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Reply to EC1

Andrea Mazzeo et al.

Author comment on "Evaluation of the WRF and CHIMERE models for the simulation of $PM_{2.5}$ in large East African urban conurbations" by Andrea Mazzeo et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-552-AC3, 2022

- *There is no discussion on why the modelling was performed on 2 km x 2 km resolution. Given the uncertainty in the models, their input data, and the usage of the results this extra step of modelling may only have introduced extra uncertainty compared to, say, retaining the 11 km x 11 km of the emissions inventories used. The authors shall also explain why they only simulated a 1-month period and not a full seasonal cycle.*

A > The performance of the modelling system needs to be compared with observations from the real world to assess their reliability. These observations are taken in different environments that can have different characteristics (e.g., rural, urban, or roadside backgrounds) and be affected by potential local sources. The necessity to bring the model prediction at high resolution is connected to the representation of the concentrations inside each grid cell. In the case of simulations at coarse resolution e.g., the 6x6km domain, the value of concentration of the pollutant X is the average of an area of 36km^2 and difficult to be representative of observations taken in a particular place. On the other hand, increasing the spatial resolution and bringing it to 2x2 km the average value inside each grid cell will be representative of a smaller area such as 4 km^2. WRF and CHIMERE model work up to a maximum of spatial resolution of 1km that represent the limit for which the numerical prediction can be considered reliable but there is a not negligible difference in computational time between 2x2 and 1x1km of resolution. The decision to simulate at final resolution of 2x2 is then the combination of higher representation of the concentrations modelled with observations, high reliability of the prediction and reasonable computational times. A new paragraph has been added in the manuscript explaining this at lines **536-551**.

The authors understand that the change of spatial resolution of the emissions could introduce uncertainty in the final emissions, but the tool used for the spatial allocation – "emisurf2016" – has been proven to be mass conservative and therefore the total amount of the emission in the original cells (original resolution) is the same than in the reallocated ones (2x2 km of resolution).

Finally, the period of simulation has been chosen in agreement with the available observations from another working strand of the same project. Observations or $PM_{2.5}$ from the analysed region for long period of times (e.g., one year) are almost impossible to find without large gap in observations due to problems in the infrastructures or not reliability of the sampling methods. Moreover, the assessment of a configuration of a modelling system can give better performance in particular season than in other due to combination of meteorological and chemical parameters.

Being this, this first work assessing a modelling system for atmospheric chemistry simulation in the East African area the author decided to limit the validation and application to a period of time for where a) continuous observations were present and b) the performance of the model could be validated with more precision. A new paragraph to clarify this aspect has been added in the text at line **268-281.**

- *In the description of the CHIMERE model there needs to be a short description on how wet- and dry deposition of gases and particles are treated.*

A > A description of the dry- and wet- deposition schemes included in CHIMERE have been added at Line **171-175** of the new manuscript.

Q.I did not find the Validation of the WRF simulation particularly impressive, and I find the statement in the conclusion to be misleading "WRF has proved capable of reproducing the main meteorological patterns for all domains considered." (Line 762). For example, the overall temperature bias for the 1-month simulation is 4.1K in Nairobi – which, I believe, is the same order of magnitude as the seasonal variation of monthly mean temperatures in Nairobi. I also lack an evaluation of the WRF-model's capability of reproducing precipitation in the different modelling domains.

A > Thanks for this valuable comment. We agree that the statement as it was written was misleading and it has changed accordingly. The new line says: *"WRF showed a variable capability in reproducing the main surface weather patterns according to the different conditions of the three domains"* (Line **846-847**).

Despite this, the authors want to highlight that the validation of the WRF configuration has been done on three different domains with large differences in terms of topography and local weather conditions. The performance of the model has been assessed from the high and dry altitudes observation point in Ethiopia to locations near the Lake Victoria in Uganda. These environments are meteorological different and the validation of an individual configuration for WRF able to describe them (albeit in a defined seasonal period) represents a valuable and useful result.

The difference in absolute temperature of 4.1°C is calculated on the average of all the stations of Kenya and it is not relative to the station of Nairobi. The statistics of the individual stations (not shown in the paper) show that the bias for Nairobi is only 1.3°C. The highest bias is found for the station of Narok where the bias in temperature is around 10°C. A new paragraph explaining this has been added at line **391-400.**

The value of mean normalised bias (MNB) has been calculated considering the individual hourly data and excluding from the computation those hours not available from observation in order to have a more precise evaluation of the distance between model and data from the real world. This way of calculation has given us the possibility to exclude some periods where the observations were unavailable due to blackout in the networks or different technical issues.

It is right that a complementary evaluation of the performance of WRF should be done also taking in account rainfall. The manuscript has been modified mentioning this as an additional possibility for the differences in concentrations and observations in Nankuki but according to the reference material from Pope*, et al.* [1] no rainfall was observed in the simulated period.

Forthcoming works will be focused on the extension of the simulations over annual period and will take include the validation of the WRF configuration accounting also for rainfall events from the long- and short rain seasons in the equatorial countries.

- There are numerous typos throughout the manuscript.

A > The Manuscript has been carefully checked in all its parts and all the typos modified and corrected.

- Several of the maps provided would, in my opinion, be more useful if they, for example, show land surface type, population density, or emissions rather than height above mean sea level. The site indicators in Figs. 3, 7 and 11 need to be clearer.

A > The maps 1, 3 and 11 have been modified according to the suggestions of the reviewer. All the indicators have been increased in size, the text near made more visible and the size of the images increased in general. Maps 7 has been deleted because the information provided in Figure 3c were the same than in Figure 7. The height above the sea level is an information useful to understand the possible transport paths for the domain of Kenya and have been kept in Figure 3 for all three domains but the MODIS land use classification has been introduced for the three domains. The land use classification shows the different surface types present in the three domains.

- There are no units specified in Tables 3 and 4.

A > The units for the variables are Temperature (ºC), relative humidity (%), wind direction (degrees), wind speed (m s^{-1}) and $PM_{2.5}$ µg/m^3 and have been added to Tables 3 and 4.

Q The specified diurnal cycle of RH is wrong (row 405)

A > The line has been modified accordingly the new line says: *"Despite this both the diurnal peaks and night lowest values seems to be not correctly reproduced by the model that tends to overestimate the formers and underestimate the latter with a mean bias between -0.1 and 0.004."* Line **466-467**

- *The denominator of Eqs. (2) and (3) are in error*

A > The parenthesis has been added in the denominators of Eq. 2 and 3 in the new manuscript.

- *Explain the strict lower baseline of ca. 2 µg/m3 in modelled and observed PM2.5 in Fig. 8*

A > The observations used to validate CHIMERE performance for Kenya comes from a previous work made by Pope et al., 2018 [1]. In that work the site of Nanyuki was chosen as rural spot in a location of minimum local air pollution influence. The date from Nanyuki, in fact, in that work have been used for the calculation of the net urban increment subtracting the rural background concentrations of Nanyuki from the urban concentrations in Nairobi.

The average concentrations around 2 µg/m^3 in the period between the 4^{th} and the 11^{th} are the levels of the rural background in absence of any external influence from meteorological parameters and in absence of local sources. The peak of concentrations visible is the other days are between 4 and 15 µg/m^3 that are in any case lower in comparison with the concentrations from the urban area.

The difference in the baseline concentrations is given by the big difference between the days with possible transport of pollutants from days where this phenomenon is not visible, but it is exaggerated by the low scale of the concentrations (0-16 μg/m^3)

References:

- Pope, F.D.; Gatari, M.; Ng'ang'a, D.; Poynter, A.; Blake, R. Airborne particulate matter monitoring in Kenya using calibrated low-cost sensors. *Atmospheric Chemistry and Physics* **2018**, *18*, 15403-15418, doi:10.5194/acp-18-15403-2018.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-552/acp-2021-552-AC3-supplement.pdf