Author's Response

First of all I would like to thank both reviewers for their time reading the manuscript and for their meticulous comments, which have been very helpful and have contributed to the improvement of the manuscript. I responded to all comments and implemented the changes in a new manuscript, which is re-structured and more readable. Below, I provide a short overall description of the manuscript changes and then I proceed to the responses.

General changes to the manuscript

1. **Restructuring of the Results**
   The structure of the Results (section 3.3) has been altered. Instead of describing the impact on each variable separately we describe the results depending on the interactions enabled:
   a) Enabling only aerosol-radiation interactions
   b) Enabling aerosol-radiation interactions in an environment where the aerosol-cloud interactions are also present
   c) Comparing the Thompson aerosol aware microphysics scheme to the non-aerosol aware Thompson2008 scheme and describing what happens when we implement both the aerosol-aware scheme with aerosol-cloud interactions and aerosol-radiation interactions and compare against control simulation that has no aerosol effects at all.
2. **Adding figure, removing tables**
   A new figure (Fig2-in the new manuscript) has been added containing the vertical aerosol profiles of the simulations. Tables 3 and 5 (original manuscript)have been moved to the supplement. Furthermore a new figures has been added in the supplement describing the single scattering albedo of the simulations.
3. **Changing color scale and number of simulations depicted in the main figures**
   We have changed the color scale in the figures (2, 3,4,5,6-original manuscript). The new colorbar has white color in the middle and makes clearer the sign of the small changes. Moreover the number of simulations depicted has been reduced. I understand that the previous figures were too packed with information. Several simulations enabling aerosol-radiation interactions have very similar behavior and are not discussed thoroughly in the manuscript. Now instead of 8 rows of plots the new figures have only 5. We keep simulation ARI_T (Tegen climatology, 1st aerosol option), ARI_Mv1 (Macv1 climatology, 2nd aerosol option), ARI_Mv1urban (urban very absorbing aerosol), the difference ARCI-ACI (indicative of aerosol-radiation interaction when indirect effect is present) and ARCI. We have omitted simulations ARI_Mv1full, ARI_MC that have very similar behavior to the others enabling aerosol-radiation interaction and also simulation ACI since the depiction of ARCI is enough to state the thing we describe about the Thompson aerosol aware scheme.

Now I proceed to answer each reviewer's comments. The original comments are with bold fonts and each response lies below the respective comment.

Reviewer 1

It is not clear for me the different between all the experiments listed in Table 1 (and section 2.4) (especially ARI_Mv1 ,ARI_Mv1urban, ARI_Mvfull)

Answer: Simulations ARI_Mv1, ARI_Mv1urban and ARI_Mv1full have the same AOD field (MACv1) but they have differences in the rest aerosol optical properties (single scattering albedo, asymmetry factor).

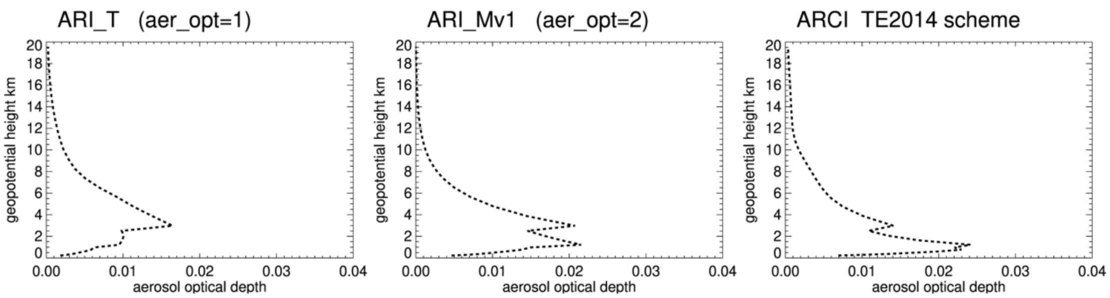
Changes in the manuscript: I have added this phrase to make it more clear (page 8, line22 new manuscript): "Within the ARI group simulations ARI_Mv1, ARI_Mv1urban and ARI_Mv1full have the same AOD field (MACv1) but they have differences in the rest aerosol optical properties (single scattering albedo, asymmetry factor)."

For someone who is not using WRF, would it be possible to explain this in a more general way, how the AOD is distributed into the different types, and what is the consequence of having rural over urban

Answer: The consequence of using rural over urban is being described in section 2.4 Model Simulations, page 7, line 8 of the reviewed manuscript. I have included a new figure (S2) with single scattering albedo maps in the supplement that will make the rural over urban difference even clearer.

Some more information about the vertical distribution would also be good.

Answer: I have added an extra figure (fig2) in the manuscript regarding the vertical distribution and a small text (page 13):



The results is a bit challenging to read, as there are many acronyms, and also it is refereeing to many figures in the supplementary text, which probably could have been included in the main text, and maybe some figures could be removed, as there are not always so large differences on the horizontal maps (I find figure S5 very useful, would it be possible to replace some of the tables and figures with this type of figure?).

Answer: I believe the restructuring of the Results, described in the beginning, is helping make the results easier to understand. Less maps are included in the main figures. I think the reviewer is referring to figureS6 (not S5) in the supplement. I have included a figure with box plots to the supplement (S1) and moved tables 3 and 5 of the original manuscript to the supplement.

Would it make sense to first present the results where the experiment CON, ARI-T, ARI_Mv1, ARI_Mv1urban, ARI_Mv1full is described, since this is focusing on the more trivial approach to include aerosol in RCMs, and it is just depending on what is having the "best" representation of the different species to represent the direct and semidirect effect. Then a separate section can be presented, where it is shown the effect of including the aerosol-cloud interaction, which is representing the indirect effect. Then these two simulations (ACI and ARCI) can be compared with the "best" aerosol representation from the first part (one of the CON, ARI-T, ARI_Mv1, ARI_Mv1urban,ARI_Mv1full). Now it is a lot of jumping back and forth between the different simulations, and it is not so easy to follow.

Answer: The reviewer is suggesting an interesting way to restructure the section of the Results. The restructure described in the beginning is very similar. First only the aerosol-radiation interactions are presented, the trivial approach to include aerosol in RCMs. Then we explore how aerosol-radiation interactions behave if we enable them in an environment where the indirect effect is also present. Finally what is the impact of the Thompson aerosol-aware scheme that enables aerosol-cloud interactions.

Two significant notes:

Simulation ARI_T uses the option aer_opt=1 to parameterize aerosol-radiation interactions. Simulations ARI_Mv1, ARI_Mv1full, ARI_Mv1urban and ARI_MC use the option aer_opt=2. Thus the impact of aerosol-radiation interactions is not just depending on the aerosol dataset used but also to the option (parameterization) used. This is why the Tegen climatology used in ARI_T leads to a similar clear-sky shortwave radiation decrease with ARI_Mv1 (for example) despite the fact that the AOD of Tegen is considerably smaller tthan that of MACv1. The aer_opt=1 parameterization has a tendency to decrease clear-sky radiation more per unit of AOD (W/m^2/AOD) than aer_opt=2.

When using the Thompson aerosol aware microphysics scheme, for example in ACI, we do not just implement aerosol-cloud interactions. We introduce a modified microphysics scheme that is designed to work with aerosol. Thus when we compare ACI to control CON (that uses the Thompson2008 non-aerosol microphysics) the impact seen is attributed to the combined effect of the introduction of a different microphysics scheme and to the possible indirect effect taking place.

After reading the manuscript, I am not so sure what is the recommendation from the study, since when there is no aerosol climatology included (as in the CON-experiment), there is a cold bias over Europe, and this cold bias is enhanced when the aerosol is included (e.g. for ARI_T, ARI_Mv1). The ACI and ARCI simulations are warmer than the CON, so there is a potential to remove the cold bias when aerosol-cloud interaction is included, but is this the take-home message that RCMs should aim for having interactive aerosol schemes? However, the impact on precipitation is very small, so in the end the aerosol treatment does not have a large impact?

Answer: I am not very worried about whether the aerosol introduction improves or worsens the bias. Of course if aerosol inclusion increased dramatically the bias this would be alarming and an indication that the aerosol parameterizations are probably not working properly. But this is not the case in our study. The final bias is of course a product of many different things such as the RCM structure and setup and the quality of the driving data. The purpose of our study is to identify the impact of the aerosol parameterizations and data used and the general behavior of aerosol implementation in the WRF model over Europe. This impact can improve or worsen the bias depending on the characteristics of each simulation. It is therefore simulation specific. For example the use of a different land scheme in our study could lead to a warm bias in the control simulation and thus enabling aerosol-radiation interactions would end up improving the temperature bias. So it is up to each WRF user to decide whether and what options to enable if they have bias improvement in mind. I can only describe their impact. Therefore I do not want to make general recommendations about using this or the other option or dataset. The same goes for the ACI and ARCI simulations that are mentioned by the reviewer. ACI is indeed slightly warmer than control and can improve the cold bias. But I would not state that it is a take-home message that RCMs should aim for having interactive aerosol schemes only because of the bias improvement. Using a different model setup, different driving data or a different domain could lead to bias increase when using interactive aerosol. It is understandable that people may want to include in their simulations all the physical mechanisms that are available and have a more detailed representation of each phenomenon (e.g. interactive aerosol). This makes the model more complete. However the extra or/and more detailed mechanism does not necessarily improve the bias. Regarding precipitation, indeed aerosol treatment does not seem to have a large impact. To conclude I can make two statements regarding the bias:

a) Aerosol introduction can have an impact on bias. However the main biases are not altered considerably. Thus aerosol do not seem to be the main source of the bias.
b) Aerosol introduction does not necessarily improve the bias. In many cases the bias is increased when aerosol are enabled in our study. This does not mean that the aerosol parameterizations have a problem. We have seen that they behave in a

physically consistent way. Whether bias is improved or not is specific to each simulation. It depends on each user to decide if and which aerosol treatment to use.

If I have understood, there is no yearly change in the aerosol (only monthly or daily data), did the authors consider to include yearly varying aerosol? I guess for 5 years of simulation, the effect is not so large, but in past studies it has been shown that RCMs that don't have transient aerosol is not representing the change in the surface radiation correctly. (Bartok et al. (2017))

Answer: The Tegen and MACv1 climatologies do not have yearly variability, only monthly variations. The MACC is a daily dataset and thus there are differences from year to year. However the impact of this year to year variability is minimal. It seems that 5years are indeed a small period to explore the effect of transient aerosol and this was not the intention of this study. It is a very nice suggestion by the reviewer to explore the effect of transient aerosol. Actually we are working on longer simulations spanning 30 years that indeed show that the inclusion of transient aerosol is important to correctly capture the trends in surface radiation. However this will be the subject of another study.

General comments:

Line 15-17 (p1): "statistical significant ".what is statistical significant (and what is meant by "in some cases"..please rephrase.

Answer: We use the phrase "statistically significant" to denote that a result is of statistical significance. As explained in the methodology we use the Mann-Whitney non-parametric test and calculate significance at the 0.05 level. I am under the impression that the phase "statistically significant result" is frequently used.

I have rephrased to: "of statistical significance".

The use of the phrase "in some cases" was meant to state that the changes that are of statistical significance are not widespread but are seen in some selective cases. I did not want to give more details in the abstract, just state the general behavior. I have added: "which in some cases, mainly close to the Black Sea in autumn".

Line 5-6 (p2): This sentence is not so easy to understand, especially if you don't know the difference between radiative forcing and adjustment (where I assume you mean rapid adjustment).

Answer: Yes indeed we mean the rapid adjustments. I do understand that it is hard to follow. I think it is better to keep it simple and just state that both aerosol-radiation and aerosol-cloud interactions add to the uncertainty while the aerosol-cloud interactions do that to a larger degree.

Rephrased to: "It states that the uncertainty due to aerosol is attributed to both aerosol-radiation (ari) and aerosol-cloud interactions (aci) with the latter having the largest contribution."

Line 15 (p5): I don't quite understand how the vertical profile of the different component is distributed in each model level. Is there some weighting doing the distribution? Would it be possible to show the vertical distribution for the different experiment?

Answer: I have added a figure depicting the vertical profile of the experiments in the supplement (S1). The option aer_opt=1 (ARI_T) uses the 3D Tegen climatology and constructs a vertical profile by adding the AOD of each aerosol type in each model level. The option aer_opt=2 (ARI_Mv1 family) assumes a specific function for the vertical profile. This is the same over each grid point and thus the vertical profile has the same shape over each grid point, only the magnitude of AOD changes depending on the total AOD provided at each grid point.

Line 21-24 (p5): The distinguishing of rural, urban and maritime component is not clear for me. What is meant that "in this work the first two component has been implemented"? is the maritime not used? And for the experiment where the different components are used (e.g. rural or urban), is this the case for the whole domain? Or can you combine this and set rural for one part, and urban for another part?

Answer: The "rural", "urban" and "maritime" types are different ways that the option aer_opt=2 uses to parameterize the single scattering albedo (SSA) and angstrom exponent (AE) of aerosol. The selected type is indeed used for the entire domain and you cannot chose one type for one part of the domain and another type for the another part. However for the selected aerosol type the produced fields of SSA and AE are not spatially homogenous but present spatial variability over the domain. This is because the parameterization takes into account the total AOD over each grid point and the relative humidity to calculate the SSA and AE values. The difference between the types lies mainly in how absorbing are the aerosols of each type. The range of the single scattering albedo is given in the manuscript in section 2.4 "Model Simulations". I have also included a figure depicting the SSA of the simulations after a request of the second reviewer. The main difference of the "rural" and "urban" type is that "urban" is considerably more absorbing all over the domain. Indeed we only used the "rural" and "urban" types and not "maritime". "Maritime" presents larger scattering compared to "rural" however the differences are not as large as those between "rural" and "urban". Since we tested the "rural" type which is the most balanced and also used the more realistic SSA of MACv1 in one simulation (MACv1full) we believe that the use of "maritime" would not provide any significant additional value and some early tests gave the same indication. We chose however to test the "urban" type since it represents a completely different situation from the "rural" and "maritime" types and would show the impact of extremely absorbing aerosol, especially the semi-direct effect.

I have added the phrase (page 6, line 7 new manuscript): "Only one aerosol "type" can be used for the entire domain."

Line 2-4 (p12): are you describing a specific figure, or just the results in general?

Answer: This is indeed a small summary of the overall evaluation. I think it is nice to clearly state the main point in the beginning and then elaborate for each variable.

Line 9-10 (p12): does this mean that the model performance is actually better when aerosol is not included in the simulations?

Answer: Yes in some cases, not only temperature, model results present smaller biases when aerosols are not included. As I have described in a previous answer I do not think that this is problematic regarding the performance of the aerosol parameterizations used. The fact that the inclusion of an additional physical mechanism in the model (aerosol) fails to improve the bias is quite interesting and reveals how models work. That they have after all a group of different parameterizations that do not always work perfectly with each other and in many cases these parameterizations are calibrated to work well at certain conditions. For example a radiation parameterization might be calibrated to produce small biases without the use of a separate aerosol parameterization in place. IN this case the aerosol impact is indirectly taken into account through the calibration process. However this calibration might be insufficient in another domain (e.g larger AOD values) under different meteorological conditions (e.g. heavy cloudiness) or different aerosol related events (e.g transportation of very absorbing aerosol).

Line 7-9 (p13): is this related to a specific figure?

Answer: Yes this is related to figure 3 (fig2 original manuscript). It has been added to the manuscript.

Line 15 (fp13) From this line, it seems as a more general summary about the results is given, so it should maybe not be under section 3.2.4 (which is about the SW).

Answer: Yes this is a very helpful comment. This is a general summary about the evaluation of the aerosol including sensitivities. It is under a separate section after finishing with the evaluation of the control simulation.

I have moved this text in a new section (3.2.5 Evaluation of the sensitivity simulations) to make clear we are talking about the sensitivity simulation evaluation.

Figure2: how about including S1 with Fig 2? Moreover, if possible, how about using a color scale which is white in the middle? (not green).

Answer: I understand this point. However the differences in the bias are small (except only for DNI) and there are several maps in the manuscript and supplement explaining the impact of aerosol on each variable. I would prefer to just have this figure alone to give emphasis on the evaluation of the control simulation. I have changed the color scale to a new one having white in the middle.

Line 9 (p28): I would be careful with using the word climate (e.g. "aerosol effect on European climate"), since only 5 years of simulation is done.

Answer: This is indeed true. 5 years are not a sufficient period to state impact on climate. I have replaced "on European climate" to "over Europe".

<u>Reviewer 2</u>

Main comments:

- The authors present eight different simulations (1 control run and 7 sensitivity experiments) which makes the paper difficult to understand, and the reader can easily get lost in the tables and figures presenting all the simulations. Besides, the author mainly focus only on ARI_T, ARI_Mv1urban, ACI and ARCI. The three other simulations (ARI_Mv1, ARI_Mv1full and ARI_MC) are not discussed in detail. Unless adding more discussions on these different simulations, I would suggest to keep only a few of them in the main text and in the tables and figures, and keep the other ones for supplementary material.

- The organization of the paper and in particular of Section 3.3 dealing with the sensitivity experiments should be improved. Indeed, the author present first temperature and precipitation, whereas the direct effect of aerosols concerns first radiation, which then has consequences on temperature. The fact to present cloud fraction at the end may also be a problem as this parameter is needed to explain aerosol effects on temperature and precipitation. I would suggest to reorganize Section 3.3, and notably start by the analysis on radiation.

Answer: I do understand the comments regarding the organization off the results and the general readability of the entire manuscript and consider them extremely important since the essence of a paper is to easily communicate information. I have tried to reorganize the entire manuscript and especially the way Results are presented by implementing suggestions by the two reviewers. The main changes are described in the beginning of the author's reply. To quickly summarize they include two key features: a)The aerosol impact is presented according to the effect explored and not for each variable separately, b) less simulations depicted in the figures and tables 3 and 5 have been moved to the supplement.

- The simulations presented in this paper last only 5 years. I wonder if this is enough to study the sensitivity of climate-aerosol interactions, notably as far as cloud-aerosol interactions are concerned. I get the impression on some figures that the signal is quite noisy, notably in terms of cloud cover and precipitation.

Answer: Very interesting question. I do believe that 5 years are enough to study the rapid climate responses (or adjustments) due to the forcing of aerosol. According to the IPCC AR5 (section 7.1.3) most of the rapid adjustments are thought to occur within few weeks. The slow adjustments (mainly the full extent of ocean atmosphere interaction) only need much larger simulation time. Other RCM studies have produced results for smaller or similar periods: Da Silva 2018-6 moths for precipitation, Zanis 2009-2 years, Nabat 2015-2 years spin up +6 years simulations. I am not very worried about the somewhat noisy signal in precipitation and cloud fraction since these variables are highly impacted by differences in the local scale especially terrain elevation.

Zanis, P.: A study on the direct effect of anthropogenic aerosols on near surface air temperature over Southeastern Europe during summer 2000 based on regional climate modeling, Annales Geophysicae, 27, 3977–3988, https://doi.org/10.5194/angeo-27-3977-2009, http://www.ann-geophys.net/27/3977/2009/, 2009.

Da Silva, N., Mailler, S., and Drobinski, P.: Aerosol indirect effects on summer precipitation in a regional climate model for the Euro-Mediterranean region, Annales Geophysicae, 36, 321–335, https://doi.org/10.5194/angeo-36-321-2018, https://www.ann-geophys.net/36/321/2018/, 2018.

Nabat, P., Somot, S., Mallet, M., Sevault, F., Chiacchio, M., andWild, M.: Direct and semi-direct aerosol radiative effect on the Mediterranean climate variability using a coupled regional climate system model, Climate Dynamics, 44, 1127–1155, https://doi.org/10.1007/s00382-014-2205-6, 2015.

- The presentation of figures should also be improved. Several figures (for example Figures 3 to 6) are composed of too many plots, which make them difficult to read, and not all of them are discussed in the paper. There are also too many references to figures in supplementary material. Some of them have their place in the main paper. Besides, the font used for labels should be higher (notably in Figures 1 and 2).

Answer: The number of the plots in the main figures has been reduced. Unfortunatelly it is difficult to avoid referencing the supplement a few times. The fonts in Figures 1 and 3 (fig2 of original manuscript) have been placed slightly higher.

Specific comments :

- Abstract: It should be clearly stated in the abstract how are calculated the different numbers which are given, in particular the fact that they rely on a comparison with a control simulation without aerosol scattering and absorption.

Answer: I added the phrase (page 1, line19 new manuscript): "The impact of aerosol is calculated by comparing against a simulation that has no aerosol effects."

- Page 2 Line 17-18: "Finally, a minority of the simulations use prognostic aerosol schemes with natural and anthropogenic emissions (dust, sea salt) online driven by meteorology". Could you precise which model has a fully prognostic aerosol scheme ? I don't see any model in the table given in footnote 1.

Answer: Only one model uses a prognostic scheme. It is UM-WRF361 (third from the bottom in the table). It states that aerosol are estimated online at every time step. I have rephrased to state that it is only one model since the term "a minority" is not precise.

Changed to: "Finally only one model uses a prognostic aerosol scheme estimating online the aerosol field."

- Page 2 Line 20: "the aerosol-cloud interactions (indirect aerosol effect) is typically not considered". This information is not given in table in footnote1. Could you justify this point ?

Answer: Indeed this is a good point. The fact that aerosol-cloud interactions are not considered was indirectly inferred by the table and a bit hasty. There is only one model (SMHI-RCA4) that states clearly that has only aerosol-radiation interactions (and of course 5 cases that have no aerosol at all (so no indirect effect) and a couple that are not sure). Most of the models however use climatologies or fixed AOD fields and usually in these cases this indicates simple aerosol sophistication and only aerosol interaction with radiation. Moreover even if aerosol-cloud interactions are considered I do not believe that the full impact of the indirect effect could be captured with a stable in time aerosol field (even with seasonal variability). Only a prognostic aerosol scheme can modify the aerosol field according to the meteorological conditions and thus fully capture the impact

But yes, the point I am trying to make here cannot be strictly justified by the information given in the table in footnote1. This statement has been omitted in the new manuscript.

- Page 2 Line 4: Could you precise here the version of WRF that you use ?

Answer: I have added: “WRFv3.8.1”

- Page 3 Line 13 (and Page 4 Line 19): the limits of the EURO-CORDEX domain given here (25S- 75N, 40W-75E) seem to be very large for Europe (in particular 25S and 40W).

Answer: Nice observation. There is a typo in the description of the domain and yes this is not the typical EURO-CORDEX domain but the domain used in the simulations. The southern limit is 25N and not 25S. It has been corrected in the manuscript in the second case stated above. In the first case (section 2.1.1) I have rephrased to simply state that E-OBS cover Europe. The 40W and 75E are not as distanced from the original EURO-CORDEX domain as they seem. Since we use a rotated grid only the domain upper left and right corners stretch to such limits. The domain used in the simulations encompasses completely the EURO-CORDEX domain.

- Section 2.1.1: Please give the horizontal resolution of the observation datasets.

Answer: It is actually stated that data on a 0.44^{o} rotated pole grid are used.

- Page 3 Line 16: What are these cases with an excess of 100% ? It is worth knowing if there is specific situations in which the E-OBS precipitation is not trustworthy.

Answer: The largest relative errors in precipitation are found mainly over mountainous areas in the Alps and mountainous parts of Norway, in North Africa due to the very small station density and in areas east to the Baltic sea. Large relative errors are also seen in some grid cells in Italy and Spain. In general results are the best over central Europe and the UK. It must be noted that these “errors” are estimated by comparison against more regional gridded datasets with higher density of stations that are thought to be closer to reality. I have rephrased the sentence in order to be more specific and provide as much information is given in Hofstra et al. (2009) as possible.

Hofstra, N., Haylock, M., New, M., and Jones, P. D.: Testing E-OBS European high-resolution gridded data set of daily precipitation and surface temperature, Journal of Geophysical Research Atmospheres, 114, https://doi.org/10.1029/2009JD011799, 2009.

Rephrased to: When compared against regional datasets with higher station density (Hofstra et al., 2009) the E-OBS dataset presented a mean absolute error around 0.5^{o}C for temperature whereas for precipitation a general tendency of underestimating precipitation amount is reported, with large (>75%) relative errors found in mountainous regions of the Alps and Norway, over North Africa and in areas east to the Baltic Sea.

- Page 3 Line 20: Please give a definition for Direct Normalized Irradiance.

Answer: I have added: DNI is the solar radiation received by the direction of the sun's rays and received by a surface that is perpendicular to that direction.

- Section 2.1.2: Please give a reference for the SARAH dataset.

Answer: A reference is actually given in the second line: (Müller et al., 2015). I have moved it to the first line in order to state it right away.

- Page 3 Line 24: "between ± 65° longitude and ± 65° latitude". Too large domain ?

Answer: I have checked this again. It is stated in the CMSAF webpage https://wui.cmsaf.eu/safira/action/viewDoiDetails?acronym=SARAH_V002 and it is actually true regarding the ultimate extent of the domain. There are variations depending on season.

- Page 4 Line 5: As the CLARA dataset is not used for the evaluation of radiation but only for cloud cover, it should be discussed if this could have an impact on the evaluation.

Answer: I understand your point. Every dataset uses its own information about cloudiness amount to calculate shortwave radiation at the surface. However the use of a different product for radiation and cloud fraction does not necessarily lead to discrepancies as long as these products are good enough in estimating the respective variable. Both SARAH for Rsds (Müller et al., 2015) and CLARA for cloud fraction (Karlsson and Hollmann, 2012) have reasonable accuracy in detecting the respective variable, so I do not believe this has a significant impact on evaluation results.

I have added the phrase (page 4, line27): "The use of a different product for cloud fraction (CLARA) than the one used for radiation (SARAH) does not impact the evaluation since both of these products have reasonable accuracy and uncertainty in estimating the respective variables."

- Section 2.2: Please explain more clearly which indirect aerosol effects are taken into account in the different simulations (Twomey, Albrecht, …).

Answer: I have added the phrase: "Thus aerosols are free to either change cloud albedo (first indirect or Twomey effect) or/and impact cloud lifetime (second or Albrecht indirect effect)."

- Sections 2.3.1 and 2.3.2: The titles of these sections are unclear, they should be clarified.

Answer: The section 2.3.1 title was changes from: "WRF Aerosol options" to "WRF aerosol parameterizations examined".
The section 2.3.2 title was changed from: "Aerosol data" to "Aerosol datasets used".

- Page 5 Lines 14-15: In the case aer_opt=1, how are other radiative properties (SSA, asymmetry parameter) defined ? Are there common for all aerosol types ?

Answer: In aer_opt=1 the other radiative properties (SSA,ASY,) are given for each aerosol type separately in lookup tables the model. Then for each grid cell, each model level and each spectral band of the radiation scheme a final value is calculated by weighing the value of each aerosol type by its AOD and then summing them all together.

<u>I have added the phrase (page 6, line 11)</u>: "The single scattering albedo and asymmetry factor are given for each aerosol type and a final value is calculated in each model level and for each spectral band of the radiation scheme. This is done by weighting the value of each aerosol type by its respective AOD and aggregating for all five aerosol types."

- Page 6 Lines 18-23: It is not clear for me how this aerosol scheme is used. Is it a full prognostic aerosol scheme with emissions, transport and deposition ?

Answer: The Thompson aerosol-aware scheme is a very interesting idea. It is a microphysics scheme that is prognostic in the sense that it explicitly predicts the number concentration of aerosols and it has emissions, transport and deposition for aerosol. It makes some simplifications (like all schemes do to some extent). It separates aerosol into two general species: droplet nucleating and ice nucleating. It uses an aerosol climatology, derived from multi-year (2001-2007) global model simulations by the Goddard Chemistry Aerosol Radiation and Transport (GOCART) model, to initialize the aerosol field in the model and provide boundary conditions. Moreover a fake surface aerosol emissions/flux/tendency is added at the surface for the droplet-nucleating aerosol. This emission flux is based on near-surface aerosol concentration and a simple mean surface wind.
To conclude: The Thompson aerosol-aware scheme is not as complex as schemes with interactive chemistry that contain features like multiple aerosol types, multiples bin sizes, realistic emissions inventory e.t.c. It is a microphysics scheme that tries to have a more simplified aerosol treatment that has no significant extra computational burden. Its purpose is not to predict aerosol concentrations with the best accuracy. It tries to retain some basic climatological features of aerosol while at the same time changing the aerosol field according to the meteorological conditions thus enhancing the realism of aerosol-cloud interactions.
<u>I have added(page 6, line26):</u> "This scheme explicitly predicts aerosol number concentrations."

- Page 7 Lines 6 and 14: what does (and mp=28) mean ?

Answer: Indeed this is not explained. This is the symbolism of each microphysics option in the model namelist. I have added: (mp=8-in the model namelist)

- Page 7 Lines 8-9: "The single scattering albedo (SSA) at 550nm of the "rural" type aerosols ranges in our experiments between 0.92 and 0.98". How are these values spatially distributed ? Maybe a map in supplementary material could be helpful to understand the spatial distribution of aerosol-radiation effects.

Answer: I have added a map of the SSA values on the supplement (S2).

- Table 1: For simulation ACI, aerosols are indicated to interact with clouds whereas the option aer_opt=0 is used. How is it possible ?

Answer: I see this might be confusing. The "Aerosol option" row in Table1 is for aerosol-radiation interactions only. Aer_opt=0 means that there are no aerosol-radiation interactions. Thus ACI has aerosol-cloud interactions since it uses the Thompson aerosol-aware scheme but the aerosol of the Thompson scheme do not interact with radiation.

- Page 9 equation2: The DRE is calculated in clear-sky fluxes, so I suggest to call it rather CDRE (Clear-sky Direct Radiation Effect) to avoid confusion with RE, which is calculated in all-sky confitions.

Answer: I think this adds to the clarity of the metric. I have renamed it to Clear-sky Direct Radiation Effect CDRE throughout the manuscript.

- Page 10 Line 13: "The climatology of Tegen has a lower AOD compared to SEVIRI, but follows the latter's seasonal spatial variability". I don't understand how the Tegen AOD follows the seasonal spatial variability of SEVIRI AOD, it is not clear from me in Figure 1.

Answer: This was meant to state that Tegen has the same main changes in seasonal AOD like SEVIRI. I have completely re-written the section 3.1 Aerosol optical depth omitting this sentence. I tried to give more precise information. We have also included the MODIS satellite dataset and use it as the main point of reference. We reanalyzed the SEVIRI dataset taking more care about averaging missing values and

- Page 10 Line 15: The use of AOD assimilation in the MACC reanalysis could be mentioned to explain the better agreement of MACC AOD with satellite data.

Answer: I have included that MACC uses data assimilation is section 3.1 (page 11 ,line 15)

- Figure 1: There is strange high AOD in Eastern Europe in SEVIRI data in winter (DJF). Please comment on this pattern. Maybe the use of another satellite product (MODIS for example) could help to ensure the robustness of satellite data.

Answer: I have also used the MODIS dataset and this is the main point of reference now. This weird AOD high over Eastern Europe in SEVIRI during winter severely confused me in the beginning. I was unable to find a reference of this. However this AOD high is fake and had to do with the way the missing values were seasonally averaged due to a bug in the code. The SEVIRI monthly mean gridded data have large areas of missing values in winter and autumn over around 45N. Few scattered grid points with valid data can be found however in higher latitudes in each month. By mistake the seasonal averaging happened also above grid points that had very few valid monthly (even one valid monthly values per the entire 5 year period was enough to be included). Thus the averaging over those areas resulted in an aerosol field that cannot be representative of the seasonal mean. In the corrected analysis only grid points that have no missing values at all are used for the seasonal averaging.

- Page 12 Lines 2-4: These lines should be rather in the conclusion of the section than at the beginning. The authors could rather introduce Figure 2.

Answer: I believe it is nice to have a quick summary in the beginning with the essence of the evaluation and then elaborate more for each variable.

- Page 12 Lines 6-7: There is on the contrary a warm bias in northern Scandinavia.

Answer: In Scandinavia over winter the averaged bias is negative and almost -1 ^{o}C. I understand however since the warm bias at the north is not discussed in the manuscript the statement of considerable negative bias in Scandinavia might be confusing.
I have rephrased to make it clearer.

<u>Changed to </u>: In the simulation CON winter temperatures are mostly underestimated (-0.5oC domain average, land only), with higher cold biases over Scandinavia (despite a warm bias at the north), the Mediterranean and the Alps (-1^{o}C) as indicated in the upper panel of (Fig.2).

- Page 12 Line 10: Please give a reference for the Noah land surface model.

Answer: I have added the reference

Niu, G-Y., Yang, Z. L., Mitchell, K. E., Chen, F., Ek, M. B., Barlage, M., ... Xia, Y. (2011). The community Noah land surface model with multiparameterization options (Noah-MP): 1. Model description and evaluation with local-scale measurements. *Journal of Geophysical Research: Space Physics*, *116*(12), [D12109]. https://doi.org/10.1029/2010JD015139

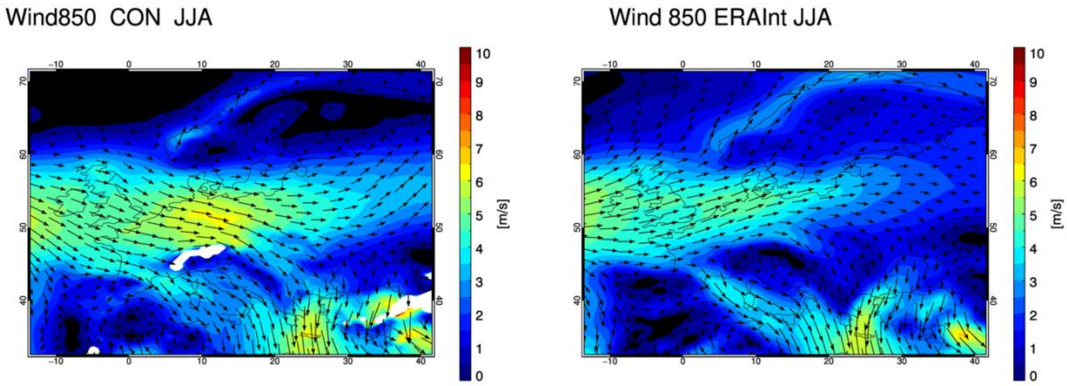
- Page 12 Line 25: Please give a reference for the WDM6 cloud microphysics scheme.

Answer: I have added the reference.

Lim, K.S. and S. Hong, 2010: Development of an Effective Double-Moment Cloud Microphysics Scheme with Prognostic Cloud Condensation Nuclei (CCN) for Weather and Climate Models. Mon.Wea.Rev.,138, 1587–1612, https://doi.org/10.1175/2009MWR2968.1

- Section 3.2.3: The bias in cloud fraction in summer could be related to a too zonal circulation ? Besides, the author could discuss if those biases in cloud cover could have an impact on AOD in the case of the simulations with aerosol-cloud interactions.

Answer: I have examined the wind field at 850hPa of ERA-Interim and found small differences with that of the control simulation CON in summer. The model does not seem to deviate much from the general circulation seen in the reanalysis dataset.



- Tables 2, 3 and 4: Please explain which domain is used for these averages. In particular, it should be stated if only land points are considered (as the model is not coupled with ocean, it would be more relevant to show only land grid points).

Answer: The domain of analysis is given in page 10 line 31. I have added that both land and sea points are considered. I have also added a description in the label of each table: Domain is defined as -10°W, 40°E and 36°N, 70°N.
The lack of coupling definitely has an impact. However I believe it is mainly seen in temperature. Thus I have included a column in table 2 that has the impact (annual) on temperature only over land.

- Page 15 Line 7: The impact on surface temperature seems to be larger in autumn than in summer, while the AOD is higher in summer. Is there a role of internal variability?

Answer: Interesting comment. This is true. For Tegen the domain averaged AOD is indeed larger in summer whereas for Macv1 and MACC the mean AOD is the same for summer and autumn. However in summer it is larger over land for all datasets, where we expect a more strong response in temperature. If we use only land points the temperature impact is still larger in autumn even though not as clearly as was when using both land and sea points. I do not think that this has to do with the internal variability of the model but with the rather complex nature of aerosol impact. Larger AOD does not necessarily mean larger reduction of radiation since clouds also play a role. The relative change in shortwave radiation is slightly larger in autumn (4 out of 5 ARI simulations) compared to summer. And I see that the impact on temperature seems to be more correlated with the relative changes in shortwave radiation (%) and not so much with the change in W/m^2.

I have added (page 21): "Despite the larger AOD in summer, the temperature impact is greater in autumn. This is probably related to the fact that the relative Rsds decrease is slightly larger in autumn (except for ARI_Mv1full). "

- Page 15 Line 9: "in cases reaching a decrease of 1.5° C" Please give more details on these cases.

Answer: I have added(page 21): "Cases of such strong reduction are not spatially extended and are seen mainly in summer and autumn within the areas of intense cooling like the Balkans and near of the Black Sea. "

- Page 15 Lines 10-12: This point should be related to a figure.

Answer: I referenced figure S3 of the supplement.

- Page 15 Lines 13-end: The results using the ARI_Mv1urban simulation should be moderated as the absorption of aerosols is not realist in this simulation.

Answer: I have added: "We must remind here that ARI_Mv1urban is more of an idealized experiment with unrealistically absorbing aerosol."

- Page 16 Line 5: Could you explain: "Contrary to the ARI group, simulation ACI using the Thompson aerosol-cloud interacting cloud microphysics and accounting for indirect effects only results in a domain averaged temperature increase (0.1 to 0.2 o C) compared to CON for all seasons except autumn" ?

Answer: ACI just implements the Thompson aerosol-aware scheme compared to CON and no aerosol-radiation interactions. The aerosol-aware scheme presents less cloudiness than Thompson2008 used in CON thus ACI has temperature increase since aerosol do not interact with radiation to decrease it. I understand that describing ACI after the ARI (aerosol-radiation interactions) simulations for each variable might be confusing. I believe that the reorganizing of the Results per aerosol effect helps to make things clearer.

- Page 16 Line 21: It should be mentioned that the Black Sea is not coupled, which could influence the results on precipitation.

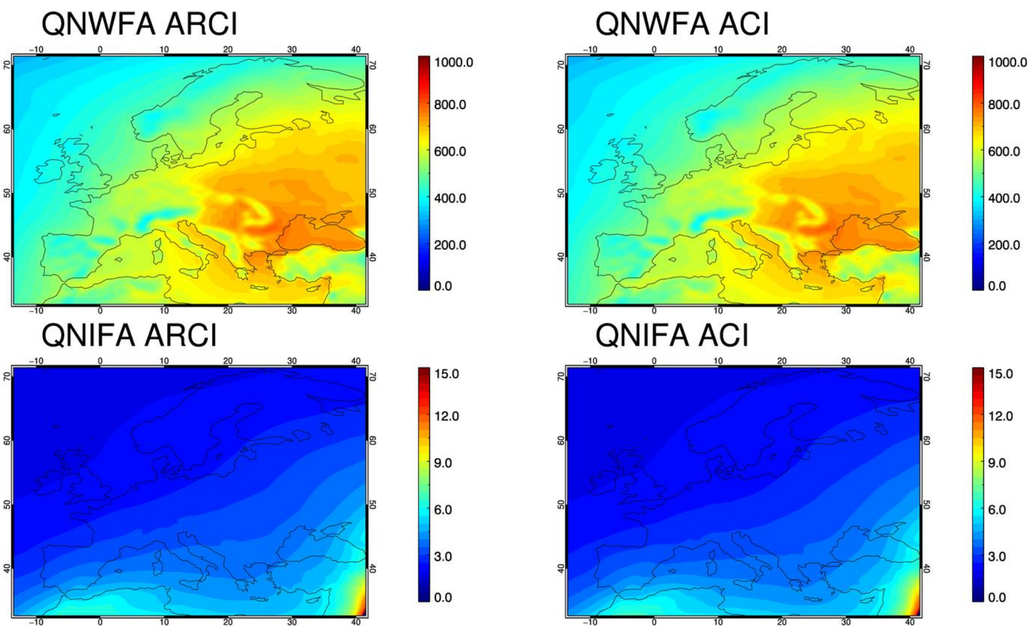
Answer: Yes it is important to state this especially in this case.

I have added the phrase (page 23): "It should be reminded however that the simulations do not have an ocean-atmosphere coupling, something that can influence the results on precipitation over the Black Sea."

- Page 17 Lines 2-4: This point should be related to a figure.
Answer: I have pointed to the figure 4 that shows the clear-sky direct radiative effect.

- Page 17 Lines 12-13: how is calculated the correlation with AOD in the ARCI simulation (with which AOD) ? The difference in AOD between ARCI and ACI could have an impact ?
Answer: Nice point. In the above case correlation is calculated between ARCI and ACI with only the ARCI field being used to determine correlation. The fields of ARCI and ACI (number concentrations, AOD is not available in the model for ACI) are very similar. Since ACI has no aerosol-radiation interactions I believe it makes sense to find the correlation between the AOD field that is active and radiation clear-sky radiation change. I would be more worried about the time evolution of the AOD in ARCI (this is stated in the manuscript), something that could make the mean AOD not sufficient for spatial correlation analysis.

- Page 19 Line 14: it is difficult to draw this conclusion as AOD is not the same in all simulations. I suggest that AOD could be added in Table 4 to discuss this point.
Answer: The aerosol field in ARCI is the only field that is interacting with radiation. Thus it makes sense to see what happens when I introduce an active I think that comparing ARCI to ACI can give a strong indication as to how the aerosol-radiation interactions are affected when the aerosol-cloud interactions are also present. Especially when we are talking about domain (and possibly seven subdomain) averages over a large period of time (multi-seasonal or multi-annual averages). The AOD field of ARCI is presented. However since ACI is not provided in the model output since ACI lacks aerosol-radiation interactions and AOD is irrelevant. However the concentrations of aerosol, both droplet nucleating (QNWFA) and ice nucleating (QNIFA) are given for each grid point and model level for both ARCI and ACI. If we calculate the total concentration over each grid point the aerosol fields are almost identical. See below for annual averages. Thus I feel confident that the ARCI to ACI comparison can give valuable information regarding the general behavior of aerosol effect over the domain over large time periods. For much smaller spatial and temporal scales the above comparison could be problematic. Finally the idea to add AOD in table 4 (table 2 –new manuscript) is very good and has been implemented.



- Page 19 Lines 21-24: I don't understand how the author come to this conclusion.

Answer: This talks about the simulations ARCI and ACI that use the Thompson aerosol aware microphysics scheme having less cloud amounts and more positive cloud forcing compared to control CON (that uses the Thompson2008 scheme). This point about the aerosol aware scheme is more evident when comparing ACI to CON since the only difference between them is the change in the microphysics scheme. ACI has less cloud fraction amount, more prominently in summer and spring (fig5 original manuscript) and more positive cloud forcing (fig6 and table 4 original manuscript) than CON. Of course these differences cannot be attributed only to aerosol-cloud interactions but mainly to the change of the microphysics scheme.

- Page 20 Line 1: what is Aer2urban ?

Answer: My apologies. The naming of the simulations was changed after initial submission due to the editors suggestions and an earlier naming remained in the text. It was changed to ARI_Mv1urban.

- Page 20 Line 8: Could you explain why the effects are stronger on DNI than on Rsds ?

Answer: <u>I have added this phrase</u>(page 18): "Since DNI comes only from the direction of the sun, any interaction with aerosol (scattering, absorption) removes radiation amount from this direction. On the other hand in Rsds radiation is reduced only when it absorbed or scattered in an angle that does not reach the surface. Thus the aerosol direct effect is much stronger in DNI."

- Page 21 Line 5: what is the specific effect of aerosols on diffuse radiation, that could be distinguished from direct radiation ?

Answer: <u>I have added the phrase (page 18):</u> "Diffuse radiation reaches the surface from all angles except from the direction of the sun (direct radiation). Thus when direct radiation is scattered by aerosol a part of it becomes diffuse radiation and reaches the surface increasing diffuse radiation amount. "

- Page 23 Lines 11-12. This result involving LWP seems to be important to understand cloud aerosol interactions. Please explain more this process.

Answer: I believe this is connected to the Thompson aerosol-aware microphysics scheme (TE2014) having in general less cloud fraction amount than the Thompson 2008 microphysics scheme and not so much about the aerosol-cloud interactions. When we change from the Thompson 2008 to the aerosol-aware scheme (e.g. simulations ACI-CON) we do not just turn on the aerosol-cloud interactions but we introduce a modified microphysics scheme that explicitly predicts aerosol. Thus the impact seen is attributed to the change of the microphysics scheme and partially also to possible aerosol-cloud interactions. Since I cannot separate these two I do not think I can attribute the decreased LWP to aerosol-cloud interactions. Moreover the aerosol-cloud interactions also rely of the aerosol field that is being produced by the TE2014 scheme. Thus I believe that the change of microphysics scheme presents the main impact. To conclude it seems that the TE2014 has a tendency to produce smaller LWP amounts than the Thompson2008 (at least in our simulations) and this decrease in LWP also leads to decreased cloud fraction amount.

- Page 23 Lines 19-20. In this kind of semi-direct effect, the internal model variability could be important. The author should mention this point.

Answer: Yes this is a very interesting comment. We have seen the same behavior (aerosol induced cyclonic anomaly) to some extent in most simulations despite different datasets and aerosol options used. However the impact of different physics parameterizations or/and different initial conditions and period of simulation can be substantial and could substantially modify this effect. I believe that also the real variability of the climate could strongly impact this result, thus longer simulations would be needed to robustly examine this effect.

I have added to the manuscript (page19): "However the internal model variability as well as the real climate variability could be very important in this kind of complex feedback mechanism. The use of different physics parameterizations, initial conditions and even different time periods can have a large impact and could potentially modify this effect. Therefore it would be interesting to see whether these results are modified in a large physics ensemble simulating a more prolonged time period. "

- Page 26 Lines 26-27. I don't understand how "the introduction of aerosol-radiation interactions" could lead to "more transparent clouds" ?

Answer: (This is actually at page 26 lines 6-7). I do understand that the way it is written it is not just confusing but it is also wrong to directly link aerosol-radiation interactions to more transparent clouds (cloud albedo). What is meant is that when aerosol-radiation interactions and/or the Thompson aerosol scheme with aerosol-cloud interactions are implemented the cloud forcing at the surface becomes more positive (more radiation at the surface). This can be due to a change in cloudiness amount due to aerosol semi-direct effect or cloud optical properties due to cloud indirect effect. I have tried to rephrase.

Changes in the manuscript (page20):
From: "Thus, the introduction of aerosol-radiation and/or aerosol-cloud interactions leads to more transparent clouds enabling larger amounts of radiation reaching the surface."

To: "Thus, the introduction of aerosol-radiation and/or aerosol-cloud interactions leads to cloudiness enabling larger amounts of radiation reaching the surface. This can happen due to changes in cloudiness amount or in cloud optical properties."

Other corrections:
- Abstract line13 and Page 19 line 6: is comprised of (instead of comprises of)
- Page 4 Line 10: an**d** underestimation
- Page 4 Line 30: incorporates aerosol**s**

- Page 10 Line 15: The MACC reanalysis is **in better agreement** with the satellite data.
- Page 10 Line 16: and the **higher** generally higher AOD
- Figure 1: Please keep the same spelling for **MAC-v1**
- Page 12 Line 11: **In particular** northern Europe is …
- Page 13 Line 21: **error** compensation between errors
- Figure 4: Space character is missing after (RE)
- Page 28 Line 2: please define NWP

Answer: All these corrections have been implemented in the manuscript.