Review: “On the ability of RegCM4 regional climate model to simulate surface solar radiation patterns over Europe: An assessment using satellite-based observations”

Having in mind the reviewer comments and the answers of the authors, I would like to provide some additional comments on the paper based on the updated document (the one that includes the track changes and author remarks).

Responses to the reviewers

Comment: “This manuscript contains a wealth of useful information, however, the main outcome / take-home message is not clearly highlighted. The manuscript would benefit from a more focused and dense presentation of the results of the analysis.” This comment is not answered but I hope it can be answered taking into account the comments below especially for figure 10 (in the end of this document).

I agree with the reviewer comment about the supplement. The figures got to have a link with the manuscript, otherwise they lose their validity even if they are necessary for future references and studies. I think what is presented in the supplement can be easily described in appropriate parts of the text.

Editor comments (in order of appearance in the document and not of importance)

Abstract

Reading the abstract I miss the quantification of the results drawn by this work. There is a general comment about good agreement based on counteracting effects but beyond this everything else is mostly a description of the work performed and not of the results. E.g. which inputs are most important? How accurate are they determined? What are the percentages of these counteracting effects ? e.t.c.

Introduction

Page 2/ line 30 you could at least mention these parameters

Page 3/ line 1: " for example" could be deleted

Page 3 / lines 13-22. I think here is a major point for the paper needed to be highlightened more on the discussion. Is the fact that previous validation efforts are focused on cloud cover and surface albedo and this study goes deeper, examining other factors too. The question also by the reviewers is: if this additional contribution (of these other factors) is significant. (Even if it Is not it is still an important finding).

Page 3 / line 29: MSG, MFG need some reference here.

Page 3 / line 31 : you are mentioning such studies later but some general references here would be helpful for the reader to exist.

Page 5 / lines 7-9 (newly added sentence) please rephrase since it is not clear

Page 5 / line 29: In order to have aerosol optical properties per layer you need a profile for each for the optical properties mentioned. For the aerosol optical thickness is there a standard profile used? For the single scattering albedo and the asymmetry factor I am not aware of any publication of such measurements, moreover having profiles of them in the spatial grids used. What exactly was used here and what is the reference for these profiles ?

Page 6: a number of different quantities/symbols used in the equations are not mentioned or defined in the text (e.g. ps, p, T, etc)

Page 7 / since single scattering albedo was used as SSA I would recommend not to be referred also as omega.

Page 11 / line 3. What do you mean similar ? are they the same or if not what are the differences ?

Page 11 line 9. This consistency you are mentioning needs a reference. For example, AERONET calculates quality assured SSA and ASY (level 2 products) only for cases of AOD>0.4, then more than 90% of the pixels of the study have mean AOD<0.4. So a reference for these parameters used is needed if you think that these parameter accuracy is crucial for the scope of this work.

Pages 15-16 The presentation of the statistics together with the figures 2 and 3 is a bit confusing.

You start the discussion with table 1 that uses normalized mean biases. From my point of view this is a good decision that gives an additional valid to the paper, since it shows clearly features like the "extreme case of NE wintertime when MOD and SAT have mean SSRs ~19 and ~12 W/m^2 and biases of ~52%. Deviations would be much larger and clear if the absolute mean bias was used (now mean bias is a sum of positive and negative differences within one chosen area and that limits the validity of the work to the assigned regions only).

Then Figures 2 and 3 and discussion on page 16 are focused on SSR absolute differences. In that case wintertime differences are small despite the fact that normalized bias differences were high. I think this section would be much more clear if it could start the discussion with SSR differences, then presenting the mean biases as a statistical tool for characterizing the performance of the model in the selected areas and finally comment on the normalized absolute biases in order to try to discuss the pixel to pixel difference impact. The approaches can be used/referred by future model users depending on the application purpose of the model user.

Figure 10.

This is a very interesting figure and probably one of the highlights of the work. There are some issues that have to be clarified/changed.

Table (3) and figure 10.

It has to be commented that the importance of each of the parameters presented here on the SSR calculation is different. The cloud related parameters play the most important role. Looking at percentages in table 3 someone have to understand that for example a hypothetical 100% change on SSA have negligible effect when AOD is very low. Then a 100% change in AOD have different impact when the change is from 0.1 to 0.2 or from 0.2 to 0.4 because AOD impact on SSR is not linear. It is difficult to quantify exactly the importance of each parameter as this is a complex function of other parameters like the solar elevation etc. But it would be useful to discuss together with the table 3 that the percentages presented here are not directly linked with the SSR differences.

Figure 10 results.

Based on the analysis of this work the differences between SAT (satellite based) and MOD (model) results could be attributed to:

a. Differences of the parameters p that are used for SAT and MOD
b. other issues related with SAT algorithm and MOD uncertainties, spatial and temporal comparison representativeness, etc

In order to assess point **a,** the authors have been using a radiative transfer model in order to quantify the contribution of each of the parameters p to the overall uncertainty. This is a very interesting approach but in order to reach more solid conclusions:

1. It would be essential to show the bias due to all factors p compared to the real bias of MOD and SAT in order to quantify the contribution of a an b. This can be achieved by using sbdart one time with all SAT (p) inputs and one time

with all MOD (p) inputs and produce monthly SSR differences for each region shown together with the real SAT - MOD SSR differences already presented.

2. To justify the contribution of each of the factors p :

Authors now state: “Then, several SBDART simulations are implemented in the same way, replacing each time only one of the aforementioned input parameters with corresponding values from CM SAF, MACv1 or ERA-Interim (SSR(p)). SSRcontrol and SSR(p) are then used in Eq. (11) to calculate ΔSSR for each month (i) and parameter (p).”

What I understand for this sentence is that you have the Model (MOD) parameters p as inputs on the SBDART and each time you replace one p with the ones used by CM SAF, MACv1 or ERA-Interim (SAT inputs). Then you use also the control SBDART (with MOD related p’s as inputs).

By changing only one parameter p at a time you end up in bars that they can not be added in order to explain reason (a) above. That is because parameters p are interdependent. So as an example: lets assume that for a specific region and a specific month SAT parameters CFC and AOD differ from the ones of MOD input. Running SBDART once changing the CFC and once changing the AOD does not simulate the real AOD contribution to the difference, since if the CFC is higher for MOD inputs, AOD contribution will be much lower than calculated/shown. Same applies with AOD, SSA and ASY. The contribution of SSA is proportional to the absolute AOD value so if AODs for MOD and SAT differ then SSA contribution is not correct. What the plot shows here is the contribution of each of the parameters if all the other parameters of MOD and SAT were the same. Which is not something that can quantify the contribution of p’s in the SSR differences.

One approach to do that would be to use the Monte Carlo method using the different distribution of p’s, as used in the SAT and MOD algorithms. Another approach could be to assume that for MOD validation purposes (which is the main goal of the paper) SAT p’s can be used as a reference. In that case,

SSR difference (A) equals SSR from SBDART using MOD p’s, minus SSR from SBDART using SAT p’s. Then the contribution of each parameter p_i to the difference A, is the SBDART output using all SAT p’s except p_i which is used from MOD minus SSR from SBDART using all SAT p’s.

Finally, the conclusions would have to be re-written based on the above recommendations. Someone also has to point out there than the significance of each parameter p on the SSR calculated differences is a different concept from the relative contribution of this parameters p on the calculation of SSR itself. For example, if CFC for MOD and SAT for a specific region/month are similar then the CFC contribution on the difference will be very small, despite the fact that CFC could be considered as the most important parameter in SSR calculations.