land-surface schemes. Moreover, we would like to emphasize that the model can reproduce both surface energy balance, surface temperatures, ground temperatures and thaw depths. This suggests that the model can simulate the ground thermal regime for the right reasons, not due unphysical adjustment of one or several of the many model parameters. In Sect. 5.1, we therefore write: "The good agreement between the modeled and measured ground thermal regime can be linked to the surface energy balance, surface temperatures and the snow dynamics, all of which appear to be adequately reproduced.**"**

Snow is considered and important variable for the overall energy balance of the ground, but is the snowmelt period really that important?

In general, the snow pack has two main effects: it strongly insulates the ground during winter, and the timing of the snow melt influences the ground thawing and the resulting thaw depths. As described above, our model validation attempts to cover several different aspects to demonstrate that the model can reproduce the ground thermal regime (which is the main target) for the right reasons. Therefore, we have also included the timing of the snowmelt in the validation.

Are there two different models described in this paper or is the Xice portion a module that can be turned on and off?

In the manuscript, we describe the basis model CryoGrid 3 and the model version CryoGrid 3 Xice that can account for excess ground ice thaw. If no excess ground is defined in the ground stratigraphy, the two model versions are equal (as for the Arga stratigraphy). The excess ground ice component can be turned off also for stratigraphies with excess ground ice (simply by modifying the Matlab code), but this will lead to the establishment of an unphysical "subsurface aquifer" upon thawing. Moreover, the respective grid cells will obtain a thermal conductivity close to the relatively low thermal conductivity of liquid water, which delays further thawing of below-lying layers. Such effects are purely model artefacts that are not relatable to observations (since ground subsidence/thermokarst will occur in reality), and we have therefore chosen not to show and discuss such simulations in the manuscript.

Natural porosity is an unusual term and should be cited: e.g. Hopkins and Sigafoos in Contributions to general Geology 1950.

The term "natural porosity" is used in a number of studies on various processes that can lead to consolidation of soils. We have added two references to the term to in order to clarify that we make use of an established concept.

The model seems to perform well, but the scales in the figures do not allow accurate assessment of the fit. For instance the 1 to 1 line in figure 2 shows a spread of over 10 degrees Celsius between measured and simulated even close to the freezing point. Is that really a good fit or maybe it is not really important to know the exact surface temperature for permafrost degradation if the micro thermal conductivity is not really known?

Due to the diffusive nature of heat conduction, it is important to reproduce the average surface temperature for longer periods, e.g. one week or similar, in order to reproduce the ground thermal regime and the seasonal thaw dynamics. Single model values which strongly disagree with measurements, will therefore not strongly influence the results. Fig 2 is clear evidence that the average surface temperature is well reproduced for most temperature intervals, and that a (small) systematic bias only occurs for the coldest temperatures. Moreover, single outliers could also be caused by erroneous measurements (see Langer et al., 2011a,b), e.g. due to partly snow-covered sensors. We agree that the thermal conductivity of the snow and ground has a pronounced influence on the model results. For this reason, however, we validate the model both against measured surface temperatures and against measured ground temperatures. The satisfactory agreement with both is a clear indication that at least strong biases in these parameters do not occur.

Why not apply the Stefan solution and some n-factors?

The Stefan solution is a highly simplified way of computing thaw depths, as it only accounts for the latent heat due to freezing soil water, not the energy required to change the subsurface temperatures. As shown in Langer et al. (2011b), this only accounts for about 20-60% of the accumulated energy change in the ground for the case of Samoylov Island. A detailed understanding of ground thermal dynamics, as intended with CryoGrid 3, is impossible with the Stefan solution. N-factors are a simplified way to account for surface offsets, e.g. caused by the insulating snow cover in winter, or systematic differences between air and surface temperature during summer. They are empirical factors, which can be highly practical in large-scale applications, where a detailed data set on model parameters (as in Table 4, revised version) is not available. For detailed process understanding in a largely physically-based model, however, they are not a practical solution. Moreover, n-factors need to be calibrated with present-day data, and it is highly uncertain that this calibration is still valid under a future climate forcing.

The argument that lateral fluxes do not mater at a small scale is not true. They are even more important on a small scale, because the portion of the lateral flux to vertical flux is greater. Therefore coarser scale models can ignore lateral fluxes more easily. The problem of scale and resolution will always be against accurate representation of the system. On the other hand more can be done in this model on a sub-grid level to improve representation of the lateral fluxes.

We fully agree with the reviewer, and all statements that could create a different impression have been modified or removed in the revised version of the manuscript. As discussed at several places in the manuscript, the key for realistic modeling will be the representation of small-scale (and thus subgrid when referring to a land-surface scheme) fluxes of water, heat and snow. With this study, we aim to lay the foundations in order to advance in this direction in the future.

The paper is publishable, because it attempts to fill a gap in the capability of existing models where the model domain itself changes as a function of boundary thermal forces. This type of model should be useful to people how may not have experience using large main frame computers that attempt to solve the entire set of physics. Ultimately though will scientists use this model if the results it provides are based simplified physics?

Follow-up studies with the presented model are already on the way for a variety of different permafrost settings. We feel that ultimately all results obtained with Earth System Models (ESMs) are based on “simplified physics”, yet they are crucial for advancing science. The aim of this study is ultimately to contribute to achieving ESM results that are based on less simplified physics compared to current ones.

The model description should be focused more on understanding the effects of the physical simplifications on the results, but maybe that is the intended goal for others to figure out?

We are not fully sure if this comment refers to the general model setup or the Xice module, but we assume it is the latter and focus on this the Xice module in our response: at the moment, we are not aware of any model that claims to account for the complete physics of excess ground ice thaw, so it is not possible to compare our results to a “reference model”. In Sect. 6.2., we mention a number of limitations of the presented scheme, and what these may mean for the model results. We are off the opinion that this is the adequate place for this discussion, not the model description. In the future, we will build some of the mentioned processes in the modeling scheme, which will then facilitate e.g. sensitivity studies towards different processes and/or parameters. This, however, cannot be incorporated in the present study without considerably adding to its content and length. We therefore feel that this is out of the scope for the present study.

Specific edits:

Equation 14 T* not described.

done

Equation 17 θ* not described

done

6941 L10 define the state of zero energy content

done

6941 L13-14 delete “corresponding amounts of”

done

6946 L1 “re” needs to be added to “mains”

done

6961 L20 delete one of the “a”

Done, thanks!!

6962 L25 should be “stable” not “stably”

The term “stably stratified” is commonly used in atmospheric studies. For lakes and water bodies, most studies only refer to “stratified water columns” and we use this term in the revised version.

References

Foken, T.: Micrometeorology,308 pp, Springer, Germany, 2008.

O'Neill, A. and Gray, D.: Solar radiation penetration through snow, The Role of Snow and Ice in Hydrology, Proceedings of the Banff Symposium, International Association of Hydrological Sciences 107, 227-240, 1972.

On behalf of all authors,

Sebastian Westermann