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Reply on RC2

Youwen Sun et al.

Author comment on "The drivers and health risks of unexpected surface ozone enhancements over the Sichuan Basin, China, in 2020" by Youwen Sun et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-664-AC3, 2021

Response to Referee #2:

Thanks very much for your comments, suggestions and recommendation with respect to improve this paper. The response to all your comments are listed below.

This manuscript described an assessment of relative influence of meteorology and emissions on the surface O_3 enhancements over the Sichuan basin (SCB) in May-June 2020 using high resolution nested-grid GEOS-Chem simulation and the eXtreme Gradient Boosting (XGBoost) machine learning model. Furthermore, the health risks of the surface O_3 enhancements in terms of various premature mortalities are also evaluated by using the exposure−response relationship. The surface O_3 enhancements over the SCB are in contrast to an overall reduction in surface O_3 level across China. The authors first demonstrated the effectiveness of XGBoost to mitigate the model prediction discrepancy over the complex terrain over the SCB. The relative contributions of meteorology and anthropogenic emissions changes to the unexpected surface O_3 enhancements are then quantified with the combination of GEOS-Chem and XGBoost models. The authors concluded that the unexpected surface O_3 enhancements over the SCB is attributed to the unexpected changes in meteorology combined with the complex basin effect, which caused an increase in the total premature mortality of 89.8% in May-June 2020 vs. 2019. In general, the topic is interesting and the majority of the works are sound. It is well organized, written and analyzed convincingly, and its topic fits well in the scope of ACP. I recommend for publication after addressing the followings comments.

Response: All your comments listed below have been addressed. Please check the point by point response as follows.

General comments:

Comment [2-1]: The assessment of the influence of meteorology and emissions is based on the premise that the GEOS-Chem-XGBoost effectively corrected the model discrepancy over the SCB for May-June 2020. Since the ozone formation is highly non-linear and has strong dependence on its precursor levels and meteorology, the training data should cover the variation range, at least, of the key ozone precursors or meteorology. The training and validation of the XGBoost with observations for a specific period may not be applicable for all conditions especially for the case that significant emissions or meteorology changes occurred. In this study, the authors use a full seasonal cycle of hourly measurements in

2019 at each site over SCB as the learning samples, and GEOS-Chem input of emissions and meteorological parameters, output concentrations of atmospheric constituents, and time information as training input data. The usage of the GEOS-Chem-XGBoost is valid only if the range of variations for the training data in 2019 cover that in May-June 2020. So a few discussion or clarification is needed to consolidate the usage of this method.

Response: In the revised version, we have included the probability density functions (PDF) of the key ozone precursors (i.e., NO_2, CO and HCHO) and meteorological parameters (planetary boundary height layer (PBLH), temperature, specific humidity) in the two most densely populated cities over the SCB (i.e., Chengdu and Chongqing). We verified that the training data (a full seasonal cycle of 2019 measurements) cover the variation ranges of O_3 precursors in May-June 2020 (Figures 1 and 2 in supplement of this response letter).

Comment [2-2]: The aggregate meteorological influence and the aggregate anthropogenic influence are quantified with the GEOS-Chem-XGBoost method. However, the analysis for the influence of each individual meteorological or anthropogenic factor, based on the differences between 2020 and 2019 over the SCB and surrounding regions, are qualitative. The analysis for the potential vorticity (PV) is needed to be verified and modified. The differences in PVU over the SCB and surrounding regions are very small between 2020 and 2019. As a result, the meteorology-induced surface ozone increase over SCB may be attributed to other meteorological anomalies rather than PV. Normally, the influence of stratospheric intrusions on near-surface ozone can be evident on a specific short period but can't last for months. I recommend the authors to modify the PV associated deduction and temper the description for its influence.

Response: In the revised version, we have double checked the analysis for the influence of each individual meteorological and anthropogenic factor. We have followed the suggestions of prof. Heini Wernli and removed the analysis for the potential vorticity. As a result, all concerns arise from the PV discussions are gone. Since we only performed very few analysis for the PV in the study, all revisions are minor. Instead, we have compared and analyzed the difference in vertical transport velocity at the PBLH between 2020 and 2019. We concluded that there is no strong evidence for the change in the horizontal transport from other regions (Figure 3(b) in supplement of this response letter) and the vertical transport from the free troposphere to the surface (Figure 4 (a) in supplement of this response letter) over the SCB in May-June 2020 vs. 2019. Please check the marked up file for details.

Comment [2-3]: There are still some grammatical errors which needs further careful check. For example, the usage of "emission" and "emissions" is sometimes misleading. Referee #1 has listed part of them.

Response: In the revised version, we have corrected all grammatical errors listed below and one of the authors with good command of English have gone through the manuscript in detail to address the rest errors. Please check the marked up file for details.

Specific corrections:

Comment [2-4]: Page 6 line 4, "All these training input data are summarized in Table S1 and have been standardized". Please describe in more details for "standardized".

Response: In the revised version, we have included detailed description of "standardized". Please check section S1 in the supplement of this response letter for details.

Comment [2-5]: It would be helpful if Figure 1a-c showed the 1std of mean value.

Response: Done. Please check the marked up file for details.

Comment [2-6]: In Figure 4 (a), the ozone variability is smoother than that in Figure 3 (a). I wonder if a certain running average is used. Please clarify.

Response: In the revised version, we have stated that values shown in this figure are 7-day running average. Please check the marked up file for details.

Comment [2-7]: Figures 1, 6, 7 should add the corresponding latitude and longitude.

Response: We have included latitude and longitude information in Figures 1, 6, and 7 . Please check the marked up file for details.

Comment [2-8]: In Figure 8, the titles of each subplot are not needed.

Response: We have removed the titles of each subplot. Please check the marked up file for details.

Comment [2-9]: Can the authors show the differences in vertical transport velocity at the PBLH between 2020 and 2019? This could help the reader to understand the vertical transport at the studies period.

Response: In the revised version, we have showed the differences in vertical transport velocity at the PBLH between 2020 and 2019 over the SCB, and the resulting discussion are presented accordingly. Please check the marked up file for details.

Comment [2-10]: Please make sure all references follow the ACP format.

Response: Done. Please check the marked up file for details.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-664/acp-2021-664-AC3-supplement.pdf