Review of manuscript:

"Towards a conceptual model of water routing for a debris-covered glacier"

by Wani and co-authors

submitted to The Cryosphere

This paper presents simulations of energy fluxes at the location of one automatic Weather Station at 4727m a.s.l. in the upper Ganglass catchment in the Ladakh region of India. Energy fluxes are calculated over a period of two years, from 1 Sept 2015 to 31 Aug 2017, using the GeoTop model. The model performance is evaluated against observations of radiative fluxes, snow depth and near surface ground temperature measured at 10cm from the surface. Then the authors analyse the energy fluxes obtained with the model and describe their temporal variability. They finally compare the average values obtained for each flux with values from the literature computed at other locations in the world, from the Tibetan Plateau to the Andes, irrespective of the sites' elevation, the type of surface (ground, glacier surface, etc) as well as the models used for those simulations.

The paper is interesting in that it presents energy fluxes at a remote location in a distinct climatic region of High Mountain Asia (HMA), dominated by very dry conditions and where permafrost has been identified as a dominant feature. The paper however is short, in its current form, of the quality needed for publication, both in terms of structure and language and in terms of content. I have several comments that the authors should address before the paper can be published. I was struck in particular by the lack of clear findings and a discussion of those beyond a simple description of the fluxes the authors obtain. In its current form the paper reads more like a report than a scientific paper. There is also a compelling lack of uncertainty analysis, which I would strongly encourage the authors to carry out (see one of my major comments below). Equally, the measurements are presented without any error or uncertainty assessment. I also have several comments on some of the methods used.

The text contains many repetitions, it is redundant at times and on the other sides lacks key information (e.g. the values of the critical surface and soil properties used to run the model). The English needs a thorough proof-reading.

In synthesis, this is an interesting paper that could represent a valuable contribution to energy balance studies, but needs very major revisions before can be accepted for publications in TC. I hope the authors will find my suggestions useful.

MAIN COMMENTS

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 abstract seems long and could be shortened and made more to the point.

There are many repetitions in the paper, e.g. lines 242-243; 344-346; 511-512; and in many other instances.

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.

2) Paper structure

I feel the paper structure needs to be improved in several places.

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.

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

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. 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.

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*".

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.

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 timeseries.

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 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).

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.

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 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.

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 Basi*n", 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.

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).

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.

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 Ladhak.

_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 authors also seem to mix together rock glaciers and permafrost. Are they using rock glaciers as a synonym for permafrost? 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.

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 differences between measured and modelled snow depth, listed in Table 1 in the SI, is very high. The authors should justify this.

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.

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.

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:

_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).

_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.

_For calculation of the latent heat flux, how is the relative humidity of the surface determined, since it was not measured it seems?

_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).

_Does the calculation of the turbulent fluxes include corrections for stability of the atmosphere?

_How is surface roughness calculated/estimated?

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 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.

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.

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.

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.

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.

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.

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 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 flkuxes, fiven that this is a point-scale application?

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.

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. 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). 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?

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.

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. 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.

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.

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.

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.

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.

DETAILED COMMENTS

_Line 47: the authors need to provide one ore preferably more references for this statement.

_Line 124: what are “strong land-atmosphere interactions”? This is vague and misleading. The authors should reformulate this.

_Table 1

Data platform: I guess the authors here refer to the datalogger?

_lines 131 to 140: can be removed, or at least substantially shortened or moved to SI.

_Line 159-160: remove from there. He authors can put this info in the Acknowledgments if they want.

_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?

_Table 4: I would provide the incoming and reflected, incoming and outgoing fluxes separately for the shortwave and longwave radiative fluxes separately.

_section 4.1: this entire section belongs to Results.

_Lines 695-697: There is no proof here that they are credible. This is a circular argument.

_Line 772: (d) high latent heat due to snowmelt that is a heat sink: not clear what the authors man here.