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 #1

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The authors used the EMEP/MSC-V model to predict the future air quality changes in India under both the climate and emission changes. The topic is not novel, especially under the RCPs scenarios. However, I do acknowledge that data analysis with high-resolution model simulations over the India are not presented frequently before.

General comments:

For the model evaluations, the authors concluded that overestimation of the ozone by 35% may be caused by the underestimation in NOX titration by the model. However, I am wondering whether the overestimation would be related to the O3 measurements the authors chose. From Fig. S1, the majority of the O3 measurement work used for the

evaluation are not adjacent to the year 2011 which was the emission year. I understand reliable observation data are scarce in India, but I presume the O3 concentration in India has been increasing for the past years. I wonder how will that affect the model evaluation performance. Please clarify.

The authors are strongly suggested to present the future climate changes in both the 2030s and 2050s, such as the temperature and precipitation. The authors discussed the effects of winds on the air pollutants. So the future changes in wind speed and directions are also necessary too.

I am not in favor of the conclusions that the O3 variations under the future climate change were caused by the O3 dry deposition changes. The authors did spend time to show the O3 dry deposition changes, but I didn't see how the authors could relate these DD changes to the O3 air quality changes. Please clarify.

I don't understand why the authors keep defining different regions for the data analysis, e.g. Figs. 7, 9, 10, 12, 13, 14. It is really not readers friendly and annoying. I have to keep going back to different figures to check which regions the authors were discussing about. I suggest the authors report air quality changes based on several larger regions consistently in the paper, or one region as the domain defined in Fig. 13.

Too many figures in the main context. I suggest move some of them to the supporting, such as Figs 8, 10, 15.

The authors should improve their writings. Lots of sentences could be combined or trimmed. I will give some examples in the specific comments.

Specific comments:

L18: change "calculate changes" to "predict changes".

L65-L66: rewrite this sentence. This is not even a complete sentence.

L77-L83: I suggest the authors to include the following two papers for summarizing the

interactions between air quality and climate change: Fiore et al., 2012, 2015.

L85: Suggest to add these two references: Silva et al., 2013; Lelieveld et al., 2015.

L94: change "but O3 has" to "and has".

L100-107: the authors should discuss more clearly about the primary PM and secondary PM as these concepts were used in the late results, otherwise it may lead to confusing. For example, in L104, the authors discussed that the "PM2.5 also includes secondary particles" which sounds to me that the authors were saying these secondary particles were at the same level as sulfate, nitrate, ammonium and so on.

L124-L128: just state the fact that this paper uses the EMEP model (rv4.9, Simpson et al., 2016), which includes some important updates such as the gas-phase reactions and aerosols compared with the previous version (Simpson et al., 2012). Discuss more in detail about the aerosol mechanisms.

L129: changed to "global scale modelling has been possible for many years (Jonson et al., 2010, 2015; Wild et al., 2015)".

L140: the author should also discuss whether the model includes the online dust module as the dust concentration would also change due to climate change too.

L144: I am confused about the model setups. So did the authors run 1-yr spin-up for each scenarios, and then run the 10 years consecutively, or did they run 1-yr spin-up for each of the 10 years simulation? "ten 1-year simulations" makes me think the authors run these 10 years simulation individually, and for each year they have their own spin-up.

L152: what does the author mean by "their respective baseline year"?

L174-L205: the authors spent great efforts to discuss the differences for the emissions between Sharma and Kumar, with ECLIPSE v5a, which makes me wonder whether the authors have chosen the best emission scenario for their simulations. Why not choose

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the emission projections under the RCP8.5 which is public available and free, and also will be consistent with future climate change used in this study (Gao et al., 2012; Zhang et al., 2016).

http://tntcat.iiasa.ac.at:8787/RcpDb

The RCPs also have the NH3 emissions.

L209: delete "since the NH3 emissions from . . .."

L217: change "in order to give confidence in" to "and give confidence in"

L233: modify the "ca.130%".

L243: change "Sharma et" to "Sharma and".

L323: show the correlation for the delta O3 and delta T.

L345-L346: I am not convinced of the VOC-sensitive regime by only seeing that NOx decreases and NMVOCs increases in winter. The decreases/increases for NOx and NMVOCs are slightly (Fig. 10), and how did the authors imply there are VOC-sensitive?

L362-363: "In both FC scenarios, an increase in surface PM2.5 concentrations is predicted for the Eastern part of the domain (Arabian Sea) and a decrease over the Western part of the domain (Bay of Bengal)." I think they should be the opposite?

L436-437: "These increments alone are comparable to, or double" Rewrite this sentence with the previous one. It's really confusing.

L455: In the conclusion, the authors should discuss more about the uncertainties associated with this study, for example why the authors didn't choose the future emissions under the RCP8.5 instead of the Sharma and Kumar, 2016. How would that affect the results? This study also didn't consider the intercontinental transport of the air pollutants on the effect of surface air quality in India, which was implied to be important source in THE US (Nolte et al., 2008; Zhang et al., 2016).

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L462: "emissions is the main cause" to "emissions are the main cause"

L467-L468: "Climate change leads to increases in the PM2.5 levels at short and medium-terms, reaching 6.5% (4.6 μg/m3) by the 2050s." So these "6.5%" change is regional average or domain average. It is confusing in both the abstract and conclusions since the authors keep define new regions for the analysis.

Page 37: change the colorbar for region1, region2. The fractions of the PM2.5 components were not clearly seen with the high y axis.

References:

Fiore et al., 2012, Chem Soc Rev, Global air quality and climate; doi: 10.1039/c2cs35095e.

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Gao et al., 2013, Atmos. Chem. Phys., The impact of emission and climate change on ozone in the United States under representative concentration pathways (RCPs), doi: 10.5194/acp-13-9607-2013.

Lelieveld et al., 2015, Nature, The contribution of outdoor air pollution sources to premature mortality on a global scale, doi:10.1038/nature15371.

Nolte et al., 2008, Journal of Geophysical Research, Linking global to regional models to assess future climate impacts on surface ozone levels in the United States, doi:10.1029/2007JD008497.

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