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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 #2

Received and published: 6 March 2018

title: The effects of intercontinental emission sources on European air pollution levels

Overview: This paper has a significant contribution, but the manuscript has many errors and organizational issues. The authors approach of tracer, CO, ozone analysis could provided a nice insight into model differences. The manuscript, however, is full of obvious errors that should not make it to a reviewer.

The evaluation in a stepwise approach provides useful insight. Perhaps the most interesting results (rows 1-3 in Figure 3) however are not correctly displayed. This leads to difficulty interpreting *the* most interesting contribution.

There are many typos, grammatical errors, and glaring omissions. As a funny example, the HTAP phases are inconsistently numbered and the AQMEII project is misspelled.

The figures are incorrectly referenced and in some cases contain the wrong content (e.g, fig 3i). Several of the models are not referenced when they are clearly relevant to a point the author is making. For example, where is AM3 on page 8 lines 240-245?

The manuscript needs major reorganization. The manuscript reads as though sections were added without consideration of what had been previously said. For example, the authors suggest that evaluation should be found in other papers which leads the reader to believe evaluation will not be discussed (section 3: Models vs measurements) and then simplistic evaluation is provided in the discussion (section 5). The organization provides little methodological context for the results in Section 2 (the HTAP2 model setup) – instead, "revealing" methodological inconsistencies in the results (e.g., Section 4.3) or model configurations in the discussion (Section 5). Section 5 is a combination of nice insights and throw away paragraphs that seem unfinished.

Lastly, the issue of titration seems important to the conclusions. The results in Figure 5 show that most models calculate local titration in many months. While that suggests low sensitivity, "contribution" as defined by source apportionment or tagging would likely produce a much larger local contribution estimate. The 20% maybe just beyond removing titration but before reaching control effectiveness. In the conclusions, the authors suggest that controls have been offset by increases in other regions. The titration still present, however, suggests that European controls have also been offset by removed local suppression. These are important considerations for highly non-linear regions like Europe that are not sufficiently addressed.

This manuscript is not ready for publication. The underlying work is clearly important and makes a contribution, but the presentation (including the writing and organization) are not ready for publication.

Specific Comments (lines: comment)

16: capitalization error. 25: missing parenthesis 29: verb agreement 46-54: The list of published papers should be used to provide context. Here it is simply a list. 60: details like model count would be better in the methods. 68-70: differ should be differences? 72-74: poorly written. 82: "etc" seems particularly poor when later you will refer to advection schemes as a causal difference. 87: missing space 92: missing space 95: missing "is" 95: How does evaluation of upwind sources affect conclusions about transport to Europe? 99: spell out GAW 101: How "high" correlations are expected given the resolutions of the models? 101-102: resolutions of all the models should be provided in the methods rather than the comparison to measurements. 98-109: How is it that CO deserves a site-by-site comparison and ozone 112: The authors should mention that they do have some surface evalaution in this paper. Currently, Table 3 in this manuscript is not referenced until Section 5. 114: There is currently no discussion of ozone results except to say they exist somewhere in the supplement. Why is this sufficient? 122-123: There must be more discussion of the basic results that will clearly affect transport. 138: Here and elsewhere the definition of regions is incorrect. Here you have NW, SW, SE, GR+TU. In the Figure, you have NW SW, E, GR+TU. Other places you have NW, SW, E, SE. Choose one, and be consistent. 139-140: Is this source apportionment the same as contribution in sections 4.4 and 4.5? 142: rate of decay is later explained, but here seems completely arbitrary. 182: Wrong figure references. 185-189: The reasonableness of this should be discussed. 205: This gets discussed in several places and is really part of the methods. 217-219: Web citation is inappropriate. Further, the lifetime of ozone is expected to vary with respect to season and altitude (Wang et al. 1998; Brasseur, Orlando, and Tyndall 1999). Estimates of lifetime at 500hPa range from 15-160d and from 40-300d at 10km. Your upper bound of 18days is misleading. Table 1.1 of the HTAP 2010 report cites weeks to months in the free troposphere. The IPCC range of values do not acknowledge the complexity of ozone transport. 242: AM3? 246-247: Provide some reference or evidence. 247: here = PBL? 254-284: Is this contribution from a simple mean within seasons? What months were included in each season? Are the numbers in the text ensemble means? What about ensemble mean RBU? MDE? EU? 290-291: Did they "too" calculate smaller "than in this study" or did they "too" calculate "smaller as in this study"? 269: MDE

appears to always be small. 272-273: Did these other studies use the same model? 277: missing parenthesis 274-280: methods? 305: HTAP1? 306-335: There needs to be a clearer connection to the previous section. In fact, you could just add two bars to Figure 5a. That would help to connect the of POD and SOMO35 to the seasonality of titration. 361-371: Terse and uninformative. 390-392: See previous comments about ozone lifetime. 400: probably? 405: Ozone?

Table 1: If mountain sites are used at readers peril, consider making room for ozone evaluation by moving them from the first data result.

Table 2: Update region definitions to be consistent with figures and text.

Figure 1: update region names to be consistent. Also, too many extra colors so it is hard to tell what is included. Is the Baltic Sea part of Eastern Europe? Black Sea? Caspian Sea? Mediterranean?

Figure 2: necessary?

Figure 3: lettering needs to be updated in the figure and in the text. What was the common grid and how was it treated when a gridcell at 1000hPa was below the surface?

Figure 3: 3i is AM3 CO not ozone. Column 3: consider a scale that does not saturate in so much of the figure.

Figure 4: North and south boundaries are unnecessarily different from figure 3. Further, this highlights that no meaningful discussion of the boundaries was made. In fact, 50E includes a lot of Russia and a lot of ocean. Column 3: consider a scale that does not saturate in so much of the figure.

Figure 5: There is no discussion about the CHASER model being the only one without apparent titration, and this should be discussed somewhere. Region definitions should be consistent with the text or the text should be consistent with the figure. The units are cutoff on the first row.

Figure 6: region definition nomenclature. I recommend showing as 3 stacked-bars (or adding to Figure 5). If I am interpreting this right, the RAIR is 84% compared to 43% from HTAP1. I suspect that all models provided annual and I think reporting RAIR would be useful (maybe in Figure 2).

References: Brasseur, Orlando, and Tyndall 1999, ISBN:978-0-19-510521-6 Wang et al., 1998 doi:10.1029/98JD00156 Figure 4 HTAP, 2010

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