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Comment on acp-2021-613

Anonymous Referee #1

Referee comment on "Dipole Pattern of Summer Ozone Pollution in the east of China and Its Connection with Climate Variability" by Xiaoqing Ma and Zhicong Yin, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-613-RC1, 2021

Review report: ACPD Ma and Yin (2021)

This manuscript discusses the dipole like pattern in summer time ozone concentrations (DPO) seen in the north and south parts of China. The authors particularly focus on the roles of interannual variability (IAV) of climate dynamics like Rossby wave activity and propagation, and Indian ocean SST (dipole mode). Since O_3 pollution, being increasingly important, is now one of the biggest issues of air quality as well as PM2.5 which is on the decrease in China, revealing underlying mechanisms of interannual changes and variability in surface O3 including precursor emissions and meteorological factors is of importance. I enjoyed much their discussions on the mechanisms that cause dipole anomaly patterns over China focusing on the roles of Arctic sea-ice and southern Indian ocean SSTs. However, the authors do seem to only focus on the aspect of climate dynamics, just looking at the meteorological parameters like pressure field, precipitation, and fail to address the actual mechanism of the dipole like O3 anomalies over China. They do not seem to answer enoughly to the important questions of how meteorological changes give a rise surface O3 anomalies in the region (upward/horizontal transport? Chemical loss? Chemical production?). Furthermore, I do not fully agree with the analyzing approach adopted in this manuscript. For the O3 variation, they use simulation with GEOS-CHEM, while for meteorological/climate parameters, another dataset like Era-5 is used instead. To keep consistency, the authors should have used the same meteorology as used in GEOS-CHEM. I rate this manuscript as “major revision” at this moment, but think this stuff could be published in ACP after additional analysis and modification are properly included.

Major comments:

M1) The authors use different meteorological datasets for ozone (DPO) and meteorological parameters (i.e. GEOS-Chem with MERRA-2, and ERA-5), isn’t it possible to fix this inconsistency by synchronizing those two? Ideally, you should close the overall discussions within the framework of GEOS-Chem model simulation. At least, they should verify consistency between the Era-5 and MERRA2 in detail.

M2) The DPO patterns are only evaluated with surface observations in China (Figure 1). But the DPO pattern could and should be further evaluated by using satellite observation like OMI/TROPOMI. This could make your later discussions (relationship with SI or SIOD) further clear and robust.

M3) As pointed out above, you should more clearly discuss the actual mechanism of DPO. At least, is should be separated into two categories: 1) transport (bot vertical and horizontal motion could affect surface O3), and 2) chemistry (production and net-production of O3 in the regions). You should evaluate the chemical roles by taking a look at chemical tendency (P and P-L) of O3 in GEOS-Chem. For transport aspect, this can be evaluated by focusing on the concurrent changes in distribution of carbon monoxide (CO) or any other inert-like tracer. And if you look at CO, satellite observation (MOPITT etc.) may further help and reinforce the discussion on the transport aspects.

M4) You need describe more about your GEOS-Chem simulations especially for the emissions. How does the model consider natural emissions like BVOCs and LNOx? In this manuscript, the authors state that they use GEOS-Chem simulations with fixed emissions. What kind of emission types are targeted for this treatment? (only anthropogenic or natural emissions also?) If the natural emissions are not fixed and follow the climate/meteorological variability, the authors should add description on how these natural emission IAVs affect DPO. On the other hand, if the natural emissions are fixed as anthropogenic ones, you should in turn discuss how the natural emissions, if they are allowed to vary in response to climate condition, would cause additional effects.

M5) For the dipole pattern over N/S China, the authors only try to attribute it to SI and SIOD. But in fact, this phenomenon should be also tightly linked to Asian monsoon and ENSO, shouldn't it? Please extend your discussions to cover the monsoon and ENSO influences.

Minor comments:

- L41: "were lower" --> "were lower by about *** ppbv"
- L46: "enhancement of natural emissions of ozone precursors" Yes it's true, but it is not clear at all how natural emissions are treated in this study.
- L98-103: The math formulation should be further explained. What are *f*, z,*U*? and what do "_x", "_xy", and prime "'" indicate? (I know what they are, but major part of readers can not catch them on)
- L136: Doesn't this version of CESM-LE consider chemistry? If so, you could discuss DPO in CESM—LE as well.
- L147-148: "the GEOS-Chem model has a good performance … Therefore EOF was applied" : I don't understand the logic here. The sentences should be rephrased.
- L152: "the first EOF pattern …": What does this 1st EOF mode stand for as

meteorological phenomenon?

- L176: “a moist, cool, … weak solar radiation were conductive to low O3” : Please check and discuss changes in chemical production and photochemical lifetime of O3 in GEOS-Chem.
- L192: “, but its effected” --> “, but its effects”
- L216: “After removing the influences of ENSO”: What are the ENSO influences like? And how did you remove them?
- L226: “82%” how did you draw this value?
- L227: “active centers” I didn’t follow this.
- L232: Note that the correlation coefficient between them was only 0.21and was not significant” : The authors claim that SI and SIOD impacts are causing independently DPO. But I don’t think so. Even if correlation is weak, years extracted for the composites for SI and SIOD may overlap each other. Please check the sample years used for making composite to verify whether enoughly different years are used for SI and SIOD for your discussion like with Figure4(c),(d) which are too similar.
- L261 “(+)” : what does this represent?
- Figure 5 (especially c,d) panels are quite busy and hard to check the description in the texts. Please improve the visibility.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-613/acp-2021-613-RC1-supplement.pdf