Review ACP-2021-165

Effects of ozone-vegetation interactions on meteorology and air quality in China using a two-way coupled land-atmosphere model

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 27 km 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.
- 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)?

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

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 semi-natural 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.
  - 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.
  - 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.

  - 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].
  - 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.

- 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 $0.8\,\mathrm{nmol\,O_3\,m^{-2}}$. 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.

- 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.

3 technical corrections

`purely technical corrections`

- L5-12: *Author affiliations* The affiliation indicated with * is missing from the list of affiliations.
- L74: *"Noah-MultiParamaterization"* Missing space and typo → *parameterization.*
- L75: *"CL M"* Remove space.
- L76: *"[...] is commonly used in to simulate [...]"* Remove *in.*
- 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.
- L153: *"[...] and cover the whole China"* Grammar is probably off. Remove article?
- L161: *"[...] and an hourly resolution that were suitable [...]"* Consider rephrasing slightly: [...] and an 1 hourly resolution suitable [...].
- L168: *"with Secondary Organic Aerosol Model"* Probably needs an article (the) here.
- L294: *"[...] in year 2017 [...] in year 2014"* Remove word "year", respectively.
- L302–303: *"Fore example, the larger values [...]"* This sentence appears to be incomplete – it misses at least a verb. Please correct.
- L325: *"[...] at similar magnitude [...]"* Change preposition: *at → of.*
- L329: *"credence"* This term sounds odd in this context. The authors may consider rephrasing the sentence and use "trust" instead.
- L349: *"units break into new line"* Ought to be fixed here and other places in the following. Probably subject to final typesetting process, though.
- L473: *"[...] used in other previous studies[...]"* The authors should consider using either *other* or *previous.*
- 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.".*

- L584–605: *Grammar and sentence structure* is slightly off in this whole section and need refinement, in particular in L587 and L595.
- 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 regards before submission.
- 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.

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 Makoto Watanabe 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