Review of the Article Tc-2019-286

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

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The paper presents the results of a monitoring and modeling study to understand the surface energy budget in relation to permafrost formation in a little studied environment, as the high elevation dry Himalayan inner range.

General comments:

This is a valuable and interesting paper, which shows an accurate modelling study of the characteristics of the surface energy budget in a poorly studied region, the inner Himalaya. Moreover, in this region permafrost is widespread and permafrost processes relevant for water resources and risks management. The topic is therefore relevant and the paper suitable for TC.
The paper shows new observations and applies with good results a hydrological model, which considers explicit water and energy budget, in a cold and dry catchment.

The modelling study is solid and well done. Model validation convincing. Therefore the methodology appears to be sound (I have only one doubt related to water budget).

However, I have several major comments that, on my opinion, should be addressed before publications, regarding the paper organization and the results discussion.

- 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.
- 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).
- 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.
- The paper is interesting, but the story is simple. I have the feeling that there are repetitions and details not needed.
- 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.

Specific comments:

1. Introduction

See general comments. More specifically:

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.

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?

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.

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.

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).

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.

L 305 – „*performance statistics* ". Okay, but it might be more concise. All is well known.

3. Results

I suggest moving the paragraph "Model Evaluation" 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.

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.

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.

L 500 - 515. This is also long and boring …

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?

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. Please consider uploading this test case in the testing suite of the GEOtop model website.

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.

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.

L 730 - 745 Ok, the story is clear! Please stop repeating.

Figure 9 Sub charts E and F. Why they are informative? I do not understand …

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.

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.

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.

Minor comments:

L 74 – "Spatio" lowercase

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 2014 or Bertoldi et al 2014. For ground temperatures, besides Gubler et al., 2013, you cold 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.

L 322 – the model was initialized at a uniform **soil** temperature

References

Herrero, J., Polo, M. (2012). **Parameterization of atmospheric longwave emissivity in a mountainous site for all sky conditions** Hydrology and Earth System Sciences 16(9), 3139-3147. https://dx.doi.org/10.5194/hess-16-3139-2012

Herrero, J., Polo, M., Moñino, A., Losada, M. (2009). **An energy balance snowmelt model in a Mediterranean site** Journal of Hydrology 371(1-4), 98-107. https://dx.doi.org/10.1016/j.jhydrol.2009.03.021

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