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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A global ozone climatology from ozone soundings via trajectory mapping: a stratospheric perspective" *by* J. Liu et al.

Anonymous Referee #3

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Review of Liu et al.: Ozone climatology

The paper describes a WOUDC sonde based ozone climatology, which is constructed by using backward trajectory mapping with NCEP/NCAR reanalysis data. It is available from 1960 to 2000 on a relatively coarse grid of 5 x 5 degrees with 1 km vertical spacing tup to 26 km altitude. It is compared with seasional and zonal mean satellite data (SAGE and OSIRIS) showing basically the same patterns. Evaluation has been done by comparing the results to 20 independent sonde profiles, which are not included in the respective climatology for evaluation. An in-situ based sonde climatology is of large value for the community, since it provides consistent information over the whole

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column. However, as the authors stated, there are data bases around of which climatologies have been produced, which include sonde data as well. It is not clear to me, what the advantage of this data set over other data bases is. The authors should better evaluate the potential error, which is introduced by the vertical motion of air parcels following trajectories in the stratosphere. Another open question is the analysis of free tropospheric ozone, where satellites have restrictions due to clouds, the stratospheric column and vertical resolution. Why do the authors not stronger emphasize the tropospheric data? Does the method give valid data there? Therefore I recommend the paper for publication after the following points have been addressed.

Major: The authors should clearly point out, what the advantage of this data set over existing data bases and climatologies is. Particularly their method, which includes stratospheric vertical motion on the basis of reanalysis data should be evaluated more in detail. What is the uncertainty which is introduced by the vertical motion in particular in stratosphere, where vertical ozone gradients are large and vertical motions from reanalysis data have a large error? Does the climatology cover the seasonality of polar ozone correct? Seasonally resolved plots of vertical cross sections would be interesting here. Also a quantification of the error introduced by the vertical wind would be useful, (by e.g. showing the variance).

Specific: p.16839: What is the resolution of the driving NCEP reanalysis data to construct the trajectories? Which time step was used for the trajectory calculation? Does HYSPLIT use the 3-D kinematic wind fields for vertical motions or are diabatic heating rates used? This is particularly important for the bias in Fig.7, which is large over elevated terrain. Is there any physical motivation to use a 4 day period for the calculation?

p.16840, l-5: What kind of tropopause is used? Dynamical or WMO? If WMO, how are the tropopause breaks included?

l.16844, l.1-5: If the terrain induced vertical motion were responsible for the large bias over mountains, why is no effect evident over the two Americas? This could be eventu-

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ally checked by comparing mean vertical velocities of the trajectories (or the variance of vertical velocity).

The authors should include a refernce to the work by Hassler et al., ACP, 2013 (which was not yet available in the final version, when this manuscript was published)

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16831, 2013.