Answer to Anonymous Referee #1

Referee comment on **tc-2021-180** : *Modelling surface temperature and radiation budget of snow-covered complex terrain* by Alvaro Robledano et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-180-RC1, 2021.

The reviewer's initial comments are written in black, and our answers are written in blue. The modifications and corrections in the paper are reported in *red* (the unchanged parts of the text are in *blue*). The line numbers, section numbers and figures correspond to those of the original manuscript.

The authors claim to estimate the LST and 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.

The authors would like to thank Anonymous Referee #1 for the general analysis of the manuscript, as well as for the useful comments and suggestions, which we have taken into account. We wanted to note that a few changes in the modelling have been done based on the review by Anonymous Referee #2 and therefore almost all figures have been updated. Several parts of the manuscript have also been modified and rewritten. We show the new figures (and captions) at the end of the document, if not mentioned before. For specific details about these changes, we kindly refer the reviewer to author response AC2.

1. The authors claim to estimate the energy budget of the snow-covered complex terrains. However, this is not discussed in the results.

In this manuscript the focus is put on the snow surface temperature of complex terrains, and to reach it we need indeed to estimate the surface energy budget. The introduction was probably not very clear about this, so we have partly restructured it in order to highlight the importance of surface temperature.

Nevertheless, in Section 3.2.2 we have evaluated a diurnal cycle of all the simulated terms of the surface energy budget, in addition to surface temperature. This evaluation has been discussed in Section 4.2 (lines ~ 420 – 440), showing several strengths and weaknesses of the assumptions that have been made.

2. Why the double channel method (split window) is not examined by the authors?

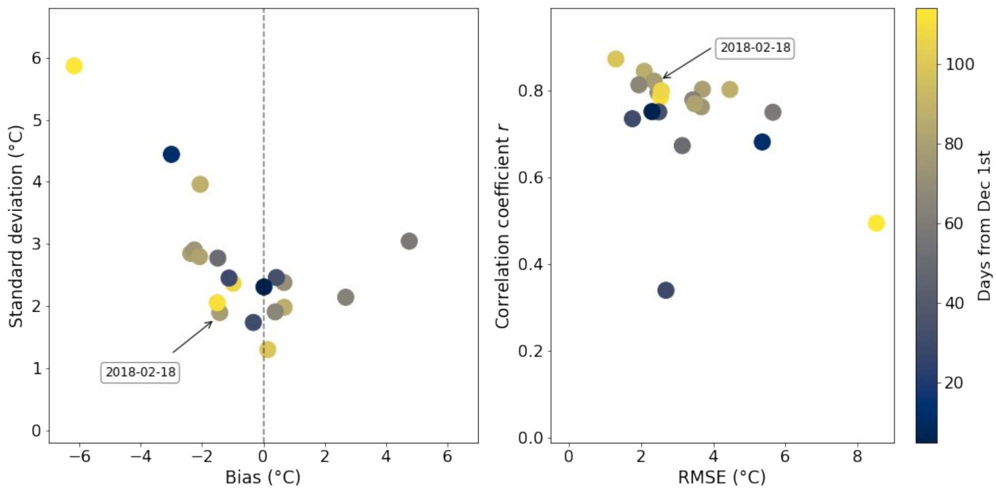
Since the study goal is the modelling of the impact of topography on surface temperature, it is not a priority to evaluate it with different remote sensing algorithms. For this reason, we selected what seemed to be the most reliable approach at the time of writing to provide the spatial variations of surface temperature.

As explained in Section 2.4 (line 265), stray light was observed on Landsat-8 thermal acquisitions (Montanaro et al., 2014), affecting Band 11. Even though several corrections have been applied afterwards (Gerace and Montanaro, 2017), we applied a method to retrieve surface temperature based on only one band, as suggested by other authors (e.g. Cristobal et al., 2018; He et al., 2019).

Indeed, the new Landsat Collection 2 Surface Temperature product relies on a single-channel approach, which supports our choice to rely on such an algorithm.

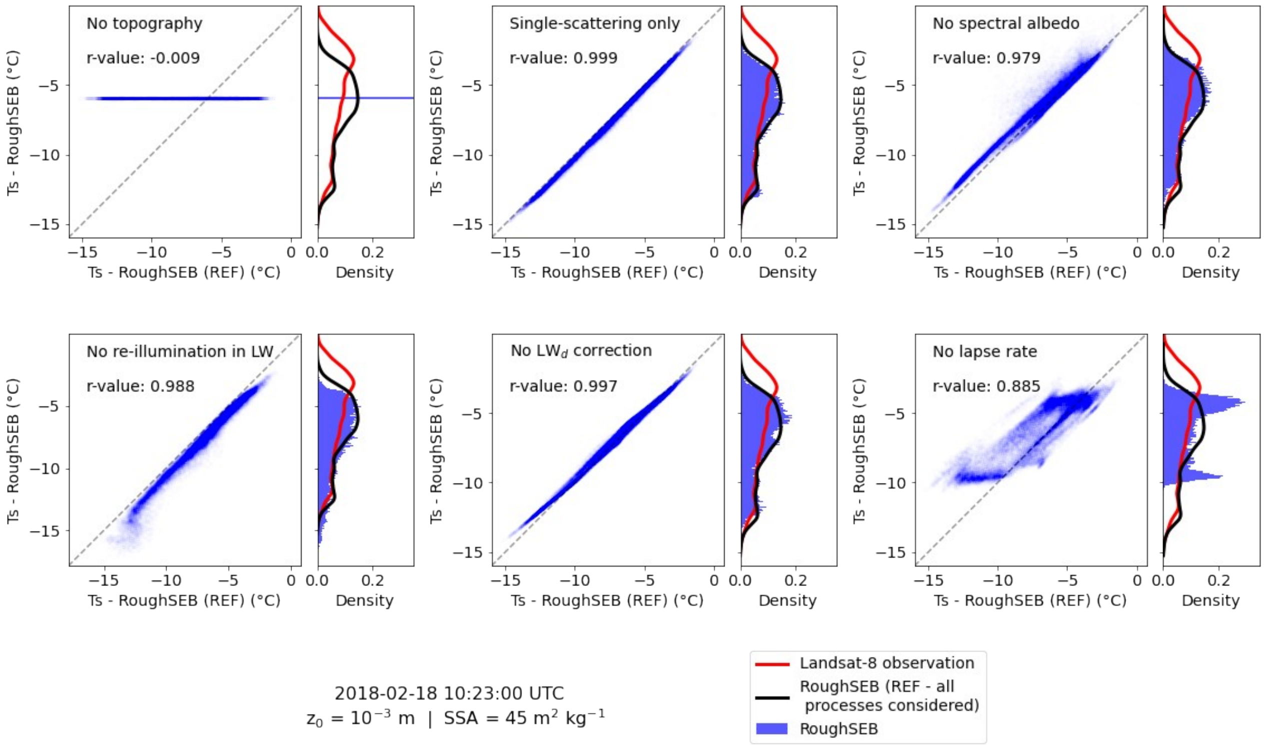
3. The discussion in the results is mostly qualitative. The analysis lacks statistical depth. The discussions include at places standard deviation, mean difference, etc., but not popular metrics such as the correlation coefficient and the RMSE. For example in Figure 8, 9 and 10.

We have updated Figures 8 and 9. The new Figure 8 includes a second panel with the RMSE and the correlation coefficient, that allows a more extensive discussion of the outliers (see comment 7d)



New *Figure 8: Comparison of the spatial variations of surface temperature between the simulations and the satellite observations for each date, computed considering the whole domain. On the left, mean bias and standard deviation of the differences. On the right, the RMSE and the correlation coefficient r.*

The new Figure 9 is changed. We compare now in the scatterplots the differences between each simulation and the reference simulation to better constrain the role of each topographic effect. We have added the correlation coefficient as suggested. By doing this, the evaluation of spatial variations with remote sensing observations is concentrated to Section 3.2.3, avoiding mixing the analysis of the topographic effects and the evaluation of the model. We have restructured the revised version of the manuscript to take this into account.



New *Figure 9: Impact of disabling a topographic effect on the simulated Ts on 18 February 2018. Every single panel corresponds to a disabled topographic effect, with respect to the reference simulation (REF) where all the effects are included. The marginal histograms show the distribution of surface temperature for each simulation as well as the observed Ts by the satellite (red) and the reference simulation (black).*

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 et al., 2017: https://doi.org/10.3390/rs9010038

Varade and Dikshit, 2019: https://doi.org/10.1029/2018WR023806

The study is focused on modelling. While we use LST retrieved from Landsat-8 to compare with the spatial variations predicted by the model, improving LST retrieval algorithms or comparing different satellite products is out of the scope of our study (and expertise). It would not benefit the main goal of this work. We clarify this aspect in the revised version (e.g. change in Figure 9). However, we acknowledge that the remote sensing topic is related and likely of interest for the readers interested by this study. For this we propose to add the reference suggested by the reviewer in the comment 5 in the Section 4.1 (line 409), as follows:

A possible future improvement would be to include a land mask to set a particular emissivity value for each pixel depending on the presence of snow, rocks, grass, etc. This is normally achieved by means of NDVI-based classifications (Li et al., 2013), that can be adapted to snow-covered complex terrains with methods that rely on the snow cover area (Varade and Dikshit, 2020).

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

Varade and Dikshit, 2020: DOI: 10.1080/10106049.2018.1520928

This alternative has been added as suggested (see above).

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.

The influence of vegetation is neglected here as its presence is very limited in the study area. The following image is a screenshot of a 360-degree webcam operating at the Col du Lautaret mountain pass, corresponding to the 18th February 2018 (one of the selected Landsat-8 acquisition dates):

For clarity, we have added the following (line 239):

The predominant orientation is S-SW, followed by N-NE facing terrain, and the slope varies mainly between 15° and 40°. Protruding vegetation is rare in the study area in winter and is neglected here. We assume that the snow cover is 100%.

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.

This study uses Landsat-8 to compare the spatial variations of LST predicted by the model, but does not rely on the absolute accuracy of the product. The bias identified at the single meteorological station available in our domain could be used to apply an offset to the satellite product but there would be no gain for the spatial variations. For the sake of simplicity and to keep the manuscript focused on the modelling aspect, we prefer to keep the remote sensing part as simple as possible.

In more details:
The Landsat Collection 2 Surface Temperature product was not available when we first started this work. However, when it was made available, we assessed its accuracy at the in-situ station and compared it with the already implemented retrieval method. Figure 5a shows that the reference

product is less accurate at this particular point, with a bias of -3.5°C (RMSE: 4.0°C). The applied single-channel algorithm seems to be more accurate at this place, with a bias of -1.3°C (RMSE: 2.0°C). On the other hand, as stated in Section 4.1 (line 413), the differences across the study area between the applied method and the official product are of 0.3 °C (median of standard deviations). Considering that both products are virtually equivalent regarding the spatial variations (i.e. the main goal of the study) and the better accuracy at the measurement station, we decided to keep the single-channel algorithm proposed by Cristobal et al. (2018) as our reference product.

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.

The manuscript will be revised beforehand by an English native speaker.
Line 28: "*terrain tilt*" has been modified in the text by *"terrain slope and orientation"*.

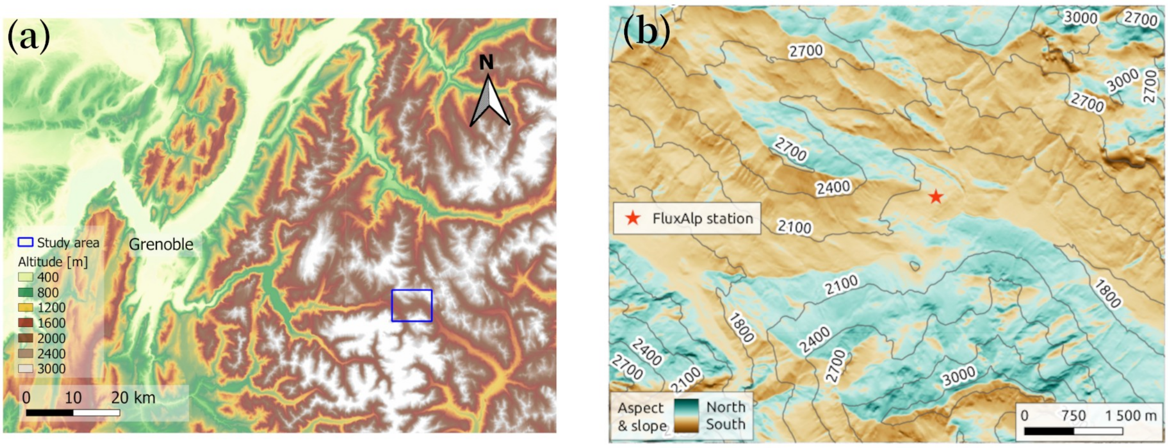
Line 214: the general form of a quartic equation is:
$ax^4 + bx^3 + cx^2 + dx + e = 0$

while the general form of a quadratic equation is:
$ax^2 + bx + c = 0$

So in our case, **quartic** equation is the appropriate word, there is no error.

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.

The Figure has been updated as suggested.



New Figure 3: *Location of the study area, around the Col du Lautaret alpine site. The blue rectangle in (a) represents the extension of the study area, shown in (b). The domain is generated from the RGE ALTI®Version 2.0 Digital Elevation Model (DEM) provided by IGN France at a spatial resolution of 5m, and resampled to 10m for this study. Slope angle is represented by the intensity of the color.*

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.

Figures and captions have been updated following the comments of both reviewers. We have removed part of the text in Figure 1 to make it more clear:

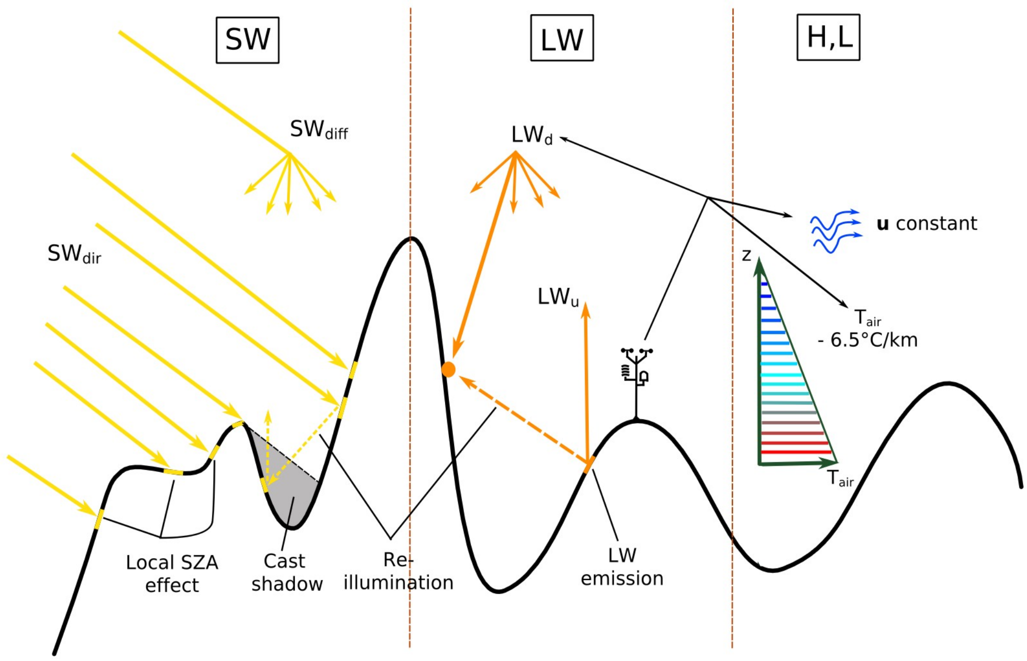


Figure 2 has been updated by enlarging the font size and modifying the caption:

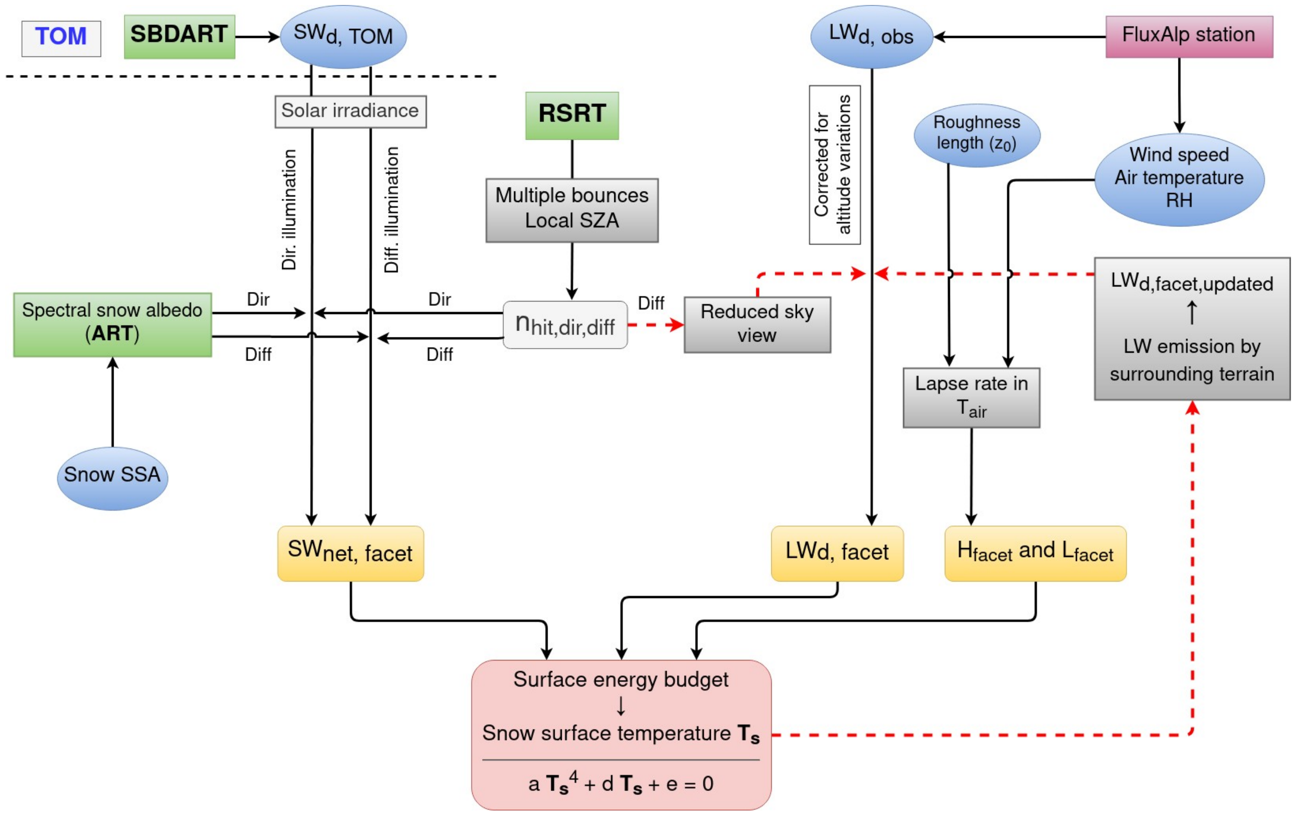


Figure 2: Flowchart of the modelling chain to estimate snow surface temperature. TOM (top of mountains) is the horizontal above the highest point in the study area. The involved models are in green, the terms of the surface energy budget are in orange, the needed inputs are in blue and the topographic effects are in grey. The red dashes lines indicate the two-step iterative process to compute the downward LW flux.

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.

Figure 8 has been updated following the major revisions suggested by Anonymous Reviewer #2 and the previous comments of the reviewer. Some outliers are indeed observed, and a plausible explanation is that in the modelling chain we consider a totally snow-covered area. However this is potentially not the case for several acquisition dates, in particular in early winter and early spring. We have modified the discussion of the outliers (~ line 340) as follows:

The simulations are slightly colder in general, with a bias principally between -3°C and 1°C. The standard deviation of the differences varies mostly between 1 and 3°C (2 and 4°C for the RMSE). Some outliers are observed, and in particular the simulation that shows the highest differences (both standard deviation and RMSE) correspond to an acquisition from late March. Such differences could be explained with an early onset of snowmelt (snow patches in the lowest areas) due to mild temperatures, a particular situation that breaks the assumptions in the model (e.g.

100% snow cover). The shallow snowpack in early winter (probable patches of bare soil) can lead to a similar situation, where the bare soil temperature would be certainly different than that of snow-covered terrain. This could explain the lowest correlation value of the dataset which corresponds to an acquisition from early December.

e) Figure 7, it would be interesting to see how a downscaled Landsat-8 LST would fare against the results from the proposed methodology.

We kindly refer to our answers to comments 2, 4 and 6.

References:

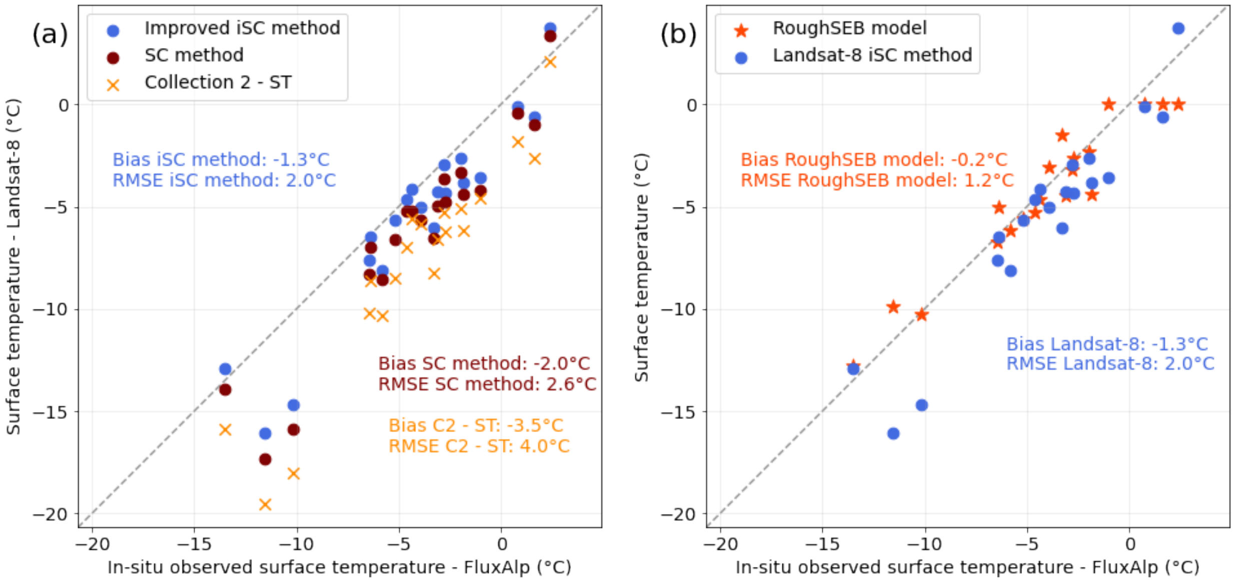
Cristóbal, J., Jiménez-Muñoz, J., Prakash, A., Mattar, C., Skoković, D., and Sobrino, J.: An Improved Single-Channel Method to Retrieve Land Surface Temperature from the Landsat-8 Thermal Band, Remote Sensing, 10, 431, https://doi.org/10.3390/rs10030431, 2018.

Gerace, A., and M. Montanaro. 2017.: Derivation and Validation of the Stray Light Correction Algorithm for the Thermal Infrared Sensor Onboard Landsat 8, Remote Sensing of Environment, 191 (Supplement C), 246–257. https://doi.org/10.1016/j.rse.2017.01.029, 2017.

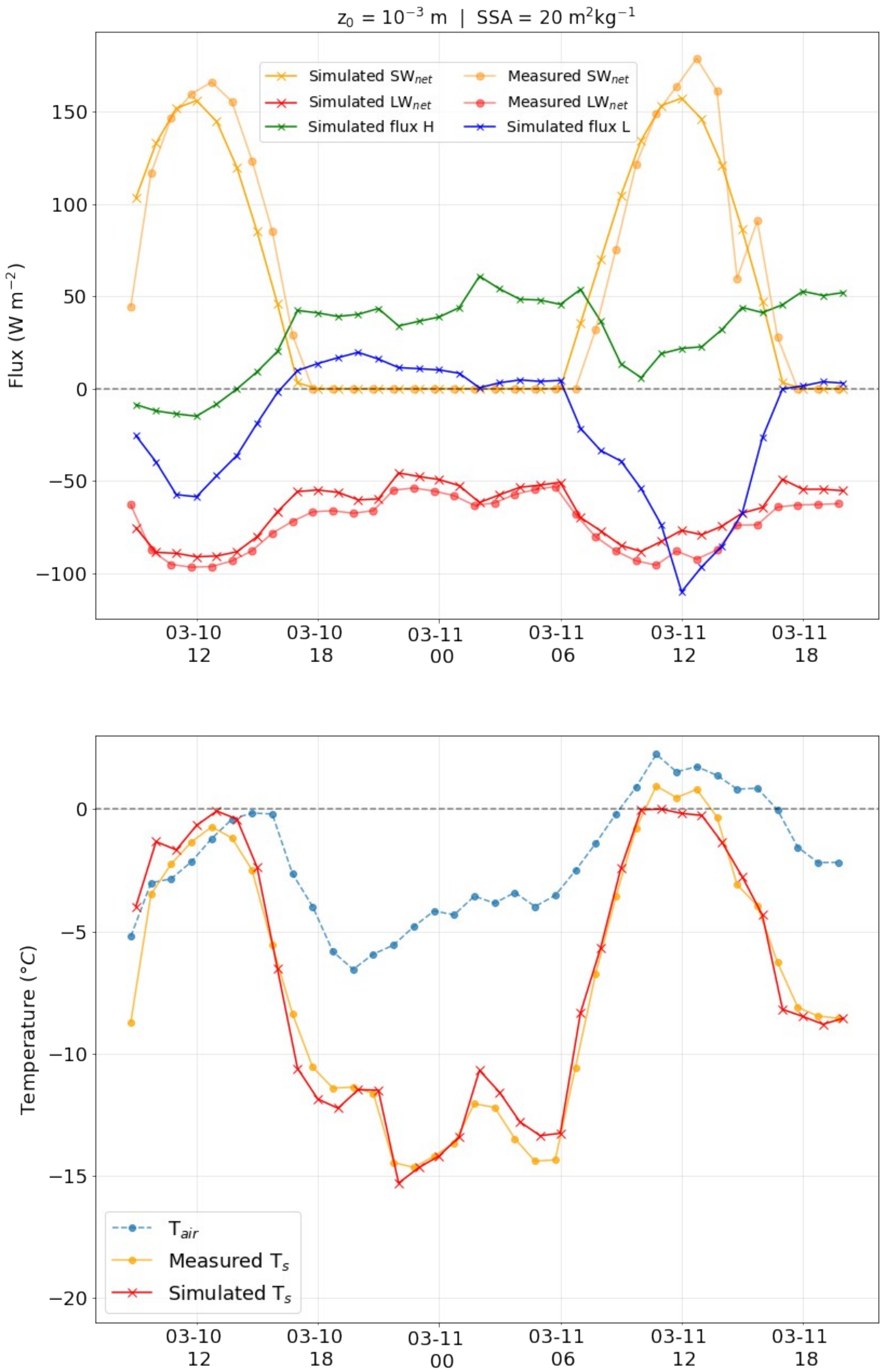
He, J., Zhao, W., Li, A., Wen, F., & Yu, D.: The impact of the terrain effect on land surface temperature variation based on Landsat-8 observations in mountainous areas. International Journal of Remote Sensing, *40*(5–6), 1808–1827,https://doi.org/10.1080/01431161.2018.1466082, 2019.

Varade, D. & Dikshit, O.: 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, https://doi.org/10.1080/10106049.2018.1520928, 2020.

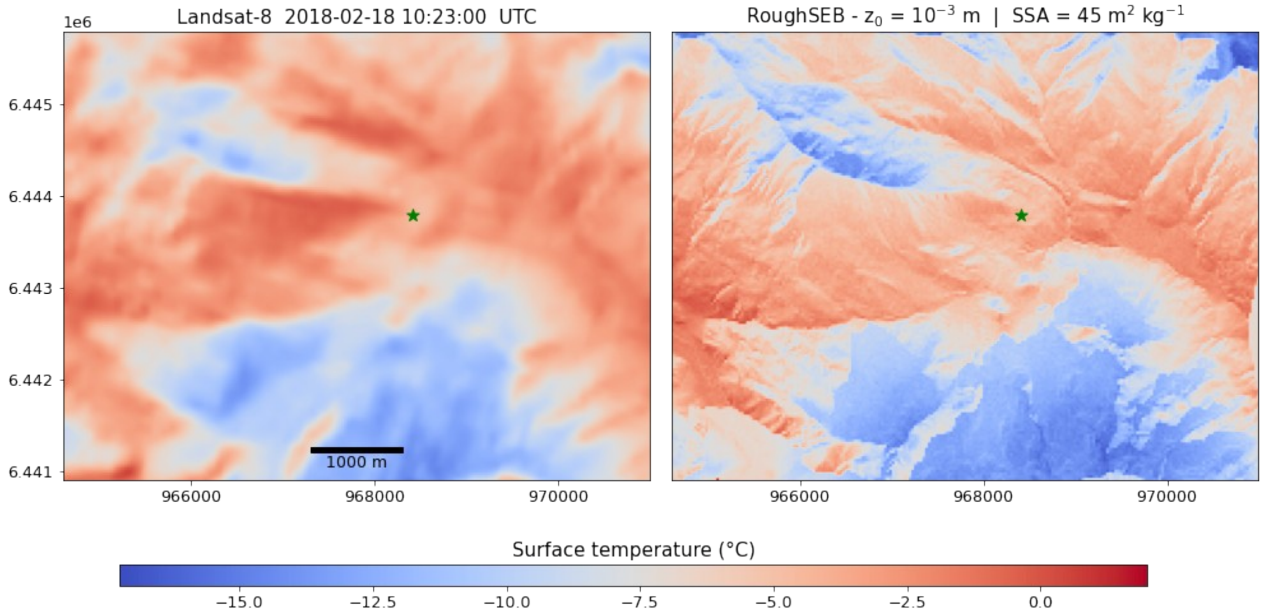
Extra figures:


(a)
Improved iSC method
SC method
Collection 2 - ST
Bias iSC method: -1.3°C
RMSE iSC method: 2.0°C
Bias SC method: -2.0°C
RMSE SC method: 2.6°C
Bias C2 - ST: -3.5°C
RMSE C2 - ST: 4.0°C
Surface temperature - Landsat-8 (°C)
In-situ observed surface temperature - FluxAlp (°C)
(b)
RoughSEB model
Landsat-8 iSC method
Bias RoughSEB model: -0.2°C
RMSE RoughSEB model: 1.2°C
Bias Landsat-8: -1.3°C
RMSE Landsat-8: 2.0°C
Surface temperature (°C)
In-situ observed surface temperature - FluxAlp (°C)

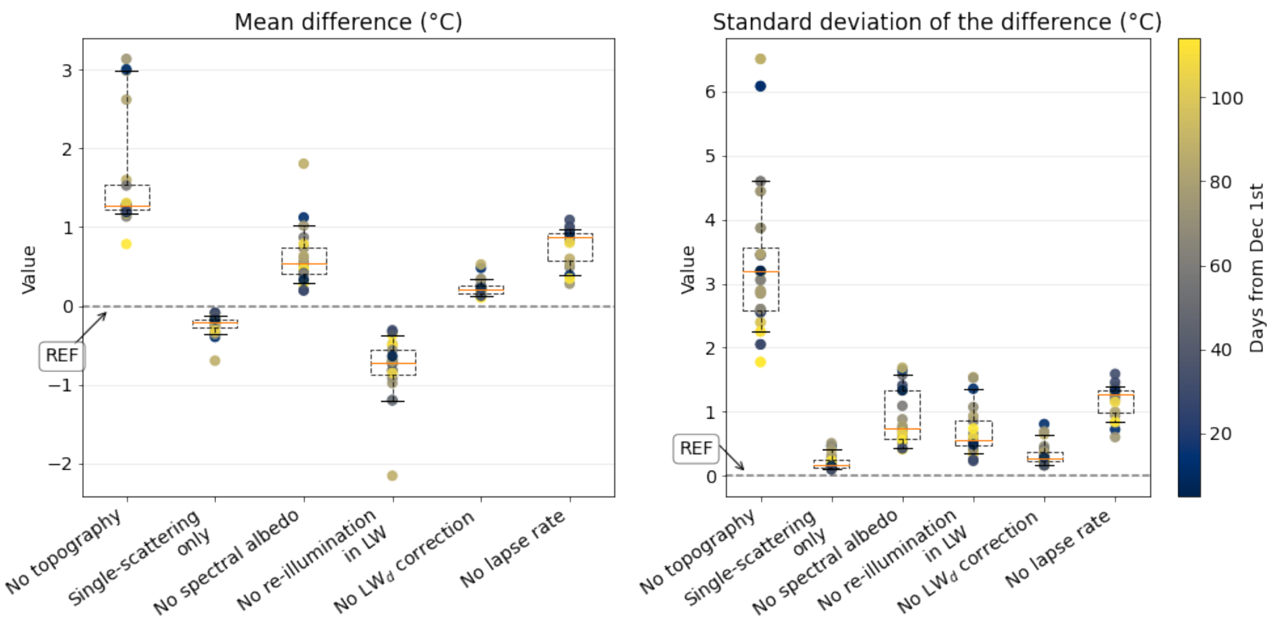
New Figure 5



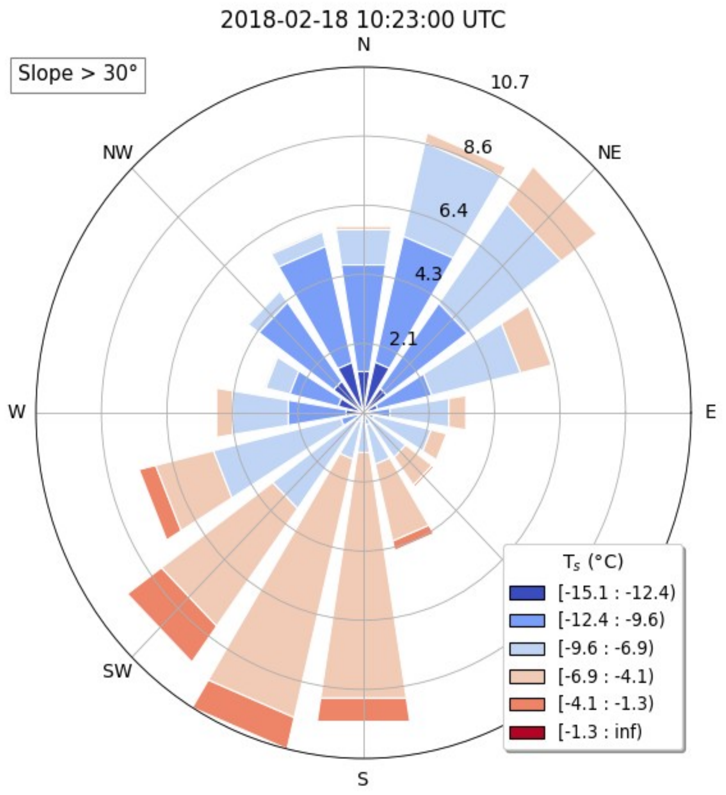
New Figure 6: *Simulation of the surface fluxes (top) and snow surface temperature (bottom) at the FluxAlp station for a ~36h long time series starting 10 March 2016. The radiative fluxes are compared to in situ measurements. All times are in UTC.*



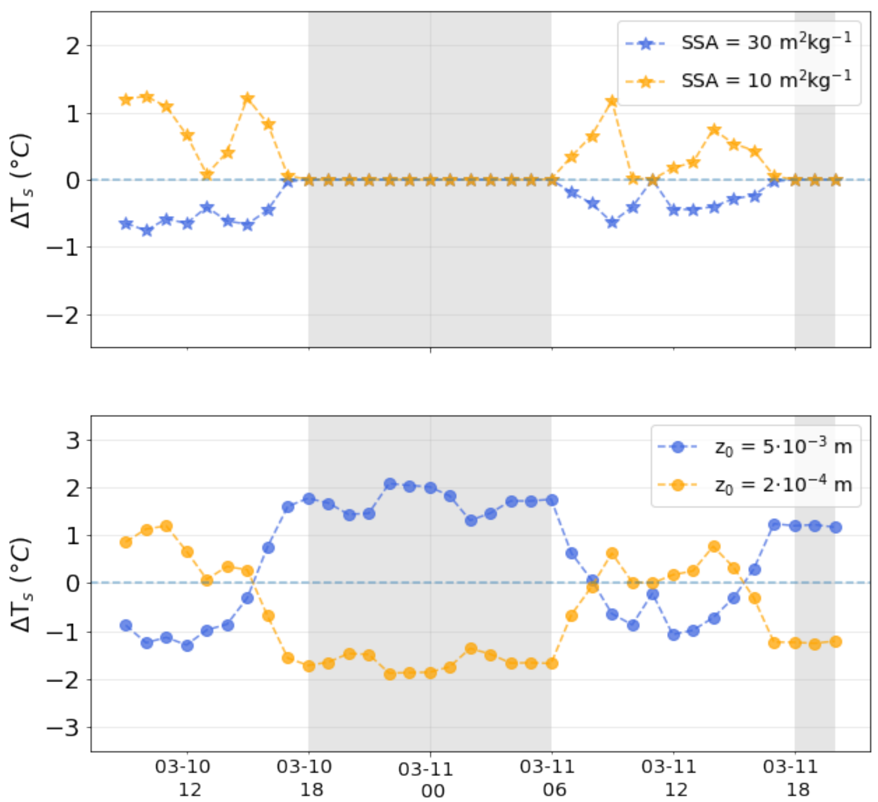
New Figure 7



New Figure 10



New Figure 11



New Figure 12