Major revisions:

(1) The new parameterization was calibrated and validated at an identical site. Although the authors separated the validation period from the calibration period, the data for the two periods are from the same site and may have similar characteristics, and thus, a more strict validation is needed. Moreover, as the parameterization does not have a robust physical basis, such a local calibration usually has a limited applicability and is not favorable. I suggest the authors testing the parameterization at different sites, in order to justify that "they may be applied to other mountainous areas with a Mediterranean climate similar to that of the study site".

We agree with the referee about the limitations that arise because of the use of only one site for calibration and validation. In fact, local applicability and, hence, scope, are the most common problems that atmospheric equations and parameterizations have to face.

However, despite this, we presented our study due to the singularities of the area, Sierra Nevada, the most southern alpine region in Europe, with semiarid and subtropical environments in the nearby surroundings. It is one of the Spanish National Parks, and it houses a global change observatory because of its particular conditions. In fact, the Refugio Poqueira station, located at 2500 m, is the first permanent scientific station in Sierra Nevada. The so far 7-yr dataset made up of continuous measurements of downward long-wave radiation in such a special and delicate environment are of great interest to researchers, and also to managers of the Park and the Observatory. We apologize for not having addressed this point, and we have thus included some comments in the revised version (lines 99-101 in the revised version).

However, we were concerned about the applicability of this study to the whole region. So, in 2009, we installed a new weather station (Contraviesa) in a nearby mountain range at 1500 m of altitude. Following this comment, which was also pointed out by Reviewer 3, we have included these data in the analysis, together with data from a new station (EN2) that has been deployed at 2500 m lately in the National Park. This station, despite its more time-limited series, can be used for validation in a similar site to Refugio Poqueira but with different sensors. Both stations are equipped with all the necessary sensors to apply and validate the parametric expressions proposed in the article. One of them is in a very similar location to RP station (EN2), while the other is at a different elevation and orientation (Contraviesa). Therefore, they are very convenient for analyzing the behaviour of the expressions at similar high mountain conditions in the National Park area, and at lower mountainous sites nearby.

Moreover, we have removed the last sentence of the article (page 3800 line 12-14 in the original version) regarding the applicability of this study to different study sites as it was simple speculation, and should have not been mentioned.

So, following this comment, we have included the validation of the proposed parametric expressions in these two new sites. This has led to changes in the abstract (lines 23-28 in the revised version), site description (lines 108-118 in the revised version), results (lines 253-256 in the revised version), discussion (lines 261-295 in the revised version), and conclusions. A new Fig. 7 with validation data at Contraviesa has been added. Original tables have been merged into one single table (Table 1) with all the statistics for every model and dataset.
The more local scope of the 3-state parametric expression versus the new modified Brutsaert equation has been emphasized throughout the article.

(2) There is a lack of inter-comparisons between the two parameterizations and other ones. Crawford and Duchon (1999), quoted by this study, presented a simple parameterization for the longwave emissivity, based on the input parameters same as in the present study. Their scheme has been evaluated as a reliable scheme in many

cases (Agricultural and Forest Meteorology 143, 49–63; Theoretical and Applied Climatology 102, 227-241), and a recent study showed it works well for high-elevation sites (Agricultural and Forest Meteorology, 150, 38-46). I suggest the authors considering a comparison of the parameterizations presented in this study with Crawford and Duchon (1999) parameterization and others.

The decision to use Brutsaert equations as the reference expression in this paper was made based on previous analysis of different expressions for atmospheric emissivity (Herrero et al., 2009), as stated in the original paper (page 3792 line 4-10). One of the conclusions of that analysis, which was in agreement with many other studies that are considered reliable references (e.g. Kustas et al., 1994; Iziomon et al., 2003; Kjaersgaard et al, 2007; Staiger and Mazarakis, 2010), is that, for clear skies, Brutsaert (1975) equation (B75 from this point on) is one of the most successful ones. The extension to all skies by Brutsaert (1982) seems also very appropriate but always limited because its N value is based on direct sky observations, which are very uncommon. That is the reason why we selected this equation as the reference point for our study.

Crawford and Duchon (1999) (CD99 from this point on) used a modified version of B75 with two changes: 1) the use of a non constant leading coefficient, varying throughout the year with a sinusoidal parameterization, and 2) the extension of clear sky emissivity to cloudy situations through the definition of a cloudy factor, which is linearly dependent on shortwave measurements. The variable leading coefficient is focused on improving the calculation of emissivity under clear skies. However, according to the references given by the referee, this expression is rejected by Kjaersgaard et al (2007) (p59: "As shown in tables 4 and 5 adjusting the model coefficients to vary with season as suggested by CD99 rendered the B75 model to underestimate long-wave irradiance under clear skies more than in its original formulation questioning the global validity of this adjustment") and directly not considered in Yang et al (2010) or Staiger and Matzarakis (2010) for clear sky situations, while B75 remains as one of the preferred performers in this sky state. Following that, we did not include the test of CD99 expression in our work, and kept Brutsaert's expression as a reference against which we could test our proposals. Nevertheless, to gain insight of these particular conditions, we have included this seasonal-variable leading coefficient in the revised version of our work, with interesting results that are presented later on in this discussion.

The parameterization under cloudy skies performed by our previous work (Herrero et al., 2009) is in fact very similar to that in CD99, which is the part most appreciated in literature. Shortwave measurements were taken as a cloudiness indicator through the use of the clearness index, a variable very similar to the "s" ratio used by CD99. We found this methodology to be a simple but really effective way to valuate cloud presence, though, however, something that could be improved. The linear dependence between CI/s and the effect of clouds on emissivity appears to be a very restrictive hypothesis, so most of the motivation of this article was founded on the intention to increase the accuracy of this parameterization without increasing the necessary data while still calculating it from screen-level measurements. Besides, CD99 stated in their paper that emissivity =1 is a limiting value "but unobserved", which obviously is not the case for instantaneous measurements on our site, and probably on high mountain ranges either, where clouds can virtually be in contact with the surface.

The referee cited a recent and interesting study developed in a high-elevation site. But it is difficult to establish a comparison between our results and those in Yang et al (2010) because, even though both works refer to high altitude areas, in the case of Tibet this is a plateau, while Sierra Nevada is an isolated mountain range highly exposed to interaction between the clouds and the terrain. And this interaction may be the key element in the results we are presenting in this paper. Another aspect, even more important, is that Yang's work deals with monthly means while we are talking about instantaneous data, where maximum and minimum values extend over a more extreme interval.

Thus, after these considerations, we followed Reviewer 2's suggestion and tested CD99 equation against the data from Refugio Poqueira (RP station) and compared its performance with the two expressions presented in this paper: the 3-state complete parametric expression and our modified Brutsaert (1982) equation. We have also included an alternative model that uses our modified Brutsaert equation along with the CD99's seasonally-variable leading factor for the clear sky emissivity in order to explore its suitability for our datasets (lines 235-252 in the revised version). The performance of the linear cloudy factor in CD99 using RP station data was found to be poor on cloudy days, as it is not able to represent the large amount of days when high emissivity, very close to 1, was measured. This has also been asserted in the new text (see lines 317-322 in the revised version). Fig. 6 and 7 and Table 1 have been modified to include the four different models at different sites.

Minor comments

(1) The equations (4-7) were regressed for three cases, respectively. Does the transitions from one case to another are mathematically continuous.

No. They are independent surface fittings for different states. The goal is to describe the apparent surface that the relationship of emissivity with other screen-level variables creates in the simplest mathematical form. In fact, these expressions have now been simplified (see Eqs. 4,5,6 and 8 in the revised version) as their complexity in the initial version of the paper did not reflect over the calculated emissivity, as Reviewer 3 suggested to us. Continuity would impose very critical restrictions that would complicate the parametric expressions.

(2) Suggest replacing "Wa" with "rh" for relative humidity.

We appreciate the referee's suggestion. We chose *W*a for relative humidity following the notation in Dingman (2002) (p 587), which is commonly used when dealing with snow hydrology and atmospheric water and we would prefer to maintain it that way.

(3) The authors classified sky conditions into three types: clear-sky, completely overcast, partial cloud cover. Is this classification according to solar radiation or according to the emissivity itself? It would be a self-circle if it is the latter case.

The question is interesting and it is worth explaining the methodology followed further in order to achieve this regionalization in detail. We performed a previous analysis of the data collected by RP station. There we detected the strong relation between relative humidity, solar radiation, and emissivity and decided to take advantage of it to delimit the three regions as defined by Eqs. 2a, 2b and 3. Once these regions had been established, we assumed them to be applicable to other sites as they were found in RP station data. Thus, it can be considered that the regionalization is parameterized too. Since emissivity data are not usually available, the regions can be simply calculated from screen-level measurements of relative humidity and CI according to Eqs 2a, 2b and 3.
Regarding this regionalization, here we need to point out another change that has been made in the new version of the work. In the previous version, the cloud index *N* of Brutsaert (1982) was applied only for values outside region A in Eq. 2; in fact, this means an additional not-direct parameterization in *N*. This regionalization has been removed, and now the only parameters that affect the modified Brutsaert equation are those in Eq. 8.