

Interactive comment on “Tropospheric ozone and its precursors from the urban to the global scale from air quality to short-lived climate forcer” by P. S. Monks et al.

Anonymous Referee #1

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This article represents a significant effort towards summarizing the state of knowledge regarding tropospheric ozone and its impacts. It is thematically comprehensive, and generally well-written. It suffers from some redundancies and hiccups typical of an article with so many contributing authors; I encourage the authors to pay particular attention to editing for this before final publication (some specific examples are included below).

My primary critique is that the article is quite Euro-centric, some sections more than others. Given the author affiliation and the association with the ACCENT project, this is not surprising, but needs to be addressed prior to publication. In present-form the article requires some caveat (“with a focus on Europe”) in the title, abstract, and throughout

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the text. Alternatively, to accurately reflect the title and goals of the paper the authors need to address this issue on two fronts. The first is including discussion of results from non-European projects. Two examples of this: in the context of biomass burning (4.2), there was no discussion of the ARCTAS project which provided significant insights into the role of boreal fires on atmospheric composition. Similarly the megacities section (4.1) made no reference to the MILAGRO campaign in Mexico City, a tremendously comprehensive megacity field campaign. Furthermore, the authors should be cautious about focusing the discussion/implications solely on impacts in Europe (given the title of the review). There are occasional references to the United States (and the Arctic is featured), but very little reference to Asia or other regions of the world where ozone has been investigated. The second front is the failure to reference the scientific literature from non-EU scientists. This is a pernicious bias which is a challenge to address, and of course the natural outcome of geography (we are all more aware of the studies authored by our collaborators, and seen presented at local conferences). But given that this is a review article, I would encourage each of the authors to examine their sections with this in mind, and return to the literature to identify earlier or critical work by non-Europeans which should be acknowledged.

Section 4 is also poorly organized – it seems a grab bag of topics. Topics such as lightning (4.7) and biomass burning (4.2), halogens (4.4) appear to belong in Section 2.3 (precursors). I suggest a re-organization.

I include some additional minor comments below.

1. Overall: inconsistency of units; suggest using ppb throughout (and converting all references using $\mu\text{g}/\text{m}^3$)
2. Pg. 32712, lines 14-29: This content doesn't seem to fit particularly well in the introduction, and is a bit redundant with the review in section 2.1. I suggest merging this paragraph into that section. It is also worth noting that the end of this paragraph is a bit misleading as it could leave the reader with the impression that this debate is still

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alive.

3. Pg 32714: The tropospheric ozone budget is quite precisely quantified here (3 significant figures on the burden!), but a discussion of uncertainty and variability in model budgets (beyond the regionality discussed in Figure 2) is lacking.
4. Section 2.1.1: It would be useful if the authors could include some discussion of the relevant timescales of processes in this section.
5. Pg 32720, line 9: language needs to be corrected.
6. Section 2.1.2d: Obvious missing reference/discussion: Kurpius and Goldstein, GRL, 2003
7. Page 32729 lines 26-30 & 32730 lines 1-3: Specify whether these are surface, profile or column concentrations
8. Page 32731, lines 1-3: This list of factors should also include meteorology beyond transport (rain, radiation, temperature, etc), as well as changing surface cover (e.g. vegetation).
9. Page 32733, lines 19-22: This sentence requires a reference – unclear if the citation from the previous sentence applies here.
10. Page 32745, lines 15-17: Sindelarova et al. is not an appropriate reference here (an application, not the model description). MEGAN v2.1 also includes an algorithm for CO₂ inhibition of isoprene emissions.
11. Page 32758, lines 16, 21: Remove the repetition of line 16 of line 21. The specific reference to rice is also odd, there are many more crops affected by ozone...a list of vegetation types affected and some references would be useful.
12. Page 32760, line 2: Tai et al. show that climate reduced global yields by 11% (not > 20%)

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13. Page 32762, line 8-9: What does “the fundamental processes involved” mean? Please elaborate.
14. Page 32763, line 1-2: Needs reference.
15. Page 32763, line 28: also domestic biofuel use?
16. Page 32764: lines 25-29: needs a reference (particularly for the role that evaporative emissions played in this event)
17. Page 32770: lines 15-17: needs reference.
18. Section 4.3: This might also be a good place to note the potential impacts of biofuels on ozone concentrations, via changes to vegetation and BVOC emissions (e.g. Ashworth et al., 2012; Porter et al., 2012).
19. Section 4.5: This section does not seem to be particularly relevant to a review of tropospheric ozone. The key elements of the role of NO_x have been discussed elsewhere.
20. Section 4.7: The basic chemistry described in this section is redundant with earlier descriptions of ozone formation. Please harmonize.
21. Section 4.7: the discussion of how lightning relates to aerosols is not relevant to this review.
22. Page 32794, lines 1-11: In addition to several studies which show high methane leakage from fracking operations, the authors should note the Allen et al., PNAS, 2013 study which provides the counter-example of low measured leakage rates.
23. Section 4.10: This section is quite long, and should be edited to re-focus on implications for ozone.
24. Page 32803, lines 20-25: A number of studies identified the role of intercontinental transport of ozone in ozone exceedances prior to the publication of the HTAP report in

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2010. As written, this sentence is a bit misleading.

25. Page 32805, lines 14-18: unclear what scenario this mortality reduction corresponds to – is this the result of reductions of European emissions only?

26. Section 5.3, 5.2, and to some degree 5.1 need to be merged and redundancies eliminated.

27. Section 5.4: Tai et al., GRL, 2013 recently showed the “climate penalty” has been substantially overestimated. The discussion should be modified in light of this.

28. Page 32808, lines 14-16: It is not clear why emissions that were constructed for radiative forcing purposes would be inappropriate for air quality purposes. Please justify any concerns about quality of these emission inventories, and why they might therefore still be appropriate for climate projections.

29. Page 32810, lines 10-22: duplication with Section 5.4.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 32709, 2014.

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