Author response R#2

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

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Response to Anonymous Referee #2

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.

Main comments:

Reviewer comments	Author response
1. English and style: The English contains grammar errors (too many to detail here, but as an example often the third person plural is used when it should have been singular) and weird sentences. The writing style is often redundant and contains many repetitions – I have indicated some below. I had started suggesting corrections to the English but then stopped as this would take many pages and a lot of time. The paper however needs a careful and extensive proof-reading of both English and writing style, and the authors should make an effort to turn the manuscript into a more readable, polished and compelling paper.	The proof-reading of the revised manuscript is done with the help of Grammarly software (Institute Premium License).
The abstract seems long and could be shortened and made more to the point.	In the revised manuscript, the abstract is shortened.
There are many repetitions in the paper, e.g. lines 242-243; 344-346; 511-512; and in many other instances.	The repetitions in the revised manuscript have been corrected.
I would strongly encourage the authors to go through the manuscript and polish/improve it substantially. In its current form, it is not appropriate for publication.	Thanks to the reviewer suggestions, the revised manuscript is presented in a much better way.
2. Paper structure: I feel the paper structure needs to be improved in several places.	Thanks to the reviewer suggestions, the revised manuscript is structured in a much

First, I would suggest that the authors separate the study site and data section from the methods section, for readability. As it is now, they need many sub-sections to accommodate all this section content and this section is very long.	better way. The study area and data section are separated from the methods section.
Second, a lot of text that belongs to the Methods is contained in the Results section, to a point that the paper is extremely repetitive. Examples are on lines XXX	The repetitive text from the results section is removed.
Third, I would encourage the authors to reconsider the way their Results section is structured: first the observations are presented, then the energy fluxes described, and then those are validated with the observations. Before any discussion of the fluxes, they should be validated – otherwise we do not know on which we can have confidence and on which we can have less.	Combining this suggestion with that of reviewer#3 (Comment: 3. Results), the model evaluation section is now moved at the start of the results section in the revised manuscript.
I also have some major comments on the figures in this section, which are repetitive and do not make a very good use of space (see comment on Figures below). Finally, most of the content in the Discussion, and most of those figures, should actually be in the Results section, as they present the actual energy fluxes that are the main focus of this paper.	The figures are improved in the revised manuscript. As suggested some part of the discussion is moved to the results section.
3. Aim of the paper: It is not clear what the paper aim is. The authors state: "we aim to provide a foundation for better understanding the micro-climatological drivers affecting permafrost distribution and temperature regimes in the area, to build hypotheses about similarities and major differences with other, better-investigated permafrost areas".	Aim of the paper is made clear as follows: 1. Understanding the SEB dynamics in a hitherto unknown permafrost area in the UIB. 2. Model seasonal snowpack response (accumulation and melting) and near-surface ground temperature (GST) giving better understanding of snow precipitation (ESILOP) and GST response. 3. Assess the reliability of GEOtop model with minimum input parameters by comparing with observed radiation components. The idea behind the comparison with other permafrost areas is to understand how different micro-climatological drivers such

	as incoming shortwave radiation, relative humidity, etc. is comparing with the Ladakh region.
First, it does not seem that this study can contribute to understanding the drivers of permafrost distribution, given that it focuses on one single location. If, however the authors think their analysis can contribute to this, they should devote the discussion to examine how their results about energy fluxes at one location can be relevant for permafrost distribution, and consider more in depth-broadly the implications of this study for permafrost distribution.	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. Furthermore, our aim in this study is not about permafrost distribution.
Second, I do not see which are the hypotheses the authors want to build. Also for this, I would encourage the authors to either reformulate their overall aim, or consider the implications of their findings beyond the pure description of the energy fluxes time series.	Based on the comparison, we draw to the conclusion that in this region the, (a) surfaces being overall colder than at a similar location with more relative humidity, (b) Increased amount of incoming shortwave radiation. This will mean that sun-exposed slopes will receive more radiation and shaded ones less (less diffuse radiation) than in comparable areas, and (c) Increased cooling by stronger evaporation in wet places such as meadows. Where there is enough water, you can cool the ground significantly. With modified objectives and improvement in the discussion, including implications as mentioned above, we addressed the reviewers concern.
With respect to the aim, I am also puzzled by their choice of the model forcing. If the paper's aim is to understand the energy fluxes (and melt and refreezing processes into the soil), then I do not understand why the authors force their model with parameterisations of the radiative fluxes given that they have all measurements available. They instead use the measurements of the four radiative fluxes as a validation of the model, showing indeed that there are differences between observed and modelled shortwave and longwave fluxes. Those differences or errors will	Following upon the suggestion, all the observed radiation (incoming and outgoing shortwave radiation, incoming longwave radiation) fluxes except outgoing longwave radiation are now used as input to the model. GEOtop model does not have the provision to give outgoing longwave radiation as input. It is estimated from modelled ground surface temperature iteratively.

translate into errors in the energy fluxes simulated, which are rather gratuitous here.	
This seems even more important considering that there is no quantification of model uncertainty (see a point below).	Uncertainty analysis of the model using PEST tool is undertaken
With the approach they use, they seem to want to test the ability of GEOtop to parameterise those fluxes. If this is their aim however, this should be stated more clearly, and the paper structured accordingly.	In addition to our other objectives, such as energy balance modelling, the reliability of GEOtop model as an objective is also added in the introduction. (Please see the response to comment no. 3, Page 2).
4. Introduction: The Introduction should be substantially improved. The Rationale for this study is not clear and the review of current studies and knowledge gaps is incomplete. There is a single short sentence about precipitation being higher than expected –referring to one single study from 2015 – and then the authors start with “Another key unknown is permafrost…” I strongly suggest that the authors provide clear motivations for their work.	The motivation behind this study in the introduction section is presented in a much better way, and more references have been added. Unfortunately, high elevation precipitation data in this region is seldom available. Added couple of references regarding this aspect.
The overall There is quite a lot of emphasis on permafrost and its potential importance, but the link then to the actual investigation conducted in this paper should be made clearer and stronger.	The revised manuscript is now structured in such a way that the paper focusses more on energy balance from a permafrost environment.
While I overall agree with the authors that “..the knowledge of frozen ground and associated energy regimes are a key knowledge gap in our understanding of the Himalayan cryospheric systems, especially in the Upper Indus Basin”, the introduction as it is now does not convey this at all, nor the authors make a compelling case for the motivations for their study.	Permafrost is not considered or appreciated for Hydrological and climate assessment in the Upper Indus regions in India. This paper is a small first step towards appraising the SEB of one site in the permafrost region so that further studies can be triggered to achieve larger goals. The Introduction section is presented in a much better way, and more references have been added.
The aim is general... a foundation for a better understanding of the .. I also question the fact that, being this a point-scale study, the authors cannot say much about the distribution of permafrost (see my point above).	Our aim in this study is not about permafrost distribution, but to understand the energy balance from a permafrost environment in conjuncture with our earlier study (Wani et al., 2020).

References and use of literature The authors make extensive use of their own publications to back general statements on the Himalayan cryosphere, but miss the major publications in the field, and especially the many excellent studies from the last couple of years, some of them key papers that have substantially advance our understanding. I find it is not very elegant to refer only to one's own publications, especially when those cannot provide the evidence the authors use them for, as they mostly refer to very local and detailed studies. I would strongly encourage them to use a less parochial approach and give credit to the many excellent studies that have come out recently.	As suggested, more references about the Himalayan cryosphere have been added to the revised manuscript. Agree to the fact that there are number of publication on Hydrology of this region (Upper Indus Basin). However, one can notice that the none of these excellent studies mention about permafrost and its role in regional climate and Hydrology. And this is our prime motivation to take up the permafrost studies in the region. (This aspect is added in the introduction of the revised manuscript).
The first example is on lines 48-50: "It is hard to propose a uniform framework for the downstream response of these rivers as they originate and flow through various glacio-hydrological regimes of the Himalaya (Thayyen and Gergan, 2010)". That definitely is not the appropriate reference for such a statement, which needs back up from more extensive and comprehensive studies at the scale of the entire Himalaya or HMA and not from one single local catchment in Ladakh.	As suggested, more references have been added in the revised manuscript.
_Argument about permafrost cover being 14 times the one of glaciers should be rephrased, as glaciers have a thickness of hundreds of meters, while permafrost of few meters. I suggest the authors revise those statements. They can still point to the large areas where permafrost is present, but I think they should compare the total amount of ice, e.g. ice volumes or total potential water equivalents and not the area.	The statement is intended to give a sense of permafrost cover in the region. Comparison with glacier ice storage and permafrost ice reserve is not intended. Area of permafrost cover/ thaw does matter in terms of high elevation microclimate and disaster potential. Moreover, what is known today is the area. Ice reserve in the permafrost is not known as yet. These numbers are based on a coarse scale assessment using reanalysis data. Furthermore, in this region, the focusses of researchers have been limited to snow and glaciers.
_ The authors also seem to mix together rock glaciers and permafrost. Are they using rock glaciers as a synonym for permafrost?	In the Himalaya, rock glaciers are studied as it is indicative of discontinuous permafrost in the region. Hence we refer to those

They should clarify why rock glaciers are mentioned here. There are two theories as regards the genesis of rock glaciers, a glacial and a paraglacial origin, and the authors should make clear that at least they are aware of both.	studies to give due regard for the past work on this subject. In Wani et al. (2020), we provided a more reliable assessment of permafrost. The rock glacier studies were referred to provide an honest sketch of the progress made in this region.
5. Determination of precipitation: The authors use a method called ESOLIP to estimate precipitation from snow depth, which is not described except for the equation used for fresh snow density. I would strongly encourage them to explain at least the basic assumptions of the method in the main text, and include a more detailed description in the SI, given that snow is an important element of the differences in the two years.	The ESOLIP method used for precipitation estimation is described in detail in the revised manuscript. All the equations used in the manuscript are added in the supplementary index material.
The differences between measured and modelled snow depth, listed in Table 1 in the SI, is very high. The authors should justify this.	In the supplementary material (Table 1), the difference between the measured precipitation and ESOLIP estimated is due to the under-catch of winter snow recorded by the Ordinary Rain Gauge (ORG).
6. Error estimates in the measurements: Both the meteo input and validation datasets lack an assessment of errors. The sensor accuracy is provided in a table but no error estimates are given throughout the paper. They should be included in all figures and tables when comparing observations and simulations.	In the revised manuscript, the instrument errors are included in the text as well as in the figures.
7. Description of the EB model: This section needs improvement. The sign convention needs to be clarified and improved. It is very confusing. There must be a convention that holds for all fluxes, and then fluxes will be positive or negative based on their direction.	As suggested, the sign convention for surface energy balance (SEB) components is changed in the revised manuscript.
This section is rich in some obvious statements, such as that the reflected shortwave radiation is the incoming shortwave radiation times the albedo; and on the other side key information is missing. Here are some of the main aspects/points that should be clearly provided/clarified for the reader to evaluate the model approach and results:	In the revised manuscript, all the observed radiation (incoming and outgoing shortwave radiation, incoming longwave radiation) fluxes except outgoing longwave radiation are now used as input to the model. GEOtop model does not have the provision to give outgoing longwave radiation as input. It is estimated from modelled ground surface temperature iteratively.

_Why do the authors model the longwave radiative fluxes if they are measured? Also, there is no need to list those fluxes' equations, they are very established ones (could be moved to the SI).	The equations describing the radiative fluxes are moved to the supplementary index material.
_On the other side, no info is provided as to the cloud transmissivity, emissivity and other parameters used in those parameterisations, which are really the difficult ones to constrain.	More information about the parameterisations used for estimation of cloud transmissivity, emissivity, etc. is added in the revised manuscript.
_For calculation of the latent heat flux, how is the relative humidity of the surface determined, since it was not measured it seems?	Saturated specific humidity at the surface is estimated using GST.
_Which are the values of the coefficients alpha and beta in equation 10? The authors should describe what the parameterisations by Pielke et al is based upon, and how the coefficients are calculated, e.g. as a function of which other parameters or variables. In general, values of all model parameters (physical and empirical) should be provided in a Table (see below).	The values of coefficients for soil resistance to evaporation (β_{YP} and α_{YP}) used in equation 10 are calculated by a function in the source code of GEOtop. More information about the parameterisation of Ye and Pielke (1993) is added in the revised manuscript. The values of all the model parameters is provided in a table in the supplementary index material.
_Does the calculation of the turbulent fluxes include corrections for stability of the atmosphere?	Yes, the atmospherical stability in GEOtop is taken care of through a parameter called as "MoninObukhov". Its values can be as follows: If MoninObukhov = 1 stability and instability considered. Similarly, 2 = stability not considered, 3 = instability not considered, and 4 = always neutrality
_How is surface roughness calculated/estimated?	In GEOtop, the surface roughness is given to the model as a parameter. In this paper, the value of 0.01m was used based on similar regions, for example in Tibet Plateau (Wang et al., 2018). Furthermore, a threshold is given to change roughness length to snow-covered values in soil area. For the bare soil, the value of the threshold is equal to zero. For snow, the default value of roughness length equal to 0.1 mm was used.

The authors should include a table, in the main paper or in the Supplementary Material, where they include all the values of the soil and surface properties that they use for the model simulations (surface roughness, albedo, conductivity, porosity, etc etc), and an explanation of how those properties were determined. This is important for repeatability but also to understand what modelling choices the authors have made, how sounds they are and how they affect the model output. Most of those properties and parameters are often affected by large uncertainty, which translate into uncertainty in model simulations, so their values should be provided and their uncertainty assessed (see below).	The values of all the model parameters such as atmospheric, soil, snow are provided in a table in the supplementary index material as suggested. Also, an explanation is provided about the determination of parameters.
The paper lacks a discussion of the amount of frozen soil that melts and of the corresponding melt water generated by permafrost thawing, which I guess could be calculated with such a model and would be a very useful information to get.	We respectfully disagree. While such a calculation is part of the ultimate motivation for this study, it would be premature at present. This is because the present study is concerned with improving and understanding our ability to predict the spatial differentiation of ground temperature. To calculate runoff, multi-decadal transient model runs would be needed, together with detailed information on the amount and stratigraphic distribution of ground ice.
8. Model evaluation: This section is in places redundant, and contains many repetitions. It should be – as the entire paper - reworked and streamlined. For the shortwave radiation: I first of all do not understand why the authors model the shortwave fluxes since they have observations that they can use directly. I think a very strong explanation is needed here if they want to maintain this model forcing. This is important especially because the modelled fluxes do not agree particularly well with the observed ones, see metrics in section 3.5.1 and Figure 5. This is bound to reflect in uncertainties in the simulated energy fluxes.	In the revised manuscript, all the observed radiation fluxes except outgoing longwave radiation are now used as input to the model. See the reply to Comment No 7 & 3 above.

Second, I would disagree with the authors choice of the mean Bias difference and RMSE, and would use instead the NSE, which is more appropriate for variables with a strong temporal cycle, such as runoff, melt rates or indeed shortwave radiation components.	As suggested, the Nash-Sutcliffe Efficiency (NSE) is added for model evaluation in the revised manuscript.
The equations of those metrics are not needed, as these are all basic, well known metrics. If they want to include them, I would suggest the authors place them in the SI.	The equations of the evaluation metrics are added in the supplementary index material.
In general, I feel that a clear rationale for the use of those many metrics is not clear and should be provided. I do not understand for instance why the authors use distinct sets of metrics for shortwave radiation and ground temperature, which both have a strong sub-diurnal cycle.	The rationale behind the use of different metrics for radiation and ground temperature is because Expressing MBD and RMSD as per cent makes no sense for temperature because the 0 point of the Celsius scale is arbitrary (in contrast to Kelvin).
9. Partition of fluxes: I do not understand how the authors can write that a given amount % of the net radiation was converted into specific percentage of turbulent fluxes: e.g. "The partitioning of energy balance components during the study period show that 47% of Rn was converted into H, 44% into LE, 1% into G and 7% for melting of seasonal snow", in abstract, line 22-24 and throughout the paper. LE in particular can be both positive and negative, as the authors also show (Table XX). How do the authors quantify percentage fluxes if they have both positive and negative fluxes at any given time? They refer to Zhang et al to calculate the fluxes – but not – I think for the partition of what amount of which flux goes where. They should provide a clear explanation here so that the reader can understand what the values they provide are.	Yes, the method used in Zhang et al. (2013) was used to calculate the proportional contribution of each flux. We thank the reviewer for pointing out the mistake. The correction is made in the revised manuscript. To quantify the percentage of fluxes, we calculated the mean annual average of each of the individual surface energy balance components (LE, H and G) and then divided these individual averages with the mean annual average of net radiation (Rn). For example: Percentage of Rn converted into LE: LE/Rn*100 The same procedure is adopted by Liu et al. (2019) (Table 1) to calculate the partition ratios.
10. Uncertainty analysis: One of my main objections to this study is that there is no estimation of uncertainty on the model simulations. I feel that model outputs without an associated uncertainty are no longer acceptable, and I would	Uncertainty analysis of the model using PEST tool is undertaken

strongly encourage the authors to do a thorough uncertainty analysis using e.g. a Monte Carlo type of approach, by varying both the meteorological forcing and soil and snow parameters.	
11. Figures: Figure 3 and 4 should be combined, or presented differently. In its current form, the authors show first the observed radiative fluxes and then the simulated ones – they should be the same or very similar. Indeed, this relates also to one of my objections regarding the forcing of the model: why is the model not forced with the observations of radiative fluxes, given that this is a point-scale application?	In the revised manuscript, all the observed radiation (incoming and outgoing shortwave radiation, incoming longwave radiation) fluxes except outgoing longwave radiation are now used as forcings to the model.
The figures showing fluxes over one day, and comparing several days, have little information content. The authors should calculate and plot sub-daily values of fluxes for sub-periods of similar meteorological conditions – if this is their aim – or of similar snow conditions, as one day is really too isolated an example to be significant and representative of a pattern or characteristic. Figure 8: It is not very informative to present those values for two separate days. I suggest the authors calculate averages for periods of similar conditions.	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.
12. Comparison with other studies: This section makes little sense to me. The authors include a comparison also with EB calculations on glaciers, which does not bring, I feel, many insights to the (very limited) discussion of this paper as glacier surface conditions are very distinct from those that the authors consider at this AWS location.	At line795, we have already mentioned about the lack of studies with data in the manuscript as: "Although aiming to represent differing permafrost environments, this comparison also includes SEB studies on glaciers for lack of other data."
The selections of the sites to include seems arbitrary, and misses numerous EB studies across the world (Wagnon et al., 2009; Pellicciotti et al., 2008; Ayala et al., 2016 Andes; Yang et al 2011, Yang et al 2017, Ding et al 2017, Mölg et al 2012, Mölg et al 2014, Zhang et al 2013 for HMA, and many more for other regions of the world).	As suggested, more energy balance studies have been added in the revised manuscript.

Also, if this wants to be inclusive: why not including studies of EB and melt regimes over debris covered glaciers, then, which are also abundant (to mention only very few and recent ones: Reid and Brock, 2010, Steiner et al., 2019; Stiglietz et al., 2020) and might be more relevant to permafrost studies than clean ice glaciers?	As suggested, more recent energy balance studies have been added in the revised manuscript.
Astonishingly, the authors in their comparison do not consider the elevation of the stations they compare, which plays a key role in determining the amount and sign of fluxes.	The elevation of stations is already taken into consideration and is available in Table 5.
I would suggest the authors either considerably strengthen this discussion with better argument and a comparison that takes into account at least the differences in elevation, or remove it.	The discussion section is now presented in a much better way in the revised manuscript.
Some of the statements provide are obvious and do not add anything to the authors discussion: such as that the albedo of locations with soil or tundra is lower than that of the AWSs on ice (lines 809-811: The mean α for all the sites where radiation balance is measured either on bedrock or tundra vegetation was smaller than those measured over firn or ice during summer"). The authors also do not need to provide those albedo values.	The statements mentioned in the comment are removed and are presented in a much better way in the revised manuscript.
13. Conclusions and main findings: This is a mostly descriptive paper, that uses a very complex models but ends up describing mostly the surface energy balance, with very little consideration of the role that permafrost plays in the surface and mass budget.	In the revised manuscript, more details about the role of permafrost and its influence on the energy balance are provided.
It is very descriptive, and looks more like a report than a scientific paper and I think it would benefit from some more in-depth and perspective. Figures are of poor quality in general, and poorly designed/selected. They often represent times series with little effort of synthesis.	Thanks to the reviewer comments, the revised manuscript is restructured and presented in a much better way.

There is a long introduction about permafrost and its importance, but the rest of the paper seems disconnected from this focus, and fluxes are not analysed in the context of permafrost characteristics, duration, thawing.	In the revised manuscript, the main focus is given to the energy balance from a permafrost environment.
The lack of findings and descriptive nature of this paper is reflected in the fact that indeed the Discussion contains mostly material that should belong to the results. The actual Discussion could definitely be improved.	The material described in the discussion of the earlier version of the manuscript is moved to the results section. The discussion in the revised manuscript is modified and improved.

DETAILED COMMENTS

Reviewer comments	Author response
_Line 47: the authors need to provide one or preferably more references for this statement.	More references have been added to the sentences mentioned in the revised manuscript.
_Line 124: what are "strong land-atmosphere interactions"? This is vague and misleading. The authors should reformulate this.	I can't find this statement in the manuscript?? This line is not present in the online version of the manuscript. And we think a much earlier version of the manuscript is sent to the reviewers.
_Table 1 Data platform: I guess the authors here refer to the datalogger?	The word Data platform in Table 1 is replaced with the data logger.
_lines 131 to 140: can be removed, or at least substantially shortened or moved to SI.	The line numbers between 131 to 140 have been removed in the revised manuscript.
_Line 159-160: remove from there. He authors can put this info in the Acknowledgments if they want.	Moved to the acknowledgements.
_line 234: strange language, and unclear ("But in Geotop (endrizzi et al., 2014) the equations are described separately"), which should be reformulated. What does it mean and does it bear any relevance for this paper? Do the authors modified some of the formulations in the mode?	In the revised manuscript, the sentence mentioned in the manuscript is reformulated for better clarity.
_Table 4: I would provide the incoming and reflected, incoming and outgoing fluxes separately for the shortwave and longwave radiative fluxes separately.	In Table 4, the incoming and outgoing fluxes are given separately for the shortwave and longwave radiations.

_section 4.1: this entire section belongs to Results.	Section 4.1 is moved to the results section.
_Lines 695-697: There is no proof here that they are credible. This is a circular argument.	This sentence is reformulated in the revised manuscript.
_Line 772: (d) high latent heat due to snowmelt that is a heat sink: not clear what the authors man here.	The heat capacities of the mineral or organic soil material, water, and ice, is relatively small by comparison with the quantity of latent heat of fusion. For example: To warm 1 g of ice to 1℃ involves the addition of 2.1 J, however, the 334 J g^{-1} of energy must be added to melt it. Therefore, snowmelt is an energy sink because of the latent heat of fusion (Zhang, 2005).

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