Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-519-RC3, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of regional climate change and future emission scenarios on surface O_3 and $PM_{2.5}$ over India" *by* Matthieu Pommier et al.

Anonymous Referee #2

Received and published: 14 August 2017

The manuscript describes numerical experiments of modelling of surface O3 and PM2.5 concentrations over India using EMEP's regional off-line chemical transport model. To facilitate comparisons between present levels of air pollutants and future concentrations -after assumed changes in air pollutant emissions and in climate- the EMEP model is fed with meteorological data from a regional climate model. To my knowledge is this the first study of its kind covering the Indian subcontinent and as such the work deserves to be published.

The manuscript is well written, without any omissions and the results are, mostly, clearly presented. The manuscript could be published in its present form but it would

definitely gain from tough editing. There is an overwhelming amount of figures included in the main text which distracts the reader from any clear take-home messages. My personal feeling is that the authors want to pack too much into the present paper – which already comes with a comprehensive Supplement. The ratio between text and figures is low; chapter 5.1, for example, discusses 3 figures (altogether 21 panels) in 14 lines of text.

General comments:

Although the average seasonal cycle of O3 seems to be reasonably resolved by the EMEP model in the reference simulation (inferred by the similarity of the curves in Fig. 2a; it is not so meaningful to calculate the correlation of the 12 monthly averages of O3), is the mean bias of O3 substantial. The authors attribute this flaw to the fact that they compare the output from a regional model with observations from urban locations. I am perfectly aware of the paucity of data from regional background stations in India but the dissimilarity of station type raises concern about the validity of the model evaluation. From Fig. 3c it is clear that O3 concentrations are also overestimated during large part of the year at the available rural stations. Can the general overestimation be attributed to imperfect boundary concentrations? PM2.5 is surprisingly well reproduced by the EMEP model.

The introduction of small, rectangular, sub-regions in Fig. 9 and onwards is confusing. The selected areas don't cover all the grid-cells with the characteristics that the authors want to highlight (e.g. positive correlation between changes in O3 deposition velocity and near-surface concentration). Re-usage of the numbers 1, 2, 3 in Figs. 9, Fig. 12 and Fig. 14 further adds to the confusion. If the different sub-regions should be retained in the presentation they should be given unique numbers.

In the discussion of the results of section 5.1 and 5.2 model results have been averaged over a rectangular subdomain (shown in Figs. 13a and 16a) covering vastly different countries, socio-economical and geographical regions. I find this choice arbitrary.

To focus the presentation I would recommend the authors to consider excluding the 2026-2035 results as I don't think they add much to the general understanding of the evolution of O3 and PM2.5 from present times into the future.

Minor editorial/technical issues:

L 278: "-6%" in Fig. 5a it is +6%

L. 290-293: "It is worth nothing . . . for Hyderabad." Unclear what you want to say with these sentences here.

L 363: "Eastern" and "Western" are shifted

The appendix is never mentioned in the main text. "Mean normalized Gross Error (MNGE)" is probably a valid term but I would perform the more descriptive term "Mean normalised absolute error". The formula for NMB is in error (1/N is missing).

It is unnecessary to label the increasing and decreasing O3 with A and B in Fig. 7. These areas are quite visible any way.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-519, 2017.