Review for Gao et al. 2020 submitted to ACPD.

General comments

In this manuscript, the authors focus on the radiative effect of aerosols on the tropospheric ozone photochemistry over China. Specifically, they address the discrepancy (reported in the previous studies) of the substantial decrease of the net ozone production due to aerosols and a much smaller magnitude of the actual ozone decrease at the surface. The authors highlight that the missing mechanism is the vertical entrainment, which enhances due to increased vertical gradients of the ozone in the PBL. They also address the possible mitigation of the recent increase in the surface ozone levels due to lower levels of the ambient PM concentrations. The apportionment method revealed that the local and adjacent regions play the dominant role.

The results of this work are based on the WRF-Chem simulations, ozone tagging technique and extensive observation. The analysis is mostly sound, the manuscript is well written, but some details are missing. I recommend a minor revision, which should give authors enough time to address the comments listed below.

Specific comments

I have one major concern regarding the mass balance technique, its description, and the interpretation of the results. Any balance equation inevitably has the residual term, which includes “other” processes (uncategorized) and the numerical error term. During the analysis, one has to show that the specified terms (for example, net chemical production or vertical entrainment) are much larger than this residual term.

Could authors, please, add the mass balance equation and the short description (possibly to the supplementary), which explicitly states all of the terms and address the following questions:

1. How well does the mass balance equation balance? What is the magnitude of the “numerical error” term compared to the other terms? Often, this term has the same order of magnitude. In this study, however, the agreement is exact (for example, Tables 3 and 4).
2. Was the dry deposition taken into account? If it is incorporated into one of the three considered terms (CHEM, ADV, or VMIX), then it has to be extracted and presented separately, or the terms should be renamed (for example, CHEM+DEP).
3. How was the NET term obtained (for example, in figure 5)? Is it merely a sum of ADV+VMIX+CHEM terms or is obtained directly from the separate WRF-Chem output variable, which represents the d[X]/dt?

Given the reasonable model-observation comparison statistics, net chemical production (CHEM) and vertical mixing (VMIX) are likely the only major drivers, but the scientific analysis has to be rigorous.

My minor concern is related to the effect of aerosols on PBL height. In section 3.2 and Figure 3, the authors contrast the clean and polluted cases. The impact of the aerosols on PBL height is vivid (Figure 3, polluted case). However, no discussion on the aerosols properties or the effect of the collapsed PBL is offered. I think that manuscript will improve if authors describe in the text the composition of the aerosols (primary type) and the absorption properties (single scattering albedo). Additionally, considering that tracers are well mixed within the PBL, the PBL reduction by a factor of 2 translates roughly in a two-times increase in tracer concentrations. What role does the PBL collapse play in the adjustment of the surface ozone concentration compared to the aerosols effect on photolysis and vertical entrainment?

Technical corrections

1. References should be formatted appropriately and numbered.
2. Please, add units to Table 2.
3. Line 224. Delete “which showed that ozone stopped decreasing”
4. Figure 6, please, update the caption, remove the CASE* and explain that data was spatially sampled and represent four cities.

I enjoyed reading the manuscript and would like to thank authors for the accurate choice of the diagnostics and figures.