Intercontinental trans-boundary contributions to ozone-induced crop yield losses in the Northern Hemisphere by *M. J. Hollaway et al.*

Response to Reviewer Comments.

<u>Anonymous Reviewer #1.</u>

The authors wish to thank the reviewer for their careful review and providing helpful comments and suggestions. We have addressed all of the comments below with the reviewer's comments written in italics and our replies shown in bold.

<u>Specific Comments.</u>

The study overall seems well conducted and shows that there are strong transboundary effects. However, I am somewhat skeptical of the magnitude of the effects because the choice of a single growing season for all crops and locations would give poor estimates of yield loss. Aside from that, I think the there would be a lot of value in calculating how the percentage reductions in losses translate into increases of production. Clearly, the results show that there are transboundary effects, but to understand the importance of those effects, they need to be expressed in different terms. For example, a regional reduction of emissions in Europe eliminates all wheat yield losses from emissions locally and eliminates about 40 % of losses in North America. There is no indication of how large those losses are though. If wheat production in North America is decreased by 1 % because of European emissions, then eliminating losses from European emissions would have almost no influence. I would like to see something like Figure 8 but showing increase in production. I do not think that would be necessary for publication, but such information would be a valuable tool for convincing international policy makers that there is real economic value in cooperating to reduce emissions, and it would likely result in a publication with considerably more impact.

The biggest problem I see is the use of the same growing season for all crops in all areas (May to July in the Northern Hemisphere with a 6 month offset in the Southern Hemisphere). The justification for this is that it "enables more comparability between the effects of the emissions reductions and the subsequent impact on ozone-induced yield losses for each of the crops." To me is seems that this was done only because it was simpler than defining a different season depending on location and crop. It makes comparing effects of emissions reductions on ozone concentration easier, as claimed in the quote, but it certainly seems to make it impossible to compare the effects of emissions reductions on yield losses between crops, despite the claim in the quote. Crops have different growing seasons depending on species and location, and ozone concentrations are highly variable spatially and temporally. The indexes calculated here will only partially reflect what the crops were actually exposed to, which means using those indexes in a dose-response function will likely give a rather poor estimate of the effect on yield. If the only goal is to show that there are transboundary effects, then this may not be that important, but I think this study could be greatly improved by going a little further and showing how eliminating regional emission would increase transboundary production, and I don't see how good estimates could be made using the same growing season everywhere for everything. Not being very familiar with the resources required to run these models, I *do not know how feasible that would be, but I do know that the SAGE group has data sets for this, although they do have substantial limitations.*

Reply:

In order to improve the quantification of the transboundary effects shown in this study we have recalculated our AOT40 and Mx metrics using growing seasons that are specific to crop location and type of crop grown (Sacks et al., 2010). We have used the SAGE group crop calendar data sets which provide planting and harvesting dates for a number of different crops, including the six major crops investigated in this modelling study.

To calculate the revised metrics we have used the filled version of the crop calendar data sets which provide planting and harvesting dates for the six crops over every land grid box on the TOMCAT model grid (we have regridded the crop data from 0.5 by 0.5 resolution to the TOMCAT ~2.8 by 2.8 resolution) and defined the crop and location specific growing season as the interval between planting and harvest date for each grid box. We then calculate global AOT40/Mx for each crop as before, using this new growing season (shown in original Figures 5, 6, and 7), and then calculate the global and regionally aggregated yield losses as previously, using the data from Monfreda et al., (2008). This updated method has been included in the text of the methods section of the revised manuscript.

As suggested by the reviewer, we have decided to represent our transboundary effects in terms of the effect of NO_x emissions cuts on crop production rather than relative yield loss. In order to do this we have used the crop production data for the year 2000 (the same year as our emissions) from Monfreda et al., (2008) for each crop on a global scale and for each of the 3 regions (N America, SE Asia, and Europe). We have then applied the regionally-aggregated yield loss values under each emissions scenario to these total crop production figures to quantify the change in crop production loss (CPL) between the control and each of the emissions reduction scenarios. This was done for each of the metrics (AOT40 and Mx, with the addition of the W126 index (see response to Reviewer 3)).

We have decided to replace Figures 8 and 9 in the original manuscript (that showed the transboundary effects in terms of change to RYL) with figures that show the transboundary effects in terms of relative change in CPL for each crop for each of the 3 metrics (AOT40/Mx and the addition of W126). These are shown in new Figures 10, 11 and 12 in the revised manuscript. In response to comments raised by Reviewer 4 regarding model error (please refer to our response to Reviewer 4 for details) we represent the results in these new figures as both the original values calculated from the model output (the solid bars) and the bias corrected results (the hatched bars) to quantify the effect of model bias on the transboundary effects.

Finally, in order to quantify the transboundary effects in terms of increase in production we calculate the mean absolute changes (averaged over all 3 metrics) in CPL (in million metric tonnes) over each region and globally, to highlight the regions where the observed transboundary effects have the largest impact on crop production. This is shown in new Figure 13 of the revised manuscript.

On page 8658, the authors make the argument that AOT40 is a less robust index for modelling than Mx indices because model biases when using AOT40 are more variable. I am not sure this is a complete argument though. The utility of the index would depend on both how well it can be modelled and how well it predicts yield loss, which is not discussed.

Reply:

In the revised manuscript we have expanded this section to now include a more complete discussion of the utility of the AOT40 metric. We have discussed the use of the index based on how well the metric can be modelled, in particular drawing attention to the differences between modelled and observed AOT40 that can result from small model biases at ozone concentrations close to the AOT40 threshold value. In addition, the inclusion of new bias-corrected results in the manuscript gives a clear illustration of the effect of model bias on the transboundary effects that we present.

In the last full paragraph of 8657, I think it needs to be specified that this is over the growing season because there are many months throughout the year where the model is outside of one standard deviation.

Reply:

As we are now using crop specific growing seasons to calculate our modelled AOT40 and Mx we have adapted the model evaluation section to discuss the performance of TOMCAT throughout the calendar year highlighting how well the model is able to reproduce the observed metrics. This section has been updated in the revised manuscript accordingly.

Technical corrections:

The sentence starting on Pg 8652 L 5 is difficult to follow.

We have rephrased this sentence in the revised manuscript to make it clear how long the model has been run for and which sets of emissions have been used.

Pg 8647 L 7, I think the description of NO_x should be done with words because the notation used incorrectly implies a chemical or mathematical relationship.

We have amended the description of NO_x to use words i.e. NO_x describes the sum of NO and NO_2.

Throughout the paper, "ozone" is used in place of "ozone concentration". It makes no sense to say, "led to increased ambient ozone over many regions."

This has been corrected in the revised manuscript.

Pg 8646, L 11, "Northern Hemispheres" should have an apostrophe, likewise for Pg 8652 L 10, "Hemispheres" and Pg 8652 L 23, "regions."

This has been corrected in the revised manuscript.

The words "transboundary" and "AOT40" are inconsistently hyphenated.

This has been corrected in the revised manuscript – hyphenation is not used.

There are a few places with comma splices, e.g., Pg 8647 L 19 and Pg 8656 L 23.

These have been amended in the revised manuscript.

The spelling of "modelled" is inconsistent.

This has been amended in the revised manuscript.

In the list of emissions scenarios on Pg 8652, it is not clear why they all start with "as control scenario."

This list was rather superfluous, and has now been removed from the revised manuscript and has been replaced with a description indicating that we have run a control simulation and then 3 additional simulations each reducing anthropogenic NO_x emissions over the source regions by 100%.

The word "data" is plural.

This has been corrected in the final revised manuscript.

Pg 8653 L 11, S.I. convention is a single space or a dot between units when they are multiplied.

This has been corrected in the final revised manuscript.

As a very nitpicky point, panel d in Figures 5, 6, 7, and all panels in Figure 10 have strange values in the legends. I doubt that they need to go out to 3 decimal places, and a nice round spacing between intervals would be nice.

We have amended Figures 5, 6, 7 and 10 to have equal spacing between intervals and all numbers now only go out to one decimal place. For Figures 5, 6 and 7 we have also rearranged the panels so that they are stacked vertically to aid clarity. As we are now using crop specific growing seasons to calculate the metrics, we represent the changes in AOT40 and M7 (to represent changes in Mx as changes in M12 are similar) for ozone sensitive (wheat) and ozone tolerant crop (rice) growing locations. We also now include changes in W126 for both maize (ozone tolerant) and soybean (ozone sensitive) growing locations. The plots for the rest of the crops and M12 are shown as supplementary material.

<u>Anonymous Reviewer #2.</u>

The authors wish to thank the reviewer for their careful review and providing helpful comments and suggestions. We have addressed all of the comments below with the reviewer's comments written in italics and our replies shown in bold.

<u>Specific Comments.</u>

The paper by Holloway et al. entitled "Intercontinental trans-boundary contributions to ozone induced crop yield losses in the Northern Hemisphere" attempts to characterize the transport dynamic and formation of ozone and its precursors in the northern continents, with the ambitious goal to define how a reduction in NO_x emission in North America, South East Asia and Europe can modify ozone concentrations and effects on crop yields for each of the northern continent. Different metrics for concentration-based ozone-risk assessment are applied, and a global atmospheric chemistry model is used to predict transport dynamics over continents. Data show that the model makes a good work in predicting ozone concentrations at regional level, although I am not familiar with the model used and its parameterization. My main concern is somehow shared with the other reviewer, since it is quite limiting to consider the same growing season for crops in the Northern Hemisphere. This is an approximation which turns in wrong estimates of the real yield loss, even assuming that the model has a perfect predictive capacity of the local ozone concentrations. However, the paper reads well and the overall message is clear. Future improvements will have to be a better calculation of crop growing seasons, as well as the implementation of metrics based on accumulated ozone flux, not just concentrations.

Reply:

Following the similar recommendation of Reviewer 1, we are now using crop-specific growing seasons. Please refer to our response to Reviewer 1 for details.

<u>General Comments:</u>

Pag. 8647 linen 18: Surface concentrations or background concentration: please clarify what it means and be consistent with the definition in the text.

This should read "background ozone concentrations at the surface". This has been corrected in the revised manuscript and made consistent with the definition in the text.

Pag. 8654 lines 22-27: It is time to parameterize flux-response relationship for global application, isn't it? First you say that concentration-based metrics may not be appropriate for location where they have not been developed, then you sat that an aim of the paper is to determine how use of these metrics impacts yield loss contributions. Perhaps I did not understand what the author meant, but it seems a little contradictory. I think it is a fair assessment just to state that large uncertainties on yield loss could derive from the application of concentration-based metrics and that future effort should be dedicated at implementing flux-based metrics because they are a better predictor of ozone damage==yield loss.

We have decided to use the concentration based metrics in this study, as at present there is a much more comprehensive set of exposure response relationships available to determine crop yield losses from these metrics. In the revised manuscript we have expanded the discussion of the metrics and their limitations. We also acknowledge the fact that future work is needed to develop and implement flux-based metrics for predicting yield losses at the global scale.

Pag 8655line 24-25 + pag 8656 lines 1-10: This part should be included in the M&M section as it describes the data sources.

This has been amended in the revised manuscript.

Pag 8657 line 18: put "concentration" after ozone.

This has been added in the revised manuscript.

Table 5. Please add the units (%) in the table.

This has been amended in the revised manuscript.

Anonymous Reviewer #3.

The authors wish to thank the reviewer for their careful review and providing helpful comments and suggestions. We have addressed all of the comments below with the reviewer's comments written in italics and our replies shown in bold.

Specific Comments.

First, I agree with the other two referees that using the same growing season for all crops/regions in the Northern Hemisphere is unrealistic, and may lead to inaccurate results given the significant temporal and spatial variability of O_3 *concentrations throughout the year. The May-July NH growing season assumption may hold relatively well for North America and Europe for most crops, but not for South and East Asia, and furthermore not for certain important crops (e.g. winter wheat). Although the paper in its current form is certainly illustrative of the potential intercontinental benefits of* NO_x *reductions in source regions, using more accurate crop calendars would make the results more robust. Growing season data has been published that could be utilized (see Sacks, W.J., D. Deryng, J.A. Foley, and N. Ramankutty (2010), Crop planting dates: an analysis of global patterns. Global Ecology and Biogeography 19, 607-620. DOI: 10.1111/j.1466-8238.2010.00551.x.), and international crop calendars for many countries are also available from the USDA.*

Reply:

Following the similar recommendation of Reviewer 1, we are now using crop-specific growing seasons (using crop calendar data from Sacks et al., (2010)). Please refer to our response to Reviewer 1 for details.

Second, I additionally agree with referee #1 that calculating crop production losses and the associated economic impact, while not necessarily required for publication, would make the paper significantly more influential and interesting to policymakers.

Reply:

As recommended by Reviewer 1, we are now representing our transboundary effects in terms of the effect of NO_x **emissions cuts on crop production. Please see response to Reviewer 1 for details.**

Similarly, calculating the benefit of NO_x *emission reductions in various source regions on a national level, even if only for the major emitters/agricultural producers in the NH (e.g. the U.S., EU, China, India), could be a useful supplement to the study and more relevant from a policy perspective.*

Reply:

In this study we have decided to focus on the transboundary effects on an intercontinental scale. We believe that the next stage in this research would be, as the reviewer suggested to take the emissions reductions to a national level to investigate the impact of pollution transport within and between

neighbouring countries. However, to achieve this it would be more appropriate to employ a finer-grid resolution simulation or a regional chemical transport model. This would constitute a valuable follow-up study to this work.

Third, I think the paper could have a greater impact (particularly in the U.S.) if results were also calculated according to the W126 metric, which was recently proposed by the Environmental Protection Agency to be used to set the secondary O_3 *standard in the U.S. (for the protection of crops and other sensitive vegetation). Although the proposed revisions were recently shelved, enactment of a secondary* O_3 *standard in the future will most likely be based on this metric, and therefore quantification of the potential contribution of* NO_x *emission reductions in various source regions to exceedance of the W126-based standard (as well as corresponding crop losses) would be especially interesting to U.S. policymakers.*

Reply:

As recommended, we have calculated the W126 index for 3 of the crops in this study (wheat, maize and soybean) using available exposure-response functions from Wang and Mauzerall, (2004). We have calculated the associated global and regional changes in crop production and represented this as an extra figure (new Figure 12 in the revised manuscript). This figure exhibits similar transboundary effects as those shown for the AOT40 index, due to its nature as a cumulative exposure index like AOT40.

We have also provided a description of the W126 index in the introduction section of the paper and also clearly describe its calculation and limitations in the materials and methods part of the revised manuscript.

Fourth, while the model evaluation is quite thorough, it would be constructive to see a comparison of model-simulated O_3 *exposure with monitoring site data outside of just Japan and Malaysia for the SE Asian region. While I understand that hourly* O_3 *concentration data in this region are difficult to find, a monthly mean comparison may be possible in India and China (for a few sites), as well as for AOT40 during certain months in India (see for example the references in Van Dingenen et al. (2009) and Avnery et al. (2011a) as cited below).*

Reply:

We have updated the model versus observed monthly-mean ozone comparison (new Figure 3) to now include monthly-mean ozone observations from China and India for the South East Asian region. Where possible we have used observations from the year 2000, however where data from this year is not available we have used observations that fall within the period 1995-2005 following a similar approach to Avnery et al., (2011a). For the comparison of modelled AOT40 & M12 versus observations we have kept the comparison in the SE Asia region to Japan only as the observations here provide the most complete dataset for the entire year with ozone measured at an hourly frequency. As we have updated the calculation of our metrics to now use crop-specific growing seasons we require a full year of hourly observations in order to assess the model's ability to reproduce the metrics over a range of growing

seasons. As we are now using the W126 metric, a modelled W126 versus observations comparison figure is now added to Section 3.1 (new Figure 6).

<u>Minor Comments:</u>

The paper could benefit from an enhanced discussion of the scalability of results to more realistic emission reduction levels (e.g. a 20% reduction in anthropogenic NO_x*) given the complex nonlinear chemistry of tropospheric* O_3 *production.*

The overall goal of this study is to investigate which of the major industrialised regions in the northern hemisphere contribute the most to crop yield losses on both a regional and global scale through their anthropogenic contributions to ozone, and in particular quantifying which regions produce the largest transboundary effect. To achieve this estimate of total contribution it was necessary to apply 100% NO_x **emission reductions for two reasons: (1) As stated by the reviewer, the production of ozone in the troposphere is complex and varies non-linearly with changes in** NO_x **emissions, meaning that scaling from a e.g. 20% reduction to total attribution is not trivial (see the recent study and discussion by Wild et al., (2011)), and (2) the threshold nature of the AOT40 index means that scalability is also complicated by differences in absolute ozone concentrations close to 40 ppbv. We have added a short discussion of these points in Section 2.1.**

On pg 8654, the last sentence in the last paragraph seems out of place and could be moved earlier in the paper as a part of the study motivation.

This has been amended in the revised manuscript with this sentence moved to the end of the Introduction section to be included as part of the motivation.

In the comparison of results section (pg 8661, lines 14-17), Avnery et al. (2011a) also use the same crop distribution data in their analysis of O_3*-induced crop losses, so the differences between this study's results and those of Van Dingenen et al. (2009) appear to be due to the different models/emissions and growing seasons used rather than the crop data.*

We have updated this section to now include a comparison of our results with those from Avnery et al., (2011a) as well as those from Van Dingenen et al., (2009) to highlight that our yield losses under the control scenarios are comparable to those found in both studies. Differences between these studies and our results arise from the use of different chemical transport models as well as different emissions data sets and crop growing seasons (both Avnery et al., (2011a) and Van Dingenen et al. (2009) employ the use of crop data from the USDA).

The sentence on pg 8666, lines 8-11 is unclear, I believe "may be reduced" is meant in line 10.

This has been amended in the final revised manuscript.

Finally, a few key references have been omitted – several suggestions for additional recent literature citations include:

(1) The papers of Avnery et al. (2011), which should be cited in the introduction and elsewhere as they also quantify global O_3-induced crop losses in 2000 and 2030:

Avnery, S., Mauzerall, D. L., Liu, J., et al. (2011a). Global crop yield reductions due to surface ozone exposure: 1. Year 2000 crop production losses and economic damage. Atmospheric Environment, 45(13), 2284-2296. doi: 10.1016/j.atmosenv.2010.11.045

Avnery, S., Mauzerall, D. L., Liu, J., et al. (2011b). Global crop yield reductions due to surface ozone exposure: 2. Year 2030 potential crop production losses and economic damage under two scenarios of O_3 pollution. Atmospheric Environment, 45(13), 2297-2309. doi: 10.1016/j.atmosenv.2011.01.00

These references have been added to the final revised manuscript, although we note that these were not published when we originally prepared the manuscript.

(2) In reference to stomatal flux indices (pg 8654, lines 20-22), tomato has additionally been parameterized. See:
Mills, G., et al. (2011). New stomatal flux-based critical levels for ozone effects on vegetation. Atmospheric Environment 45, 5064-5068.

This reference has been added to the revised manuscript to provide additional description of stomatal flux-based indices, although we note that these were not published when we originally prepared the manuscript.

(3) In the discussion of O_3 impacts below the 40 ppb threshold (pg. 8648, lines 17-20), the following paper could be cited:
Mills, G., et al. (2011). Evidence of widespread effects of ozone on crops and (semi-)natural vegetation in Europe (1990–2006) in relation to AOT40- and flux-based risk maps. Global Change Biology, 17(1), 592-613.

This reference has been added to the revised manuscript, although we note that this was not published when we originally prepared the manuscript.

(4) The new HTAP studies should also be cited in the discussion of intercontinental O_3 transport (pg 8649, lines 15-18), e.g.:
Dentener, F., Keating, T., Akimoto, H. (eds.). 2010. Hemispheric transport of air pollution. Part A: Ozone and particulate matter. Economic Commission For Europe, United Nations, Geneva.

This reference has been added in the revised manuscript.

Anonymous Reviewer #4

The authors wish to thank the reviewer for their careful and thorough review and providing helpful comments and suggestions. In particular we thank Reviewer 4 for their careful reading of the manuscript, and picking up various errors in presentation style that should have been corrected before submission. We have addressed all of the comments below with the reviewer's comments written in italics and our replies shown in bold.

Specific Comments.

1. Use of AOT40?
Much of the analysis and discussion seems to centre on the AOT40 metric results, as this gives the largest impact. Yet at the end of Section 3.1 it is stated, "from a modelling perspective, AOT40 may be a less robust metric [compared to the Mx indices]". Also, Figure 10 shows how sensitive the metric is to small changes. Based on this, perhaps the authors could provide more justification for the fact that this metric is the one used to give the "headline" results.

Reply:

The overall goal of this study is to use a range of metrics to quantify the intercontinental contributions to ozone-induced crop yield losses. The range of results highlighted in the abstract and discussion includes all of the metrics calculated (which now includes the W126 index following a recommendation from Reviewer 3, in addition to the AOT40 and Mx indices used in the original manuscript). Much of the discussion focuses on AOT40 in this study as this is the concentration based metric that we have exposure response relationships for a large number of crops grown worldwide (Mills et al., 2007). In addition, the AOT40 index is currently applied throughout the European Union and the United Nations Economic Commission for Europe (UNECE) to assess the impact of ozone damage on crops and to set air quality standards, as highlighted in the recent HTAP report (Dentener et al., 2010). The full range of results from all three types of metrics are given in the abstract as the 'headline' results. In addition, the final summary figure of the paper (new Figure 13), shows the crop production changes calculated as a mean of the metrics used.

2. NO_x emission sources
Section 2.1 should explicitly point out that biomass burning and natural NO_x emission sources are not removed in the emission scenarios. (What happens to aircraft emissions?) Currently, this fact is not really highlighted until the Discussion section, but it is obviously important in the analysis of the results. For instance, Section 3.4 mentions that removing local emissions does not totally eliminate local yield losses in North America.
Can you say whether this is because of the remaining emissions, or because of transboundary pollution? Similarly, are the residual NO_x emissions responsible for keeping SE Asia's yield loss at 87.8% (P8658, L20)?
Related to this (and maybe assisting the analysis), Table 1 could be replaced with a 3-panel figure showing (a) the "full" NO_x emissions, (b) NO_x emissions after removing the anthropogenic component in each region, and possibly (c), the difference between (a) and (b).

Reply:

In Section 2.1 of the revised manuscript we have now made it clear that the only emissions we have reduced are anthropogenic surface NO_x emissions. We specifically state in this section that biomass burning and natural NO_x emissions are not changed from the control scenario. We have not applied reductions to aircraft emissions. This is because it is difficult to regionalize aircraft emissions in terms of stating which region the aircraft emissions are attributed to.

In the Results section (p 8665) we had already discussed the likely role for biomass burning emissions in controlling crop ozone exposure in the Western US. We have amended the Results section to discuss more clearly the effects of natural and biomass burning emissions of NO_x in maintaining yield losses. To aid this section of the discussion we have replaced Table 1 with a 4 panel figure that shows the following. (a) The full NO_x emissions, (b) the NO_x emissions under the NA_EMSCUT scenario, (c) the NO_x emissions under the SEA_EMSCUT scenario and (d) the NO_x emissions under the EUR_EMSCUT scenario. This new figure (new figure 1) clearly shows that after the emissions cuts over N America and SE Asia, biomass burning NO_x emissions of reasonable magnitude remain.

3. Comment on more realistic NO_x reductions?
While I see that 100% NO_x reductions are a useful model sensitivity exercise, it would be interesting to consider the impact of smaller (more achievable?) reductions. Since ozone production/concentration is not a simple linear function of NO_x, presumably any relationship with yield reductions would also be non-linear. Could inferences be drawn from other experiments conducted as part of HTAP? (Perhaps if AOT40/Mx was found to roughly scale with the monthly mean model output that might be available.) At the very least, I think this should be included in the discussion.

Reviewer 3 makes a similar point. We have added discussion on this. Please see our response to Reviewer 3 for details.

4. Error
Is it possible to put the change in the metrics from the emission scenarios in the context of the model error? E.g. hypothetically, what does it mean if there is a 20% change in AOT40, but the model bias is 25%?

In order to take the model error into account, we have applied a model bias correction to our calculation of the AOT40, Mx & W126 metrics. For each of the source/receptor regions, we have applied the regional average biases based on the observation/model comparisons from Section 3.1, and for other locations we have applied the global-mean bias. These bias-corrected metric values are then used to calculate the changes in crop production. In the final figures showing change in crop production under each scenario (replacing Figures 8 & 9 in the original manuscript) we present both the original model

results and the bias-corrected results to demonstrate the effect of model bias on the transboundary effects estimated in this study.

<u>General Comments:</u>

1. Introduction.
This is very long and we do not learn of the object of the study until the end. I would recommend (1) briefly saying what the study is about near the beginning and (2) separating out the description of the metrics into its own section. I think this should make it more readable, especially for interested atmospheric chemists who might not be familiar with these terms.

This has been amended in the final revised manuscript.

2. General clarity, reducing repetitiveness, tightening the text.
E.g. the last paragraph on P8654 is very long, confusing, and states facts that were already mentioned previously (e.g. plant damage from ozone below 40 ppbv). Also, the last paragraph of P8656 is long and quite tedious. There really is no need to describe every nuance in the figure – try and limit to highlighting a few key results. Also, the paragraph finishing at the top of page 8649 – what is the take home message here? Shorter sentences would also improve clarity.

In the revised manuscript for the last paragraph on P8654 we shortened this paragraph to clarify the reasoning behind using the concentration based indices in this study. In response to issues raised by Reviewer 2 (see response to Reviewer 2) we have acknowledged in the revised manuscript that future work is needed to implement flux based metrics into predicting global-scale yield losses. We have also moved the final sentence of this paragraph to the introduction section as part of the study motivation.

For the last paragraph on page 8656 (L11-) we have tightened the text to only discuss the key areas where the model predicts observed ozone well and the key areas where there is significant model bias.

The paragraph that finishes at the top of page 8649 (L1-9) is discussing the relative advantages and disadvantages of using the relative metrics to estimate crop damage. We have edited the revised manuscript to clarify this.

<u>Minor comments / corrections:</u>

- Where there are multiple references, it's better to put them in chronological order to emphasize any progression in understanding.

Corrected in revised manuscript.

- Refer to references as people rather than books. E.g. "It is shown by Liu et al." rather than "It is shown in Liu et al." (P8667, L7)

Corrected in revised manuscript.

- Check parentheses for the references. E.g. same example as above "(Liu et al., 2010)" should be "Liu et al. (2010)". There are several instances of this.

Corrected in revised manuscript.

- Check apostrophes (e.g. worlds -> world's on P8647, L13; also: AOT40's, region's, Northern Hemisphere's).

Corrected in revised manuscript.

- Should "N America" be "North (N) America" the first time? Same for Southeast (SE) Asia.

Corrected in revised manuscript.

- Abbreviation for Northern Hemisphere is used inconsistently.

Corrected in revised manuscript.

- Check for consistency of US/British spelling (Presumably the latter is preferred for this journal: "modelling" -> "modelling"). Also "parameterized" or "parameterised"?

Corrected in revised manuscript.

- Say "year 2000" (or similar) rather than just "2000", which could be ambiguous (e.g. P8651, L2, L13).

Corrected in revised manuscript.

- Check tense consistency, especially in the method section.

Corrected in revised manuscript.

P8646: Consider cutting the abstract to less than 300 words.

We have shortened and amended the abstract to be more concise.

P8647:
L4. Spell out "US EPA" the first time

This has been corrected in the revised manuscript.

L10. While Prather et al. (2001) talk about changes in ozone from the PI, they also say how uncertain the trends are. Maybe say "likely led to increased…"

This has been amended in the revised manuscript.

L13. "(NO_x in particular)" – You've already defined NO_x above.

This has been amended in the revised manuscript.

L17. "Royal Society" (space), but weren't there authors/editors for this report? (Fowler et al.?)

We have added the editors to this report in the revised manuscript and corrected the reference list accordingly.

L18 and L23. Pick either "AQS" or "AQSs" (I think the latter reads better)

We have made the acronym of air quality standards consistent as AQSs throughout the revised manuscript.

L19. "…for human health, to which enhanced…detrimental. However, compared to those for vegetation, these tend…" (There are several other places where sentences should be split – please proof read carefully.)

These have been corrected in the revised manuscript.

L21. "impact processes" is a bit vague – what do you mean?

We have amended this sentence in the revised manuscript to indicate that AQSs for human health tend to focus on acute ozone pollution episodes rather than chronic long term exposure.

L24. Spell out "EEA"

This has been amended in the revised manuscript.

L25. What is "ozone risk"?

We have amended this sentence in the revised manuscript to read "The AOT40 (accumulated exposure over a threshold of 40 ppbv) metric has been adopted in Europe (EEA, 1999), to assess risk to vegetation from ozone exposure, and has been used in East Asia as well as globally to assess ozone risk (Van Dingenen et al., 2009;Wang and Mauzerall, 2004)"

L28. Are the units of AOT40 correct? (ppm -> ppmv, but also "h daylight hour"?)

The units of AOT40 are usually calculated in ppm.h as highlighted in Van Dingenen et al. (2009).

P8648:
L12. Not equal weighting to ***all*** *concentrations surely, since it is only calculated for a certain time periods (?)*

In this sentence we are indicating that the Mx index is a mean index calculated during daylight hours over the crop growing season hence taking into account all ozone concentrations during that time. The AOT40 index however only

accounts for hourly ozone concentrations (again during daylight hours) that are over 40ppbv hence has a bias towards higher ozone concentrations that occur during the daylight period. We have amended the sentence accordingly to clarify this argument in the revised manuscript.

L15-L17. This is not clear. What is the “Mx Weibull relationship”?

We have added the definition of the Weibull relationship into the revised manuscript indicating that yield loss essentially varies exponentially with changing Mx rather than linearly as with AOT40.

L23. “less biologically significant lower ozone concentrations” – but you just said that concentrations < 40 ppbv matter!

This sentence is intended to convey that while larger ozone concentrations (> 40ppbv) are more important in terms of plant damage, some damage can still occur at lower concentrations. The Mx indices take some account of this, whereas the AOT40 index can only take account of ozone concentrations larger than 40 ppbv. We have reworded this section in the revised manuscript to make this point more clear.

L26. “As such….” Isn’t this sentence a bit obvious?

We have omitted this sentence.

P8649:
L26. Is it also due to the lack of a land/ocean boundary between Europe and Asia?

The lack of the land/ocean boundary is one of the reasons there is less efficient long range transport of pollutants from Central and Eastern Europe compared to the North American and Asian east coasts. Stohl (2001) shows that warm conveyor belts associated with frontal systems originate most frequently over warm water pools along the eastern seaboards of the North America and Asia. Therefore the lack of a land/ocean boundary over eastern/central Europe results in lower WCB activity and hence pollution transport to the free troposphere, which ultimately affects long range pollution transport.

L27. “…exceedences of AQSs…”

This has been amended in the revised manuscript.

P8650:
L9. “Mills et al. (2007) review….” (?)

This has been corrected in the revised manuscript.

L24. “….emission scenarios. In particular they focus on the impact…” The rest of this sentence could be made clearer (what are “production losses”?)

This sentence has been clarified in the revised manuscript to indicate that this study focused in particular on the impact of relative yield losses on crop production losses in terms of reduced yield due to ozone damage and the associated economic losses.

L27. Define what is meant by yield losses. As I understand it (from later in the text), it means the reduction in yield compared to a theoretical case where there is no pollution (…?)

This has been amended in the revised manuscript to clarify that yield losses are the relative reduction in yield based on a case where there is no crop damage from ozone pollution.

P8651: L13. "with" -> "against"

This has been amended in the revised manuscript.

P8652:
L1. Use commas rather than dashes.

This has been amended in the revised manuscript.

L6. Why is "2000" in quotation marks?

This has been amended in the revised manuscript.

L8. Spell out "POET"

This has been amended in the revised manuscript.

L12. Colon at the end of this sentence.

This sentence has been amended in the revised manuscript to now include a brief description that the model was run for a control scenario and 3 further runs where a 100% emissions cut were applied to NO_x over each of the source regions shown in Figure 2 of the revised manuscript. The list of the scenarios has been omitted from the revised manuscript, as it was unnecessary.

L26. Aren't the model levels defined as mid-points anyway? Also, is this a hybrid-sigma model? If so, I guess the position of the lowest level above the surface will depend on the surface pressure – i.e. it will vary globally. (Not too big a deal if you're mostly near sealevel.)

The TOMCAT model defines its 31 vertical levels using hybrid sigma-pressure levels. Surface pressure is used to calculate the position of the model levels, hence this does vary globally. We output our surface ozone field at the lowest model level the mid-point of which lies at approximately 30m above the surface.

P8653:

L3. Spell out "LRTAP Convention" (authors/editors though?)

This refers to a manual on the methodologies for calculating ecosystem damage from deposition from chemical transport models. The citation does not include authors or editors. We have spelt out the definition of LRTAP in the revised manuscript.

L9. Perhaps point to Figure 1 here.

We have amended this sentence in the revised manuscript to point to new Figure 2 to indicate the extent to which the crops are grown worldwide. This figure is now updated to also include crop production data.

L11. "..50 Wm-2. Values are…"

This has been amended in the revised manuscript.

L28. Are there references for the statements about "ozone sensitive" and "moderately ozone tolerant" crops?

References have been added to support these statements. E.g. Mills et al., (2007).

P8654, L7-8. "…such that the relative yield is equal to 1."

This has been amended in the revised manuscript.

P8655:
L2. "…using data from…" (you mention the nature of the data below)

This has been amended in the revised manuscript.

L16. Consider an introductory paragraph to let the reader know what to expect in the results section.

We have added a brief summary at the start of the results section covering the major results that will be shown.

P8656, L2. "…Data Centre…"

This has been corrected in the revised manuscript.

P8657:
L10. "…Fig 2, except that SE Asia has been replaced by Japan, since the latter is…"

This has been amended in the revised manuscript.

L19. Reference for "model failing to decouple the shallow nighttime…"?

This sentence has been amended in the revised manuscript to state that surface model ozone may be biased high in regions where model vertical mixing may be suppressed, as also suggested by Avnery et al., (2011a) with regard to their model / observation comparisons for ozone over NE USA.

P8658:
L6. "While larger than the biases…"

This has been amended in the revised manuscript.

L13-. Are you using 3-month mean AOTs? Please clarify.

The text has been edited to indicate that the AOTs are calculated over the crop growing season which have now been defined in the revised manuscript as the time between the crop planting and harvesting date for each specific crop type.

P8661:
L11. "are" -> "were"

This has been amended in the revised manuscript.

L14. "These differences could (?) be explained by the use…"

This has been amended in the revised manuscript.

L20. Quite likely different ozone distributions. E.g. see Stevenson et al. (2006) JGR, ACCENT ozone intercomparison.

We agree with this assessment. We have added a comparison between our findings and those of Avnery et al. (2011a) to highlight that differences between our results and those of previous studies are likely to also arise from the use of different chemical transport models as well as different emissions data sets and crop growing seasons (both Avnery et al. (2011a) and Van Dingenen et al. (2009) employ the use of crop data from the USDA).

P8662:
L20 (and throughout). Percentages of percentages are a bit confusing. How about just saying the absolute changes in relative yield?

In the revised manuscript we have decided to represent our transboundary effects in terms of % changes to crop production loss (CPL) which is calculated in million metrics tonnes (please see response to Reviewer 1). In the revised manuscript we discuss the transboundary effects in terms of relative changes in CPL.

L22. I'd just quote values to 1 d.p.

This has been amended in the revised manuscript.

P8664, L22-. This is all repetition from the previous section.

We agree that this section contains repetition from the introduction section, and have omitted this in the revised manuscript.

P8665, L8. "Potato" -> "potato"

This has been corrected in the revised manuscript.

P8666:
L18. "Assuming that under…"

This has been amended in the revised manuscript.

L25-. Not clear what this is getting at.

This sentence is indicating that due to the Weibull nature of the exposure-response relationships for the Mx indices, the magnitude of the response of relative yield loss to a given change in ozone concentration is larger if the initial absolute concentration of ozone is larger. This has been clarified in the revised manuscript.

P8667, L12-17. Section before "An additional factor" should be in the same paragraph as the statements about climate/crops in the previous paragraph.

This has been amended in the revised manuscript.

P8668, L25. "However, it is important to consider…"

This has been amended in the revised manuscript.

P8669, L3. So, do you think that the results from this study are conservative estimates?

The purpose of this statement is to indicate that as the exposure response relationships were derived for European and N American crops, there is uncertainty as to the sensitivities of Asian crops to ozone exposure. We have clarified this statement in the revised manuscript to indicate that there is some uncertainty in the sensitivities of Asian crops and that they may differ to those grown over Europe and N America.

Table 1: Delete "receptor" (as these are also source regions, right?)

Table 1 has now been replaced with a figure to show global NO_x emissions under the control and each of the emissions reduction scenarios.

Table 2/3: Delete redundant reference column and add to title (since all the same references). Are there any errors quoted for the functions' coefficients?

This has been amended in the revised manuscript. Mills et al. (2007) do not provide uncertainties on the coefficients of the relationships.

Table 4: Remove URLs and add as table footnotes (save repeating the same information)

This has been amended in the revised manuscript.

Table 5: Please add units.

% units have been added in the revised manuscript.

Figures 2/3/4: Spell out what "NMMB" is in the caption.

This has been amended in the final revised manuscript.

Figures 5/6/7. I would consider having the panels stacked vertically, with the CONTROL results at the top (own colour bar), followed by the sensitivity studies underneath. Also, could better values be chosen for the colour bars (i.e. integers)?

We have amended the figures accordingly in the revised manuscript and have replaced the numbers in the colour bars with more sensible values. As we are now using crop specific growing seasons to calculate the metrics, we show the changes in AOT40 and M7 (to represent changes in Mx, as changes in M12 are similar) for ozone sensitive (wheat) and ozone tolerant crop (rice) growing locations. We also now include changes in W126 for both maize (ozone tolerant) and soybean (ozone sensitive) growing locations. The plots for the rest of the crops and M12 are shown as supplementary material.

Figure 10. Describe that the standard deviation refers to a normal distribution in the caption. As with the colour bars, how about having better values for the contours? Integer percent values? Maybe a colour plot would help to read what the values are in the top left panel.

In the revised manuscript we have kept the figure as it was previously however we have replaced the contour labels with values that only extend out to one decimal place. The description that the standard deviation relates to a normal distribution has also been added the caption.

Supplementary material: Is this really needed? Could just leave it with the statement that AOT30 and AOT60 gives similar results in the main text (P8659). Figure 2 of the material might be better in the main text too. (If this material is kept, I would suggest adding proper figure captions.)

We agree that the yield loss changes under different AOT thresholds is not necessary to include as a plot. In addition, we have merged the information from Supplementary Figure 2 with new Figure 2 (old Figure 1) in the main manuscript, to include crop yield data as well as location and emissions regions.

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