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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Scorched earth: how will changes in ozone deposition caused by drought affect human health and ecosystems?" *by* L. D. Emberson et al.

Anonymous Referee #1

Received and published: 4 December 2012

This paper describes a modelling exercise to assess the impact of differences in assumptions of O3 dry deposition on premature mortality rates and vegetation damage. For this purpose a highly detailed and advanced O3 deposition model (DO3SE) is linked to a state-of-the-art air quality model (CMAQ), to provide online deposition velocities for O3. The model ozone is verified against a small set of air quality observations at rural stations, distributed over the UK. Model surface ozone has been post-processed in a model which estimates the dependency of health risk to elevated ozone levels, as well as to a model to assess the vegetation damage of increased O3 levels. These estimates were accompanied by two sensitivity studies: the minimal and maximal edge of possible dry deposition. This resulted in an estimation of approx. 460 premature deaths due to decreased dry deposition rates during June-July 2006 in the UK. While abstract and conclusions are rather firm in their statements, a considerable section is devoted to a discussion of the obtained results, where uncertainties in the followed approach are described.

General comments:

1. It appears that the actual O3 dry deposition rates for period investigated here (June-July 2006) are close to their minimal values, comparing the various statistics of actual vs minimal dry deposition (e.g. Table 3 and Fig. 7). However, it remains unclear whether simulation with large dry deposition velocities is realistic, in the sense that it is close to a normal, climatological, summer. Therefore any increase of surface ozone, and derived quantities such as health risk and vegetation damage, as computed as the minimum versus maximum dry deposition seems irrelevant, and a pure model exercise. Additionally, giving so much attention to these sensitivity studies using the simple assumptions of extreme (low/high) dry deposition divert from the novelty of the current system, and its evaluation. I suggest the authors to replace their sensitivity study using lowest edge dry deposition with a sensitivity study using 'climatologically normal' dry deposition for summertime situations. This would better quantify the sensitivity of the dry deposition changes on O3 concentrations during this heatwave episode.

2. Model validation is performed against a small set of O3 stations in the UK, with variable success. I have a number of questions / comments on the chosen strategy for evaluation, in general I would like to see a more profound evaluation of the system in terms of surface O3, considering this plays a pivotal role in the evaluations that follow.

a. Why are so few stations selected? It is mentioned that only stations with >90% observations are selected. Why not include also stations with >70% observations, when this brings in valuable information for the time periods that data is available? Furthermore, it would help the reader if the location of the different stations is given on a map.

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b. It appears that the authors have chosen to evaluate their model to rural observational stations. This is common for many air quality models, with the valid argumentation that models cannot reach the high spatial resolution. Nevertheless, the method of the authors to quantify the health risk due to surface O3 depends on the quality of the system to model urban O3 concentrations, as can be seen from Fig. 4. To my opinion an evaluation of model urban O3 concentrations should be included, rather than referring to other studies.

c. Figure 1 shows a scatter plot of the total days of O3 exceedance between May and July 2006. Nevertheless, it is unclear whether the actual exceedance at a specific day was modelled correctly, which is an important feature of an air quality model. This can be quantified by hit and false alarm rates, see, e.g., Savage et al., GMDD 2012. I believe the authors should replace their evaluation given in Fig. 1 with an assessment of the hitrates, which is a more accurate metric.

d. Apart from an evaluation of surface ozone, an evaluation of soil moisture deficit, as a crucial parameter in the dry deposition parameterization, should be quantified better, rather than referring to other material.

e. While much attention is given to the parameterization of O3 deposition, dry deposition of other trace gases is not discussed, except for a small note in Sect. 4. This raises the question whether the modelling approach is out of balance by putting so much attention to one type of parameterization, while other sensitivities are omitted. Summarizing, the authors may consider resubmission of this manuscript to GMD, in view of the strength of their model parameterization in this work, while its validation is relatively poor.

3. The way the results are presented by the authors is to some respect misleading and should be improved. Model uncertainties, as described in the discussion, should be given a more prominent position, e.g. mentioning them in the conclusions and abstract. At several locations in Sect. 2 and 3 the authors should refer to the discussion

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section, e.g. when describing the uncertainties related death statistics using the 35 ppb threshold level in Table 1. The discussion section itself contains furthermore many excursions to subjects that are only marginally relevant for the current work, and this section should be condensed. The title of the manuscript (‘Scorched Earth’) seems not appropriate and should be reconsidered.

Specific comments:

pp 27852, l. 5 “equally well O3 deposition and precursor emission estimates”.

pp 27856, l9-l10 “EMEP/NAEI”: Please provide a reference, and specify the year for which the emission inventories have been compiled.

pp 27858, l27. “PLA”: what does this acronym stand for?

pp 27860, l7: “f_light was assumed to be that for clear sky” : How realistic is this scenario to obtain a lower limit of the stomatal resistance? Maybe it’s interesting to see a figure of actual (mean) evolution of g_sto, along with the maximum and minimum variants.

pp 27860, l17: “90%” why not include more observations, e.g. those with availability > 70%? Also, it might be interesting to differentiate the statistics in Tablet 2 per season, to assess whether biases are more prominent in summer or winter. Finally, It seems that model validation is performed for rural stations, while the method for estimating health effects depends on its quality in urban environments.

pp 27861, l19: “less than 30% of AWC remaining”: Why not show AWC in Fig. 2, rather than SMD, and compare this directly with observations, or the other model results?

pp 27862, l4: “estimates for the whole of the UK of exceedance of the DM100”: To me this does not sound like a very strict formulation. Could you clarify? Also the table suggests a strong sensitivity to the choice of the threshold. This is only discussed in Sect. 4, while I was struggling with an interpretation at this location.

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pp. 27863, l. 14: “reduced under the no stress scenario by 410 premature deaths”: How realistic is this scenario?

pp 27866, l 4: “increased by almost two thirds”: Where does this number come from?

pp 27866, l6: please modify to: ”under the stress drought scenario, compared to the no stress scenario. We…”

pp 27866, l13-l22: To my opinion this section can be removed.

pp 27866, 25: The question whether a threshold for O3 effects exists should be mentioned earlier.

pp 27867, l15: “Therefore, the importance of O3 dep. pn human health risk is largely independent of the threshold value chosen”: While this is true for absolute values, this percentual contribution of health risks is much decreased when removing a threshold. This suggests that introducing a cut-off will artificially exaggerate the impact. Can you comment?

pp. 27868, l2: “The work presented here…” The authors write that O3 deposition should be considered to estimate impact of changing climate on O3. I wonder whether the dry deposition parameterization is that crucial as compared to uncertainties in other parameterizations, such as emissions, transport, meteorology. Please comment.

pp 27868, “AOT40”: The authors suggest that the AOT40 index causes problems. To substantiate this, it would be good to include a figure. Otherwise, to my opinion this section can be removed, as it seems beyond the scope of this manuscript.

pp 27869, l19 : This section can be removed, or condensed.

pp. 27870, l10 – l18: This can be removed, as it seems beyond the scope.

pp 27871, l1- l17: This section may be removed, or condensed. The sentence on the performance of SMD (“Büker et al., 2012”) and the outlook (“More testing is required”) could go to the conclusions.

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Interactive Comment

pp. 27872, l27: "...can lead to at least $\sim$460 excess deaths": change to: "are estimated to exceed $\sim$460 excess deaths in the UK, in a worst case scenario."

pp 27973, l1 "damage to vegetation will likely be reduced", change to " ...be reduced, although it is acknowledged that the NPP is also decreased."

pp 27874, l12: "reference Carslaw, D." : This reference seems incomplete.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27847, 2012.

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