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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-model evaluation of short-lived pollutant distributions over East Asia during summer 2008" *by* B. Quennehen et al.

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Please find below detailed replies to your comments. Since we have made significant changes to the manuscript, we invite you to read our replies together with the new version of the manuscript attached as a pdf.

This paper describes the evaluation of a number of global and regional atmospheric composition models over the East Asian region during 2008. It focuses on the summer period, coinciding with the Beijing Olympics, and addresses comparisons with both gas-phase and aerosol observations from a range of different measurement platforms and locations.

The paper is interesting, and is potentially very useful in characterizing the behavior of

the models involved. However, the major weakness is that it does not provide sufficient interpretation and attribution of why models differ from observations or each other. Reproducing observations over this highly polluted region requires a good simulation of emissions, chemical processing, local dynamics, and regional meteorology. The analysis presented includes suggestions of the causes of particular differences (e.g. that emissions of NOx are too high), but these are not firmly supported with clear evidence or convincing argument to back them up. A more thorough attribution of model differences is needed, so that developers and users of the models involved can make some progress towards model improvement, and so that others attempting a similar comparison are aware of the critical factors involved. This would provide a stronger message and make the paper much more useful to a wider audience. Authors' reply: We thank the reviewer for their insightful comments and suggestions. We agree that the submitted manuscript needed to be improved in terms of a more in-depth analysis of the model evaluation results and the attribution of causes to explain modelled discrepancies compared to observations. We have substantially revised the paper to include, in particular, two new sections discussing the reasons for model differences in ozone and other trace gases (Section 3.6) and aerosols (Section 4.3). The Abstract and Conclusions have also been substantially revised to more accurately represent our findings. In addition, the Introduction has been updated in order to explain more clearly the motivation and context of this study. We describe the rationale employed to assess the performance of models used for air quality and climate assessments using a range of different observations. Since we have made substantial modifications to the manuscript, as requested by the reviewers, we highlight the main changes to the text that are relevant to the reviewer's specific points in the following:

" Models show systematic positive biases in ozone, especially at rural surface locations, and compared to satellite data downwind of major Chinese emission regions. The general underestimation of CO over and downwind of emissions is linked to this, most likely due excessive destruction by OH, suggesting that CO lifetimes are too short. Reasons for ozone differences varies between models but appears to be linked to model ability

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to simulate VOC and NOx regimes in polluted and less polluted environments. This may also be linked to inter-model spatial variability in compared to NO2 surface data and NO2 satellite column data. The latter, however, indicates a possible underestimation in NOx emissions over Korea and Japan as well as under (over)-estimation of emissions to the south/east (west) of the Chinese NCP emission region. These findings point to the need to employ adequate model resolution to improve simulated responses to emissions when moving from ozone titration to ozone production regimes within large polluted conurbations, their surroundings and downwind. Overestimation of Asian ozone and its transport downwind implies that radiative forcing from this source may be overestimated. Sensitivity analyses, based on one model, suggest that emission mitigation over Beijing cannot explain these discrepancies.

Satellite-derived AOD measurements were reproduced quite well by the models over China even if evaluation of individual aerosol components over Asia overestimate ECLIPSE model-mean surface BC and sulphate aerosols in urban China in summer 2008. The effect of the short-term mitigation measures taken during the Olympic Games in summer 2008 is too weak to explain divergences between the models and observations. Our results rather point to an overestimation of emissions close to the surface in urban areas, particularly for SO2. A potential reason for this is the fact that the spatial distribution of power plant emissions has been changing dramatically in the last decades in China Liu et al. (2015), a change that has not been captured well in the ECLIPSEv4a dataset. A strong underestimation by ECLIPSE models of aerosol loadings has been identified over northern India, suggesting that the emissions of BC and precursors of other aerosols are underestimated in the ECLIPSEv4a inventory. Improvements have already been included, such as higher emissions from kerosene lamps, in the ECLIPSEv5 dataset. Model deficiencies in the representation of pollution accumulation due to the Indian monsoon may also play a role. The underestimation of the scattering aerosols in the lower troposphere, above the boundary layer, suggests too much vertical transport of pollutants towards the free troposphere and/or insufficient deposition in the boundary layer, leading to overestimated aerosol residence times in

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models."

General Comments The purpose of the model evaluation is briefly outlined at the end of the introduction, but includes several very different issues (air quality, climate change, long-range transport) which have different evaluation requirements. However, these issues are lumped together in this study. Please provide a clear indication of which parts of the evaluation are relevant to which issue, so that the wider implications are immediately clear. Authors' reply: The Introduction has been substantially revised to better explain the rationale for this study and to provide a clearer description about the different observations that were used to evaluate model performance on a range of scales and at different locations. The ECLIPSE models have been used, in related studies (summarized in Stohl et al., 2015), to estimate present-day and future chemical composition and their impacts on air quality and climate. The models used in these assessments range from chemistry-climate models to chemical-transport models, also including aerosol treatments. One goal of ECLIPSE was also to evaluate these models over emission source regions (Europe, Asia). As such the models need to be evaluated both in urban and rural locations for air quality as well as over source regions and downwind for climate change impacts. Comparison of model results with a range of different datasets including surface data, vertical profiles and satellite data allows assessment of model performance over a range of scales relevant for air quality and climate.

East Asian emissions are rising rapidly and are less well quantified than in many other parts of the world, and they were temporally reduced by a poorly-known amount during the Olympic period. Given these large uncertainties, it is unclear why this region and period were chosen for the study. Please provide a clear justification for the choice. The impact of the Olympic emission reduction is one aspect of interest in many previous studies, but is not exploited here. Authors' reply: Here, we present results from the evaluation of the ECLIPSE models over East Asia. As noted above, this region was targeted due to its still high pollution levels, climate impacts and as a region where SLP

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mitigation options are being actively considered. It is also a region where signiïňĄcant uncertainties surround model estimates of radiative forcing. For example, Kinne et al. (2006) showed important underestimation of observed AOD by multiple models over East Asia in summer and pointed out that uncertainties in the direct radiative forcing could be larger than inter-model differences in AOD suggest. Even in the recent AeroCom model comparison, inter-model variation in radiative forcing is largest in this region (Myhre et al., 2013a). Samset et al. (2014) pointed to overestimation of modeled BC compared to aircraft observations in the upper troposphere downwind of Asia over the PaciïňĄc suggesting that BC lifetimes are too long in current models and that BC direct radiative forcing is overestimated by about 25%.

The ECLIPSE model evaluation over East Asia focuses on the summer period (August and September 2008). This was motivated by the availability of intensive observations from the CAREBEIJING 2008 measurement campaign (the only campaign conducted during 2008 and 2009 for which data were available) and by the fact that severe ozone pollution episodes still occur over this region, and the wider Northern China Plain (NCP) at this time of year.

We have revised the text in the Introduction to include: “The ECLIPSE model evaluation over East Asia focuses on the summer period (August and September 2008). This was motivated by the availability of intensive observations from the CAREBEIJING 2008 measurements campaign (Huang et al. 2010, Zhang et al., 2014) and by the fact that severe ozone pollution episodes occur over NCP at this time of year even if the maximum is generally earlier in the late spring for trace gases (Naja and Akimoto, 2004; Li et al., 2007; He et al., 2008; Safieddine et al., 2013) and aerosols (Cao et al., 2004; Sun et al., 2004; Yang et al., 2005; Huang et al., 2006). During the summer months, whilst the monsoon circulation brings cleaner air from the Pacific Ocean into southern and eastern Asia reducing pollutant concentrations (Lin et al., 2009; Kim et al., 2007). However, high pollution episodes with enhanced aerosol concentrations and decreasing visibility still occur in coastal regions, due to increases in relative hu-

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midity increasing aerosol sizes (Flowers et al., 2010). The monsoon flux also induces transport of high ozone concentrations inland (He et al., 2008)."

To assess the possible impact on our results of emission mitigation measures applied in the context of the Beijing Olympic and Paralympic games, the WRF-Chem model was run for 2 weeks (1 to 15 August 2008) with reduced pollutant emissions from the transport, industrial and solvent use sectors, following the mitigation strategy during the Olympics described in Wang et al. (2010). According to the related literature (e.g. Zhou et al., 2010; Worden et al., 2012), the impact of such emission mitigations was mostly local and only affected the Beijing area. Thus, regarding our observation datasets, only the in-situ observations in Beijing (at the Peking University, Fig. 4 and 10) were affected by them. For example, emissions of all species in the transport sector were reduced by 75% in Beijing and 20% in the area 200 km from Beijing, corresponding to eight model grid cells around Beijing in this model. Emissions linked to the industrial sector or to solvents were reduced by 50% in the same region. Most pollutant concentrations are reduced resulting, for example, in lower CO, by about 30 ppbv, locally in and around Beijing in the emission reduction run compared to the base run. This results in ozone reductions of up to 6-7 ppbv in the region of Beijing. With regard to aerosols, results from the WRF-Chem simulations with reduced emissions due to additional mitigation measures in the Beijing area show that the measures taken for Olympic Games leads to small reductions in surface BC, OC and sulphate concentrations by 0.3, 1 and 1μg m$-$3 , respectively. Based on these results, it appears that these reduction measures cannot explain the discrepancies between the models and the observations discussed earlier.

Fig. 1 shows the difference between surface concentrations in the base simulation and surface concentrations in the simulation where the emissions have been reduced during the Olympics for two pollutants: CO and ozone. Concentrations of CO, ozone, SO2, BC, OC, sulphates decrease, except for NO2 that increases due to a competition between NO and NO2, in Beijing. Overall, we find that the impact of emissions abate-

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ment is too low to explain discrepancies between the models and the observations. These results are described in sections 3.6 (trace gases) and section 4.3 (aerosols).

Fig. 1: Differences between modelled surface concentrations in CO (left) and O3 (right) in the base simulation and surface concentrations in the simulation where emissions have been reduced during the Olympics using the WRF-Chem model.

The main discussion sections (e.g. Section 3.5) are insubstantial, major biases are missing, and the links between biases that are described are not clearly and logically laid out. The result is that these sections are inconclusive and provide the reader with little new information about why the models and observations differ. While it may not be possible to diagnose model biases completely with the limited information available, clearer analysis of the data will provide new and useful information. One issue is that the satellites provide estimates of NO2 (not NOx) and that emissions are of NO (not NOx). Biases in chemical processing thus strongly influence comparison with measurements, and biases are expected in this region where aerosol loadings can be very high. Another issue is that no attempt is made to assess how representative the surface or aircraft measurements are of the wider region, and thus whether it is reasonable to expect the models to match them. A clearer and more detailed discussion of the expected chemistry and transport biases would make the paper much more useful. Authors' reply: We agree with reviewer #2 that we needed to analyse our results in more detail in order to provide more concrete and useful conclusions. For trace gases, we have now included a new Section 3.6 that examines possible reasons for model discrepancies in much more detail. We identify several rather robust and systematic biases that are common to the majority of models and which have implications for model ability to correctly assess air quality and climate impacts of Asian pollution. In order to examine the causes of the discrepancies in different models, we make use of modelled trace gas ratios (e.g. CO/NOx, ozone/NOz) together with a more detailed analysis of the model results (e.g. modelled ozone diurnal cycles). With regard to the specific point about NO2, we agree with the reviewer and have clarified this in the text

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throughout the new version of the paper. With regard to aerosols, we have included a new section 4.3 which discusses reasons for the discrepancies in the model aerosol simulations compared to the observations and their implications for assessment of climate and air quality impacts.

With regard to the representativeness of the model/observations comparison we now explain, in several places in the text, which observations we have used, for which purpose and which spatial scales they represent.

For example, in the Introduction: “In order to assess model performance over East Asia for air quality, as well as climate, we use a variety of different datasets covering the urban, regional, and continental scales. Ozone, aerosol and precursor data at surface sites in urban and rural locations are used, together with CAREBEIJING aircraft data collected in the lower troposphere south of Beijing, to evaluate model performance in terms of local and regional pollution from major emission regions. Continental scale horizontal and vertical transport of ozone and aerosols, important for radiative impacts, are assessed downwind of the main emission regions using aerosol lidar data as well as satellite aerosol lidar and tropospheric ozone, CO and NO2 column data.”

We agree that it is difficult to compare global models with surface data collected in urban locations, for example. On the other hand, it is important to assess these models over a variety of scales and locations ranging from urban to rural and on continental scales. Such models need to be able to capture average patterns in terms of gradients from polluted to background environments as well as vertical distributions of pollutants. These models have been used in the ECLIPSE project to assess air quality and climate impacts together. Analysis of the CAREBEIJING aircraft data is described in Sections 3.5 and contributes to the discussion in Section 3.6. We make it clearer that this data was collected to the south of the main Beijing conurbation, in air masses largely originating from the south. See also our reply to the specific comment below.

Specific Comments

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Abstract, l.6: "emissions of NOx are too high": or that chemical timescales or transport are incorrect. Addressing the contributions of these different biases is the key to strengthening the paper, and simultaneous evaluation of several variables across a number of models in this study should permit this. Authors' reply: We agree with the reviewer that this particular conclusion was too general and required further analysis. The Abstract has now been substantially revised to better reflect the conclusions of our study and to take into the account the additional analysis of the trace gas results presented in the new Section 3.6 and the aerosol results presented in Section 4.3. See also replies above.

Abstract, l.13: I agree with the statement "These results have important implications for accurate prediction of pollution episodes...", but the reader needs to know what the implications are, and how they could be resolved. Authors' reply: As noted above, the Abstract has been substantially revised. Overall, the ECLIPSE model evaluation highlights several signiïňĄcant biases in the model results, based on simulations using the same emissions. The Summary (Section 5) has also been revised which we invite the reviewer to read - the final paragraph from this section now states: "In summary, the ECLIPSE model evaluation highlights significant differences between the models and observations, even when models are run using the same emissions over East Asia. Nevertheless, an important finding is that overall the Earth System Models show a similar level of performance as the Chemistry Transport Models, which is encouraging for the further use of the Earth System Models for determining both climate and air quality impacts. Somewhat better general agreement is found for trace gases compared to aerosols, for which model simulations are very variable. For both trace gases and aerosols, models have difficulties reproducing gradients between urban and rural∼(downwind) locations and vertical distributions. Improved model resolution as well as improved understanding and model treatments of processes affecting pollutant lifetimes are needed. Model evaluations using a variety of observations are required so that difference aspects of model behavior can be tested. Results from this study suggest that significant uncertainties still exist in chemistry-climate simulations which

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has implications for the use of such models in the assessment of radiative effects of short-lived climate forcers on climate and regional/global air quality."

Section 2.4: It would be helpful to include a statement on the purpose of the CAREBeijing flights to provide an indication of how representative they were of the spatial and temporal scales resolved by the models. Were they intended to sample urban outflow, or rural regions? What biases have previous analysis of these flight data identified? Authors' reply: The paper analysing the CAREBEIJING aircraft observations (Zhang et al., 2014), did not provide the intended target of the flights nor did they identify clear biases. However, a back-trajectory analysis conducted by Zhang et al. (2014) highlighted 4 different origins for the air masses sampled during the flights: south, north, east and mixing. More polluted air masses that were sampled originated from the south. These airborne observations are representative of the somewhat less polluted background atmosphere in China with only a partial influence from urban areas. Section 2.4 has been updated. We also added the following text describing how the model results were extracted and their representativeness:

"Model results were extracted along the flight paths corresponding to 2 or 3 model pixels (depending on the model) using hourly (or 3-hourly) output. This allows a fairer evaluation against the observations especially since trace gases have important diurnal cycles. Whilst the model results are an average over fairly large spatial scales, such a comparison provides useful insights into the vertical distribution of pollutants simulated by the models over a region which more representative of the less polluted background."

Section 2.5: The meteorological data are highlighted as a source of model differences, but no indication is provided of how good they are (how close are they to observations?) or what differences they introduce. Fig 1 shows that the NorESM fields are very different from the others. What can we learn from pollutant comparisons if the underlying transport patterns are substantially different from those observed? Authors' reply: The models were forced, primarily using meteorological analyses and reanaly-

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ses from ECMWF, or NCEP in the case of WRF-Chem, in order to nudge the model results closer to reality in the case of the large scale dynamics and humidity, for example. These meteorological analyses have also been corrected with assimilated observations a posteriori. At this time of the year, the ïňĆow over the southern part of East Asia is inïňĆuenced by the Asian summer monsoon with dominant synoptic winds blowing from the south linked to the Anticyclonic circulation over the PaciïňĄc Ocean to the east. This results in high levels of relative humidity over the region. ECMWF and NCEP wind ïňĄelds are rather similar suggesting that differences in large-scale transport patterns are not the main cause of differences in trace gases and aerosols discussed in later sections. As pointed out by the reviewer, NorESM was not nudged to meteorology, but forced using reanalysed sea surface temperatures for 2008. Figure 1 shows that this model does have a monsoon circulation, but that it penetrates to far to the north over China. NorESM was included in this evaluation to provide consistency with companion papers (e.g Stohl et al., 2015) and because it was used to derive present-day and future emission impacts on air quality and climate. NorESM relative humidities are also higher over China, for example, compared to ECMWF and NCEP analyses which is likely to affect ozone photochemistry as well as aerosol formation. These factors, linked to model representation of the Asian monsoon, are discussed in the results sections.

Section 3.1.1: Are these spatial or temporal statistics/correlations? This should be stated clearly here, and in the captions to the relevant tables. I assume they are spatial statistics, in which case the start of the section (p.11060, l.6) should state clearly that it is the spatial patterns which are being evaluated here. Authors' reply: These are spatial statistics/correlations. We have revised Section 3.1 and merged the original sub-sections to make the text clearer. The point about the use of IASI data to evaluate models on continental scales is covered in the introduction to Section 3 and elsewhere in the manuscript. See also the reply to the next comment. The Table captions have also be updated.

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p.11061,l.14: What does the good agreement actually tell us here? Does it just reflect the latitudinal variation in tropopause height, or is there a more interesting story? Authors' reply: We agree with reviewer#2 that the good agreement is actually reflecting the latitudinal variation of the tropopause height since the highest concentrations are observed and simulated in the north of the domain, where the tropopause height is lower with higher amounts of stratospheric ozone included in the 0-20 km column. But, the main finding here is the fact that the overestimation of the 0-6 km column ozone by the models is not due to a general overestimation in the stratospheric ozone ïňĆux in this region, since models show good agreement with 0-20 km IASI ozone columns (high correlation coefïňĄcients (R > 0.93). The main discrepancies reported in the modelled tropospheric columns are mostly linked to photochemistry and transport rather than stratospheric intrusions (as discussed in Sect. 3.6). Section 3.1 has been re-written to make this point clearer.

p.11062,l.26: observed "spatial" patterns? Authors' reply: Yes, the text has been updated (see above).

p.11063,l.29: Biases are substantially worse at 0-6 km than at 0-20 km, as expected, as they are not dominated by stratospheric influences. What can you say about the source of the biases here? (e.g., overestimation over East China Sea, etc.) Authors' reply: Reasons for model biases compared to IASI, and the other datasets used in the evaluation, are now discussed in detail in the new discussion Section 3.6.

Section 3.5: As noted in the general comments above, a clearer and more detailed discussion of the expected chemistry and transport biases in this section would make the paper much more useful. Authors' reply: The section (3.6) discussing the results and causes for model discrepancies has been totally rewritten to include an analysis of possible causes for biases in the different models. We attempt to draw out some systematic biases in the results and to attribute these to particular issues in different models. Note that we have also included a more complete discussion of aerosol discrepancies in Section 4.3.

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Section 4.2: How are the aerosol composition biases identified here likely to influence the optical properties assessed earlier? The large overestimation of BC is a particular issue. Comparing the performance in different variables at the same time should allow stronger conclusions to be drawn. Authors' reply: In general, biases in the aerosol composition mostly concern BC in urban environment. This bias is expected to increase the AOD and Rapp values, although its impact is limited because of (i) the small BC contribution to the total aerosol mass, and (ii) the smaller contribution to extinction of a given BC particle compared to similar (in mass) OC and sulphate particles. More specifically, some models show important biases in very scattering species (OC and sulphate) which are correlated with excessive values of AOD and Rapp, e.g. EMEP and NorESM (over Korea only). We have added a point about the BC/SO4 ratios in the discussion in Section 4.3: "The BC/SO4 ratio observed at Beijing is almost constant (âĹij 0.2) with enhanced values detected episodically (02-03 August, 16-17 August, 01-02 September), where the ratio can reach 1. All models reproduce this ratio reasonably well (not shown), but two (HadGEM and EMEP) show high oscillations between 0.1 and 6 (mean value of âĹij 2). Over Gosan, the observed BC/SO4 ratio is lower (âĹij 0.1) underlining that Gosan is a more remote site from local sources. Models also present a good agreement, except EMEP (âĹij 0.4) and TM4-ECPL (âĹij 0.2), which overestimate the ratio. Such discrepancies may affect model responses to emission perturbations and thus radiative forcing." We also highlight evidence for : "a general underestimation of scattering aerosols in the boundary layer associated with overestimation in the free troposphere pointing to modeled aerosol lifetimes that are too long. This is likely linked to a too strong vertical transport and/or insufiňĄcient deposition efiňĄciency during transport or export from the boundary layer, rather than chemical processing (in the case of sulphate aerosols). The top-heavy distribution of sulfate implies substantial errors in the simulated aerosol-cloud interactions, with too weak effects if not enough of the sulfate impacts the boundary-layer clouds."

p.11075,l.21: The short lifetimes aspect is important here (see general comment about Section 3.5), but you don't explain why this might be the case. Author's reply: We agree

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with the reviewer that it is important to identify biases in modelled lifetimes of ozone precursors, such as CO. Our analysis suggests that this more likely linked to excessive ozone production than to transport errors. This is explained more clearly in the new version of the manuscript where we have included further discussion in Section 3.6: "The ECLIPSE models also systematically underestimate CO downwind compared to surface data over Korea, Japan, and compared to IASI CO data over Japan and downwind over the north-western Pacific Ocean. Whilst inclusion of additional seasonality in the ECLIPSE emissions (already included for domestic combustion), might improve agreement in winter and spring (Stein et al., 2014), this is unlikely to explain these summertime differences. Low model CO appears to be linked to the clear overestimation in modeled ozone at rural sites and compared to IASI 0-6∼km column data. Excessive ozone resulting in too much destruction of CO by OH and may suggest that modeled CO lifetimes are too short. This hypothesis is consistent with the findings of Monks et al. (2015) who concluded that, in models run with the same emissions, differences in OH∼(chemical schemes) are a more likely cause of the systematic CO underestimation in the Northern Hemisphere and the Arctic than differences in vertical transport. Indeed, we find that surface August mean modeled OH (not shown) is higher in the NorESM model (due to the penetration of the monsoon flux) compared to, for example, TM4-EPCL and WRF-Chem over the main Chinese emission regions. In contrast, excessive modeled CO over the central Pacific, where concentrations are low, may be due to the position of the Pacific anticyclone in the meteorological analyses used by the majority of models. A shift in the position of the anticyclone to the south could result in this pattern of negative (positive) biases over the north (south) Pacific potentially as a result of transport that is too zonal. This may also explain low modeled CO in the Arctic noted by Monks et al. (2015)."

p.11075,l.25: Coarse resolution models are not appropriate tools for representing regional air quality, so this sentence simply supports the findings of many previous studies. Authors' reply: We agree with reviewer#2 's comment and removed this sentence. We have also added the following sentence in the Introduction : "The ECLIPSE global

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chemistry-climate models may not be the most suitable tools to assess air quality impacts, however they are the tools used to evaluate climate and air quality impacts together. To address this point, a regional model is also included in the evaluation and one of the global chemical-transport models is run at relatively high horizontal resolution (50 km) compared the other global models." Based on results from this study, we have however included in the summary section : "This evaluation has important implications for accurate assessment of air pollutants on regional air quality and global climate based on global model calculations. Overall, Earth System Models perform as well as Chemical Transport Models in terms of atmospheric composition, which is encouraging for the further use of the Earth System Models in determining the climate impacts. Ideally, models should be run at higher resolution over source regions to better simulate urban-rural pollutant gradients/chemical regimes, and also to better resolve pollutant processing and loss by wet deposition as well as vertical transport. Discrepancies in vertical distributions require further quantiïňĄcation since these are a key factor determining estimated radiative forcing from short-lived pollutants."

p.11076: The final paragraph of the conclusions makes a number of very vague and general statements about model weaknesses and about improvements needed, but there are no specific new conclusions derived from this study. Please sharpen up this paragraph to include some firm guidance on how weaknesses should be addressed. Authors' reply: Conclusions have been rewritten accordingly with modifications in Sections 3.6 (trace gases) and 4.3 (aerosols). We have substantially rewritten the paper providing additional scientific analysis that was, as the reviewer points out, missing from the submitted version of the manuscript.

Table 3: The NMB of the model mean is a lot larger than that of any of the contributing models. This looks odd, please check. Authors' reply: We thank the reviewer for pointing this. The correct value is 2.5, not 25.0. It has been replaced in Table 3.

Typos p.11052, l.11/l.26: "Hong Kong" p.11052, l.21: missing "the" before industrial p.11055,l.5-6: "allow to control" would be clearer as "address" p.11056, l.18: gram-

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mar incorrect: perhaps remove "description"?p.11067, l.6: Monks reference should be 2015. p.11075, l.13: "climate" not needed.

Authors' reply: All the typos have been corrected.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C12272/2016/acpd-15-C12272-2016-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11049, 2015.