Dear Reviewers,

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

Best regards

Pierre Sicard

Anonymous Referee #2

Scientific comments:

(1) The main conclusion of the work as stated in the last sentence of the abstract (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 O_3 ecosystem impacts.

The reviewer is right. We will remove this sentence in the abstract and reformulate the section "Conclusions".

(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.

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 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.

(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.

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.

(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 statements 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.

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.

(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.

We would like to thank the reviewer for this comment. The potential O_3 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 O_3 as the product of the sensitivity coefficient and the O_3 concentration. We consider this impact as a potential one, in the worst-

case scenario, where all the ozone is entering into the leaf. After a deep review of both papers, i.e. Reich (1987) and Ollinger et al. (1997), is an empirically derived O_3 response coefficient (dimensionless value) representing the proportional change in photosynthesis and biomass growth per unit of AOT40. 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. A statement will be added to clarify this issue and the units will be modified.

IO3 = α× AOT40 => IO3 in ppb h
IO3 = α × ($AOT40_{21st\ century}$ - $AOT40_{present}$) / ($AOT40_{present}$) x 100=> IO3 in %

(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.

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

(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.

We will reformulate the abstract.

(8) Overall, whilst the extensive discussion of the variation in surface O_3 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.

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

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.

Anonymous Referee #3

L142 "when the stomatal conductance is greater than 0": what do you mean? Do you mean the "leafy season"? Please rephrase it.

The reviewer is right. 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 and we have removed this sentence.

L147 “the overestimation of AOT40 does not affect our results”: it is not clear why. Please rephrase it.

The aim of this study is to assess how O_3 stress to vegetation changes between historical period and future. By calculating AOT40 year-round, an overestimation can be observed over polluted region. Even if the AOT40 is misestimated at a given model grid point, as we compared the mean change between present and future at the same model grid point, thus the change is consistent. We rephrased to stress that an overestimation of AOT40 does not affect our main conclusions (instead of “results”) about the percentage of change in the potential O_3 impact on photosynthetic assimilation.

L170 not “per unit of ozone-uptake” but “per unit of AOT40”

The reviewer is right. Alpha is an empirically derived ozone response coefficient.

L172 Again you did not use “ozone-uptake” in Eq. (2). You can describe it as “regressions of the photosynthesis response to ozone (Reich, 1987)”.

The reviewer is right. Data from the literature demonstrate strong relationships between cumulative ozone exposure and reductions in both net photosynthesis and plant growth. Figures from Reich (1987) show the percentage of change of photosynthesis in relation to ozone exposure, so we reworded as suggested.

L173 What are the “other vegetation types”? And please justify why the photosynthetic responses to AOT40 are same between deciduous trees and “other vegetation types”.

We would like to thank the reviewer for this comment. The photosynthetic responses to AOT40 are not the same between deciduous trees and “other vegetation types”. We now clearly explained in the text that the relationships between cumulative ozone exposure and reductions in both net photosynthesis and plant growth vary among and even within species (Reich, 1987; Ollinger et al., 1997; Anav et al., 2011). Differences in response per unit uptake tend to be greater in magnitude between functional groups (e.g., hardwoods vs. conifers) where leaf structure and plant growth strategy differ most widely (Reich, 1987).

From the Global Land Cover Facility (GLCF) data at 1degree of spatial resolution, we grouped the vegetation in 3 categories and then we used the following factors: conifers, crops and deciduous trees. Ollinger et al., 1997 derived a leaf-level ozone response equation for broadleaved deciduous species (2.6×10^{-6}) and we used 0.7×10^{-6} for coniferous and 3.9×10^{-6} for crops (Reich, 1987).

L464-465 Nemani et al. (2003) and Zhu et al. (2016) did not show the ozone impacts. Please revise it.

The reviewer is right. These analyses (Nemani et al., 2003; Zhu et al., 2016) focused on impacts of global environmental changes (e.g. climate, land-cover, nitrogen deposition, CO_2 fertilization) on vegetation. We have reworded as suggested.

L480-481 “In these areas, the increasing effect of a warming: : :”: where can we refer for this result? Please specify it.

We compared the GPP reduction (from - 10 to - 20%) due to O_3 (Sitch et al., 2007) and the strong increase in NPP and LAI due to climate change (Nemani et al., 2003; Zhu et al., 2016)

over Amazon forest. We have reformulated as "*In these areas, we observed an increasing effect of a warming climate on forests (e.g. increase in greening, NPP, LAI) as compared to a reduction in GPP due to O_3 (Sitch et al., 2007)*".

L491-496 "mainly due to the lack of empirical data about the response of different species to O3": We have to say that this is a weak rationale. In fact, Sitch et al. (2007) considered five plant types (broad-leaved tree, needle-leaved tree, C3 crops, C4 crops and shrubs; please see the Table S1 of their paper). But we can find a marked difference in estimated ozone concentration in 2100 between this study (Fig. 1) and Sitch et al. (2007). A major advantage of this study is a comparison between the models and scenarios. The authors should reconsider the sentence and should emphasize what is the need to explore future potential impacts of ozone.

The reviewer is right. The ozone concentrations over Amazon forest are lower in Sitch et al., (2007), i.e. 75-90 ppb in summer (present) and more than 90 ppb by 2100. In our study, the annual O_3 mean is around 15-20 ppb by 2100. In this section, we added in the text explanations about the overestimation in GPP reductions simulated by Sitch et al. (2007) in summer such as the estimated O_3 concentration in 2100, the lack of empirical data about the response of different species to O_3 the non-inclusion of the nitrogen limitation of growth.

L553-560: I agree with the statement. However, if so, readers are wondering why AOT40 was targeted in this paper. The authors can put more "take-home messages" for readers. For example, what is a climatic condition (arid/humid) in high AOT40 regions? How about the need for a parameterization of the ozone dose-response relationships in tropical plants? ...etc.

The reviewer is right and we decided to add a few more information about the AOT40 limitations at global scale (e.g. factors affecting stomata e.g. water availability).

L578"the lower risk areas include evergreen broadleaf forests ": we cannot find the description about the parameters in evergreen broadleaf forests (lines 170-174). Did you target this plant type?

We did not focus on evergreen, as now clearly explained in the text. By using the land-cover data (GLCF), we can observe that the lower O_3 risk areas (Figure 3) correspond to areas with evergreen broadleaf forests.

Figure 3 legend: "the potential ozone impact on vegetation": of what? Maybe photosynthetic assimilation. But please specify it.

Indeed, we added "the potential O3 impact on photosynthetic assimilation".

L551 "..South Asia they may..": you had better put ", and" before "they may".

Done

L552 not "were" but "was"

Done