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Comment on acp-2021-552

Anonymous Referee #4

Referee 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-RC2, 2022

The paper presents a promising strategy for air quality forecast, which they have applied to large regions of three east-African countries including their capitals Kampala, Addis Abeba and Nairobi (with a special focus on Nairobi, capital of Kenya). Studies on air quality modelling are extremely scarce, so this work is, by itself novel, particularly in the fact that the authors present results for three large cities along with the corresponding data.The numerical methods used by the authors and their tools are exposed clearly and their results in terms of comparing model to observations are good (and sometimes extremely good).

However, I feel like there is more potential with this study if the authors would spend less time and space in presenting too many statistics (sometimes not relevant as I will discuss below for specific points). Statistical discussion should be refocused in model average, observation average, MFE and MFB which as the authors themselves recognize are better suited to their study than the metrics they use first. It is interesting that the model performs correctly relative to observations, but once this is done the reader feels he has the right not to see more statistics bu to get insight on the physics and chemistry that are at play : if the model provides a relatively correct assessment of the PM time series, then it would be very interesting to know what the model tells us on the composition of PM (and therefore, possible, on its sources). Is it made of mineral dust, primary anthropogenic contaminants, SOA ? If the model is correct on specific station, then we would like to see a map of contamination that it produces for the entire simulation domain (that of Fig. 7 for example) : is Nairobi the maximum for the entire domain, are there other hot spots ? How id pollution channeled – or not – between the mountain slopes ? In its current shape, this article sometimes looks like a technical report on the feasability of a particular forecast system for specific regions, which is not really what is expected from a research article. I think that with the additions above, this article could give many more indications on the specificities on Particulate matter composition in this region, and yield more interesting questions for future research.

I feel this article will deserve publication because they obtain a great performance in reproducing pollution in areas where this has rarely been attempted ; Once major changes are brought (making the statistical discussion more straightforward and give more scientific material from the model outputs), I feel that this may become a beakthrough article for air quality modelling in Africa.

MAJOR COMMENTS

A. Statistics on wind

l. 362 : it is unclear what is meant by wind direction » and its unit. The vocabulary used i not appropriate for wind direction (« higher » wind direction has no meaning in my opinion). Mean Normalized bias has no clear meaning either in this sense (if as I think wind direction is in degree). Authors should explain how they build their indicators for wind. For calculating the RMSE and MNB, how do they account for the difference between a wind oriented at 1° and one at 359° ? they are close but the difference between them is large. In general, MNB and RMSE are not adapted to deal with angles. Even the average does not make sense (average between 1° and 359° is 180° which does not make any sense…). I suggest that the authors remove the statistics they have done for wind direction (and possibly replace them by a more meaningful way to do these statistics, e.g. average and standard deviation of the geometric angle between observed and modelled wind speed). An alternative is to rely mostly on Fig. 5.

B. Error on wind speed ?

Wind speed seems largely and critically false for the Kenya domain (Table 3). Could the authors double-check their numbers ?

C. Use the same metrics throughout the paper

l. 528 : here the authors argue (insightfully in my opinion) that MFB and MFE have less problems than MNB and NRMSE which they use above. Why not use MFB and MFE throughout the paper ? MFB and MFE could be calculated in time-average foe all stations in the first place, given in table 4, and Table 4 could bbe used to discuss the results relative to the Boylan and Russel criteria. This would be less confusing than the current presentation, and would avoid needing Fig. 6 which is an unusual figure style and, lacking the time dimension, does not bring much more understanding to the reader.

D. What do the authors mean by « coupled models » ?

Usually, coupled modelling means that the chemistry-transport model is able to give a feedback to the meteorogical model (both models runing at the same time, similar to a general circulation model with atmosphere + ocean). This is not the case here, so the expression « coupled models » is confusing and should be removed from the text.

Minor comments / typos

l. 75 : lon-term ☐ long-term

l. 148 : is it really two-way nesting (do the small domains retroact on the large one?) ? Otherwise this is one-way nesting.

l. 343 : why such a massive underestimation for temperature in Kenya ? Such a difference would strongly affect photochemistry isn't it ?

l. 392 MNB is not really significant here, it would depend if temperature is expressed in K or °C. In any case, 0.1 MNB is not small at all (relative to an average value of 20° this is a 2° bias which is not small). I see something strange in the numbers presented in Table 3. There is a negative bias of 4.1° relative to an observed mean of 23.2° so with the typical definition of the mean bias ((<M> - <O>) / <O>) I would expect a MNB ~0.18 while the authors claim the MNB is 0.1 here. This looks underestimated. I am aware of the difference between NMB and MNB and I don't have the data to recalculate the MNB here (see the definitions in e.g. https://www.bnl.gov/envsci/schwartz/pres/metrics.pdf). Could the euthors explain what exact definition they have retained for the mean normalized bias and how they deal with missing data ? This all the more surprising as for UGA2K and ETH2K I find exactly the same number as in Table 3 by calculating <M> - <O>) / <O>. This is also an argument to just drop MNB which behaves in a confusing way, as suggested in my major comment C.

l. 453 : « negligible » is not the correct appreciation for biases up to 4°C in temperature.

Table 4 : The figures don’t always make sense. For Addis Abeba, there is a MNB 0.1 for daily data but 0.88 for hourly data, this is much likely affected by data points with an extremely small denominator driving the entire average. If the model yields 2.0 and 2.0 , and if the observations yield 0.1 and 3.9 then the MNB would be 0.5 * (1.9 / 0.1 – 1.9 / 3.9) ~10 which hardly makes any sense. Again, see my major comment C.

Fig. 6 : This figure is not really useful, just the statistics on MFB and MFE would be sufficient for the understanding. I do not think their distribution as a function of concentration really brings something to the discussion.

Fig. 8 : please use the same x-axis for both panels to ease reading the figure

Fig. 9 : It is not useful to compare modelled values in Nanyuki to observed values in Nyeri, 60km away in a mountain / plateau environment. No statistical link between the two timeseries can be expected a priori. I do not understand the point of the authors here, this should maybe be explained more.

l. 643 : the authors claim that Nyeri is 0.43°N but n 10 on Fig. 7 seems to e at 0.43°S (or at least clearly nor 0.43°N)