Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-849-RC2, 2022

Comment on acp-2021-849

Anonymous Referee #2

Referee comment on "North China Plain as a hot spot of ozone pollution exacerbated by extreme high temperatures" by Pinya Wang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-849-RC2, 2022

In this manuscript, the authors present an interesting study that can be a valuable contribution to the existing understanding of ozone pollution in China and its connections to health and climate. The study is carefully thought-out, conducted through a series of original analyses, and arrives at significant findings. In addition, the manuscript clearly describes the work conducted. Several major comments and some additional minor points that should be addressed by the authors prior to publication are included below.

1. The authors rely on the 90th percentile at each grid cell to define O3 and temperature extremes. Given that the exceedance of air quality standards and health impacts are dependent on O3 concentrations rather than a percentile score, would a consistent threshold across all grid cells not be a more relevant metric for extreme air pollution? By relying on the 90th percentile for each individual cell, high O3 pollution cells are not consistently defined across the domain and thus a location with a large number of O3 pollution days as defined in the study may be experiencing lower total O3 pollution than one with a smaller number of O3 pollution days. A clearer description of this "local-specific" threshold, including justification and implications of its selection, is needed.

2. For the study's estimates of health impacts, a deeper discussion of the epidemiological studies and β coefficients selected is needed, including:

• How do the β coefficients compare to others reported by different epidemiological studies, including those most commonly used internationally? How extensively have the coefficients used here been applied, and why would they need to be specifically derived from data in China?
• The assumption of no lower threshold for O3 mortality (C_0=0) is not applied in other analyses. Rather, an assumption that a threshold for ozone effects is likely near the lower limit of ambient ozone concentrations in countries like the US is often considered. Given that all results presented here are based on the ratio of relative risks, it would appear that defining a C_0 (and T_0) threshold is not necessary and can be avoided.
• While in Discussion and conclusions, the study acknowledges the uncertainty associated with combining relative risks of O3 and temperature without considering coupled effects,

this limitation should be mentioned earlier when describing the methods. To what extent do each of the studies of O3 and T mortality from which the β coefficients are taken control for the other variable?

3. While temperature is a key driver of O3 formation, emissions of O3 precursors also play a major role in O3 pollution. The discussion of emissions in the paper is minimal. How do emissions, including anthropogenic and biogenic precursors, vary temporally and spatially? Beyond meteorological factors, could variability in emissions be partially be driving for the frequency and geographic differences in the co-occurrence of high O3 and temperature?

4. The authors acknowledge the role of interannual variability in the analysis of the 2014-2019 period, noting that trends reflect interannual variability rather than a long-term warming trend. Recent work has shown the significant influence that internal variability can have on long-term projections of both temperature and O3 concentration. Here, 5-year periods are used to characterize future temperature and air quality at midcentury and the end of the century. The length of these periods is insufficient to confidently distinguish a forced signal in temperature and O3 from the noise imposed by natural variability. How do the climate simulations used account for internal variability and to what extent may internal variability be affecting the climate-related findings of this study? At a minimum, the authors must acknowledge the large uncertainty imposed by natural variability on the projected coupled extremes.

5. Evaluation of model results against observations, for GEOS-Chem and CIMP6 simulations, should be expanded beyond a visual comparison of the spatial pattern and country total number of OPCs. Established model performance statistics (e.g., normalized mean bias, normalized mean error (NME), and correlation coefficient) can more definitely determine if the models indeed "reasonably capture" observed values and meet accepted performance standards. For the GCM simulations specifically, the models are known to often have high biases in modeled O3. Would bias-correcting the projected concentrations alter the findings?

6. The spatial resolution of the analysis (1 degree) is coarse and the model simulations are even coarser. However, resolution is not discussed. To what extent may the resolution of the data fields affect the results? Further discussion of the potential limitations imposed by coarse resolution is necessary.

Other comments:

• Line 41: Listing PM2.5 as an example (e.g.) of particulate matter is confusing.
• Line 60: The 2003 example described occurred nearly 20 years ago. Is it still relevant given the significant changes that have occurred in China since?
• It is unclear why it is necessary to "standardize" meteorological variables as described on line 116; explain the intent further.
• Define the resolution of GCMs (line 147 and table S1) in terms of degrees rather than

the number of model cells.
• Figure S2: Change the 'SC' label on panel (b) to YRD
• Figure 4: What are the units of the plots?
• Line 173 (and throughout): A better term than "mortality ratio" can be used. One that is more is more descriptive of what the ratio represents, enhanced O3 mortality for coupled extreme O3 and temperature days, should be selected.
• Figure 5 (and text in 3.3): Are the mortality ratios shown average values for all of China?
• Section 3.3: Approximately to how many deaths does the increase in mortality risk due to coupled O3 and temperature correspond?
• Line 295-296: Are the thresholds based on the historical observed or modeled values? If based on modeled values, are the thresholds unique for each model?
• Line 376: The value of radiative forcing of tropospheric O3 seems irrelevant, especially for the next-to-last sentence of the article.