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 #3

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This paper summarizes an effort to diagnose recent changes in tropospheric ozone over the North China Plain from balloon-borne measurements (ozonesondes). This analysis is coupled with output from the Chemical Lagrangian Model of the Stratosphere (CLaMS) to help identify contributions from stratospheric ozone to observed changes over time. The analysis is mostly straight-forward, but it is not clear to me if this paper amounts to a substantial contribution. Similar recent studies have diagnosed the observed long-term changes using different datasets and have gone into somewhat greater detail to elucidate the processes responsible and the significance of diagnosed trends. I believe this could be a more meaningful contribution with greater communication of its novelty throughout and a bit deeper dive on the mechanisms responsible for

observed changes. Some specific concerns are outlined below.

General Comments:

1. The trend quantification in Section 5 is quite underwhelming. It is void of any statistical significance testing, which is necessary to diagnose meaningful changes. Moreover, trends are diagnosed for relatively short time periods, which provides little confidence in the result and leads to overfitting where there is substantial year-to-year variability. Confidence intervals (e.g., 95 and 99 percent) would be especially helpful here to demonstrate the degree to which diagnosed long-term changes (and reversals from the first to second half of the time period) are meaningful. Without statistical evaluation here, clear conclusions cannot be made for the diagnosed trends and the use of the term "significant" throughout the paper is inappropriate.

2. There are at least two claims based on comparison of observations and CLaMS output that are not justified based on the analyses conducted. First, the authors claim at lines 124-125 that CLaMS overestimates transport from the stratosphere to the troposphere. This is based on comparing ozone concentrations in the model to that observed and assuming a certain missing control by tropospheric chemistry. Without additional analysis (or citations to other more thorough evaluation), I do not find this claim to be justified based on the analysis presented in this paper. Second, at lines 261-266 it is argued that a reduction in stratospheric ozone found near the ozonesonde location in CLaMS is a result of ENSO, but there are certainly several alternative explanations for this change that are not acknowledged. Notably, Beijing is near the climatological mean latitude of the tropopause break (the sharp discontinuity in tropopause altitude from tropics to extratropics). Latitudinal migrations of the tropopause break could result in Beijing being more on the tropical side in later years, thus less exposed to downwelling stratospheric air. The latter can certainly be evaluated using the CLaMS output.

3. There is substantial repetition in the discussion of the time-evolving role of NOx.

Namely, a succinct analysis and discussion is given and then followed shorlty after by a less clear rehashing of essentially the same points while pointing to other work – lines 151-155. Perhaps the authors intend to make a slightly different point, but this is not clear.

Specific Comments:

Lines 34-35: delete "which transports stratospheric ozone into the troposphere and tropospheric ozone into the lower stratosphere" as it repeats the previous words in this sentence. Also, this sentence is incomplete. What about the exchange of ozone between the stratosphere and troposphere?

Line 66: "relative" should be "previous"

Line 74: The accuracy and precision of the ozonesonde data should be listed here.

Line 86: It is not clear where the "D" comes from in the ASAD acronym. A quick Google search shows that this should be defined here as "A Selfcontained Atmospheric chemistry coDe (ASAD)". Please revise.

Lines 140-148 and Figure 5: Details on OMI data used belong in the data and methods section.

Figure 6: It is not clear what exactly is being done to/with the data. Are the time series based on a three-month average of monthly means or something else?