Review of the manuscript entitled *Investigating the sensitivity to resolving aerosol interactions in downscaling regional model experiments with WRFv3.8.1 over Europe*, **by V. Pavlidis et al.**

This manuscript, submitted to *Geoscientific Model Development*, presents a sensitivity study about the impact of the aerosol representation in a regional model on regional climate. The objective of the study is to evaluate different regional simulations carried out with the WRF regional model using different aerosol configurations, and to show the consequence of these options on radiative budget, temperature, clouds and precipitation. Several observation datasets (E-OBS, CMSAF radiation data) are used for this evaluation.
Results show a strong impact of aerosols on surface shortwave radiation in all simulations compared against a control simulation without aerosols. Their radiative forcing is negative at the surface due to direct aerosol effect, only partially counteracted by a slight positive semi-direct effect. Temperature is therefore also reduced by aerosols, between -0.1 and 0.5°C, but no significant effect has been found for precipitation. Aerosol-cloud interactions modulate this impact on radiation. All these results are interesting and provide important conclusions for regional climate modelers, and in particular the WRF community. However, the organization of the paper is not very clear and not very easy to understand, and the following essential comments need to be corrected before considering a publication in GMD.

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 sensitivty 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.

- 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.

- 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).

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.

- 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.

- 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 ?

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

- 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).

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

- 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.

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

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

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

- 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.

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

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

- 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 ?

- 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 ?

- Page 7 Lines 6 and 14: what does mp=8 (and mp=28) mean ?

- 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.

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

- 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.

- 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.

- 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.

- 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.

- 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.

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

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

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

- 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.

- 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).

- 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 ?

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

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

- 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.

- 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” ?

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

- Page 17 Lines 2-4: This point should be related to a figure.

- 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 ?

- 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.

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

- Page 20 Line 1: what is Aer2urban ?

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

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

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

- 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.

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

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