

Review of “Impacts of Biogenic Emissions on Summertime Ozone Formation in the Guanzhong Basin, China,” Li et al., ACP (2017)

Summary

This study utilizes a regional model to separate the impacts of anthropogenic and biogenic emissions on ozone concentrations in a populated region of central China. It is found that that the synergistic effect of bio + anthro emission is comparable to the anthro. impact along when considering daytime O₃ concentrations. The paper is organize fine and the English is mostly OK. The number of figures/tables seems a little high and dilutes the main message (Fig. 9) somewhat with a lot of details. Publication is recommended after consideration of the following minor comments.

General Comments

Treatment of uncertainty: Aside from several brief mentions of NMB or similar metrics, there is little discussion of model uncertainties. For example, uncertainties in emission inventories surely propagate into derived O₃ partitioning. In particular, it would be helpful to estimate the confidence in the values shown in Fig. 9, which is the key result of the paper.

NO_x measurements: the “NO_x” measurement uses a hot molybdenum converter, which is known to convert a lot more than NO₂ (probably most of NO_y). This point may or may not impact the model-measurement comparison shown in Fig. 3 depending on the NO_y partitioning, but it should be acknowledged and addressed.

Specific Comments

Sect. 2.2.1: It should be made clear hear that only 6 samples total were collected.

Page 7, Line 5: This mechanism seems outdated given the recent leaps in understanding of isoprene chemistry. Of particular concern is treatment of alkyl nitrates (which can be temporary or permanent NO_x sinks, depending on the mechanism) and the general assumed fate of Isoprene RO₂ radicals. The authors should consider either 1) justifying why this does not impact their results, or 2) assessing potential uncertainties arising from use of an outdated mechanism.

Page 9, Line 5: “Brute force” does not, to the reviewer’s knowledge, refer a specific method of source characterization. Please refine or define.

Page 10, Line 22: Does the model use assimilated meteorology? If so and the model is being nudged with observations, this agreement may not be especially remarkable.

Sect. 3.2: There are only 6 observations here, and looking at Table 1 the model seems to have little skill in capturing the variability of those. It would be worthwhile to point this out and justify why that's not a big deal for the present analysis.

Page 11, Line 30: "agree well" seems optimistic. Looking at Fig. 4, 5 of the 11 non-rain days show model-measurement disagreement by more than a factor of 2, and it is not evident that the model captures observed day-to-day variability.

Page 13, Line 7: Is this JNO₂ calculation based on model output? Please clarify.

Page 13, Line 15: presumably, aerosol lifetime is also longer than NO_x.

Page 15, Line 18: Calling this the "original state" is confusing. If it is the anthropogenic simulation, just refer to it as such.

Page 16, Line 10: It is not clear that the discussion of the "actual" contributions adds much to the message of the paper. Indeed, it is a bit confusing because "pure" and "actual" have similar connotations, and because it convolutes several of the separated contributions.

Sect. 4.3: The lack of a "synergistic" contribution to PM is noteworthy and may deserve a few more sentences of discussion, especially given its importance for air quality. In other regions (e.g. the SE US), there seem to be relatively strong links between anthro/bio emissions and PM.

Table 2: Not sure this is necessary for the story. Could be moved to supplement. Your call.

Technical Comments

Page 12, Line 14: discuss

Page 16, Line 20: delete "and discussion"

Fig. 12: suggest modifying colorbar to better separate NO_x vs VOC controlled regions (e.g. blue-white-red gradient) and mentioning the limits for each regime in the caption.