Reviewer 3

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 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.

The authors would like to thank the reviewer 3 for the detailed comments, which help to improve the manuscript. We have tried to clarify the points raised by the reviewer and to answer all remarks. Our responses are written in blue in this document. Please note that Figs 8, 10, and 15 have been moved to the supplement. Furthermore, the former Figs. 14 and S10 do not present the distributions for the FCE2030 scenario anymore.

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.
The model description of Sect.2 has been re-written as below:

"The EMEP model is a 3-D Eulerian model described in detail in Simpson et al. (2012), but for global scale modelling, some important updates have been done. Although the model has traditionally been aimed at European simulations, global scale modelling has been possible for many years (Jonson et al., 2010; 2015a; Wild et al., 2012). These updates, resulting in EMEP model version rv4.9 as used here, have been described in Simpson et al. (2016) and references cited therein. The main changes concern a new calculation of aerosol surface area (now based upon the semi-empirical scheme of Gerber, 1985), revised parameterizations of N_2O_5 hydrolysis on aerosols, additional gas-aerosol loss processes for O_3, HNO_3 and HO_2, a new scheme for ship NO_x emissions, and the use of new maps for global leaf-area (used to calculate biogenic VOC emissions) – see Simpson et al. (2015) for details. The value of the N_2O_5 uptake coefficient (γ_{N2O5}) is very uncertain, but here we use the 'SmixTen' scheme described in 2015, which seemed to provide the best predictions of O_3 for global O_3 sites with this model version. In addition, the source function for sea salt production was updated to account for whitecap area fractions, following the work of Callaghan et al. (2008).
The domain of each simulation covers the latitudes 5.6°N-40.7°N and the longitudes 56.2°E-101.7°E, and the horizontal resolution of the simulations follows the resolution of the meteorological data described in Section 2.1. However, the studied region is more centered over India (e.g. Fig. 4b).
As in the standard EMEP model, the boundary conditions for most $PM_{2.5}$ components are defined as prescribed concentrations (Simpson et al., 2015), and O_3 boundary conditions (lateral and top) are defined by the climatological O_3 data from Logan (1998). For dust, concentrations

from a global simulation for 2012 (EMEP Status Report 1/2015) have been used as boundary conditions. The influence of the changes in inflow of O_3 or $PM_{2.5}$ from outside the Asian domain is not taken into account.
PM emissions are split into EC, OM (here assumed inert) and the remainder, for both fine and coarse PM. The OM emissions are further divided into fossil-fuel and wood-burning compounds for each source sector. As in Bergström et al. (2012), the OM/OC ratios of emissions by mass are assumed to be 1.3 for fossil-fuel sources and 1.7 for wood-burning sources. The model also calculates windblown dust emissions from soil erosion, but these emissions are negligible over our studied domain compared to the dust transported from the boundary conditions.
Secondary $PM_{2.5}$ aerosol consists of inorganic sulphate, nitrate and ammonium, and SOA; the latter is generated from both anthropogenic and biogenic emissions (ASOA, BSOA respectively), using the 'VBS' scheme detailed in Bergström et al (2012) and Simpson et al (2012).
The main loss process for particles is wet-deposition, and the model calculates in-cloud and sub-cloud scavenging of gases and particles as detailed in Simpson et al (2012). Gas and particle species are also removed from the atmosphere by dry deposition. Calculations of O_3 deposition in the EMEP model are rather detailed compared to most chemical transport models. We make use of the stomatal conductance algorithm (now commonly referred to as DO3 SE) originally presented in Emberson et al. 2000, 2001), which depends on temperature, light, humidity and soil moisture. Calculation of non-stomatal sinks, in conjunction with an ecosystem specific calculation of vertical O_3 profiles, is an important part of this calculation as discussed in Tuovinen et al. (2004, 2009) or Simpson et al. (2003). The methodology and robustness of the calculations of O_3 deposition and stomatal conductance have been explored in a number of publications (Tuovinen et al. 2004, 2007, 2009, Emberson et al., 2007, Büker et al., 2012).
An initial spin-up of one year (2005) was conducted, followed by ten 1-year simulations from 2006 to 2015. Each simulation was used as spin-up of the following year of simulation. The initial spin-up (2005) was excluded from the analysis. To conduct the evaluation on the impact of future climate, similar runs were done with spin-ups of one year (2025 and 2045), followed by ten 1-year simulations from 2026 to 2035 and from 2046 to 2055, respectively. In this way, short-term (towards 2030) and medium-term (towards 2050) future climate changes have been analyzed. These short-term and medium-term Future Climate (FC) scenarios used the same anthropogenic emissions as the reference scenario. In addition to the climate change, the impact of the future emission scenarios was investigated by using anthropogenic emissions for the 2030s and the 2050s. These simulations, referred to as Future Climate and Emissions (FCE) scenarios, were run for the same time periods as the FC scenarios, but used emissions for their respective baseline year (2030 for the 2030s and 2050 for the 2050s). In order to simplify the reading, the four future scenarios are named as FC2030, FC2050, FCE2030 and FCE2050."

With the corresponding references:
- Bergström, R., Denier van der Gon, H. A. C., Prévôt, A. S. H., Yttri, K. E. & Simpson, D., Modelling of organic aerosols over Europe (2002-2007) using a volatility basis set (VBS) framework: application of different assumptions regarding the formation of secondary organic aerosol, Atmos. Chem. Physics, 2012, 12, 8499-8527
- Callaghan, A., de Leeuw, G., Cohen, L., and O'Dowd, C. D.: Relationship of oceanic whitecap coverage to wind speed and wind history, Geophys. Res. Lett., 35, L23 609, doi:0.1029/2008GL036165, 2008.
- Gerber, H. E. Relative-Humidity Parameterization of the Navy Aerosol Model (NAM) Naval Research Laboratory, Naval Research Laboratory, 1985.

- Simpson, D., Tsyro, S., and Wind, P.: Updates to the EMEP/MSC-W model, Transboundary particulate matter, photo-oxidants, acidifying and eutrophying components. EMEP Status Report 1/2015, The Norwegian Meteorological Institute, Oslo, Norway, 2015, 129-138, ISSN 1504-6109, 2015.

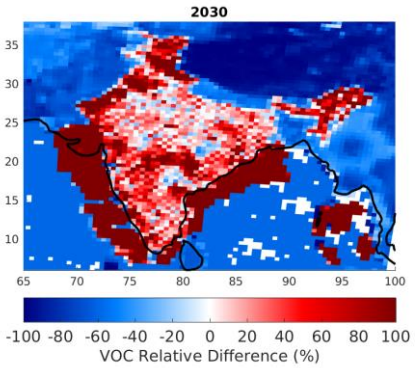
Line 140: any contribution from sea salt?
We have clarified this point by changing the paragraph:
"As in the standard EMEP model, the boundary conditions for most $PM_{2.5}$ components are defined as prescribed concentrations (Simpson et al., 2015), and O_3 boundary conditions (lateral and top) are defined by the climatological O_3 data from Logan (1998). For dust, concentrations from a global simulation for 2012 (EMEP Status Report 1/2015) have been used as boundary conditions. The influence of the changes in inflow of O_3 or $PM_{2.5}$ from outside the Asian domain is not taken into account."

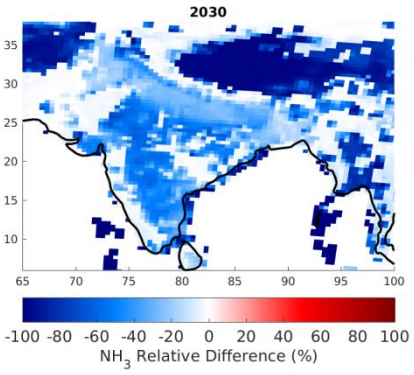
Line 191: language – higher/stronger instead of much more?
It has been changed to "larger".

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?
The following maps, representing the relative difference between the emissions used for our FCE scenarios and the RCP8.5 emissions for the corresponding year, show the larger increase in the emissions compared to RCP. Only the NH_3 emissions, which are from ECLIPSE, are lower than the RCP8.5 emissions.
In the RCP8.5 emissions inventory, only elemental carbon and organic carbon emissions are reported and not $PM_{2.5}$ and PMcoarse, as explained in Zhang et al. (2016). Moreover, as described in Amann et al. (2013), the RCP scenarios were mostly designed for greenhouse gases, they were not developed with a primary focus on air pollution concerns.
The RCP scenarios employ a range of assumptions on climate policies and also assume for all countries additional control measures for air pollutants in the future beyond those currently included in national legislation.

Amann, M., Klimont, Z., and Wagner, F.: Regional and Global Emissions of Air Pollutants: Recent Trends and Future Scenarios, Annu. Rev. Environ. Resour., 38:31–55, doi: 10.1146/annurev-environ-052912-173303, 2013.

Zhang et al., 2016, Atmos. Chem. Phys., Co-benefits of global and regional greenhouse gas mitigation for US air quality in 2050, doi:10.5194/acp-16-9533-2016.





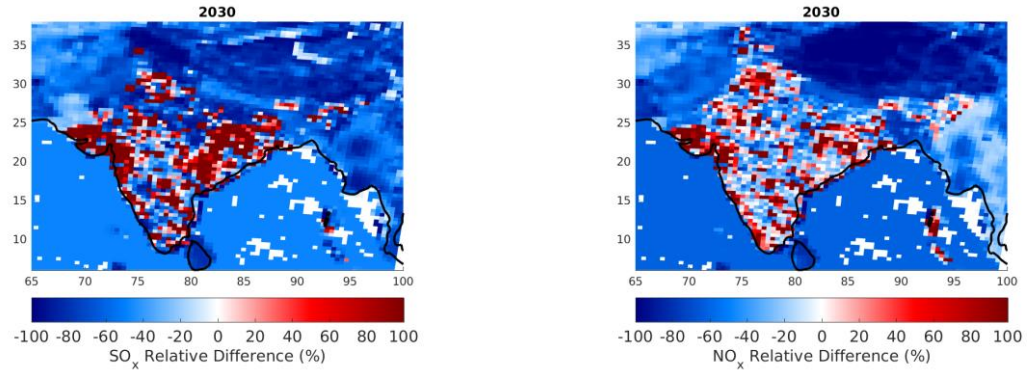


Fig. Relative difference (in %) between the NMVOC, NH_3, SO_x and NO_x emissions used in our work for the FCE2030 scenario and the RCP8.5 (for the baseline year 2030). The relative difference is calculated as: [(our work – RCP8.5) / RCP8.5] × 100%.

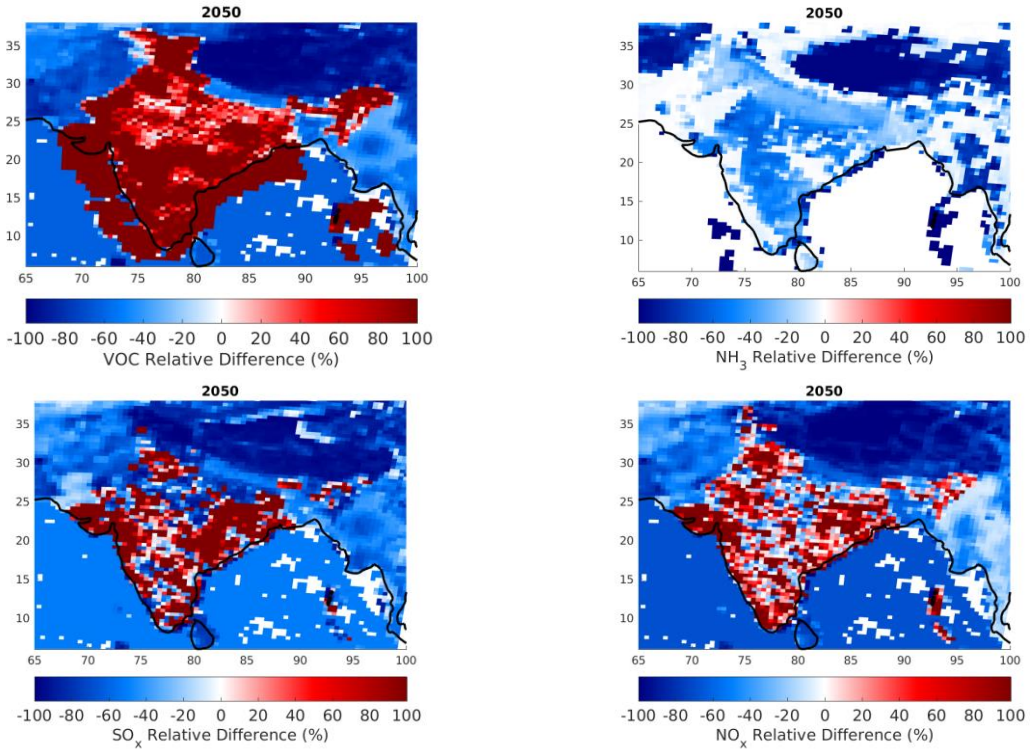


Fig. Relative difference (in %) between the NMVOC, NH_3, SO_x and NO_x emissions used in our work for the FCE2050 scenario and the RCP8.5 (for the baseline year 2050). The relative difference is calculated as: [(our work – RCP8.5) / RCP8.5] × 100%.

We have added these sentences:
"It is also interesting to note that the emissions used in the FCE scenarios are higher than the emissions used in the RCP8.5 scenarios for all species over India, except NH_3 (not shown). One of the drawback of these RCP8.5 emissions is that only elemental carbon and organic carbon emissions are reported and not $PM_{2.5}$ and PMc_{oarse} emissions (e.g. Zhang et al., 2016). Moreover, the RCP scenarios were not developed with a primary focus on air pollution concerns but for greenhouse gases (e.g. Amann et al., 2013)."

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).
- To compare with remote sensing data is a good idea; however, we decided not to perform such a comparison for three main reasons:
1) Our study focuses on surface O_3 and the satellite retrievals are mainly sensitive to the tropospheric column as the 0-6 km column for IASI (e.g. Safieddine et al. 2016) or the lowermost troposphere (0-3 km, see Cuesta et al., 2013).
2) To perform a proper comparison with satellites, we'd need to use their averaging kernels. To do so, we'd have to apply the averaging kernels to our O_3 profiles. To calculate and to store this amount of data (O_3 profiles) corresponding to a 10-yr period with our domain horizontal resolution would be too time and diskspace demanding.
3) Moreover, as the model levels are limited to ~20km, we'd have to complete these O_3 profiles by a climatology or other information for the altitudes above, in order to apply correctly the satellite AKs (e.g. see similar issues with aircraft profiles in Pommier et al., 2012). That's possible, but this climatology will have a no negligible impact on the comparison.

- Cuesta, J., Eremenko, M., Liu, X., Dufour, G., Cai, Z., Höpfner, M., von Clarmann, T., Sellitto, P., Foret, G., Gaubert, B., Beekmann, M., Orphal, J., Chance, K., Spurr, R., and Flaud, J.-M.: Satellite observation of lowermost tropospheric ozone by multispectral synergism of IASI thermal infrared and GOME-2 ultraviolet measurements over Europe, Atmos. Chem. Phys., 13, 9675-9693, https://doi.org/10.5194/acp-13-9675-2013, 2013.
- Pommier, M., Clerbaux, C., Law, K. S., Ancellet, G., Bernath, P., Coheur, P.-F., Hadji-Lazaro, J., Hurtmans, D., Nédélec, P., Paris, J.-D., Ravetta, F., Ryerson, T. B., Schlager, H., and Weinheimer, A. J.: Analysis of IASI tropospheric O3 data over the Arctic during POLARCAT campaigns in 2008, Atmos. Chem. Phys., 12, 7371-7389, https://doi.org/10.5194/acp-12-7371-2012, 2012.
- Safieddine, S., Boynard, A., Hao, N., Huang, F., Wang, L., Ji, D., Barret, B., Ghude, S. D., Coheur, P.-F., Hurtmans, D., and Clerbaux, C.: Tropospheric ozone variability during the East Asian summer monsoon as observed by satellite (IASI), aircraft (MOZAIC) and ground stations, Atmos. Chem. Phys., 16, 10489-10500, https://doi.org/10.5194/acp-16-10489-2016, 2016.

- The work done by Quennehen et al. (2016) over East Asia already gave an indication of an overestimation in O_3 by EMEP. However, this study and our work are difficult to compare, since the domains studied are not the same, the model version and the emissions are different and the work done by Quennehen et al. (2016) only focused on summer 2008. Moreover, no comparison with surface O_3 over India is presented in Quennehen et al. (2016).

- The bias in O_3 was already shown. We have added these sentences (in bold):
"The overestimation in O_3 found in this work is in agreement with previous studies (e.g. Kumar et al., 2012; Chatani et al., 2014; Sharma et al., 2016), although of course there are many differences in both emissions and models between these studies. **It has also been noted that the EMEP model slightly overestimates O_3, especially with the global version of the model in spring and in winter (e.g. Jonson et al., 2015b). This bias can however be impacted by the parameters used as for example the boundary conditions and the emissions. Stadtler et al. (2017) who used PANHAM anthropogenic emissions also reported an overestimation in O_3 over different regions such as Asia.**"

With the corresponding references:

- Jonson, J., Semeena, V., and Simpson, D., Global ozone bias Transboundary particulate matter, photo-oxidants, acidifying and eutrophying components. Status Report 1/2015, The Norwegian Meteorological Institute, Oslo, Norway, 115-128, ISSN 1504-6109, 2015b.
- Stadtler, S., Simpson, D., Schröder, S., Taraborrelli, D., Bott, A., and Schultz, M.: Ozone Impacts of Gas-Aerosol Uptake in Global Chemistry Transport Models, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-566, in review, 2017.

Line 255: "lack of aerosol effects"? Please clarify/expand.
The sentence has been changed (in bold):
"Some possible reasons for this might be problems with the anthropogenic and/or biogenic emissions, or over-active chemistry, e.g. over-predictions in photolysis rates for Indian conditions (**as EMEP photolysis calculations assume standard atmospheric conditions, and thus do not account for attenuation of radiation due to enhanced aerosols over polluted regions**) or problems with heterogeneous reactions."

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?
Yes, indeed. The following sentence has been added:
"It is also probable that a change in the emissions and thus in the observed $PM_{2.5}$ concentrations between the periods of both data sets has an impact on the comparison."

Line 295-300: even with more recent inventories, uncertainties in emissions persist, which could be worth noting/discussing.
We have added this sentence:
"It is also important to recall that, even with the use of recent inventories, uncertainties in emissions may persist (e.g. Saikawa et al., 2017)."

Saikawa, E., Trail, M., Zhong, M., Wu, Q., Young, C. L., Janssens-Maenhout, G., Klimont, Z., Wagner, F., Kurokawa, J., Nagpure, A. S., and Gurjar, B. R.: Uncertainties in emissions estimates of greenhouse gases and air pollutants in India and their impacts on regional air quality, Environ. Res. Lett., 12, 6, 065002, https://doi.org/10.1088/1748-9326/aa6cb4, 2017.

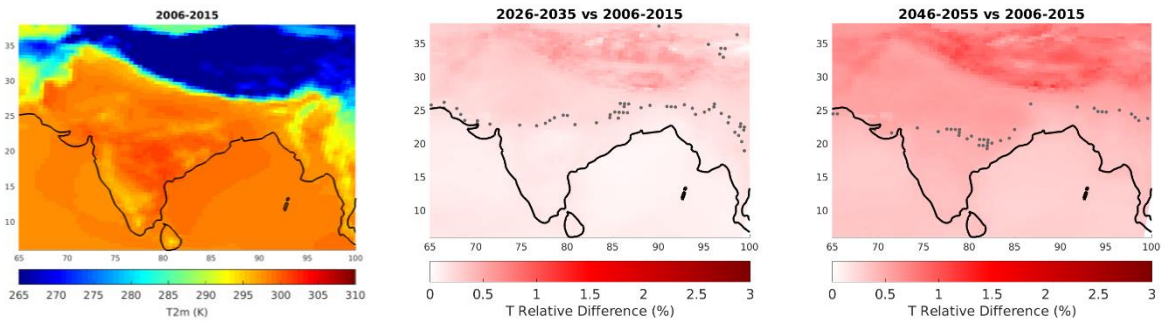
Line 315: as well as potential changes in vegetation in a different climate.
Yes, we have added that comment into the sentence; see our answer to your next comment.

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?
Yes, this is an important issue but the uncertainties cannot really be quantified or properly addressed. We have however added the following text (in bold):
"As our model does not include any CO_2 inhibition effect on isoprene emissions (e.g. Guenther et al., 1991; Arneth et al., 2007), **or potential changes in vegetation in a different climate**, these biogenic emissions are simply a function of temperature and increase in the FC scenarios. **The uncertainties associated with these assumptions are however difficult to quantify**. **For example, Hantson et al., (2017) found global isoprene emissions for the period 2071-2100 to be 544 TgC/yr without CO_2 inhibition, but only 377 TgC/yr with this effect (i.e -31%). For monoterpenes the equivalent figures were 35.7 TgC/yr and 24.8 TgC/yr (also -31%). Young et al. (2009) estimated even bigger changes for isoprene, from 764 TgC/yr to 346 TgC/yr, and showed that this uncertainty can indeed have strong effects on surface O_3 levels. The largest changes were found in South America and Africa, though annual**

changes over India were only around 5-10%. Although significant, these changes are model estimates only. The experimental data behind the CO_2 inhibition effect are extremely limited, and as noted in Simpson et al. (2014) and reference therein, current knowledge is insufficient to make reliable predictions on this issue."

With the corresponding references:
- Hantson, S., Knorr, W., Schurgers, G., Pugh, T. A. M., and Arneth, A.: Global isoprene and monoterpene emissions under changing climate, vegetation, CO_2 and land use, Atmos. Env., 155, 35-45, https://doi.org/10.1016/j.atmosenv.2017.02.010, 2017.
- Young, P. J., Arneth, A., Schurgers, G., Zeng, G., and Pyle, J. A.: The CO_2 inhibition of terrestrial isoprene emission significantly affects future ozone projections, Atmos. Chem. Phys., 9, 2793-2803, https://doi.org/10.5194/acp-9-2793-2009, 2009.

Line 323: could the authors include some information about the temperature change and its statistical significance in 2030 and 2050?
In the following figure, we have plotted the distribution of the temperature at 2 meter and the relative difference for both FC scenarios:



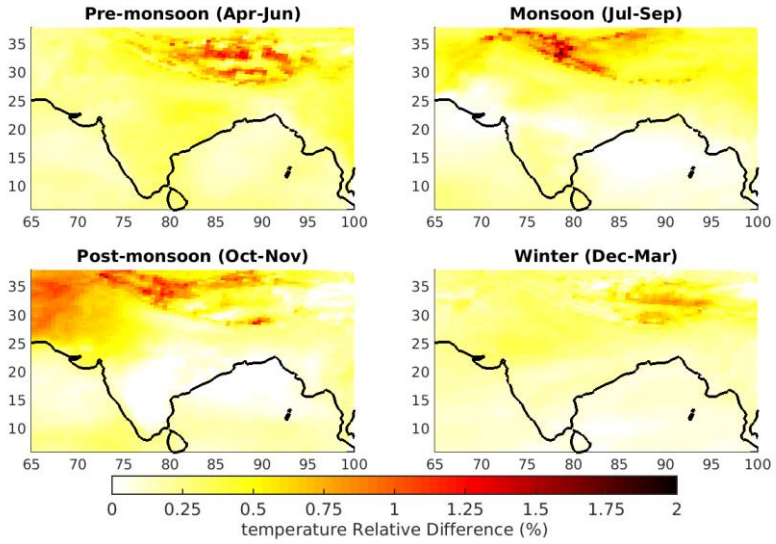
Note that the grey points, on the distribution of the relative difference, show the grids that do not satisfy the 95% level of significance.

We have added this information (highlighted in bold) in the text:
"This shows that for both FC scenarios, **even though the change in temperature is statistically significant (not shown),** other processes are occurring which impact on the thermal influence on the photochemical production of O_3."

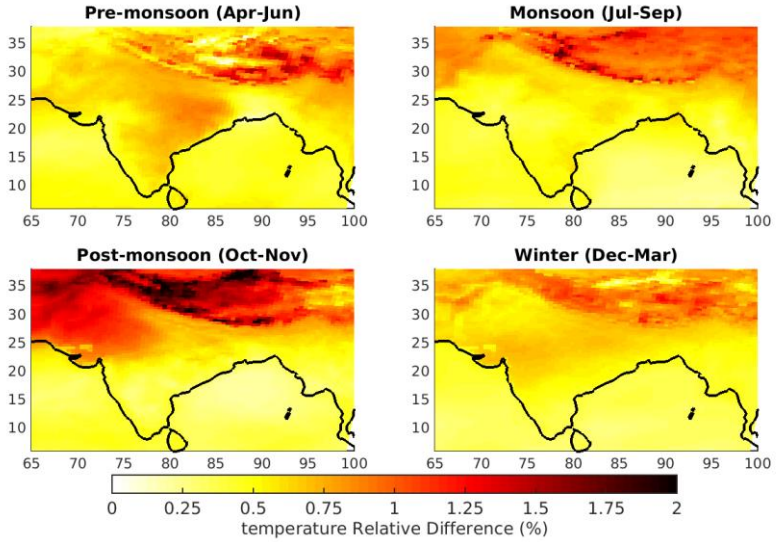
Line 353: the three regions highlighted in the 2030 results are not the same as in 2050, but are there similar explanations? Please elaborate. Could it be that the changes seen in these quite small regions are more random, than caused by the development in climate and emissions?
We have added this sentence at the end of Section 4.2:
"The change in location of the three regions between the 2030s and the 2050s shows that the local meteorology has an impact on the change in the chemistry, such as the surface temperature. Indeed, the changes in temperature are not homogeneous over the domain and vary with the seasons."

The seasonal changes (relative difference in %) in surface temperature for the 2030s and the 2050s are plotted below:

2030s:



2050s:



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

Thank you for noticing this error. The correct word is "except". It has been corrected.

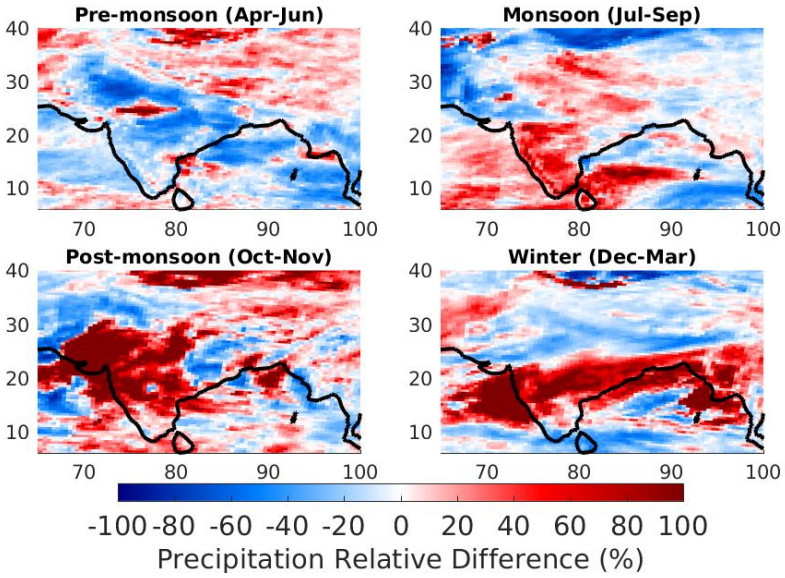
Section 4: I would like to see some discussion about the uncertainties in model representation (meteorological data) of monsoon and projected future changes, and how this could affect the results.

We have added these sentences at the end of Section "2.1. Downscaled meteorological data": "For the future scenarios, NorESM1-M predicts an increase in temperature close to the mean of the CORDEX South Asia ensemble. For many areas there is no consensus concerning the sign of the precipitation change, except during the monsoon and the post-monsoon (October-November) in the 2050s where most of the models, including NorESM1-M, predict an increase

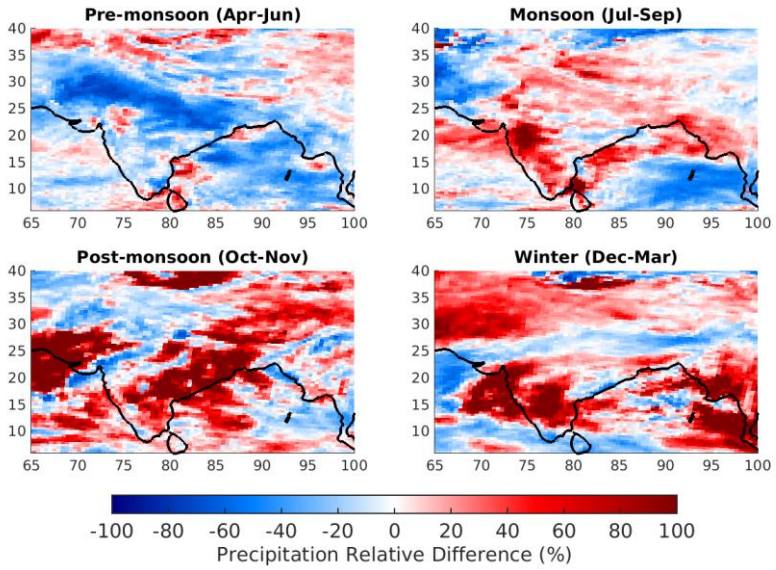
in precipitation over the major part of India, in comparison with the 2006-2015 period. During the pre-monsoon (April-June) in the 2050s, half of the models, including NorESM1-M, show a decrease in precipitation which is larger over the Indo-Gangetic Plains. NorESM1-M also presents this decrease in the 2030s. In winter (December-March), the western coast is characterized by an increase in precipitations, even if this change is lower in NorESM1-M than in the other models (not shown)."

These seasonal changes in precipitation can be seen in these figures:

Changes for the 2030s

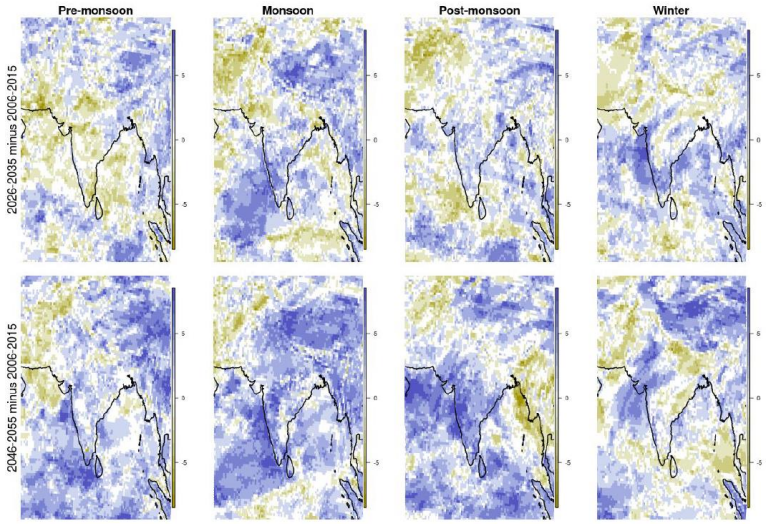


Changes for the 2050s



While this following figure shows the agreement between all 8 models (including NorESM1-M) in the sign of changes in precipitation. Blue means an agreement in more precipitation,

while brown shows an agreement of less precipitation. Zero (white color) means that there are 4 models predicting an increase and 4 predicting a decrease.



We have also written in section 4:
"It is important to recall that uncertainties in the representation of meteorological conditions can impact our chemical results even if consistencies in the projections were simulated, especially during the monsoon and the pre-monsoon, as explained in Section 2.1"
And in Section 4.2 (in bold):
"Indeed, region (1), representing mainly a rural area, is subject to a large decrease in $PM_{2.5}$ by 8% during the monsoon. This is mainly due to the reduction in dust, representing 55% of $PM_{2.5}$, largely scavenged by the increased precipitation (+36%) **(as explained in Section 2.1).**"

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

Line 391: changes in precipitation and wind speed will affect the dust production as well. Have the authors looked at this?
That is correct but while the increase in precipitation by 36% influences the decrease in dust, we have noted a slight increase in wind speed (by 1.6%) during the monsoon over region (1).

Line 401: should this be "change" instead of "variation"?
"variation" has been substituted by "change".

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.
Within UN-ECE and for integrated assessment modelling in Europe, the recommended ozone metric for health effects is SOMO35.

The SOMO35 is the indicator for health impact assessment recommended by WHO and is defined as the yearly sum of the daily maximum of 8-hour running average over 35 ppb. For each day the maximum of the running 8-hours average for O_3 is selected and the values over 35 ppb are summed over the whole year.
A_8^d denotes the maximum 8-hourly average ozone on day d, during a year with Ny days, and SOMO35 is defined as:
SOMO35 (in ppb.days) = $\sum_{d=1}^{d=Ny} \max(A_8^d - 35\ ppb, 0\ )$
where the max function evaluates max(A−B, 0) to A−B for A > B, or zero if A ≤ B, ensuring that only A_8^d values exceeding 35 ppb are included.

We have added these sentences in Section 5.1:
"This substantial increase in O_3 leads to a large increase in the ozone health indicator, SOMO35. The SOMO35 metric is defined as the annual sum of daily maximum running 8h average O_3 concentrations over 35 ppb. The SOMO35 levels for the reference scenario are already higher (Fig. S13) than over Europe (e.g. van Loon et al., 2007; EMEP Status Report 1/2017) probably related to the warmer climate and the large emissions of O_3 precursors over India, and the overestimation in O_3 from the model as shown in Section 3.1. SOMO35 is predicted to significantly increase for both FCE scenarios (Fig. S13)."

With the corresponding references:
EMEP Status Report 1/2017: "Transboundary particulate matter, photo-oxidants, acidifying and eutrophying components", Joint MSC-W & CCC & CEIP Report, 15-36, ISSN 1504-6109, 2017.
And:
van Loon, M., Vautard, R., Schaap, M., Bergstrom, R., Bessagnet, B., Brandt, J., Builtjes, P. H. J., Christensen, J. H., Cuvelier, C., Graff, A., Jonson, J. E., Krol, M., Langner, J., Roberts, P., Rouil, L., Stern, R., Tarrason, L., Thunis, P., Vignati, E., White, L., Wind, P. : Evaluation of long-term ozone simulations from seven regional air quality models, their ensemble, Atmos. Env., 41 (10), 2083-2097, doi:10.1016/j.atmosenv.2006.10.073, 2007.

And figure:

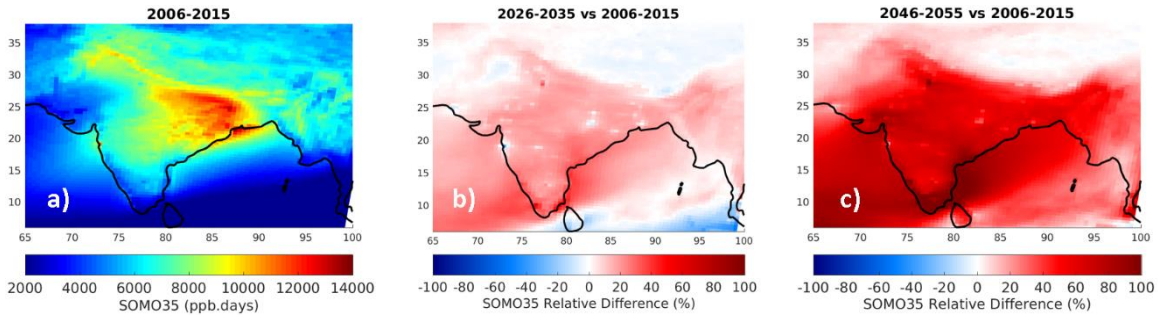


Figure S13. Distribution of SOMO35 levels for the reference scenario (a), and of the relative difference in SOMO35 between the reference scenario and the FCE2030 scenario (b) and the FCE2050 scenario (c).

Regarding $PM_{2.5}$: this is the reason there was this sentence in Section 5.2 (lines. 437-438):
"These increments alone are comparable to, or double, the annual threshold that WHO recommends not to exceed, i.e. 10 µg/m^3."
To clarify this point, it has been modified:
"These increments alone are comparable to the annual threshold that WHO recommends not to exceed, i.e. 10 µg/m^3, for the FCE2030 scenario, and the double for the FCE2050 scenario."

Line 446: please add boxes indicating regions in Fig. 16.
It has been done for Figs 13 and 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)
Yes, the EC increases but it still represents a limited amount of $PM_{2.5}$ (3%). The sentence was confusing. Now it reads:
"It is also worth noting that even though the PPM are high for the three scenarios (close to 20% of $PM_{2.5}$), the amount of EC within these PPM remains low, around 15%."

About the split between EC and OM, please see our answer to your 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.
This is actually a complex point. We have added these sentences at the end of Section 5.2:
"It is interesting to note that even under increasing anthropogenic emissions a significant fraction of $PM_{2.5}$ comes from sources (dust and some fraction of SOA) that are challenging to control through policy measures. Still, even biogenic, SOA is partly the product of anthropogenic emissions (and certainly land-use policy, e.g. Tsigaridis and Kanakidou, 2007, Ashworth et al., 2012), and dust is also partly a function of land-use and climate change, but such interactions are beyond the scope of our study."

And the corresponding references:
- Ashworth, K., Folberth, G., Hewitt, C. N., and Wild, O.: Impacts of near-future cultivation of biofuel feedstocks on atmospheric composition and local air quality, Atmos. Chem. Phys., 12, 919-939, https://doi.org/10.5194/acp-12-919-2012, 2012.
- Tsigaridis, K., and Kanakidou, M.: Secondary organic aerosol importance in the future atmosphere, Atmos. Environ., 41, 4682–4692, doi:10.1016/j.atmosenv.2007.03.045, 2007.

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.

Huang, M., Carmichael, G. R., Pierce, R. B., Jo, D. S., Park, R. J., Flemming, J., Emmons, L. K., Bowman, K. W., Henze, D. K., Davila, Y., Sudo, K., Jonson, J. E., Tronstad Lund, M., Janssens-Maenhout, G., Dentener, F. J., Keating, T. J., Oetjen, H., and Payne, V. H.: Impact of intercontinental pollution transport on North American ozone air pollution: an HTAP phase 2 multi-model study, Atmos. Chem. Phys., 17, 5721-5750, https://doi.org/10.5194/acp-17-5721-2017, 2017.