Reviewer 1

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.

The authors thank reviewer 1 for the thorough review. A detailed point-by-point reply (written in blue) is provided hereafter.

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.
We agree that the partial lack of coincidence between available measurements and emission data adds to the uncertainty in the model evaluation. We have added the sentence:
"The discrepancies between the periods of all the stations may have an impact on the evaluation, since the measurements do not necessarily match the emissions year used for the reference scenario."
We can note though that the overestimation is most pronounced for the urban stations (44%, Fig. 3a), as rural stations show an overestimate of only 15% (Fig. 3c). Although there are major uncertainties in the base emissions, trends, and indeed measurements, this strongly suggests that titration is a serious problem in this comparison. This may be a sub-grid problem that would require finer-scale emissions and modelling to resolve.

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.
The changes in precipitations in the 2030s and the 2050s are already presented in fig. 8.

About the temperature, we have plotted hereafter the distribution of the temperature at 2 meter and the relative difference for both FC scenarios:

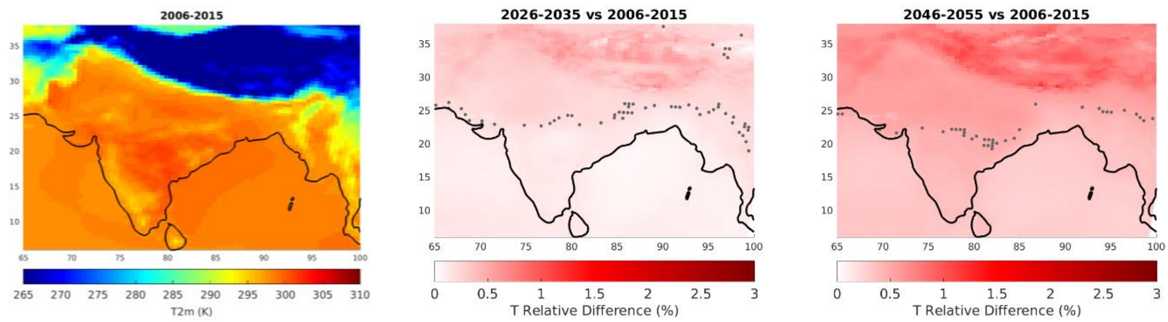

Fig. 1 Distribution of the temperature at 2 meter and the relative difference for both FC scenarios. Note that the grey points, on the distribution of the relative difference, show the grids that do not satisfy the 95% level of significance.

We do not show these maps in the manuscript but we have added relevant information (highlighted in bold) in the text, in section 4.1:
"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."

The maps in Fig. 2 show an increase in the wind speed over the Bay of Bengal and over a large part of the Thar Desert. There is also a decrease in the wind speed over the Indo-Gangetic Plain and the Northern part of Arabian Sea.
These changes do not match the changes in O_3 and $PM_{2.5}$ shown in Figs 7 and 11 of the ACPD manuscript, but we have added the following sentence (in bold) at the beginning of Section "4.2 $PM_{2.5}$":
"Climate change is predicted to lead a fairly homogeneous rise in surface $PM_{2.5}$ levels over India, especially for the FC2050 scenario, by up to 6.5% (4.6 µg/m^3) (Fig. 9**). This maximum increase is located over the Indo-Gangetic Plain where a decrease in surface wind speed is predicted (not shown). The decrease in wind speed may limit the emission of dust and the dispersion of the $PM_{2.5}$ emitted over this area**."

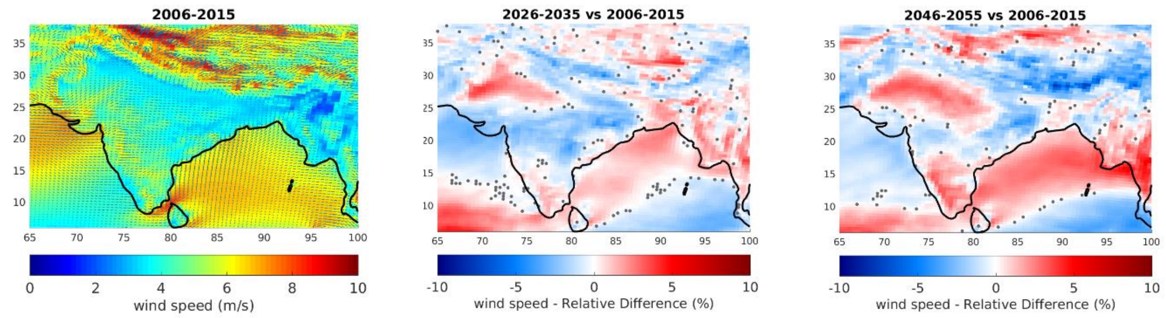

Fig. 2 Distributions of surface wind speed in m/s with the wind direction for the reference scenario (left), relative difference between the FC2030 scenario and the reference scenario in % (middle), relative difference between the FC2050 scenario and the reference scenario in % (right). Note that the grey points, on the distribution of the relative difference, show the grids that do not satisfy the 95% level of significance.

The change in wind direction is small. Thus, it is not easily visible on maps. We have decided not to show the changes in the wind direction in Fig. 2, but we present hereafter the wind direction (with 10m winds) for the 3 regions presented in Fig. 12 of the ACPD version of the paper.

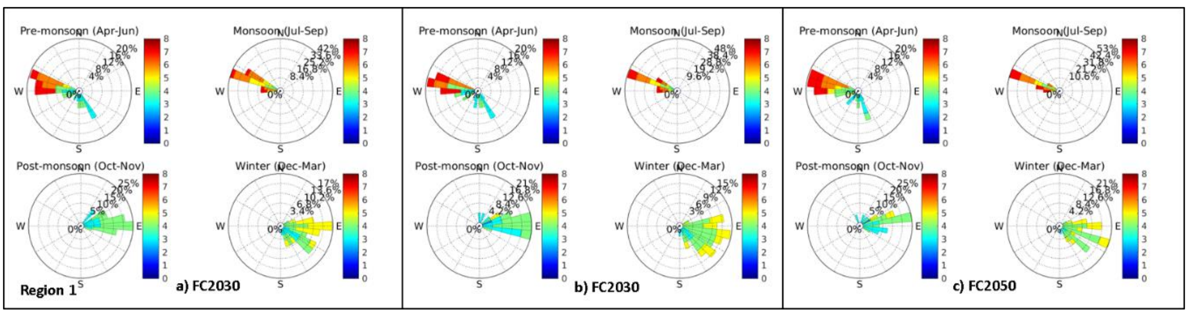

Fig. 3 Wind rose based upon 10 m winds for the Region 1 (defined in Fig. 12 in the ACPD version) presenting the wind direction for the reference (a), FC2030 (b) and FC2050 (c) scenarios, for the 4 seasons. The colorbar shows the wind speed in m/s and the percent corresponds to the distribution of the probability of the wind speed.

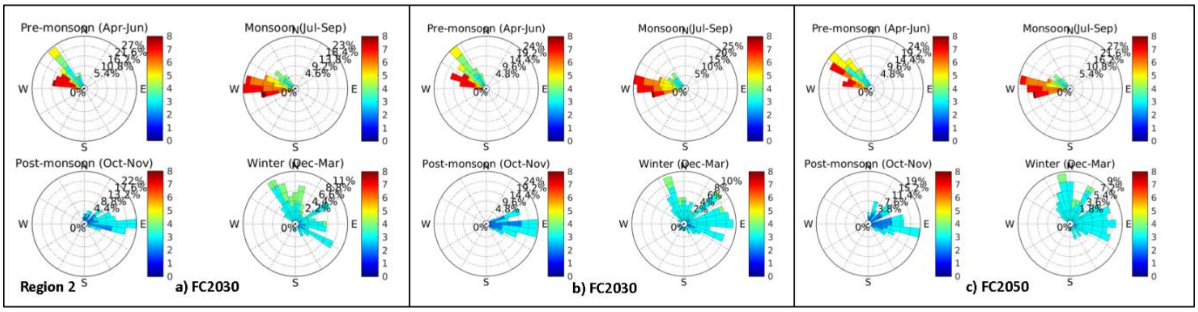

Fig. 4 As Fig. 3 for Region 2.

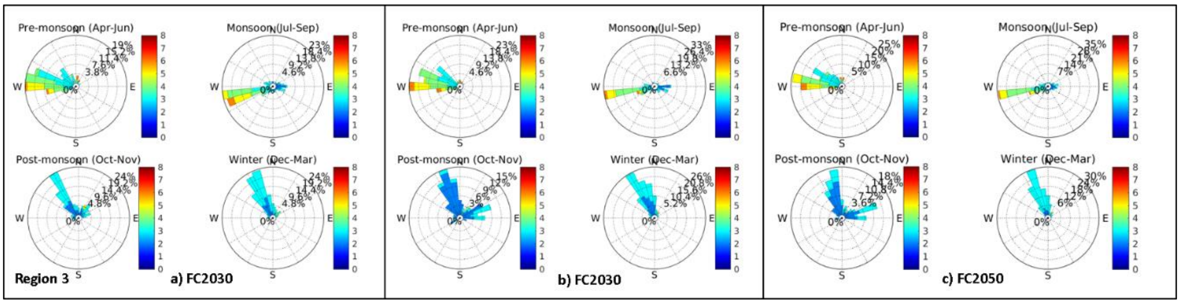

Fig. 5 As Fig. 3 for Region 3.

Figs. 3-5 show the limited impact of the climate on the wind direction over the 3 regions and for each season. A small change in wind speed is observed as an increase in surface wind speed over Region 1 during the pre-monsoon and the monsoon periods, and as a decrease over Region 3 during the post-monsoon.

The increase in wind speed over region 1 was already mentioned in lines 394-395 of the manuscript. For region 3 we have added this information (in bold):

"This increase is caused by the rise in both SIA (+29%) and OM (+21%) **and probably by the reduction of the dispersion as predicted by the decrease in the surface wind speed by 5%."**

The change in surface wind speed over these 3 regions presented in Figs 3-5 is confirmed by the following seasonal maps of the relative difference:

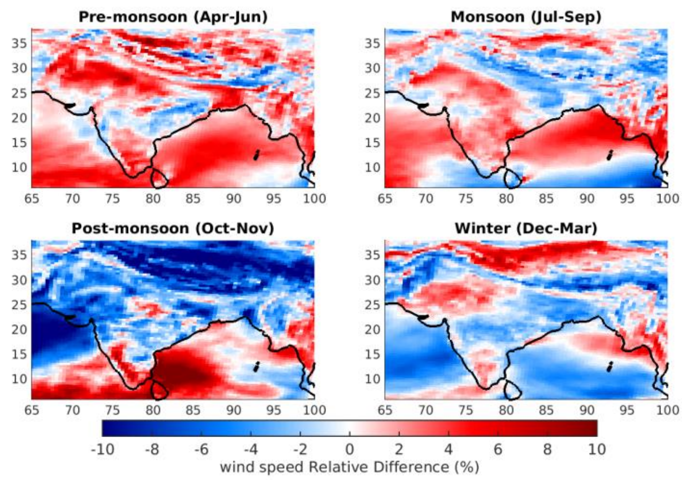

Fig. 6 Seasonal distribution of the RD (in %) in the surface wind speed between the reference scenario and the FC2050 scenario. The relative differences are calculated as: ([FC2050 – reference] / reference) × 100%.

It is important to note that the distributions in Fig. 6 do not match the change in O_3 presented in Fig. 9 of the ACPD manuscript but they do match the change in the O_3 deposition velocity presented in the same Figure.

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.

We agree with the reviewer that a perfect correlation between changes in O_3 deposition velocity and changes in O_3 concentration cannot be expected, although we find good anti-correlations except in the three focus areas (labelled as 'A/B/C' in the new Fig. 8) where the changes in NMVOCs may explain the changes in O_3.

But it is clear that climate change will cause changes in soil moisture, and changes in soil moisture impact ozone deposition. Changes in soil moisture are thus necessarily a climate change-related factor that impacts O_3, although they are of course not the only one. A decrease in O_3 will in general not only be due to an increase in dry deposition, but it will be influenced by it. We are thus careful in stating that O_3 changes are only 'partly related to changes in O_3 deposition velocity". In the abstract we now write 'assumed to be' rather than 'found to be', based on our analysis of ozone change and dry deposition change.

Abstract: "This variation in O_3 is **assumed** to be partly related to changes in O_3 deposition velocity caused by changes in soil moisture and, over a few areas, partly also by changes in biogenic NMVOCs."

Moreover, as shown with the scatterplots in this Fig. 7, even if there are areas where the changes in O_3 deposition velocity and the changes in O_3 concentration are correlated, by choosing the model grids over land within the region 70-85E - 10-35N, there are clear anti-correlations between both parameters:

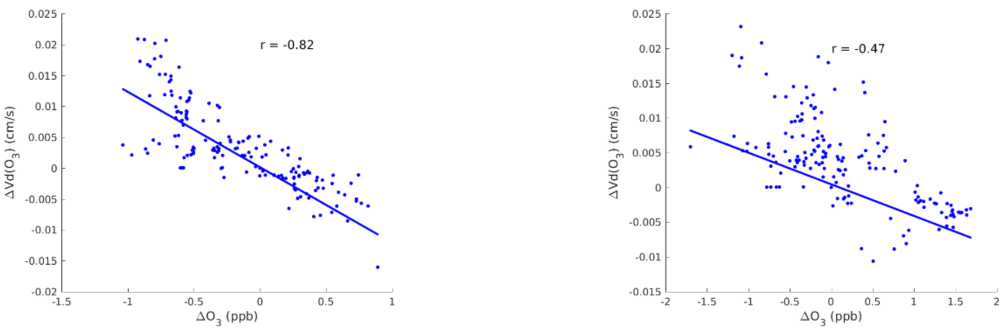

Fig. 7 Scatterplot between $\Delta Vd(O_3)$ and ΔO_3 over land grids for the FC2030 scenario (left panel) and the FC2050 scenario (right panel).

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.

It is difficult to select the same areas for each analysis since the purpose of these distinct regions was to describe and interpret:

- the change in O_3 due to the climate

- the change in $PM_{2.5}$ due to the climate
- the composition of $PM_{2.5}$ and the change in O_3 and $PM_{2.5}$ over a larger domain for the FCE scenarios.

But we fully agree that using different regions with identical labels is confusing. We have decided to change their names to clarify our analysis in the revised manuscript.

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

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".
It has been changed.

L65-L66: rewrite this sentence. This is not even a complete sentence.
This was a typing error. The dot located after "(www.worldbank.org)" has been replaced by a comma as below:
"With a population of 1.3 billion inhabitants, a density of 420 inhabitants per km^2 (12 times the population density of the United States) and a Gross domestic product (GDP) growth of 7.6% per year in 2015 (www.worldbank.org)**,** India is one of the fastest growing economies in the world."

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.
We have decided to add the reference "Fiore et al. 2015" as it seems more relevant to highlight the impact of the climate change on air quality, and not the impact of air quality on climate as also described in Fiore et al. 2012.

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

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

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.
We have rephrased this part in order to clarify our description. Now it reads:
"$PM_{2.5}$ consists of both primary and secondary components. Primary $PM_{2.5}$ components include organic matter (OM), elemental carbon (EC), dust, sea salt (SS) and other compounds. Secondary $PM_{2.5}$ comprises compounds formed through atmospheric processing of gas-phase precursors. This includes various compounds as nitrate (NO_3^-) from NO_x, ammonium (NH_4^+) from ammonia (NH_3), sulphate (SO_4^{2-}) from sulphur dioxide (SO_2), and a large range of secondary organic aerosol (SOA) compounds from both anthropogenic and biogenic VOCs. Important sources of both primary and secondary $PM_{2.5}$ emissions in India are domestic heating in winter, wood burning (mainly used for cooking), road transport with contributions from both

exhaust (mostly diesel) as well as non-exhaust emissions from brake and tyre wear, and industrial combustion. The main sink of $PM_{2.5}$ is wet deposition, associated with rain-out and wash-out by precipitation."

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.
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.

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

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.
Yes, there is a dust module. See our answer to your comment entitled L124-L128.

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.
We agree that this was confusing. Additional information has been added (in bold):

“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.”**

L152: what does the author mean by “their respective baseline year”?
To clarify the sentence we have added this information (in bold):
“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)**.”

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 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.
The emission estimates from Sharma and Kumar (2016) are based on local situation in India by accounting for sectoral growth rates envisaged by the Govt. of India in energy scenarios and also the interventions taken in different sectors up to 2014.
Moreover, by comparing emissions used with other studies shows closeness with other estimates, as shown in the following Table.

Table 11.1: Comparison of Emission Inventory (Million Tonnes) in This Study with Others

Study	Year	PM10	SO2	NOx	NMVOC	CO
This study	2011	10.6	5.6	7.0	11.4	46.4
Garg et al. (2006)	2005		4.6	4.4		41.7
Streets et al. (2003)	2000		5.5	4.0	8.6	51.1
Ohara et al. (2007)	2003		7.0	5.0	9.7	84.4
Zhang et al. (2009	2006	4.0	5.6	4.9	10.8	61.1
EDGAR 4.2a	2008	10.9	8.5	6.4	10.6	46.3
Kurokawa et al. (2013)	2008	4.7	10.0	9.7	15.9	61.8
Purohit et al (2010)		8.2	6.4	5.0	15.1	
Lu et al. (2011)	2008		8.0			
Klimont et al. (2009)	2005		6.4	5.0		

[a]http://edgar.jrc.ec.europa.eu/);

Table extracted from Sharma and Kumar (2016).

In the opposite, as reviewed by Amann et al. (2013):
- The RCP scenarios include emission projections for SO_2, NO_x, VOC, BC, OC, CO, and NH_3, but they were not developed with a primary focus on air pollution concerns. They were developed for greenhouse gases.
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.
- Thereby, these scenarios internalize additional air pollution policies, which might or might not materialize in the future. However, because the RCP scenarios explore a wide range of future climate policies, they provide indications about the impacts of GHG reductions on air pollutants.

- Scenarios that do not assume additional air pollution policies beyond current legislation indicate a potential rebound of emissions after 2030, whereas emissions decline in scenarios that assume autonomous further reductions in emission factors on the basis of the environmental Kuznets hypothesis. Thus, although air pollution might appear as a diminishing issue in the widely used RCP scenarios, this positive development will only occur if environmental policy interventions are enhanced in the future.

- 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.

Moreover, 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 your cited reference (Zhang et al., 2016).

L209: delete "since the NH3 emissions from...."
We have decided to keep this sentence since it is important to remind that the NH_3 emissions are from ECLIPSE. It also explains why in Fig. 1 the NH_3 emissions are identical.

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

L233: modify the "ca.130%".
It has been changed. Now it reads: "around 130%."

L243: change "Sharma et" to "Sharma and".
It has been changed.

L323: show the correlation for the delta O3 and delta T.
See for example the scatterplot for the area 70-85E, 10-35N (same region as in Fig. 7):

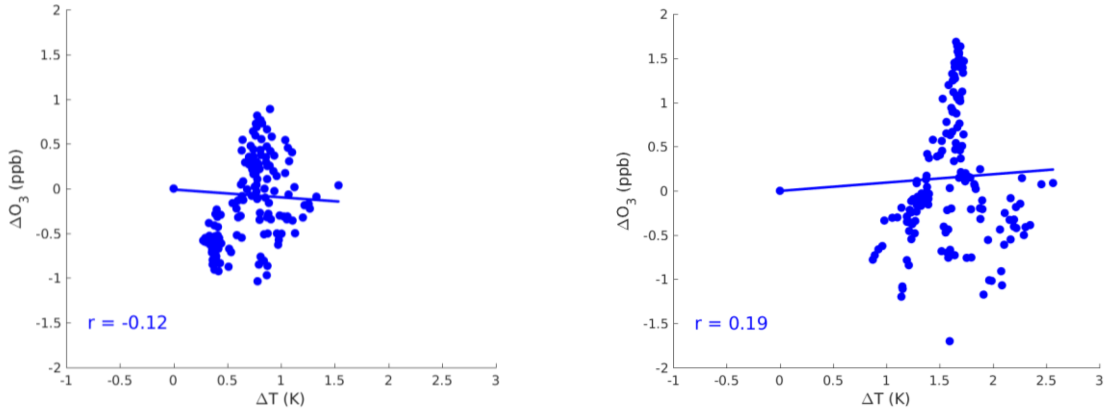

Fig. 8 Scatterplot between ΔO_3 and ΔT over land grids for the FC2030 scenario (left panel) and the FC2050 scenario (right panel).

We have noticed an error in the text. It is not "spatial" change but temporal. It has been changed in the text.

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?
We agree that our sentence was unclear. We have added the missing information (in bold) to the following sentence:
"**Combined with the increase in O_3**, this result gives an indication of the presence of a VOC-sensitive regime."

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?
Indeed. We have corrected this typing error.

L436-437: "These increments alone are comparable to, or double" Rewrite this sentence with the previous one. It's really confusing.
The sentence has been changed. Now it reads:
"These increments alone are comparable to the annual threshold that WHO recommends not to exceed, i.e. 10 $\mu g/m^3$, for the FCE2030 scenario, and the double for the FCE2050 scenario."

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).
- About the choice of emissions from Sharma and Kumar (2016), see our previous response.

- The following figure shows that RCP8.5 emissions for 2010 have less NO_x than the emissions used for the reference scenario in our work. These lower NO_x emissions will probably lead to too much O_3 over polluted areas, and our study shows EMEP already overestimates O_3 over cities.
As previously highlighted (see our answer to the comment named L174-L205), the RCP8.5 NO_x emissions are probably too optimistic (e.g. Amann et al., 2013) and we have preferred to rely on emissions estimates conducted by national experts.

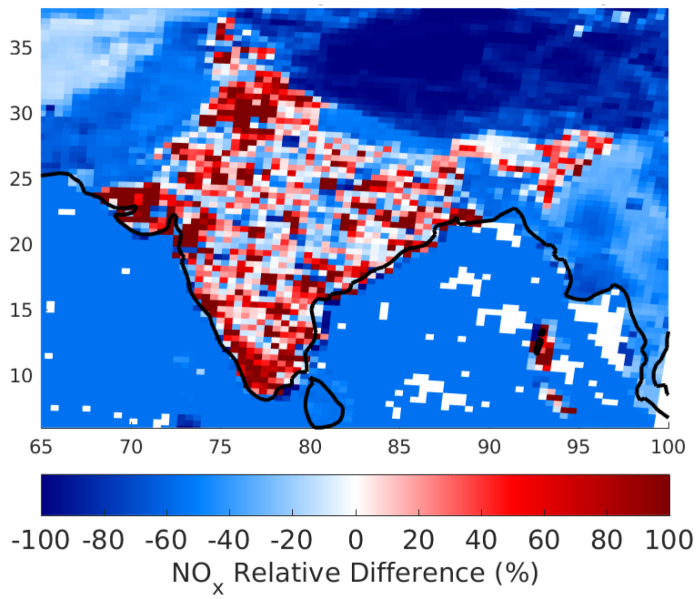

Fig. 9 Relative difference (in %) between the NO_x emissions used in our work (Sharma and Kumar, (2016) over India and ECLISPE 2010 for the other countries) and the RCP8.5 (for the baseline year 2010). The relative difference is calculated as: [(our work – RCP8.5) / RCP8.5] × 100%.

- It is true that our work does not study the impact of the intercontinental transport of pollutants, as explained by the sentence "The influence of the changes in inflow of O_3 or $PM_{2.5}$ from outside the Asian domain is not taken into account."

We did not add the discussion in the conclusion but we have added these sentences in Section "2.2 Emissions":
"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)."

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

L467-L468: "Climate change leads to increases in the PM2.5 levels at short and medium-terms, reaching 6.5% (4.6μg/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.
This number corresponds to the maximum increase in $PM_{2.5}$ over all land grids within 06-38N, 68-98E. This maximum is located over the Indo-Gangetic Plain.

We have added the information (in bold) in the abstract:
"Our calculations suggest that $PM_{2.5}$ will increase by up to 6.5% **over the Indo-Gangetic Plain** in the 2050s. **The increase over India is** driven by increases in dust, particulate organic matter

(OM) and secondary inorganic aerosols (SIA), which are mainly affected by the change in precipitation, biogenic emissions and wind speed.
The large increase in anthropogenic emissions has a larger impact than climate change, causing O_3 and $PM_{2.5}$ levels to increase by 13% and 67% in average in the 2050s **over the main part of India**, respectively."

In section 4.2:
"Climate change is predicted to lead a fairly homogeneous rise in surface $PM_{2.5}$ levels over India, especially for the FC2050 scenario, by up to 6.5% (4.6 µg/m^3) (Fig. 9). **This maximum increase is located over the Indo-Gangetic Plain where a decrease in surface wind speed is predicted (not shown). The decrease in wind speed may limit the emission of dust and the dispersion of the $PM_{2.5}$ emitted over this area."**

In the conclusion:
"Climate change leads to increases in the $PM_{2.5}$ levels at short and medium-terms, reaching **a maximum of** 6.5% (4.6 µg/m^3) **over the Indo-Gangetic Plain** by the 2050s."

Page 37: change the colorbar for region1, region2. The fractions of the PM2.5 components were not clearly seen with the high y axis.
We believe it is better to have a common colorbar for three regions to avoid confusion. However, in order to improve the reading of the fractions of the components, we have changed the y-axis for regions 1 and 2 (see below).

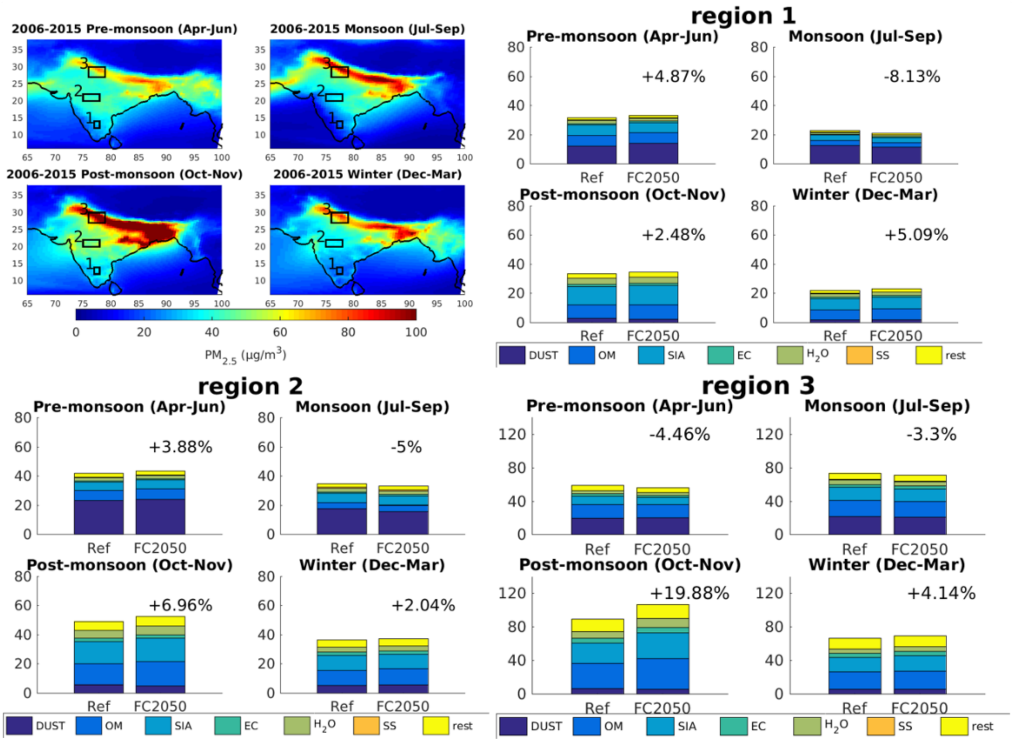

Figure 10. Seasonal distribution of surface $PM_{2.5}$ concentrations (in µg/m^3) for the reference scenario, and seasonal composition of $PM_{2.5}$ (in µg/m^3) for the three regions highlighted by black boxes on the map for the reference and the FC2050 scenarios. The black percent corresponds to the relative difference in $PM_{2.5}$ between both scenarios for each region. **Note the different y-axis for Region 3.**

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