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very interesting paper, some minor corrections are needed

Mark Weber (Referee)

Referee comment on "Effects of reanalysis forcing fields on ozone trends and age of air from a chemical transport model" by Yajuan Li et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-182-RC2, 2022

This paper reports on ozone results from TOMCAT chemistry-transport model (CTM) runs using two different meteorological ECMWF reanalyses as input, ERA5 and its predecessor ERAI. While agreement of model runs driven by ERA5 appear to agree better with total ozone observations, biases with resepect to observations are larger in ozone profile data for ERA5 driven TOMCAT than for ERAI driven. In additon recent ozone trends in total columns and stratospheric ozone differ depending on the renalsys data used. Their main conclusion is that the current ERA5 reanalysis is not able to reproduce in CTMs the observed ozone changes, in particular, in the lower stratosphere.

The topic of this paper is very relevant and is within the scope of ACP. Recent stratospheric ozone changes are governed by changes in ozone-depleting substances (chemical contribution) and circulation/transport (dynamical contribution), the latter strongly influenced by changes in greenhouse gases. In particular, the differences in the reanalyses mainly affect the circulation pattern in the model and ozone transport. Both CTMs and trend regression models applied to observations rely on input from meteorological analyses (forcing and proxies) in order to separate the dynamical and chemical part of the overall ozone trends. Uncertainties in the reanalyses therefore can affect trend estimates derived from CTMs and, possibly, observations.

I recommend publication in ACP after addressing the following points.

l.138: The sentence "Sofieva et al. and Steinbrecht et al. ..." does not belong here as they report on profile trends not total column trends.

l.281: The table caption is confusing (what is the meaning of "..based on ..."). What is shown here are the correlations (r2) between the regression model and the data timeseries.

Eq. 1 (and Fig. 5): I would suggest to use ILT (independent linear trends) rather than PWLT. PWLT are quite sensitive to the turning point (time of maximum of stratospheric halogen content) being quite close to the Pinatubo period. It could be that differences between the models and observations could become more pronounced with the ILT approach.

l. 362: here the large differences in temperatures between A_ERAI and B_ERA5 before 1998 is seen as a proof that ERA5 upper stratospheric temperatures are improved (due to some corrections in the assimilation of MLS). Has this been validated? A comparison of ERAI/ERA5 with MLS or other temperatures could show this.

l.368: "Thus, the differences in the upper stratospheric temperatures from the reanalysis data sets drive the differences in ozone anomalies in this region." Please explain why this is the case, e.g. cooler (warmer) temperatures produce more (less) ozone.

l.482: "The increasing AoA in B_ERA5 after 1998 as well as the older age in the NH lower stratosphere, suggest that other transport pathways (such as downward transport/reduced transport in the troposphere) might have been responsible for the increasing ozone in the NH extratropical lower stratosphere in B_ERA5". It should be mentioned that the aging of AoA in the NH appears, however, consistent with SWOOSH (observational) trends and the notion of reduced downward transport. Could this mean that there is some model issue here (uncertainties in transport patterns in the model). Please discuss.