

***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 #2**

Received and published: 13 December 2012

General comments:

While this is not the first study to consider the effect of dry deposition to vegetation on the concentration of ozone at the surface, the application of dose-response relationships to estimate the implications for human health and potential crop damage marks a substantive extension of previous work. The methodology employed is valid and builds on a significant body of research within the epidemiological and plant physiological communities. The results are discussed at length and the implications of the findings to policy-relevant questions explored. Overall, this is a well thought out, well conducted and well presented piece of research, and I recommend publication once

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the issues listed below are addressed.

Specific comments: 1. Confidence intervals: Epidemiological studies of the human health impacts of ground-level ozone routinely include estimates of the 95% confidence intervals for dose-response relationships, human mortality and morbidity. This is a relatively straight-forward calculation and I would expect at the very least that the confidence intervals for the health impacts are given here and discussed with the uncertainties, although the authors should also highlight how these figures have been derived and make clear that other sources of uncertainty (e.g. within the models used) would further widen the confidence intervals. (With a projected increase in mortality of at most 400, the confidence intervals could be of a similar order of magnitude, and if this is the case could the authors please comment on the implications of this to their conclusions.)

2. Threshold: Analysis of the health impacts of O<sub>3</sub> routinely employ a threshold concentration. While the authors are correct in their assertion that the validity of such a threshold is physiologically unlikely, epidemiological studies show that the correlation between O<sub>3</sub> concentrations and increased mortality is unconvincing below this level. As such, the authors' estimation of 16000 plus deaths attributable to O<sub>3</sub> is unconvincing (particularly in light of the (only) 22000 deaths estimated to be attributable to O<sub>3</sub> across all of Europe). Can the authors comment on why they have chosen to include these calculations? If the results of these calculations are to be included in the paper they should be put into context (i.e. by quoting the 22000 deaths across Europe).

3. Modelled vs. measured [O<sub>3</sub>]: The authors have taken pains to demonstrate the agreement between modelled and measured ozone concentrations at a number of rural sites across the UK, and note that, in general, agreement is good. However, they also state that the model has a tendency to over-estimate O<sub>3</sub> concentrations by between 2 and 14 ppb (p27861). An over-estimation of 14 ppb in mean O<sub>3</sub> concentration at a rural site is substantial, particularly as these concentrations are then used to derive cumulative threshold-based metrics. The authors need to calculate and show the

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effect that these over-estimates have on the threshold-based metrics that they use to estimate the impacts. How do the modelled 8-hr ozone and POD metrics compare against those calculated from measured O<sub>3</sub> concentrations?

The authors must further demonstrate that the use of these over-estimated concentrations does not significantly affect their estimations of mortality and vegetation impacts. I would like to see some exploration of the sensitivity of their results to more realistic O<sub>3</sub> concentrations (either by “correcting” the bias in model output data or by using the measured concentrations). The former could be applied over the whole domain; the latter extrapolated from the individual sites and closely adjoining areas.

Furthermore, the failure to include any urban measurement sites is of concern here as the study goes on to calculate the impact on human health. In this context, the ability of the model to correctly simulate O<sub>3</sub> concentrations in urban areas is of more importance than rural ones. Can the authors demonstrate that agreement between modelled and measured O<sub>3</sub> is good in centres of population?

4. Soil moisture status: The authors have stated that soil moisture is critical to stomatal flux and deposition and that the available water content calculated by the CMAQ-DO<sub>3</sub>SE model tallies with that from the Met Office (based on observations). However, there is a difference of 5% between the available water content calculated by the two models (i.e. DO<sub>3</sub>SE over-estimates soil moisture by 20% if MORECS is taken as “truth”, thereby over-estimating stomatal conductance and hence uptake of O<sub>3</sub> by vegetation). How sensitive is the DO<sub>3</sub>SE model of stomatal flux to soil moisture deficit? Please demonstrate that the 5% difference/20% over-estimate really does have a negligible effect on the projected impacts.

5. AOT<sub>40</sub>: The authors include a paragraph in the discussion section (p27868, L25 onward) specifically commenting on the use of the AOT<sub>40</sub> metric to assess vegetation damage. However, this metric has not been introduced, explained or discussed previously in this manuscript. If the authors wish to include it here they must explain what

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it is and how it is calculated. Furthermore they should explain why it would lead to increased rather than decreased O<sub>3</sub> concentrations. Personally, I would recommend removing this section, particularly as the authors choose not to show the data they refer to (the differing patterns of risk – p27869, L3-6). If they wish to retain this section I would urge them to include the figure referred to here.

Technical corrections: At times, the style of the manuscript is rather ponderous and convoluted, making it difficult to follow and obscuring the important message it has to deliver to policy-makers. I would strongly urge the authors to give the revised manuscript to a colleague in a related field to read and comment on in this light.

Title:

Please consider revising the title slightly, perhaps to “Scorched Earth: How will changes in the strength of the vegetation sink to ozone deposition affect human health and ecosystems?”, to make it easier to read/interpret.

Abstract:

p27848, L19-20: Is this the actual number of exceedances days or the difference between scenarios again? p27848, L23-25: Please make it clearer that when you refer to “protection” you are, in contrast to the previous sentence, now considering damage to vegetation. Perhaps “... of vegetation damage, will lead to a reduction in the impact of O<sub>3</sub> on vegetation across the UK.”

1 Introduction:

p27849, L6: Please revise “loss of atmospheric O<sub>3</sub> concentrations” to “loss of atmospheric O<sub>3</sub>” or “reduction of atmospheric O<sub>3</sub> concentrations”. p27849, L17: Please break this sentence between (SMDs) and these. p27849, L23: Put the increase in O<sub>3</sub> concentrations of 25-30 ppb into context either by giving the % increase or the baseline O<sub>3</sub> concentration for comparison. p27850, L1: The Solberg citation should not be in parentheses -> “... regional modelling of Solberg et al. (2008) ...” p27850,

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L10: To demonstrate that this association is well established requires more than one reference. Please provide more references. p27850, L17-20: Please provide references for the non-linearity and species dependent increase in biogenic emissions with temperature, and the possibility that high temperatures combined with drought could decrease emissions. p27851, L10: Is this really 2.5 to 80%; if so, perhaps writing as 80.0 would make it look less like a typo. p27851, L25-26: "This metric is capable ..... sensitivity of O<sub>3</sub> to vegetation ....." Surely the metric measures the sensitivity of vegetation to O<sub>3</sub>? p27851, L29: Consider replacing the word "events" with "factors" or "conditions". p27852, L2-5: It also requires development and application of finely resolved process-based predictive bVOC emissions models.

#### 2 Methods:

p27853, L8: Punctuation required: ".../CMAQ model" . "Central to this ...." p27853, L22: Punctuation required: "into CMAQ's MCIP." p27854, L6: Move (Rgs) to go between "surface/soil" and "resistances" p27854, L7: Insert (rext) between "external" and "resistances" p27855, L25-27: From what initial resolutions were the land cover data aggregated? p27856, L9: Insert "and" between "50 km" and "the". p27856, L25: Sanderson (2002) reference is incomplete. p27858, L26: Typo: "fromed" -> "formed"

#### 3 Results:

p27860, L25: Too many parentheses: (R)) -> (R).

#### 4 Discussions:

p27868, L25: Consider splitting this into two paragraphs between "... O<sub>3</sub> deposition." and "The study ...". p27871, L3: Punctuation required: "... DO<sub>3</sub>SE model's ...". References: p27878, L1-2: Sanderson reference incomplete.

#### Figures:

Fig 3 – Fig 6: Please improve the position of the scales on all of these figures. The scale for each map should be closest to the map it refers to. The scales would also be

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better to the right of each map so that the colour bar is immediately beside the map and the numbers are to the right of that. Furthermore, I would recommend the use of a single bar for all maps in Figures 3 and 4 as the scales are the same for all three. I also suggest that you make clear in the captions of Figures 5 and 6 that the scales for wheat and "stress" scenario respectively are different from those for the other maps in each figure.

#### Supplement (S1):

Formatting: Please ensure that the formatting (use of subscripts) is correct and consistent when referring to the terms in the equation in the text (e.g. p1 rsto should read rsto, p2 fphen -> fphen, etc). Parameters: Please ensure that all the parameters in the equations are clearly defined in the text. For example, on p1, what are LAIs and LAIe, on p2 what are fphen\_a, etc, ...)

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