

***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.**

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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

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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.

Response: We are trying to show the potential effect of dry deposition on [O₃] and human health. As such, the scenarios we use in the study are not trying to simulate realistic conditions, but rather show the importance of the dry deposition term and the need to make sure this is modelled as accurately as possible. Therefore, we don’t think it is appropriate, or possible, to use the approach suggested by the reviewer of defining a ‘climatologically normal’ dry deposition, this would require definition of ‘normal meteorological’ conditions, and perhaps more importantly, associated ‘normal’

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O3 concentrations which is not possible and would not provide the information we are seeking.

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. Response: We agree that an extended evaluation of rural O3 concentrations would be useful. Therefore we propose to extend the number of rural sites from 9 to 19, with the selection being based on sites considered to be appropriate and reliable for model evaluation by Defra who identified the same sites for use in a UK national model evaluation exercise (Defra model inter-comparison exercise, <http://uk-air.defra.gov.uk/research/air-quality-modelling?view=intercomparison>). Now, rather than showing a comparison of the annual hourly surface O3 concentrations we focus on the health metrics and compare the reliability of the modelled values for different times of the year (winter, spring, summer, autumn). We also propose to include a map with Figure 1 of the original paper) showing the location of sites which clearly demonstrates the good coverage across the UK (see also next comment and a proposed new Table 2)

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

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than referring to other studies. Response: We agree with this reviewer (and the #2 reviewer, see later comments) that an evaluation of O₃ concentrations for urban sites would be an extremely useful addition to the paper. We therefore propose to extend the evaluation to include 73 urban site comparisons (only urban background, central urban and sub-urban locations). This evaluation will be included in proposed new Table 2 which focuses on the models predictive ability to estimate annual and seasonal metrics relevant to the health risk assessment (i.e., daily maximum 8-hr mean O₃ concentration as well as DM100) at 73 urban sites across the UK. Data are derived from the UK national (AURN) and London Air quality Monitoring Network (LAQN). The analysis also covers a larger numbers of rural sites (i.e., from 9 to 19 sites) across the UK (see response to reviewers comment above).

The statistical measures of model performance in predicting DM100 (number of days that the 8hour mean O₃ concentration was > 100 $\mu\text{g m}^{-3}$) and attributable mortalities due to acute O₃ exposure at 73 urban and 19 rural sites will be shown in a new table that we propose would substitute the original Table 2. Re-analysis shows that overall, the FAC2 values indicate >70% of modelled data are in within factor of 2 of the measurements. Although the model has tendency to over predict the DM100 by approximately 5 $\mu\text{g m}^{-3}$ at urban sites and 2 $\mu\text{g m}^{-3}$ at rural sites, the NMB values show both annual and seasonal model data are within +/- 0.2 considerably acceptable (Derwent, et al., 2009). Despite positive biases of the DM100, the model slightly under predicts number of days that DM100 > 100 $\mu\text{g m}^{-3}$, i.e., approximately 2 (-7%) and 4 (-13%) days at urban and rural, respectively. The attributable deaths at urban sites are over predicted by small margin due to the tendency of over predicting DM100. The attributable deaths at rural sites are under predicted and are likely to be driven by negative biases of DM100 in spring.

Finally we also propose to replace the original Figure 1 showing exceedance of the DM100 with new Figures 1a and 1b which show a modelled vs measured daily maximum 8 hour mean by season (more relevant for both types of health risk assessment

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conducted in the study) and also extends the original analysis to include additional rural sites and new urban sites in the scatter plot comparisons. The site location (Figure 1c described above) would be included with these figures.

References: Derwent R., Fraser A., Abbott J., Jenkin J., Willis P., Murrells T., 2010. Evaluating the Performance of Air Quality Models - www.airquality.co.uk/reports/cat05/1006241607_100608_MIP_Final_Version.pdf.

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 hit rates, which is a more accurate metric. Response: We believe that the ‘hit rates’ analysis is more useful to measure the skill of a model in forecasting. It does not provide information as to whether a model is able to predict the magnitude of O₃ concentrations accurately which is very important for our health and ecosystem risk calculations. We feel that adding ‘hit rate’ analysis may cause confusion in model performance interpretation as it is possible that a model that has high ‘hit rate’ score (high skill) predicts larger biases in magnitude as compared with a model that has lower skill (as seen in Savage et al., 2012). As such, we would hesitate to include the ‘hit rate’ analysis.

Reference: Savage, N. H., Agnew, P., Davis, L. S., Ordóñez, C., Thorpe, R., Johnson, C. E., O’Connor, F. M., and Dalvi, M.: Air quality modelling using the Met Office Unified Model: model description and initial evaluation, *Geosci. Model Dev. Discuss.*, 5, 3131–3182, doi:10.5194/gmdd-5-3131-2012, 2012.

d. Apart from an evaluation of surface ozone, an evaluation of soil moisture deposition, as a crucial parameter in the dry deposition parameterization, should be quantified better, rather than referring to other material. Response: We agree this is an important

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issue. We propose to include, in addition to the map provided in the original Figure 2, figures that show the seasonal profile of evolution of SMD for beech and grasslands. These figures will be used to indicate the sensitivity of the model to +/- 20% changes in SMD (since this is within the range of difference found between the MORECS and CMAQ-DO3SE model) and resulting influence on stomatal O₃ flux (by indicating the values of SMD above and below which stomatal flux will be affected), this also helps address the comments raised by reviewer #2). Although this will not bolster the evaluation aspect of the SMD work (which is complicated by the fact that no other spatial datasets for 2006 have been found) it will provide valuable insight as to the sensitivity of the SMD module and influence on flux and hence O₃ deposition and human health and ecosystem effects.

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. Response: Not exactly clear what the reviewer is referring to here. We would agree that other pollutants whose deposition is also controlled by stomata will be affected by the simulations presented in this study. For example, NO_x deposition may also be affected (reduced) by stomatal closure, with subsequent consequences for chemical titration of O₃. Would suggest adding a sentence to this effect in the discussion where the effects of heat wave conditions on other atmospheric constituents that may also influence [O₃] are described. Dealing with this is an area for future research.

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. Response: We argue against this summary since we propose substantial additions to the surface O₃ (both rural and urban) as well as new information describing the potential influence of the soil moisture stress effects which will provide a strong model parameterization as well as model validation and sensitivity assessment

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of the study.

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 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. Response: We will make mention of the key uncertainties in the conclusions and abstract. We will improve cross referencing between Sect. 2 and 3 and the discussion. We feel the issues dealt with in the discussion are relevant but agree they could benefit from being made more succinct and suggest some editing of the text to improve this (this also will help to address some of the 'specific' comments of reviewer #1 and some of the comments of the #2 reviewer of this paper). We feel the title is appropriate but will make the revision suggested by reviewer #2 to clarify the issue dealt with in the paper.

SpeciiñÅç comments: pp 27852, l. 5 "equally well O3 deposition and precursor emission estimates". Response: OK, will change but will maintain emphasis on improvement of O3 deposition since the paper is making the case that to date this has not been given the same attention as other factors that determine O3 concentrations.

pp 27856, l9-l10 "EMEP/NAEI": Please provide a reference, and specify the year for which the emission inventories have been compiled. Response: EMEP archive: <http://www.ceip.at> and NAEI archive: <http://naei.defra.gov.uk>. Both emissions are 2006.

pp 27858, l27. "PLA": what does this acronym stand for? Response: OK will change to make clear PLA stands for "projected leaf area"

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pp 27860, I7: “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. Response: Can add some text here to be clear what limitation this will cause.

pp 27860, I17: “90%” why not include more observations, e.g. those with availability > 70%? Also, it might be interesting to differentiate the statistics in Table 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. Response: Please see answers and new model analysis at urban sites in 2a and 2b

pp 27861, I19: “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? Response: Spatially explicit observations of AWC are not available and are only interpreted from the details of the MORECS model parameterisation and the description of the range of soil moisture provided for the country. However, we propose to deal with this important issue as described above in response to comment 2d.

pp 27862, I4: “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? Response: This refers to the UK average number of days that the daily maximum 8-hr mean O₃ concentration exceeded 100 $\mu\text{g m}^{-3}$. Will change to make this clearer in the text.

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. Response: The approach is indeed very sensitive to choice of the threshold and considered as its uncertainty. As mentioned in Sect. 2 (p27858, l. 14-16) using the 35 ppb cut-off is likely to underestimate the effects of O₃, we therefore included the results of the estimates without threshold (which is in Sect.3) to show an upper estimate of the attributable effects of O₃ on mortality. The uncertainty of the threshold seems more

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appropriate to be discussed in detail in section 4. We will improve cross referencing between Sect. 2, 3 and 4 to aid the interpretation.

C10100pp. 27863, l. 14: “reduced under the no stress scenario by 410 premature deaths”: How realistic is this scenario? Response: See response to general comment 1.

pp 27866, l 4: “increased by almost two thirds”: Where does this number come from? Response: Will clarify as being from Table 3 (Jun-Jul), “stress” – “no stress” DM100.

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

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

pp 27866, 25: The question whether a threshold for O3 effects exists should be mentioned earlier. Response: OK. Sect. 2.4 p 27858, l10-16 is meant to indicate the uncertainty of threshold. However, it is probably better to explicitly state the existence of the effect of the threshold.

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? Response: This is possibly true but we can also consider it in the other way round that having no threshold will artificially reduce the impact. As it is worth pointing this out, we would also stress that there is more confidence in the estimates with threshold based on the reason quoted in Sect. 2.4 p27858, l. 10-13 and discussion in Sect. 4 p paragraph 4.

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

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parameterizations, such as emissions, transport, meteorology. Please comment. Response: Yes. . .can add a comment here. There is no doubt that dry deposition process is as important as other processes in the sense that it is considered in most numerical models and has an instant role in a policy development. Our study has highlighted its significant roles on ambient O₃ concentrations and its effects on health and ecosystems. Within the paper, we have quoted a few references that send out the same message. The dry deposition process is driven by a number of factors such as surface characteristics, meteorology and particularly plant species-specific phenology and responses. The accuracy and availability of these factors are considered as a foundation of its uncertainties and therefore deserved our attention.

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. Response: We would argue that by referencing other papers this comment is validated and should remain; it also helps to explain why we didn’t use AOT40 which many of the ozone community not so familiar with ecosystem effects may still view as the index of choice.

pp 27869, l19 : This section can be removed, or condensed. Response: OK. . .can shorten but think it is important to retain reference to this issue

pp. 27870, l10 – l18: This can be removed, as it seems beyond the scope. Response: OK. . .although we think this is important we can see that it is not relevant to the current study

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. Response: OK

pp. 27872, l27: “. . .can lead to at least 460 excess deaths”: change to: “are estimated to exceed 460 excess deaths in the UK, in a worst case scenario.” Response: OK will modify

pp 27973, I1 “damage to vegetation will likely be reduced”, change to “. . .be reduced, although it is acknowledged that the NPP is also decreased.” Response: OK

pp 27874, I12: “reference Carslaw, D.” : This reference seems incomplete. Response: This is a working draft document on the ERG archive, the web site link can be added to the reference list. Carslaw, D.: Defra Phase 2 regional model evaluation, 2012 (www.erg.kcl.ac.uk/downloads/Policy_Reports/regionalPhase2.pdf)

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

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