Review report for the manuscript-
Robledano et al., "Modelling surface temperature and radiation budget of snow-covered complex terrain"

The authors claim to estimate the LST and the the energy budget of snow-covered complex terrains, in order to evaluate the significance of the different processes in influencing the spatial variations of the LST. The strategic analysis is interesting and significant for the scientific community. However, some issues remain to be discussed and some revisions are required before the manuscript could be accepted for publication. My specific comments are as follows.

1. The authors claim to estimate the energy budget of the snow-covered complex terrains. However, this is not discussed in the results.
2. Why the double channel method (spit window) is not examined by the authors?
3. The discussion in the results is mostly qualitative. The analysis lacks statistical depth. The discussions include at places standard deviation, mean difference etc., but not popular metrics such as the correlation coefficient and the RMSE. For example in Figure 8, 9 and 10.
4. The authors mentioned and illustrated the effects of the topography on the estimated LST. However, they did not consider any literature on orographic corrections. For example as follows which is replicable for LST in a similar manner.

   Bento, V.A.; DaCamara, C.C.; Trigo, I.F.; Martins, J.P.A.; Duguay-Tetzlaff, A. Improving Land Surface Temperature Retrievals over Mountainous Regions. Remote Sens. 2017, 9, 38. https://doi.org/10.3390/rs9010038

   Varade, D., & Dikshit, O. (2019). Improved assessment of atmospheric water vapor content in the Himalayan regions around the Kullu Valley in India using Landsat-8 data. Water Resources Research, 55, 462– 475. https://doi.org/10.1029/2018WR023806

5. The authors mentioned the limitations of the NDVI thresholds method for the estimation of emissivity. The authors may explore the following alternative,

   Divyesh Varade & Onkar Dikshit (2020) Assessment of winter season land surface temperature in the Himalayan regions around the Kullu area in India using landsat-8 data, Geocarto International, 35:6, 641-662, DOI: 10.1080/10106049.2018.1520928

   Further, the authors missed the influence of the vegetation or the forest cover in their analysis, which is significant on the LST and the atmospheric water vapor content.

6. Since, the comparison is made against the Landsat-8 derived LST, it is imperative that the used reference product is at the most best quality. I would recommend the authors to calibrate this product from a series of ground station data if available.

7. Comments regarding the write-up.
    a) The language of the manuscript is extremely poor. It is difficult to understand because of the poor language used. The following checks are required by the authors
        i. Missing punctuations. Example- Line 1,5 in abstract.
        ii. Grammatical mistakes, usage of incorrect articles.
        iii. Usage of appropriate words. For example, Line 28, “Terrain tilt”, I believe should be “Terrain orientation” or “Terrain slope” . The sentence is very difficult to understand and there are several such sentences in the manuscript.
        Another example, Line 214 it should be “quadratic”. And so on.
    b) Figure 3, instead of showing the chart in the left image, the authors can show the slopes and their directions using directional gradient filters applied on the DEM.
    c) Abbreviations/symbols needs to be defined in Figures, For example in Figure 1 and 2. In some cases, the definitions of these come after several paragraphs or in other sections.
    d) The discussion of the some of the Figures and corresponding results is not sufficient. For example, in Figure 8, bulk of the points are between σ of ~1-3 ^{o}C, Hoverever, some outliers are also observed. These are not discussed in the manuscript. Any particular reasons for this.
    e) Figure 7, it would be interesting to see how a downscaled Landsat-8 LST would fare against the results from the proposed methodology.