Response to Referee #1

1 general comments

evaluating the overall quality of the discussion paper

- The authors study the effects of ozone damage on vegetation, air quality, and temperature (among others) in China with regional model simulations at 27km horizontal resolution.
- The manuscript comprises results from model simulations with a "revised version of the WRF-chem model" (based on version 3.8.1) limited to 4 growing seasons (2014−2017).
- The authors use (implemented?) a two-way coupling between the atmospheric chemistry module (WRF-chem) and the land module (Noah-MP).
- The ozone damage scheme is based on the work of [1] (decoupled stomatal conductance and photosynthesis under cumulative ozone uptake).
- The used WRF-chem model setup (among myriads of reasonable choices) is listed in a comprehensive manner.
- Overall, the manuscript is decently written. The language is concise and comprehensible but needs refinement in terms of proper grammar (see an incomplete listing in Section 3).
- The results are presented in a comprehensible manner but more discussion is needed.

Response: We thank the referee for the positive comments and valuable suggestions that are helpful for improving our manuscript. All the questions and concerns raised have been carefully discussed and addressed. We have provided a point-by-point response to the reviewers' comments below in blue color.

- Regarding the overall quality of the method. Large biases are reported between modeled concentrations of various species and observation. This is neither discussed nor are implications on the later results given. Hence, there are two major concerns (presented in more detail in Section 2):

1. The large model biases are not discussed but made light of. Regardlessly, averages over all stations are computed. It would improve the point of the paper, if only those stations were taken into consideration for further analysis, which show a low model bias.
2. The authors will not be able to fix the underlying issues in the model, but they should at least put an effort into discussion some of the reasons for the huge discrepancy between model and observation - maybe there are even quality issues with observations (representativeness)?

Response: Thanks for your constructive suggestions. Yes, we agree that averaging the results of all stations may cause more biases. A more detailed model evaluation and discussion talking about the discrepancies have been conducted and now included in the revised manuscript (Line 303–309, Line 311–332) and in the supplement (Tables S4–S10, Tables S12–S23).

- The authors point out that they use a "revised WRF-Chem model" but are missing a section about **code availabilit**y completely. Information on this has to be added.
  Response: This section is now added in the revised manuscript.
- A section about author contributions seems to be missing, too.
  Response: Added with thanks.
- A section about competing interests is also missing (see manuscript composition on https://www.atmospheric-chemistry-and-physics.net/submission.html#templates).
  Response: Added with thanks.

2 specific comments

individual scientific questions/issues

- Section 2.3

L264−268: "[...] each simulation was conducted from 24 May to 1 September [...] the days in May were discarded as spin-up [...] active growing season of plants." Considering that ozone damage in the used formulation [1] is accumulative, the described method above potentially results in an underestimation of ozone uptake (before May 24), in particular for evergreen natural and seminatural vegetation which is expected to be active even before May 24. The "active growing season" the authors refer to is for of crops, perhaps? How did the authors account for this potentially unaccounted ozone uptake? At least, it is highly recommended that they discuss this matter.

Response: Thank you for the comment. It is true that the simulation period of our study could not cover all the periods when O_3 damage happens and may not cover the growing season of all vegetation types, which may cause uncertainties. We have revised the sentence in Section 2.3 to make this point clearer. The sentence in Line 276–277 in Section 2.3 has been modified as follows: "JJA was selected because of the most severe O_3 pollution in this season and because it is within the active growing season of the plants."

We have also acknowledged the uncertainty caused by this problem in the conclusion section in Line 543–549 as follows: "In this study, the summertime simulation period of JJA was selected due to the high O_3 pollution in this season and the overlapping with vegetation growing season to capture the severe O_3 damage on vegetation. Nevertheless, uncertainty may still arise from that our simulation period may not cover the growing season of all vegetation types and may not cover all periods that O_3 damage happens, which may represent an underestimation of the full scale of O_3 damage. Future work should be conducted for longer time periods and for all seasons, which will help us better understand O_3-vegetation interactions in China."

L270−282: "Atmospheric forcing 2014−2017; anthropogenic emission 2014; [...] these years were selected based on high O_3 concentrations pointed out in previous studies [...]. [...] evaluated using available in-situ observations in China. [...] mean biases (MB), and correlation coefficients (CORR) [...]"
There is a contradiction between the chosen method and the intention described in the text. I assume anthropogenic emissions have been held constant at 2014 levels in these simulations. This is not properly expressed.
Furthermore, computing the average divergence between model and observations in this case is not reflecting the model bias (as described by the authors). Given you'd have a perfect model and perfect anthropogenic precursor emissions for 2014, you would still expect larger divergence between model and observation for any other meteorological year than 2014. This is actually the case, as can be seen in Table 3 (MB values). Hence, the authors actually study the impact of different meteorological conditions (2014−2017) on pollutant concentrations in 2014. The authors should elaborate on this and rewrite these parts of their manuscript accordingly.
Response: Thanks for this comment. This part has been revised in Section 2.3 in Line 274–275 as follows: "For each simulation in the four years, anthropogenic emissions were kept at 2014 levels, while meteorological fields were changing every year."
This sentence has been deleted: "These years were selected based on the high O_3 concentrations that were pointed out in previous studies (Li et al., 2018; Lu et al., 2018; Silver et al., 2018)."

It is true that the changes in air pollutants are mainly driven by changes in meteorological fields since anthropogenic emissions are kept in 2014 levels. We have tried our best to use the most updated anthropogenic emission inventory that we have in our simulations. We would like to clarify that this study did not aim to investigate the role of emissions or the role of meteorology on air pollutants. The main scope of this study is to investigate the effect of O_3 damage on vegetation and the following changes in meteorology and feedbacks on O_3 concentration. We recognize this limitation could cause uncertainties and it should be mentioned in the paper, so we added the following in Line 566–568 in the discussion section as follows: "It should also be noted that keeping the anthropogenic emission inventory fixed in 2014 levels may be another limitation because of the nonlinear chemistry involving biogenic and anthropogenic precursors."

L319−330: "The results indicate general overestimation by the model of most air pollutants except for CO. [...] but the spatial distribution of both meteorological variables and air pollutant concentrations are reasonably simulated by the model, lending credence to the use of the model for sensitivity studies [...]"
CO is highly underestimated (Table 4). The bias in ozone concentrations in 2014 between the default model version and observations is of the same size as the observed concentrations. In conjunction with the large underestimations in CO concentrations, this may point to issues with either the ozone chemistry (to low titration perhaps) or a generally too low dry deposition in the model. In particular, the latter is affected by the implemented two-way coupling between ozone-induced damage on vegetation and the atmosphere and thus the main subject of this manuscript. The authors need to elaborate more on this and properly discuss reasons for the divergences (systematic uncertainties in both model and observations) and implications on the results.
Response: Thank you for this comment. We agree that more explanation should be included in the revised manuscript. The explanation is now added in Line 316–318 in the revised manuscript as follows: "The underestimation of CO can be explained by either O_3 chemistry, which points to the problem related to low titration, or in the underestimation of dry deposition by the model, which is also affected by the modification of the model."

L350−352: "Comparing the changes in RSSUN and RSSHA, [...], reflecting the larger sensitivity of shaded leaves to O_3 damage." The authors should perhaps cite relevant articles, e.g. [2].
Response: Thanks for the references, which are now included in the revised manuscript in Line 353.

L372−380: "[...] where original PSN values are small [...]" This seems to be mainly the case for arid regions in western

China where the main vegetation type is grasslands. The text would benefit from referring not only to regions but also the associated types of vegetation in this regard.
Response: Thanks for this comment. The text has been revised in Line 367–369 in Section 3.2 in the revised manuscript as follows: **"**In western China where the dominant vegetation type is grassland and the original PSN values are small, more than 40% of PSN is reduced due to O_3 damage (Fig. 3c)."

- Section 3.3

L474−477: "[...] we resort to use the more universal O_3 threshold of 40 ppb [...]"
The authors are referring to the ATO40 metric used for risk assessment of potential ozone damage on plants. ATO40 is an exposure-based metric not taking the actual uptake of ozone by the vegetation into account. In the context of this work, the actual damage is modeled and has been quantified as reduction in NPP/GPP. In L234, the flux threshold is given as 0.8 nmolO_3 m^{-2}. Something does not add up in this paragraph, since these two thresholds are probably not interchangeable. I assume, that the authors are trying to say that previous studies used the AOT20/40 metric to assess potential ozone damage indirectly from modeled ozone concentrations. In their study they are able to directly assess the impact. And they consider the chosen limit on the flux (detoxification) as more conservative than studying AOT20. Then it is not the AOT threshold which is affecting the meteorology but the flux-threshold (L234). The authors should elaborate on this paragraph.
Response: Thanks for this comment. The paragraph has been rewritten in Line 438–444 in Section 3.3 in the revised manuscript as follows: "However, in their study, Li et al. (2016) assumed that O_3 damage to plants happens when O_3 concentration is over a threshold of 20 ppb to imitate a weaker detoxifying effect of plants, instead of the 40 ppb threshold that was commonly used in previous studies. Considering the severe O_3 air pollution in China, we resorted to use the more universal O_3 threshold used by previous studies (Lombardozzi et al., 2015; Sadiq et al., 2017; Zhou et al., 2018) to represent a more conventional detoxifying effect, instead of lowering the threshold value that would cause much larger changes in the surface fluxes and meteorological fields."

- Section 3.4

L497−499: "O_3 concentrations increase the most (by up to 6 %) [...] with the maximum increment of 6 ppb." Considering a model bias in ozone of the order of 100% (biases of the same size as observation), this is not significant and should be clearly stated.
Response: Thanks for this comment. This has been included in Line 473–475 in the revised manuscript as follows: "It should be cautiously noted that in terms of magnitude alone the model biases in O_3 are comparable and sometimes larger than the up to 6 ppb systematic enhancement caused by O_3 damage, which represents be one major source of uncertainties in our study."

3 technical corrections
purely technical corrections

- L5-12: Author affiliations. The affiliation indicated with * is missing from the list of affiliations.
  Response: Modified with thanks.
- L74: "Noah-MultiParamaterization" Missing space and typo-> parameterization.
  Response: Modified with thanks.
- L75: "CL M" Remove space.
  Response: Modified with thanks.
- L76: "[...] is commonly used in to simulate [...]" Remove in.
  Response: Removed in the revised manuscript.
- L129: "[...]A comprehensive study of how O_3 affects meteorology and air quality [...] is still limited but highly warranted." This sentence is slightly unclear. Particularly, warranted might not be the right term in the context. The authors may consider revising it.
  Response: The sentence has been revised in Line 133–137 as follows: "However, a comprehensive study of how O_3 affects meteorology and air quality through O_3-vegetation interactions in China at high spatial resolutions, especially under severe O_3 pollution, is still limited but highly needed. Moreover, there have been limited studies focusing on the feedbacks of O_3-vegetation coupling on O_3 concentration itself, especially in China, which is one of the main scopes of our study."
- L153: "[...] and cover the whole China" Grammar is probably o_. Remove article?
  Response: Removed with thanks.
- L161: "[...] and an hourly resolution that were suitable [...]" Consider rephrasing slightly: [...] and an 1 hourly resolution suitable [...].

Response: Revised with thanks.

- L168: "with Secondary Organic Aerosol Model" Probably needs an article (the) here.
  Response: Added with thanks.
- L294: "[...] in year 2017 [...] in year 2014" Remove word "year", respectively.
  Response: Removed with thanks.
- L302−303: "For example, the larger values [...]" This sentence appears to be incomplete - it misses at least a verb. Please correct.
  Response: Corrected in the revised manuscript.
- L325: "[...] at similar magnitude [...]" Change preposition: at -> of.
  Response: Modified with thanks.
- L329: "credence" This term sounds odd in this context. The authors may consider rephrasing the sentence and use "trust" instead.
  Response: Modified with thanks.
- L349: "units break into new line" Ought to be fixed here and other places in the following. Probably subject to final typesetting process, though.
  Response: Fixed in the revised manuscript.
- L473: "[...] used in other previous studies[...]" The authors should consider using either other or previous.
  Response: Modified with thanks.
- L573: "In this study, we found in China [...]" This sentence is hard to read, the authors should elaborate on it. Maybe: "In this study, we found that reduced dry deposition in China is mainly due to enhanced stomatal conductance, while enhanced isoprene emissions are mainly due to enhanced surface temperature and the corresponding increase in O_3 concentration.".
  Response: Modified as suggested.
- L584−605: Grammar and sentence structure is slightly off in this whole section and need refinement, in particular in L587 and L595.
  Response: The paragraph has been rewritten in Line 545–572 in the revised manuscript as follows: "Nevertheless, uncertainty may still arise from that our simulation period may not cover the growing season of all vegetation types and may not cover all periods that O_3 damage happens, which may represent an underestimation of the full scale of O_3 damage. Future work should be conducted for longer time periods and for all seasons, which will help us better understand O_3-vegetation interactions in China. Uncertainty may also arise from the O_3 scheme employed in this study in terms of the CUO calculation and the consideration of O_3 detoxification mechanism of different vegetation types. The calculation of CUO heavily relies on the O_3 threshold. Considering the sensitivities of different vegetation types to O_3 damage, CUO threshold should be varied with different vegetation types. However, a constant O_3 threshold was employed in our study for the whole simulation domain and for all vegetation types, which may either underestimate or overestimate the actual O_3 damage. Moreover, following the work of Lombardozzi et al. (2015), we classified all the vegetation types into only three groups, which may be too coarse to investigate O_3 damage effects on regional or local scales. For example, Zhou et al. (2018) pointed out that Lombardozzi et al. (2015) treated tropical and temperate plants equivalently, which might lead to possible biases. More studies should be conducted to derive more appropriate O_3 thresholds for CUO calculation and make them available for regional scales or for different vegetation types. Another source of uncertainty may arise from the lack of representation of the direct effect of O_3 on isoprene emission. As pointed out by Gong et al. (2020), including the effect of O_3 damage on isoprene emission may reduce O_3 concentration by influencing precursors, but increase O_3 concentration at the same time through weakening the shortwave radiative forcing of secondary organic aerosols, which would help constitute a more complete feedback mechanism between O_3 and vegetation. Moreover, uncertainties may also come from that the effect of soil moisture deficit was not considered in this study, which may underestimate the reduction in dry deposition sink of O_3. It should also be noted that keeping the anthropogenic emission inventory fixed in 2014 levels may be another limitation because of the nonlinear chemistry involving biogenic and anthropogenic precursors. Despite these uncertainties and limitations, our study provides detailed and comprehensive results whereby O_3-vegetation impacts will adversely affect plant growth and crop production, contribute to global warming, worsen the severe O_3 air pollution in China via feedbacks, and identifies the hotspot areas in the country. Our findings clearly pinpoint the need to consider the O_3 damage effects in both air quality studies and climate change studies."

- Page breaks: Some tables and figure captions are spread over several pages. Though, subject to final typesetting, this is slightly unpleasant. The authors may check their future manuscripts in this regard before submission.

Response: Thanks for this comment. The problem has been fixed in the revised manuscript.

- Diverging color bars:
  The two colors associated with the highest negative divergence are too similar (not distinguishable on printout). The authors may consider fixing this.
  Response: Thanks for pointing out this problem. Figures have been modified in the revised manuscript.

References

[1] Lombardozzi, Danica and Levis, Samuel and Bonan, G. and Hess, P. and Sparks, Jed, Temperature acclimation of photosynthesis and respiration: A key uncertainty in the carbon cycle-climate feedback, J. Climate, vol. 28, pp. 292−305, 2015, doi: 10.1175/JCLI-D-14-00223.1

[2] Yoshiyuki Kinose and Yoshinobu Fukamachi and Shigeaki Okabe and Hiroka Hiroshima and MakotoWatanabe and Takeshi Izuta, Photosynthetic responses to ozone of upper and lower canopy leaves of Fagus crenata Blume seedlings grown under different soil nutrient conditions, Environ. Pollut., vol. 223, pp. 213−222, 2017, doi:10.1016/j.envpol.2017.01.014