Review of Robledano et al. “Modelling surface temperature and radiation budget of snow-covered complex terrain”

The topic of the manuscript is very interesting and challenging. The authors claim that their developed modelling procedure, which involves several steps and the use of different schemes, enables the calculation of the surface temperature and surface energy budget over snow-covered mountain areas at high spatial resolution (10m). Although some of the results seem encouraging, the method is not clearly described, and there are macroscopic shortcomings in the modelling, the biggest ones being that the applied downward longwave radiation is not corrected for variations in altitude, and that the equation to extract the surface temperature from the surface energy balance equation is totally obscure and seems rather arbitrary (looks like that the Ts dependency on air specific humidity and shortwave flux are ignored?). Also, the solar infrared flux for wavelengths longer than 2000 nm is neglected, without explaining neither the reason for the neglection nor the implications of this neglection in the results (actually, this is the case also for other applied approximations). Finally, the model seems applicable only for clear-sky conditions, but this is not discussed. I feel that, given the large number of shortcomings, the results are not very meaningful, and they are probably mostly driven by the dominant role of the applied high-resolution digital elevation data.

In addition to these methodology deficiencies (and more of them are described in the detailed comments below), the paper is poorly written and organized, in some parts it is difficult to read and impossible to understand. A newer version of the paper will require a thorough proof-reading. I believe that the work is still too immature for publication. Here below are more detailed comments.

Detailed comments.

Introduction: it is currently a review of previous publications on the topic more than an introduction to the addressed issues. It should be synthesized, with focus on the issues that are addressed in the paper and on the gaps that the presented work will fill.

line 30-34: “Nevertheless, even if the literature for the smaller scales– that of the ripples, dunes, sastrugi and penitents – is usually distinct and scarcer, the principles equally apply to all the scales because the radiative transfers between faces are invariant by scale change” This is an example of tortuous sentence that need to be rephrased.

line 39: “…of the solar irradiance” It should be “of the direct solar irradiance”.

line 51-52:” Arnold et al. (2006) also pointed out the role of the anisotropic reflectance of snow and ice, i.e. the fact that albedo is higher at higher solar zenith angles (Warren and Wiscombe, 1980)”. This is a wrong explanation for the albedo dependence on the solar zenith angle. Anisotropy of snow reflectance has nothing to do with it (albedo is the integral of the directional reflectance over all azimuth angles). Albedo is higher at larger solar zenith angles because photons have larger probability of escaping to the atmosphere when they hit the snow at grazing angles. I have to say that this wrong explanation is also given in Arnold et al. (2006), who applied the correction factor of Lefebre et al (2003) to express the increase of snow albedo with increasing solar zenith angle, but wrongly attributed it to the nonisotropic reflectance properties of the snow.

line 53: “absorption enhancement is an additional effect…” You should specify that you refer to the absorption enhancement of solar radiation due to the orographic roughness. There are many other processes causing enhancement of absorbed energy…

line 61-63: “A simpler approach to account for multiple bounces is by assuming that the neighbouring faces are illuminated as if they were flat (Lenot et al., 2009; Olson et al., 2019). More importantly, the absorption enhancement is not uniform on the surface.” This is an example of unclear and puzzling sentence: how do you possible account for multiple scattering between facets if the facets are not facing toward each other? I don’t understand what you mean. Also, why the following sentence start with “More importantly”? More importantly than what?

line 76-77: “…finding deviations in surface solar fluxes on the order…” Deviations from what?

Figure 1: Please remove from the figure all the text that does not refer to the considered topographic effects and that is not referred to in the main text (name of models, energy fluxes, temperature lapse rate (text and diagram), wind speed and relative humidity).

Table 1: The title of the second column does not correspond to the content: you should replace “Spectral domain and illumination” with “Energy fluxes”. Also, what is the difference between “self shadows” and “cast shadows”? They are not described in the text. And please replace “anisotropy of reflectance” with “solar zenith angle” effect (see above).

line 114-116: “The energy budget comprises (Arya, 1988): (i) the net radiation fluxes, which are split into the contributions of the short-wave radiation from 0.3 µm to 2 µm (SWnet) and the longwave radiation from 2 µm to 100 µm (LWnet)”. I did not check the cited reference, but the correct wavelength intervals are 0.3 - 3µm for SWnet and 4-40 µm for LWnet. In fact, the downward longwave flux applied in the paper is measured by a CNR4 net radiometer, whose pyrgeometer senses the 4-42 µm wavelength window. Also, the irradiance in the 2-2.5 µm window is clearly part of the solar radiation spectrum, and not part of the thermal (longwave) radiation emitted by atmosphere and earth. By excluding the 2-2.5 µm window from the calculations of shortwave fluxes the authors significantly underestimate the surface net shortwave flux, as snow albedo is very low in this wavelength region. This is one of the major problems in this study.

Figure 2: it is too difficult to read. Please enlarge the font size and explain in the figure caption the meaning of TOM and of the terms in blue and grey.

Line 128-130: “The simulations are run in both direct and diffuse illumination conditions (noted with subscripts dir and diff), and the atmospheric effects (i.e. atmospheric attenuation) are neglected within the studied area (between the surface and TOM).” Here the text suggests that simulations are done in both clear and cloudy skies, which is clearly not the case, as all simulations are only done in clear-sky conditions and the model in developed for clear-sky conditions. This should by stated and clarified in the abstract, in the introduction, and here, when describing the modelling approach. Instead, I had to discover it only when the Landsat temperature scenes were described. The text could be improved by clarifying that the radiative transfer calculations are done separately for the direct and diffuse components of the clear-sky shortwave irradiance.

Section 2.1.1: the major problem of this section is that equations 5 and 6 are not sufficiently explained. What is the meaning of α_{diff}^{i} and of the summation term in both equations? And the explanation given for the term $n_{hit,d,f}^{(i)}$ is not clear at all. The sentence “The RSRT model can indeed compute the number of times a photon has hit a given facet regardless of the albedo (and so of the wavelength), according to the bounce order of the photon (first reflection, second reflection, ...)”

sounds odd: how can the number of scatterings of a photon on a facet be independent on the albedo of the facet? And how this is related to the derivation of $n_{hit,d,f}^{(i)}$? It is mentioned that some assumptions are made, but it is not explained what has been assumed. Finally, the explanation on how $I_{dir}$and I_{diff} are calculated is provided only in section 2.2., which in fact should be merged into 2.1.1.

Section 2.1.2: the main problem of this section is that the downward longwave flux is not corrected for variations in altitude over the 50 km^2 domain. I believe that this approximation is too crude, as it can cause an error in surface temperature of some °C when the differences in altitude are over 1000m (looking at the map, this difference seems to occur in the studied area). I recommend the authors to apply the correction, as done for instance by Arnold et al (2006). Another problem is the derivation of $LW_{u,scene-average}$: it is presented as a constant representing the average upwelling longwave flux from each facet. It is not explained how this quantity is calculated. The authors write that it is estimated according to Arnold et al (2006) so I went to read that article and found out that it is set to equal to the elevation-corrected air temperature in the surface grid mesh. Hence, it is not constant. The authors should explain in the paper how the variables are calculated, without requesting the reader to read the referred literature.

Section 2.1.4: this is the central and most problematic section. It should show how the surface energy budget is solved for Ts, but is totally unclear how equations 12, 13 and 14 are derived. It looks like that the Ts dependencies on air specific humidity and shortwave flux are ignored: is this the case? The extra equations in Appendix A3 are not of any help to understand the mathematical passages or the underlying assumptions, as they only show relationships between coefficients and not between Ts and the variables of the surface energy budgets.

Section 2.2: it should be merged to 2.1.1

Section 2.3: this section should describe the study area and the in-situ measurements, but it does not clarify which measurements were finally used. It is mentioned that meteorological and radiation data from FluxAlp station in Pre des Charmasses were used as input to the modelling chain, but which data were used from Col du Lautaret? And what is the elevation of these two stations? Automatic and manual measurements of SSA are mentioned, but it is not explained where and when they were measured (were they measured in each of the selected clear-sky days?). Since topography is the dominant feature addressed in the paper, it would be important to describe it more quantitatively: distribution of altitudes, distance between slopes, sizes of slopes. This quantitative information is also needed in the discussion, to explain the applicability of the method in other topographic environments.

line 282: “list in the appendix” should be “list in Appendix C”

Sections 3.2.1 and 3.2.2: In my opinion, validation of model simulations cannot be done with the same data used as input to the model. Hence, these two sections are meaningless and should be removed. The only aspect that could be saved is the comparison between modelled and observed shortwave radiation at FluxAlp, as in this case the simulation is independent from the observations. Actually, the comparison shows that the simulated net shortwave radiation is strongly underestimated, as expected because the simulation neglected the flux at wavelengths larger than 2000 nm (while the CNR4 pyranometers measure the radiation in the 300-3000 nm range).

Given the above considerations, I don’t further comment the discussion and conclusion sections because I think they should be entirely rewritten once the listed methodological issues are solved.