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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term Variations in Ozone Levels in the Troposphere and Lower Stratosphere over Beijing: Observations and Model Simulations" *by* Yuli Zhang et al.

Anonymous Referee #4

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The paper uses ozone soundings in eastern China from 2002 to 2020 to analyze trends in different altitudes, 0-3km, 3-9km and 9-15km. The authors conclude on different trends at these altitudes and particularly increasing trends before 2011-2012 in the troposphere and decreasing trends afterwards. In the lower stratosphere observations show a slight increase before 2012 and constant values afterwards and some "superposition" of lower tropospheric and stratospheric trends in the free troposphere. To explain potential reason for these trends they compare the data with the Chemical Lagrangian Model of the Stratosphere (CLAMS). Notably CLAMS it is able to simulate the stratospheric ozone distribution, but has no tropospheric chemistry. To do the comparison of trends they calculated the relative changes for each period and season

and selected the strongest changes of these tendencies (before and after 2011-2012). They compare the ozone tendencies before and after 2011-2012 in both, model and observations. They conclude, that the stratospheric impact also significantly has contributed to the trends in the free troposphere (They termed 'superposition layer'). They also conclude, that trend changes in the emission strengths are the key driver for ozone changes in the lower troposphere corresponding to NO2 observational trends.

The main problem with the paper, which I see is, that it does not use any robust statistical metrics or error estimate. Trends are evaluated over time periods of very few years (2002-2011/2012 and afterwards) and the year to year variability is high. The tendencies, which are shown and discussed remain vague. E.g. Fig 3a) shows a trend of observed O3 of zero DU after 2012 (9-15km), in the conclusions (l.274/275) the negative trend in the stratospheric dominated regime is mentioned. Also the criteria to define time periods of trend changes are not motivated and seem to differ in different plots (Fig.6).

The observations are interesting in some parts, but the most interesting part, which is the change of ozone in the 9-15km layer, remains unexplained and is not analyzed. The authors discuss some links with ENSO without providing additional analyses and make no link to the tropopause location or jet, tropical widening. Potential tropospheric circulation aspects could in principle also play a role changing tropospheric long range transport of air masses with high stratospheric ozone from non-local downward transport.

As such I do not know what the key finding of the paper is. If so, is it the stratospheric change of trend? Is it its impact on the free troposphere? Given the methods and the coarse analysis I don't see the manuscript meeting the standards of an ACP publication in its current form, although some observations are interesting.

Major comments:

As I said the data record is interesting, but the analysis is more than coarse. The

authors should at least provide some statistical valid metrics for the significance of trends.

1) There are no significance or error estimates of the 'trends' (the authors state, that the the time series is too short for this, which is weird, since the focus of the paper is on trends)

2) The selection of time intervals to calculate trends seems arbitrary and different in different altitudes. Criteria are not clear and seem to differ (Fig. 6).

3) They should also explain more clearly the role of meteorology when interpreting the seasonally resolved trends in the free troposphere (note that the whole manuscript does not contain any mentioning of the monsoon, convection, tropopause, jets).

4) They use the CLAMS model, which has no tropospheric chemistry to compare ozone (as mentioned correctly by the authors). How do the authors exclude potential changes of tropospheric ozone sources, circulation changes and long-range transport, which could potentially also lead to different variability and trends? The fact that an incomplete model sometimes agrees with observations, does not automatically exclude other processes, which are not included in the model, to explain the observed ozone tendencies.

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