Author response R#3

“The surface energy balance in a cold-arid permafrost environment, Ladakh Himalaya, India”

John Mohd Wani, Renoj J. Thayyen, Chandra Shekhar Prasad Ojha, and Stephan Gruber

Response to Referee #3: Giacomo Bertoldi

Thank you very much for your review and your constructive comments on this manuscript. I hope that the explanation given below, and the changes to the manuscript, will provide an adequate response.

General comments:

Reviewer comments	Author response
• I suggest to move the model validation section before the discussion of the results. The reader before wants to understand the model´s reliability, and then look to the results on the energy budget.	As suggested, the model validation section is moved before the discussion section in the revised manuscript.
• The presentation of the results is rather long and with many repetitions. The main message of the paper is rather simple. In Ladakh mountain the environment is dry, cold and sunny. Therefore, this leads, compared to other sites, to little incoming longwave and more direct solar radiation which helps permafrost. Snow comes relatively late and major differences are related to the snow duration. This could be explained in a more concise way, leaving space for a more quantitative discussion (see specific comments).	The revised manuscript is rewritten more concisely. The author response to specific reviewer comments depicts the same.
• For the methodology, it is not clear to me if soil moisture is explicitly modelled or not (see specific comment at line 210). This has strong implications on the interpretation of the results.	Yes, the soil moisture is modelled using the parameter “*WaterBalance* = 1” in the GEOtop input parameter file.
• The paper is interesting, but the story is simple. I have the feeling that there are repetitions and details not needed.	Permafrost research in this area is in a very nascent stage, and we aim to generate wider acceptability of permafrost in the Ladakh region and provide a basic understanding of SEB processes for the first time. To remove the repetitions, the revised manuscript is rewritten more concisely. The

	author response to specific reviewer comments depicts the same.
• I think that the paper could be strongly improved if the model is used also for numerical experiments for quantitatively understand role of climate and possible changes for future permafrost development.	Presence and implications of permafrost and its thaw in the UIB region, including Ladakh, is not appreciated so far. Our first aim is to provide irrefutable evidence of permafrost and related processes. This paper is a step towards that effort and used only two years of data, which is available. We highly appreciate the suggestion of the reviewer but feel that it is beyond the scope of this paper. We will certainly attempt this after generating better data and understanding.

Specific comments:

Reviewer comments	Author response
1. Introduction See general comments. More specifically:	Addressed as above.
L75 "*The energy balance at the earth's surface drives the Spatio-temporal variability of ground temperature*" This is an important point, which needs further clarification, since it motivates the rationale of this work. This is mediated by the ground heat flux (both in term of heat diffusion and heat transport by water). A little bit more of basic theory or an equation could help.	The more theoretical explanation is added in the revised manuscript.
2. Material and methods	
L 125 – 135: catchment description. All this information on geology is ok, but at the end what matters are the implications for soil and shallow rock hydraulic and thermal properties. What do you know about them?	The properties (thermal and hydraulic) of soil and rock were not available in our catchment, and we adopted the values of these properties from Gubler et al. (2013). The work of Gubler et al. (2013) and Engel et al. (2017) provides a good starting point for the selection of values for many parameters.

L 210 "*In this study, only the energy fluxes over the snow cover and the ground surface in one-dimensional (1D) mode of GEOtop are used*." Here is not clear to me if you run GEOtop only in energy budget mode or you are also simulating the soil column water budget. This has strong implications on the interpretation of the results. In the first case, the soil is assumed always saturated and therefore ET from soil could be only potential. In the second case, the soil can become dry and ET is real and can be low in dry snow free periods. Please clarify this important point.	Yes, we are estimating the energy budget inclusive of simulating the soil column water budget.
L 246 – „*Albedo*". It could be interesting for the reader to explain briefly how albedo is changing with respect to snow age and solar angle in GEOtop.	More theoretical details about the description of albedo in GEOtop such as its change with respect to snow age and solar angle have been added in the revised manuscript.
L 295 – „*Heat equation*". Is GEOtop able to simulate also the heat transport by the water into the soil? This is a very relevant process for permafrost melting (see recent Ph.D. work of Alessandro Cicoria).	The GEOtop does not simulate the heat transport by water into the soil.
L 305 – „*Snow modelling*". A little bit more details could be useful. At least to say that GEOtop uses a multi-layer, energy based, Eulerian snow modelling approach.	More theoretical details about the snow modelling approach used in the GEOtop model have been added in the revised manuscript.
L 305 – „*performance statistics* ". Okay, but it might be more concise. All is well known.	Combining this suggestion with that of Rev-2, the description of performance statistics is written more concisely, and the equations of the evaluation metrics are added in the supplementary index material.
3. Results	
I suggest moving the paragraph "Model Evaluation" at the beginning of the results section.	As suggested, the Model Evaluation section is moved at the beginning of the results section.
3.1 Meteorological characteristics.	
A lot of details, some of them are not necessary. May be a chart with the difference GST – TA is more informative than many words.	This sub-section "Meteorological characteristics" is rewritten more concisely in the revised manuscript.

L 433 - 445 Precipitation. This section is quite confusing. You have a "measured total precipitation" and then a "precipitation estimated with ESOLIP". It is not clear the difference and the meaning. I guess your measured precipitation is only the liquid precipitation measured by the (unheated?) rain gauge. The ESOLIP precipitation is the sum of the liquid precipitation of the raingauge (with some wind under catch corrections too ?) and of the solid precipitation estimated from snow height data. At the end, later (Figure 6) you find that the ESOLIP precipitation is a more correct estimation. Is this right? Please rephrase this part. If the model evaluation section is before, then the story becomes clearer.	Observed precipitation is from Ordinary Rain Gauge (ORG). In summer, rainfall is measured directly and in winter snow periods snow w.e. is measured after melting the ORG catch which is certainly underestimated. In winter snow depth is measured using SR50. So yes ESOLIP presented here is liquid precipitation plus SR50. Here, we had the time resolution problem between total measured precipitation (ORG) and other meteorological forcing's including SR50 snow depth (hourly and recorded by automatic weather station). In ESOLIP we considered liquid precipitation on daily basis only. Furthermore, we run the model twice: (a) first model run was made with precipitation data measured in the field, and (b) second model run was made with the ESOLIP estimated precipitation as input. During the evaluation, we find that when using ESOLIP estimated precipitation as input model performance match very well with the snow depletion (Figure 6).
L 473 Albedo. This is super low! Over snow covered terrain albedo should be 0.9 – 0.7 minimum, over bare soil around 0.2. Your value is so low because the assumption albedo=0 during the night? During the night albedo is not defined.	We thank the reviewer for pointing out the error in albedo, and this is now corrected in the revised manuscript. The lower values of mean daily albedo in the previous version of the manuscript were due to wrong averaging (used 24 hr.). Now it is corrected.
L 500 - 515. This is also long and boring …	These lines are rewritten more concisely in the revised manuscript.
Figure 4. Nice Figure. Your story is already there but the reader needs to wait the discussion to figure out what is striking from the Figure. Interesting is the very high sublimation (typical of arid climates – see Herrero works) and the relevant energy absorbed by snow melt (evident in Table 4) in snowy winters which is not going into the soil and therefore is not available for permafrost. However, I have a question. More snow melt means also more water infiltrating in the soil. How is this water affecting the permafrost?	This is certainly an interesting question, and critical for regional hydrology and permafrost response. However, we do not have an answer at this stage as we are working with a limited data set in this paper. With more years of data, we have plans to run the GEOtop model in distributed mode to study the role of infiltrating snowmelt, routing and hydrology.

3.5 Model evaluation.	
Please move this section before. In general, the model performs quite well, and his estimation of the surface fluxes could be considered reliable.	As suggested, the Model Evaluation section is moved at the beginning of the results section.
Please consider uploading this test case in the testing suite of the GEOtop model website.	As and when the review process is complete, a test case will be shared with the developers of the GEOtop model.
4 Discussion	
Figure 8: Choosing two arbitrary days is not very informative. It could be nicer to show the average daily cycle for many snow covered and not snow covered days for the two seasons.	During our seasonal analysis, we saw that all the days without cloud cover during the particular sub-season show more or less same patterns in the amplitude of the energy fluxes. That's why we choose to show two arbitrary days instead of an average. In the revised manuscript, the average seasonal diurnal values of energy fluxes are shown.
L 714 - 720 1% difference seems to be not so significant, given the high uncertainty in surface fluxes estimation. However, the difference from the Figures is quite evident. I do not understand this section.	The idea behind was to give an overview of the partitioning of the surface energy balance and at the same time, its difference during the two contrasting years.
L 730 - 745 Ok, the story is clear! Please stop repeating.	The repetitions have been removed in the revised manuscript.
Figure 9 Sub charts E and F. Why they are informative? I do not understand …	Figure 9 (Sub-Plots E and F) describe the monthly average variability of turbulent fluxes (H and LE) during low and high snow years. These subplots give a better overview of how the freezing/thawing processes affect the turbulent fluxes and their variability in the seasonally frozen ground and permafrost regions. For example, in early October (Figure 9E and F), the LE began to weaken up to the December for both the years as the seasonally frozen ground began to freeze. Also, during the summer months, the LE starts to increase due to the availability of moisture. Therefore, the seasonal freezing/thawing of the ground affect the LE causing its rapid decrease/increase.

	Similar variability is also reported from the seasonally frozen ground and permafrost regions of the Tibetan plateau (Gu et al., 2015; Yao et al., 2011).
4.2 Influence of snow cover.	
The comparison among two years is interesting, but two years is too less. More years are needed to have general conclusion.	Unfortunately, data is limited. Data is being generated, and we will be able to provide more detailed analysis in the coming years. Please see the answer to comment number 5 (page 2 of this response document).
Line 778 and **Figure 10.** "*Not linear behavior*" Interesting, but the simulated period is too short. You could take advantage from the calibrated model to generate many synthetic years with more and less snow cover. In this way you can generalize the relationship with a numerical experiment … for example increasing or decreasing the precipitation to generate different snow duration and then derive the relation of Figure 10 in a more robust way.	This section has been removed from the revised manuscript due to non-availability of data for more years.
4.3 Influence of snow cover. The comparison is interesting, but the characterization of the sites is very different. It seems a part put there having the feeling there is too less in the paper. If you want to make the paper more robust, I suggest performing numerical experiments.	Please see the reply to the above comment.

Minor comments:

Reviewer comments	Author response
L 74 – "Spatio" lowercase	Changed as suggested.
L 205 – GEOtop model references – "*Previous studies have successfully applied GEOtop in mountains regions, e.g., simulating snow depth and ground temperature (Endrizzi et al., 2014), snow cover mapping (Dall'Amico et al.,2018; Dall'Amico et al., 2011; Zanotti et al., 2004), ecohydrological processes (Bertoldi et al.,2010), modelling of processes in complex topography (Fiddes and Gruber, 2012), permafrostdistribution (Fiddes et al., 2015) or modelling ground temperatures (Gubler et al., 2013*)" Major GEOtop reference, besides Endrizzi et al (2014) is Rigon et al (2006). For ecological processes better cite Della Chiesa et al	Added more references that have successfully applied GEOtop as suggested.

2014 or Bertoldi et al 2014. For ground temperatures, besides Gubler et al., 2013, you could cite Bertoldi et al 2010, which deal on LST modeling in complex terrain. For full reference list please see: https://github.com/geotopmodel/geotop/blob/master/README.rst	
L 220 – "*But in the GEOtop (Endrizzi et al., 2014) the equations of SEB are described separately*" This sentence seems isolated from the context and needs to be revised.	The sentence mentioned is revised in the revised manuscript.
L 322 – the model was initialized at a uniform **soil** temperature	Added the word "soil" in the revised manuscript.

References

Engel, M., Notarnicola, C., Endrizzi, S., & Bertoldi, G.: Snow model sensitivity analysis to understand spatial and temporal snow dynamics in a high- elevation catchment. Hydrological Processes, *31*(23), 4151-4168, doi: 10.1002/hyp.11314, 2017.

Gubler, S., Endrizzi, S., Gruber, S. and Purves, R. S.: Sensitivities and uncertainties of modelled ground temperatures in mountain environments, Geosci. Model Dev., 6(4), 1319–1336, doi: 10.5194/gmd-6-1319-2013, 2013.

Gu, L., Yao, J., Hu, Z. and Zhao, L.: Comparison of the surface energy budget between regions of seasonally frozen ground and permafrost on the Tibetan Plateau, Atmos. Res., 153, 553–564, doi: 10.1016/j.atmosres.2014.10.012, 2015.

Yao, J., Zhao, L., Gu, L., Qiao, Y. and Jiao, K.: The surface energy budget in the permafrost region of the Tibetan Plateau, Atmos. Res., 102(4), 394–407, doi: 10.1016/j.atmosres.2011.09.001, 2011.