Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-519-RC1, 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 #3

Received and published: 11 August 2017

In this paper, the authors use the EMEP/MSC-V chemical transport model and investigate potential impact of changes in climate and emissions to 2050 on surface levels of ozone and particulate matter over the Indian sub-continent. This is the first time the EMEP model is used over this region, and the simulated present-day distribution of ozone and PM2.5 is evaluated against a range of observations. The model is then run with downscaled meteorological data and emission scenarios for 2030 and 2050.

While both climate/chemistry interactions and future pollution levels have been extensively studied, this paper contributes with additional, detailed information over a region where emissions are expected to contribute to increase strongly in the near-future. The

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Discussion paper

paper also present a useful documentation of model performance in a region where measurements have been less readily available. The paper is well-structured and well-written. I have some comments and questions for the authors to consider before the paper can be accepted, but I believe these are relatively straightforward to incorporate.

Comments:

Section 2: The model set up section should include a brief description of how aerosols are treated in the model. While possible to find in the cited literature, it will be very useful for the reader to get this information here.

Line 140: any contribution from sea salt?

Line 191: language – higher/stronger instead of much more?

Line 206: the downscaled meteorological data comes from model runs with the RCP8.5 emissions and a brief comparison of the 2030/2050 emissions in this scenario would be useful – do they differ considerably from the scenarios used in the FCE simulations?

Line 244: could the authors compare with remote sensing data? The EMEP model was part of the multi-model study by Quennehen et al (2016) – anything to learn from this? Related to this, has there been studies with EMEP over other regions that show similar problems with ozone? (e.g., Huang et al. 2017).

Line 255: "lack of aerosol effects"? Please clarify/expand.

Line 277-284: presumably there has been an increase in emission over the two different periods covered by the measurements. Could that have an impact of the comparison?

Line 295-300: even with more recent inventories, uncertainties in emissions persist, which could be worth noting/discussing.

Line 315: as well as potential changes in vegetation in a different climate.

Line 314-317: This is an important source of uncertainty. Are there any estimates in the literature of the potential magnitude of uncertainties introduced by this caveat? Is it large enough to affect the conclusion in this paper?

Line 323: could the authors include some information about the temperature change and its statistical significance in 2030 and 2050?

Line 353: the three regions highlighted in the 2030 results are not the same as in 2050, but are there similar explanations? Please elaborate. Could it be that the changes seen in these quite small regions are more random, than caused by the development in climate and emissions?

Line 353-354: there is something strange with this sentence. Is “expected” the right word here, or should it be “except”?

Section 4: I would like to see some discussion about the uncertainties in model representation (meteorological data) of monsoon and projected future changes, and how this could affect the results.

Line 368: see first comment – some information about how the model treats wet removal would be useful.

Line 391: changes in precipitation and wind speed will affect the dust production as well. Have the authors looked at this?

Line 401: should this be “change” instead of “variation”?

Section 5: since the motivation for this paper is partly the detrimental effects of air pollution on human health, it could be interesting to also quantify changes in terms of variables such as daily maximum 8-hour concentration if high temporal resolution model data is available and to discuss PM2.5 concentrations in terms of current air quality standards. This would make a nice, policy relevant addition. Right now, the paper focuses more on the details surrounding the smaller impact of climate change, making it somewhat unbalanced.

Line 446: please add boxes indicating regions in Fig. 16.

Line 454: Presumably, the overall PM2.5 change results in an increase in the absolute amount of EC as well, so I'm not sure about the phrasing here, i.e., "amount of EC remain low". Suggest rephrasing. Given the importance of EC/BC from a climate perspective this is an important distinction. How are PM2.5 emissions split between EC and OM in the model? (see also first comment)

Lines 449 – 454: it is interesting to note that even under increasing anthropogenic emissions, a significant fraction of PM2.5 comes from sources (dust and SOA) that are challenging, if not impossible, to control by changing policy.

References: Quennehen, B., Raut, J.-C., Law, K. S., Daskalakis, N., Ancellet, G., Clerbaux, C., Kim, S.-W., Lund, M. T., Myhre, G., Olivié, D. J. L., Safieddine, S., Skeie, R. B., Thomas, J. L., Tsyro, S., Bazureau, A., Bellouin, N., Hu, M., Kanakidou, M., Klimont, Z., Kupiainen, K., Myriokefalitakis, S., Quaas, J., Rumbold, S. T., Schulz, M., Cherian, R., Shimizu, A., Wang, J., Yoon, S.-C., and Zhu, T.: Multi-model evaluation of short-lived pollutant distributions over east Asia during summer 2008, Atmos. Chem. Phys., 16, 10765-10792, https://doi.org/10.5194/acp-16-10765-2016, 2016.

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