General Comments

The authors have chosen to look at the impact of emissions changes on January and July ozone in China. The manuscript is improved over the first draft, however there are still some organizational changes that could be made to improve the impact of this paper. Specifically, shifting the discussion to first focus on absolute ozone changes, and then focus on the drivers of those changes, could help avoid inconsistencies. Finally, the way the paper is written, it is not clear why the 'TOTAL' case belongs in the supplement instead of the main paper. The paper would be more impactful as well if the abstract clearly spelled out the big picture relationship between their emission scenarios, and the impact on both gas and aerosol-phase. Currently the abstract only discusses ozone but none of the complexity related to aerosol impacts for example on HO2 uptake.

Specific Comments

Abstract – The authors make no mention of their conclusions related to aerosol changes.

Line 258 – The authors state "A distinct increase in the surface mixing ratio of HO2 radical is derived in southern China (by up to 5 pptv or 60%; Fig. 1b). This enhancement is related to the increased mixing ratio of the OH radical found in urban areas, resulting in enhanced HO2 levels via VOCs oxidation, and a reduced HO2 loss via the aerosol uptake, as the aerosol load is reduced (see Sect. 3.2.3) (Song et al., 2021)." However the increased HO2 appears to be present almost everywhere, while the increase in OH is highly localized to urban areas as the authors state. The authors should be more clear that the broader increase in HO2 must be due to the reduced HO2 loss via aerosol uptake. If the authors tracked production from that chemical pathway, they could show a map of that rate decrease which could help. The authors could also consider a plot of the decrease in PM2.5.

Figure 1&2 – I would suggest adding the 'TOTAL' case to Figure 1 and Figure 2. Figure S4b appears to be mis-labeled as January instead of July.

Line 304 – What do you mean by "These changes are affected by meteorological parameters including the temperature, the water vapor abundance, and the solar radiation intensity, which affect the oxidative processes (Dai et al., 2023)." Is meteorology not the same in both simulations?

Line 342 – It looks like the abundance of OVOCs is reduced in "all" regions, not "most" regions.

Line 347 – Can you explain why this is? "the decrease is the most pronounced in the concentration of ketones" Why does the concentration of alcohols for example not seem to change at all? Is the model budget of alcohols really dominated by BVOCs, and if so, could that really be correct?

Line 376 – Is it more effective OVOC production? Or biogenic emission of OVOCs? Maybe a budget of OVOCs (AVOC vs. BVOC vs. secondary production) would help?

Line 411 – There is still a lot of VOC-limited area. Instead of "tend to be converted", maybe say x% of VOC-limited is converted to transition or NOx-limited?

Line 414 – Just confirming that your HO_2 uptake reaction does not produce H_2O_2? Does it produce H_2O?

Line 420 – Against suggest pulling the 'TOTAL' case in to the main text.

Line 431 – Can we learn something from Guangzhou? Does Guangzhou have differences in emissions compared to the other cities that would explain why it remains VOC-limited? In the N+A and TOTAL cases, this applies also to Shanghai? What is the difference compared to Beijing and Chengdu?

Figure 6 – Shouldn't the unit be (ppbv) not (pptv)?

Table 2 shows that in winter, NO_x reduction results in ozone increase in all cities, and AVOC reduction results in ozone decrease in all cities. The N+A and TOTAL cases result in ozone increases in all cities. In summer, NOx reduction results in ozone increases in all cities while AVOC reduction results in decreases in all cities. In the N+A and TOTAL cases, ozone decreases in Beijing, Shanghai, and Chengdu, but not in Guangzhou. According to Figure 5, in July, in the N+A and TOTAL cases, Guangzhou and Shanghai remain VOC-limited while Beijing and Chengdu shift to transitional conditions. Given that you get a different picture from Table 2 vs. Figure 5 (in Table 2, Guangzhou stands out) but in Figure 5, Guangzhou and Shanghai are different from Beijing and Chengdu, it might help to start with the ozone changes in Table 2, and use your other analysis to explain those changes, rather than starting with radical changes and NOx vs. VOC-limited changes.

Figure 7 – I think it would be better if 7b was on the same scale as 7a and 7c.

Line 499 – The meaning of this is unclear "followed by effect of NO_{4+}".

Line 515 – Cite Dai et al., 2023 here for this model bias evaluation?

Line 550 – Please add some discussion of the model HO2 uptake parameterization and uncertainties in the strength of this uptake (for example, is gamma 0.2 or 0.1)? Previous studies have reduced this gamma to better fit observations (e.g., Yang et al., 2023).

Line 555 – Can you better describe the calculation of AOC? Is there an equation you can add here?

Line 557 – There is nothing in the discussion below to support this statement: "This parameter allows us to characterize the formation process of O_3 and can be used as an indicator to design mitigation policies for reducing ozone pollution." I think this comes in better in the conclusions where you describe the relative importance of different VOCs to AOC. How is this different/better than the use of OH reactivity? The conclusions mention that AOC helps you to pick out "alkenes, aromatics, and unsaturated OVOCs, especially methanol and ethanol." It would help if the identification of those VOCs were discussed in Section 3.3 and not solely placed in the conclusions.

Line 640 – Better to name the specific cities and instead of 'slight' give the actual increase.

Line 651 – Does ozonolysis really have a net impact of increasing ozone levels? Just need clarification here on the suggestion that the net effect is positive.

Line 653 – Do you mean enhance the level of OH? Otherwise this is a nice schematic and helpful description.

Line 704 – The authors state: "The modified code in the WRF-Chem model is available upon request to the corresponding author." Best practice now seems to be to put modified code on Zenodo.

References

Yang, L. H., Jacob, D. J., Colombi, N. K., Zhai, S., Bates, K. H., Shah, V., Beaudry, E., Yantosca, R. M., Lin, H., Brewer, J. F., Chong, H., Travis, K. R., Crawford, J. H., Lamsal, L. N., Koo, J.-H., and Kim, J.: Tropospheric NO_2 vertical profiles over South Korea and their relation to oxidant chemistry: implications for geostationary satellite retrievals and the observation of NO_2 diurnal variation from space, Atmos. Chem. Phys., 23, 2465–2481, https://doi.org/10.5194/acp-23-2465-2023, 2023.