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Comment on acp-2021-942

Anonymous Referee #2

Referee comment on "Impact of eastern and central Pacific El Niño on lower tropospheric ozone in China" by Zhongjing Jiang and Jing Li, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-942-RC2, 2022

Review of "Impact of Eastern and Central Pacific El Niño on Lower Tropospheric Ozone in China" (MS# acp-2021-942) by Z. Jiang and J. Li

This manuscript examines the effect of natural ENSO meteorological variability on lower tropospheric ozone over China. The authors use satellite data (IASI ozone retrievals) and GEOS-Chem model simulations, along with meteorological reanalysis data, to examine the effects of Eastern Pacific versus Central Pacific El Niños. The mechanisms responsible for the simulated (and observed) ozone changes are investigated by looking at the changes in meteorological variables (including solar radiation, relative humidity, temperature, sea-level pressure, and winds) and in ozone budget terms (primarily transport and chemistry, which are found to be the dominant drivers). This study extends past examinations of ENSO teleconnections to the middle to high latitudes, to examine effects on atmospheric chemical pollutants such as ozone. The study concludes that El Niño generally results in a decrease of lower tropospheric ozone over China, although with some regional and seasonal changes that differ between Eastern Pacific and Central Pacific El Niños. This work provides a useful new contribution to the literature examining ENSO teleconnections to the extratropics and is relevant to air pollution control policy in China. This paper would be suitable for publication in *Atmospheric Chemistry and Physics* with revisions to address concerns detailed below.

Major Comment

GEOS-Chem simulations: The study uses several sets of simulations with GEOS-Chem. First, a transient simulation is conducted for 1980-2017 with anthropogenic and biomass burning fixed at year-2000 levels, in order to assess the effects of ENSO-driven meteorological variability on ozone. The use of fixed emissions from biomass burning, which is known to exhibit large ENSO-driven variability, is a limitation of this study that should be discussed and justified more fully. (An additional set of simulations with interannually varying biomass burning emissions would add greatly to this study, but might be prohibitive for the authors to conduct at this stage.) A second set of simulations is conducted using composite meteorological fields (compositing over 3 EP El Niño events, 4 CP El Niño events, and a full 30-year period). The use of composite meteorological fields, which will wash out most synoptic variability, seems like an odd choice, as opposed to simply compositing the results from the EP and CP El Niño years in the transient simulation. This approach needs to be justified and discussed more fully. Comment also on the implications of this meteorological compositing (versus compositing over events from full transient run) for issues of signal-to-noise in your results.

Minor Comments

1. Introduction

Page 2, line 34 – Besides meteorological conditions, note that ozone concentrations are also largely controlled by precursor emissions (including anthropogenic emissions, which do not depend strongly on meteorology).

P.2, l.35 – Circulation and ventilation (i.e., transport) should also be listed as an important meteorological control on ozone.

P.2, l.37 – Change “prominent interannual climate variabilities” to “prominent modes of interannual climate variability”

P.3, l.65-69 – This is a sentence fragment. Please rewrite.

2. Data and Methods

P.6, l.134 – Give months here. Should “Autumn” be “August”? Also, how do you start in September 2007, if as stated above, IASI-A started providing operational products in October 2007? (These dates are also mentioned at lines 163-164.)

P.8, l.159-160 – Is this validation/evaluation done using the transient simulation with fixed year-2000 emissions? Would you expect results from such a simulation to match observed LTO values? You need to mention any caveats associated with this methodology here, and provide justification for why this approach was used.

P.8, l.169-174 – As mentioned in Major Comment above, the approach of using composite meteorological fields needs to be explained more fully and justified here.

3. Results

P.9, l.187-188 – Add "for ozone" after "climatology state." Why are the seasons in Fig. S3 labeled with 0,1 subscripts. This is a climatology, not a composite of ENSO events, right?

P.9, l.193 – State reference period (Sep 2007-Aug 2017?) in Figure 1 caption.

P.10, l.219-220 – Change to "poor *representation* of *the* Brewer-Dobson circulation." Explain how the B-D circulation influences lower tropospheric ozone here. Is there evidence that the distribution of ozone in the stratosphere is biased? Or, do you just mean here that the stratospheric influence on LTO is poorly represented (e.g., from biases in stratosphere-troposphere exchange, or high-lat to mid-lat mixing in the troposphere)?

P.12, l.260 – "insufficient" compared to what? Perhaps change wording to "negligible."

P.12, l.265, Figure 5 – Explain what is being plotted here. This figure is quite confusing. Is the absolute value of the tendency due to each process taken in each grid box, or after the full field is summed over the study domain?

P.12, l.266, Figure 6 – Confusing. Does this figure show a composite of tendencies due to these processes, or just the values from a single simulation with composite meteorology? Clarify in figure caption.

P.12, l.270 – “Southwestern” ☐ “Southwesterly”

P.13, l.278 – “Northwest” ☐ “Northwesterly”

P.13, l.291-294 – Run-on sentence. Split into two sentences.

P.14, l.313 – “Southernly” ☐ “Southerly”

P.16, l.343-345 – This manuscript uses acronyms very heavily. This is generally fine as a convenient shorthand–but occasionally, as in this sentence, it makes it very difficult for a reader to follow: “Controlled more by local Pacific than IO, the SLP center shifts eastward compared to AAC in EP, and the positive LTO anomalies also move eastward accordingly.” Try to rewrite.

4. Conclusions and discussion

P.16, l.362 – Write out "western North Pacific anomalous anticyclone" on its first use in this section to aid readers looking only at the Conclusions.

P.17, l.365-368 – Run-on sentence. Please split into two sentences or rephrase.

P.17, l.371 – Write out "western pacific subtropical high" on first use in this section.

P.17, l.389-390 – As mentioned in Major Comment above, the omission of interannually varying biomass burning from this study is a significant issue in assessing the effects of ENSO on LTO over China. You should elaborate on how inclusion of biomass variability could alter conclusions of this study, and comment on previous studies on the effects of this variability (if any)