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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A global tropospheric ozone climatology from trajectory-mapped ozone soundings" *by* G. Liu et al.

Anonymous Referee #1

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This paper uses the trajectory mapping approach to extend the utility of the worldwide ozonesonde database, creating a global tropospheric ozone profile climateology dating from the 1960's to the present. The paper also presents a series of decadal maps so that readers can easily identify long-term changes in the ozone distribution.

The authors advocate the use of this climatology for model initialization and validation and for use as an a priori climatology for satellite instrument data retrieval algorithms.

The work builds nicely upon previous uses of the trajectory mapping technique and does a credible job demonstrating that the output data product shows reasonably good agreement with ozonesonde stations (Figure 4). The manuscript presents a succinct summary of a considerable body of work – running trajectories at multiple levels for

52,000 ozonesonde profiles. The paper is strongest in summarizing the advantages the resulting data product yields, but remains less convincing in the utility of this data product for trend studies. The good news is that with the trajectories run, re-tagging parcels in the model is relatively easy. Depending on the answers to a few of my queries below, that may be necessary. I also provide a few suggested avenues the authors may or may not choose to pursue in the revision of this work before its publication in ACP.

Major concerns:

1) The authors use the WOUDC database for ozonesonde profiles. Many of the stations impose a correction factor on the ozone concentrations throughout the profile, as indicated by data in the file headers. It is not clear that these correction factors should be applied to the profile, a point to which the authors refer themselves. Please see the work of (Morris et al., 2013) in ACP that discusses the impact of the correction factor on the Japanese sounding data. My suggestion would be to remove all of the correction factors from the tropospheric portions of the ozone profiles to identify what (if any) impact you find on the resulting data products. The particular problem in the Japanese data which you may or may not find elsewhere was an unexplained trend in the correction factors themselves.

2) The authors reference different outcomes with respect to their own trend analysis than found in other studies, which they cite. However, they present only a series of maps and one figure with no real statistical analysis in the content of the paper to show the trends (Figure 10 is the only attempt, and it contains no error bars). Perhaps histograms of the differences are worthwhile. Given the change in geographic extent of the ozonesonde network over the last 40 years, I am not sure what to make of the global ozone mean changes – in particular, the addition of the SHADOZ network data in the 1990's greatly increased coverage in the tropics over previous decades. I think the trend analysis could be an important part of this paper, but it is not nearly thorough enough at this point. I would advise either greatly expanding and deepening this analysis or removing it from the paper. If the authors choose the former, this paper

becomes more important, but they need to not only improve the statistical rigor of their analyses, they also need to show how their analyses result in differences with prior analyses. How do the zonal means change? How do the zonal means compare with prior analyses? What do the trends look like as a function of altitude, both with the trajectory mapped data and with just the original profiles? Histograms would be helpful to the analyses. How do you correct for the sonde technology changes? How do you correct for changes in solution strengths and the corresponding impacts on ozone measurements?

Some detailed comments:

Page – line number

11474 – 5-6: I would revise this sentence to, "The number of stations results in a sparse geographical distribution."

11474 – 9: The lifetime is on the order of weeks in the free troposphere, a distinction the authors need to recognize here and throughout. In the boundary layer, especially near industrial centers, the lifetime will be much shorter. Also, convective events may result in rapid redistribution of ozone and lightning ozone creation either by coronal discharge (Minschwaner et al., 2008) or through photochemical production resulting from lightning NOx (e.g., (Cooper et al., 2006; Schumann and Huntrieser, 2007; Chameides et al., 1977; DeCaria et al., 2005; Morris et al., 2010))

11475 – 6: A few more references for human health impacts (Bell et al., 2004; Lippmann, 1991; McConnell et al., 2002)

11475 – 28: Include or reference a graph of the O3 lifetime as a function of altitude for the troposphere.

11476 – 7-8: You say "difficult to compute accurately," and I think you mean, "difficult to model accurately."

11477 – 3: See the (Morris et al., 2013) paper in ACP.

11479 – 18 – 23: Here's your reference to the correction factors and other potential factors to screen data. Other than reducing the number of profiles in the analysis, which the authors imply would have a negative impact, are there potentially positive outcomes from excluding any data based on any criteria?

11480 – 5-10: I had trouble following the math here from "Four days of both…" at 1 hr. time intervals…and getting to 32 additional ozone mixing ratio values. Could you clarify?

11480 – 10 – 12: You describe the gridding process here as "averaging" in 5X5 bins. How does the bin size affect the results? Did you try a different gridding technique (i.e., a $1/r$ or $1/r^2$ weighting of the data from the grid points)? How might those affect the results?

11481 – 29: "…less than 40% for almost all cases." That's not small. Again, a histogram showing the differences would be helpful in conjunction with Figure 2c. The histogram would be very helpful in characterizing the differences.

11483 – The spherical functions fitting….Personally, I'm a bit uncomfortable using functions to extrapolate data beyond locations in which it is taken or to which the trajectories take the air parcels. Essentially, your approach now does a double extrapolation, with the trajectories being the first step, then the spherical functions the second step. For example, in Figure 5 at 5.5 km, the spherical functions fill in the local maximum in O_3 between Africa and South America seen in other data sets. Yet looking at the corresponding panel to its left, there is very little data in that region at all.

11483 – 21 – 24: The differences you identify here are similar in magnitude to those in the forward/backward trajectory comparisons.

11483 – 11484: All of this analysis is done with the smoothed, extrapolated data. Convince me that I shouldn't be concerned about the extrapolation process…

11485 – 16 – 21: See my major comment about the trend analysis.

11487 – The NOAA Hysplit model requests the following reference: "The authors gratefully acknowledge the NOAA Air Resources Laboratory (ARL) for the provision of the HYSPLIT transport and dispersion model and/or READY website (http://ready.arl.noaa.gov) used in this publication." References are included in the list below (Rolph, 2013;Draxler and Rolph, 2013)

11498 – This figure shows well the power of the approach.

11507 – Would it not be fairer to only use the stations that existed throughout?

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