

Interactive comment on “Projected global tropospheric ozone impacts on vegetation under different emission and climate scenarios” by Pierre Sicard et al.

Pierre Sicard et al.

pierre.sicard@acri-he.fr

Received and published: 22 April 2017

Dear Reviewer,

Thank you for giving us the opportunity of a reply. We hope that we have satisfactorily addressed all their concerns.

Best regards

Pierre Sicard

Bullet (1) - The main conclusion of the work as stated in the last sentence of the ab-

C1

stract (and in lines 585-587) is the recommendation that improved evaluation of regional exposure of ecosystems to O₃ requires improved chemistry-climate modelling systems, fully coupled with dynamic vegetation models. This is a conclusion that (a) provides no additional insight to the reader – it could have been written down before reading this study, and (b) is not based on data provided by this study – the authors do not demonstrate in this study that these modelling improvements improve the modelling of O₃ ecosystem impacts.

Reply - The reviewer is right. We will remove this sentence in the abstract and reformulate the section “Conclusions”.

Bullet (2) - The core of this paper is the calculation and use of the AOT₄₀ value but the description in the Methods section of how AOT₄₀ values are calculated in this work is currently very unclear (line 143 and onwards). Was the calculation of an AOT₄₀ for a model grid for all hours in the year, or for hours between 08.00 and 20.00 for all days of the year, or for the local ‘daylight’ period for all days of the year? At one point the text refers to calculation during ‘daylight hours’ (Line 144) but in another place ‘daylight’ is defined as 08.00-20.00 (Line 137), and the formula presented in Equation 1 implies calculation using all hours in the year. Derived values of AOT₄₀ depend on this issue. If using a 08.00-20.00 time-stamp to define daylight, the authors should confirm that this is reference to the local time for that grid cell. If using local daylight hours then the authors should confirm how this was defined as function of latitude and day-of-year.

Reply - We realize that it was not clear that we computed the AOT₄₀ for a model grid for hours between 8am and 8pm (local time) for all days of the year. We will state it clearly in the text. Conventionally, two major growing-season time windows are used, i.e. six months (April to September) for temperate climates, e.g. in Europe (CLRTAP, 2015) and all-year round for Mediterranean, subtropical and tropical-type climates where vegetation is physiologically active all along the year (Paoletti et al., 2007). UNECE (2010) supports the use of a growing season, but a fixed time-window does not allow incorporating the changes in the growing season due to climate change

C2

and would thus not be well suited when investigating changes over time. In addition, AOT40 is widely used not only in Europe (e.g. Anav et al., 2016; De Marco et al., 2016) but also in South America (Moura et al., 2014) and Asia (Hoshika et al., 2011).

The use of the fixed time-window 8-20 (as defined by the Directive 2008/50/EC) all-year round allows skipping the use of a latitude model, which would increase the level of complexity and uncertainties. We believe that this approach is valuable as it can be easily applied at global scale. We will include this justification in the text.

Bullet (3) - As noted above, it appears that in this work the AOT40 value is evaluated for all days of the year, whilst, as the authors have noted, for application of AOT40 to evaluate potential vegetation damage the AOT40 value needs to be evaluated over a certain period only during the year, a period which is different for different vegetation types. The authors state that calculating AOT40 for all days of the year in this work is not an issue when they are considering changes (in AOT40) between historic to future simulations. But surely this is not true. The seasonal distribution of ozone concentrations will change between different scenarios so relative changes in AOT40 computed for all-year will very likely be different from the relative changes in AOT40 computed for a sub-set of the year, as AOT40 values for vegetation damage assessment should be calculated. This issue needs much more discussion and justification.

Reply - Selecting a common time window at global level is an issue because the growing season is highly variable across the latitude. Rather than introducing further uncertainties by using a latitude model to simulate the growing season, we applied here a simplified approach with a year-long growing season which should be considered as a worst case study. This way, we were able to compare the historical and projected potential risk to vegetation. We will introduce a note of caution about the limitations of the present study.

Bullet (4) - Having stated in the Methods section that even if they overestimate AOT40 their study is focused on the relative changes in AOT40, they then later make state-

C3

ments about extent of exceedance of absolute AOT40 critical values. For example line 547 in the Conclusions states: “[The] most important results from the study are the significant overrun of exposure metric (AOT40) in comparison with the AOT40-based critical level for the protection of forests (5 ppm.h) and crops (3 ppm.h). Furthermore, they appear to fail to acknowledge or take account that the AOT40 critical values for forests and crops require calculation of AOT40 over defined months, not the full year as their method in Equation 1 has done.

Reply - We would like to thank the reviewer for this comment. We will introduce a note of caution when discussing the AOT40-based critical levels for the protection of forests and crops. Most important results from the study are the spatial pattern and projected changes in global AOT40 and risk areas for vegetation under the RCP scenarios. This is a very novel result of our study.

Bullet (5) - The authors apply an alpha factor to their (all-year) AOT40 values to calculate a potential ozone vegetation risk factor IO3 (Equation 2 in Line 167). The units of alpha are quoted as per mm per ppb. When alpha is multiplied by an AOT40 value, which has units of ppb.h, this means that the IO3 metric has units of h/mm (i.e. dimensions of time per length). Can the authors explain the physical/biological basis for a photosynthetic assimilation risk factor having these dimensions? When I check the associated citation (Anav et al. 2011, GCB) I note that the equivalent formula in this latter paper also includes a stomatal conductance variable g , which is not present in Equation 2 and not mentioned in the current manuscript.

Reply - We would like to thank the reviewer for this comment. The potential O3 impact on photosynthetic assimilation (IO3) is expressed through a dimensionless value. As for the variable g (i.e. stomatal conductance), ACCMIP models do not provide this variable as output, thus we can only compute the likely impact of O3 as the product of the sensitivity coefficient and the O3 concentration. We consider this impact as a potential one, in the worst-case scenario, where all the ozone is entering into the leaf.

C4

After a deep review of both papers, i.e. Reich (1987) and Ollinger et al. (1997), α is an empirically derived O3 response coefficient (dimensionless value) representing the proportional change in photosynthesis and biomass growth per unit of ozone-uptake. IO3 is the simulated percentage changes (%) in the potential ozone injury on vegetation between that expected at the end of the 21st century (RCPs simulations) and present.

$$\alpha \times \text{AOT40 in ppb h} \text{ IO3} = \alpha \times (\text{AOT40}_{21\text{st century}} - \text{AOT40}_{\text{present}}) / (\text{AOT40}_{\text{present}}) \times 100 \Rightarrow \text{IO3 in \%}$$

A statement will be added to clarify this issue and the units will be modified.

Bullet (6) - The conclusions section is almost 3 pages long and much of it is discussion/statement of prior literature and not conclusions from this work. For example, lines 552-560, lines 573-582 and lines 592-598 are generally re-statements of previous published findings and conclusions, not conclusions from this work.

Reply - We will shorten the section "Conclusions" taking into account the referee comments.

Bullet (7) - The abstract contains contradictory text. The first sentence states that concentrations of surface O3 are expected to increase in the future. Later in the abstract it is stated that for two of the RCP scenarios investigated ozone concentrations and vegetation injury decreases in the future.

Reply - We will reformulate the abstract.

Bullet (8) - Overall, whilst the extensive discussion of the variation in surface O3 mixing ratios (geographically, with model, and with scenario) is valid (but probably also described in other publications that have emanated from the ACCMIP project), I am not convinced that statements made about changes to ozone ecosystem injury are quantitatively valid.

Reply - It is not the purpose of this study to offer a quantitative estimation of the ecosystem injury due to ozone but to highlight the world areas at higher risk in a worst case

C5

scenario, and how they change relative to the historical situation. A statement will be added to clarify this issue.

Bullet (9) - Minor technical/typographical corrections:

All requested technical and typographical corrections were carried out. We have mentioned "No contour data" in the panel for two models for which the data were missing under RCP4.5.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-74, 2017.

C6