I assume the authors more or less accepted my concerns but really did not change the text at critical points. Just reading the 'revised abstract' the summary oversells.

We agree in some of the reviewer major concerns and added thus pertinent comments in revised manuscript to provide the reader with a better understanding of the strengths and weaknesses of our approach, as can be checked in the track-changes version that was submitted previously. However, we disagree in the negative vision that the reviewer has manifested regarding the validity and usefulness of the parameterization. The aerosol parameterization is targeted for limited area models, which conduct weather simulations at small spatio-temporal scales. In such conditions, it is required to have a method that provides not only average aerosol conditions (as is the case for global simulations), but also exceptional situations such as highly absorbing aerosols in polluted spots or sporadic wildfire events. The effectiveness of the method is supported by a validation process based on data from 5 radiometric sites in the US with the highest data quality that is currently available (BSRN network). The validation is twofold: first, a benchmarking against an ideal control case is conducted; second, a direct comparison with the measured irradiance is shown.

To be a bit more specific regarding the changes introduced in the manuscript, we would like to add the following:

We agree in that the two aerosol mixtures included in the parameterization do not cover all the situations that occur globally. However, in practice, including any aerosol situation would involve an infinite number of reference aerosol models. The current implementation of the parameterization lacks very importantly of an aerosol model dominated by the coarse mode, and we warn on this in the conclusions section (Sect. 6): *"At this point, the two aerosol mixtures do not allow the simulation of aerosol situations having a dominant coarse mode, such as what is typical with sea salt or desert dust. The inclusion of these other types of aerosol mixture is already in progress"*. The reasons for the lack of the coarse mode aerosol were already exposed in the previous reply to the reviewer comments. The lack of the coarse mode aerosol model however does not invalidate the validity of the parameterization or the feasibility of the method.

The reviewer has manifested his concerns regarding the highly absorbing aerosols included in the parameterization. Certainly, the urban aerosol model is very absorbing. An aerosol climatology however does not solve completely the aerosol issue for surface solar radiation assessment in a model such as WRF because WRF is run at reduced temporal and spatial scales, where aerosol situations off-the-mean occur frequently. Having available a highly absorbing aerosol model is useful and necessary in a model such as WRF. We provide an example of such situations in Sect. 4.2 of the revised manuscript, which was caused by wildfires in the nearby of one of the validation sites: *"Conversely, at the TBL site, the urban aerosol model yields better results, even though this is normally an unpolluted site. The clear days that were originally selected for that site were associated, by chance, to low values of observed SSA. The anomalous preponderance of absorbing aerosols can be likely explained by wildfires, which originated from the Arapaho-Roosevelt National Forest (50 to 100 km away), and were particularly active during those days (Short, 2013). Nonetheless, this particular case serves to show that the urban mixture may be useful under specific circumstances, and not just to represent urban or industrial environments"*

Unlike the urban aerosol model, the rural model shows very similar scattering properties to the mean records in main AERONET sites. At this respect, we provide the following comment in

Sect. 3.2 of the revised manuscript: "*Dubovik et al. (2002) presented an evaluation study of the aerosol optical properties observed during several years at various AERONET sites characterized by distinct aerosol types. At most of these sites, the measured SSA at mid-visible wavelengths was about 0.95 (such as in Greenbelt, US; Crete-Paris, France; Bahrain; Solar Village, Saudi Arabia;...) which is a value roughly coincident with that provided by the rural aerosol mixture (see band 10 in Table A3). Only at those sites affected by high pollution (such as in Mexico City) or biomass burning (such as Zambia) the measured SSA at mid-visible wavelengths was smaller than 0.90. To describe such conditions, the urban aerosol mixture would be more appropriate (see band 10 in Table A4), even though it seems to be too much absorbing*". In summary, we judge necessary to include in the parameterization not only the rural aerosol model, but also a more absorbing version (the urban aerosol model).

The validation dataset, which is also criticized by the reviewer, comprises an entire year of AOD observations at 5 main AERONET sites and 5 main radiometric experimental sites with the highest-quality data (BSRN network) in the US. It thus reflects typical situations in this large region that can be compatible with situations also presented in areas of Europe, for instance. We provide evidences that the method works in these conditions in the results section and Figs. 3-5. Therefore, we disagree with the critics shown by the reviewer.

We totally disagree with the negative vision of the reviewer regarding the uncertainty of the solar irradiance data. As it has been already commented, we are using the highest quality solar radiation data available in the US. In the replies to the first revision of the manuscript, we provided the reviewer with references supporting our estimation of the solar radiation measurements as a 5%.

In summary, we are not ignoring the reviewer concerns. We agree with him in that some points had to be clarified to make the reader more confident on the strengths and weaknesses of our approach. However we don't agree with the negative vision of the parameterization regarding the "little benefits to the community". Our method will make possible to improve in WRF the description of surface solar radiation in mountain areas (and this will positively impact the representation of surface energy balance and lower layers of the atmosphere) and will make possible to evaluate and forecast the direct solar irradiance, which is input to concentrating solar energy plants.

I had a look at the abstract and added comments for (as I feel NEEDED) modifications. In a similar way the conclusions should be toned down (otherwise we may have a lot of frustrated users.)

Our comments and rebuttals at this respect are the following:

- replace 'surface direct and diffuse irradiances assessment' by "downward solar broadband fluxes at the surface"

The terminology we use is appropriate for the audience of this paper, e.g. solar radiation forecasting and energy applications. Indeed, the term "flux" may be ambiguous since the term "flux density" should be used instead. The term "surface irradiance" is appropriate. See for instance Liou (2002). If the concern of the reviewer is with the fact that the parameterization is intended for surface direct and diffuse irradiances computation, we don't agree with him. Figure 3 probes how the parameterization largely reduces the bias with respect to the case without aerosols included in the DNI and DIF computation. Therefore, we don't judge convenient to change the title in this point.

- ... and add to the title "for regions dominated by urban pollution"

This comment is not appropriate since the proposed method is mostly applied to rural/continental aerosol conditions, for which the rural aerosol mixture is intended. Urban (very polluted or biomass burning) areas are also modeled (by choosing the urban aerosol mixture), but amount to only a very small fraction of the area at the continental scale envisioned here. See the comments above at this respect.

- replace 'surface direct and diffuse irradiances' by "fluxes at the surface"

See rebuttal above.

- the authors claim anyway said that the diffuse aspect to the solar dn fluxes is not that important (and I would not trust those with for the very absorbing aerosol type).

As has been already said, the urban aerosol model is not intended for general use. It is restricted to exceptional situations, such as the case shown at the Table Mountain site, which is affected by smoke from wildfires nearby. We show how, for this case, the urban aerosol provides much better estimates of the DIF irradiance than the urban aerosol model. In the rest of sites, in contrast, the rural aerosol model provides much better estimates of the DIF irradiance than the urban aerosol model. This means that, of course, the rural aerosol model provides more reliable estimates of DIF irradiances, in general, and the estimates provided by the urban aerosol model are less reliable. It is because of this reason that the urban aerosol model shouldn't be used in the general case. However, an aerosol model such as the urban model is necessary in a limited area model such as WRF to represent situations with exceptionally high absorbing aerosols.

The reviewer seems to be generalizing the application of the urban aerosol model to any case, and that is a very wrong interpretation of its use that we don't recommend.

- remove 'and the type of the predominent aerosol'.

Although somewhat arbitrary, the attribution of a predominant type of aerosol (e.g., rural, maritime or urban), is necessary to better define the Angstrom exponent, SSA and asymmetry factor. Even though AOD is the main driver of aerosol extinction, the latter 3 parameters are important to correctly evaluate the diffuse irradiance. Additionally, the Angstrom exponent is a driver of the accuracy of direct irradiance. Thus, we judge necessary to resort to a type of "predominant" aerosol that helps to describe these parameters.

- The aerosol impact on dn solar fluxes is controlled mainly the AOD (at a particular wavelength) and the AOD spectral dependence. Since the assumed aerosol type uses a fixed size the application for regions outside those dominated by polluted areas is likely to fail (and the strong absorbing absorption case should be removed as relevant, and certainly for larger AOD cases when significant flux reductions can be expected.

The reviewer correctly points out that the surface irradiance is mainly controlled by AOD and its spectral dependence. Since the latter is difficult to obtain at continental scale in practice, we use the assumed aerosol type as a proxy.

However, the reviewer seems to be wrongly interpreting that the use of the urban aerosol is general. In a typical WRF simulation the rural aerosol model should be used. The urban aerosol model is only recommended when a high absorbing aerosol is foreseen.

The rural aerosol model has SSA values at mid-visible wavelengths of about 0.95 at RH=80% (see Table A3, Band 10). These values are not suspected of being strongly absorbing on the light of empirical evidences at AERONET sites (Dubovik et al, 2002).

Moreover, our analysis does not focus on urban areas, contrarily to what appears to be suggested by the reviewer, and is absolutely not limited to them. That is actually why the validation we provide, using high-quality irradiance data from rural sites, confirms the validity of the method.

- I would leave the sentence out. The Angstrom and asymmetry factor relate to ONE size-distribution and sensitivity to fluxes (with respect to size), when variability in the spectral dependence can be expected, is NOT part of the paper.

The authors completely disagree with this restrictive view. We provide a parameterization of the aerosol spectral extinction effects, based on limited information at continental scale. Of course, this parameterization does not pretend to be perfect and to correctly evaluate the aerosol extinction under very specific spatio-temporal conditions. Our goal was to improve the current situations in WRF or similar models, where the aerosol effects were based on very crude aerosol climatologies, or even no aerosol information at all. Aerosol climatologies do not provide the necessary temporal information. Outputs from chemical transport models usually do not provide enough absolute accuracy in AOD or its spectral dependence. Our approach has the merit to overcome these current difficulties.

- add "for data over the continental US"

Agreed

- replace 'however' with "in principle"

Agreed

- remove 'very'

Agreed

- replace 'surface direct and diffuse irradiances' by "surface fluxes"

We do not agree. See rebuttal above.

- I suggest to delete this sentence. The authors claim is based on relative large measurement errors. I am sure folks of well-maintained radiometers will strongly object (for instance contact Joe Michalsky from NOAA Boulder)

We totally disagree. We collaborate with Joe Michalsky, and we know a lot about solar radiation measurement quality. We use data from BSRN, ARM and SURFRAD precisely because these are widely known as the best sources of quality irradiance data in the US in particular, and worldwide, in general.