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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The effects of intercontinental emission sources on European air pollution levels" *by* J. E. Jonson et al.

Anonymous Referee #1

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Review of The effects of intercontinental emission sources on European air pollution levels, submitted to Atmos. Chem. Phys by Jonson et al., 2018

The manuscript submitted by Jonson and co-authors address the issue of the relative importance of local (continental in this case) and remote (hemispheric) sources to surface ozone levels. It builds upon the modelling works undertaken by the UNECE Task Force Hemispheric Transport on Air Pollutants. The questions addressed by HTAP have a clear political relevance. It also relies on a vast amount of scientific work well illustrated by the number of articles in the current special issue. In fact, the topic chosen by Jonson et al. is perhaps one of the most relevant, at least from a European perspective. Unfortunately, the article as it stands today is not of satisfactory quality to allow publication in ACP. Major revisions are needed, and my review below explains

why. The approach overall scientifically sound, but there are a number of omissions that must be addressed. Also, the quality of the presentation is far below the usual ACP standards. Nevertheless, for the reasons listed above in terms of motivation for the study, the authors should be offered a chance to resubmit their work in a revised form.

Specific comments

The aim of the article and its structure are not well explained in the introduction. The structure in L66-70 seems not to match the actual content of the section (Section 5 became a very superficial discussion on model resolution, it seems that the authors forgot that they originally expected to suggest improvement in the experiment design in that section). It is unclear why comparison with measurements come back in Section 5, while it was introduced in Section 3. At the end of the introduction the reader is already sceptical to what extent the paper will address the problem at hand.

L23: define here the CTM acronym, which usually refers to chemistry transport models rather than chemical tracer models

L25: TF-HTAP is organized under the EMEP programme of LRTAP

L50: add that the region targeted in that paper is Europe, but that (unlike in HTAP) the contribution is assessed by model tagging rather than sensitivity experiments

L53-54: the sentence on additional papers is not relevant, suggest removing

L60-63: should be moved to the experiment description part (ex: L78)

L63: rephrase "secondly we look at CO" to better introduce the actual chemical compound in opposition to the CO-like tracer

L87: For transparency and reproducibility concerns, but also with regards to the HTAP requirements, GFDM_AM3 should not be included if it is not part of the database.

L92: The Galmarini et al. article in the special issue is focused on the complementarity of global and regional models rather than model evaluation. In the version currently in discussion, only a Taylor diagram is given with models not labelled. Therefore it cannot be considered as a satisfactory reference regarding the capability of HTAP models in capturing surface ozone. Such an analysis should be included here if not covered elsewhere. The scatter plots in supplementary material is a good start, but further discussion is needed. The selection of Airbase sites is very questionable at this scale.

L104: a reference is needed to conclude that GAW sites are affected by local sources. Similarly, one could question why engaging in such analysis if global models are not capable to capture regional sources. If the author conclude that it is the case, it would be a major conclusion of the paper.

L116: what is the source of ozone profiles ?

L118: how “approximate” is the temporal matching between model and observations?

L123: more quantitative results are needed to support the “tendency” for underestimation in tropospheric summertime ozone.

L154: why is GFDL_AM3 included in Figure 3 for CO_tracer but not in Figure 2?

L159: why would EMEP have a too strong convection ? If the comparison with measurement suggest that EMEP performs better than other models (L348), maybe the other models have a too weak convection ?

L162: the larger vertical mixing seems to occur mainly in winter for EMEP, isn’t that conflicting with the hypothesis about the role of convection ? Maybe more discussion would be needed on the vertical diffusivity and resolution of the various models.

L183: The difference between CO_tracer and CO seems larger for EMEP than for the other models. Would it also be the case in terms of relative increase, and if so, why?

L195: the model differences for OH are very impressive (Fig 9 of the supplementary material). To the extent that one may wonder the relevance (and need) to produce

a multi-model mean. Further discussion and external references are needed for that section. The sensitivity to upper boundary conditions, especially for EMEP that seems to behave differently.

L213 : a reference is needed to support the statement on the relative contribution of stratospheric/tropospheric ozone.

L245: the discussion on aircraft emissions is interesting, but it seems that there are more important differences, such as the role of surface titration (why EMEP seems the less sensitive despite the higher resolution). Or the fact that the O3 response of Chaser is actually very close to that of CO_tracer (or is it a mistake in the Figure?)

L256: the fact that CH4 is excluded from the experiments should appear before in the experiment description (Section 2).

L259: the explanation of figure 5 needs to be re-worked in the text, in particular to explain that even if some sources are not isolated in some models, their contribution is still accounted for in the “remaining” fraction.

L266: more quantification is needed regarding the relative role of external/European sources. Figure 5 indicates that the external contribution seems indeed to exceed European contribution, but they are actually not that far. The percentage contribution (with error bar) should be given.

L275: the comparison the HTAP1 is too weak. It is very frustrating not to better understand the added value of the new experiment and to what extent the earlier conclusions still hold. The benefit of having engaged in a complete new experiment should be better assessed. For instance by looking at a subset of models having participate to both and investigating clusters of regions for the reference/sensitivity simulations to conclude on the importance of (i) emission changes, (ii) region definition, (iii) participating models.

L301: it is surprising to say that a comparison should not be made, when it is made by the authors. There were no similar discussions on the realistic aspect of sensitivity experiments for non-CH4 species, so from an academic perspective regarding chemical sensitivity the comparison does hold.

L330: the results related to ozone indicators are interesting and worth being highlighted in the abstract. It is frustrating that only one model can be used here, especially given the apparent different behaviour with regards to titration. More efforts should be given to investigate the HTAP database in order to include more models for a comparison of summertime mean of daily ozone maxima, or at least summertime mean ozone.

L350: according to Section 3.2, EMEP is not the only model to display an overestimation of tropospheric ozone.

L360: how can a lower diffusion can lead an underestimation of surface CO, the opposite would be expected

L367: what is the rationale for a relaxation to zero in the GEOS-Chem adjoint?

L372: Section 5 is very descriptive and lacks a clear outcome

L399: From the results provided in Section 5, it can not be concluded that the results are not sensitive to model resolution.

L406: in Section 4.1 it is rather convection that is put forward rather than chemistry. The following sentence (L407) also goes in that direction. It is quite surprising to read that the conclusion and the content of the paper seem contradictory.

L419: from Figure 5, it seems that about half of ozone can be mitigated with European sources, isn't that "sizeable" already? More precise quantifications must be given in the conclusion and abstract with key figures and associated error bar across the multi-model ensemble.

Table 1: what is given on the right part of the table? From the text, it appears to be correlation, but that should be stated clearly. Is CHASER_rel actually Chaser_t42 according to table 1 of the supp. Mat. A uniform model labelling would be appreciated.

Table 2: ibid about CHASER_rel. The labels of regions should be consistent with Fig 1.

Table 3: swap the first and second sentences. Use boldface rather than larger signs for important deviations.

Figure 2: GFDM_AM3 missing for CO_Tracer

Figure 3: panels g) and i) appear identical

Figure 4: the panels are truncated

Figure 5: the panels are truncated

Figure 6: add that the results to 20% perturbation are plotted.

Supp. Mat. Table 1: all models are in bold, not the first 7, since 7 models are displayed. The information about spin up should be given in the experiment description, not in Table 1. Footnote #2 is not references in the text.

Supp. Mat. Figure 1: What happened in Heimaey in May in observations?

Supp. Mat. Figure 4: More details are needed in the legend about the source of data and the indicator displayed

Editing

L26: dot missing after the parenthesis

L42: define GCM, isn't it rather Climate Chemistry Models that would be referred to ?

L48: organic aerosols are OA

L50: problem with the reference

L73 dot missing after the parenthesis

L73, L75: dot missing after etc.

L87 : space missing before GFDL

L92: space missing after measurement

L111: AQMEII

Hyperlinks should be in footnotes rather than in the text

L360: lower cap for underestimates

L366: the notation “20+ percent” is not appropriate for a scientific paper

L369: incomplete sentence

L402: d missing in “and also”

L409: “in” missing: “steps in improving”

L435: EMEP and Airbase data should also be acknowledged here.

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