

## ***Interactive comment on “The origin of ozone” by V. Grewe***

### **Anonymous Referee #1**

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I appreciate the author’s extensive reply, which contain valuable information. Especially the description of the upper boundary conditions should be added in the manuscript.

The author state that ozone does not have an upper boundary flux. Since in the real atmosphere a flux at 10 hPa does exists, the author admits there is a boundary issue. Generally this artifact is “solved” by tuning of the wave dissipation. Therefore the upper boundary is linked to the dissipation strength and can thus not be separated from each other, as the author does on page S4200.

Concerning the described evaluations, I think the author should be careful. The NCEP data can no longer be used for evaluating the stratosphere (Manney et al., 2005). The NCEP variability of meteorological key parameters (wind and temperature) in the stratosphere seem to be too poor. This especially refers to the Steinbrecht et al. (2005)

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paper. I also see significant differences between both ECHAM versions in this paper, in contrast to the impression of the author. Moreover, total ozone variability is different from the quantity of discussion here (see further below). I see no validation of the model in Dameris et al. (2005), although the author gives this impression. Further, Austin et al. (2003) emphasized qualitative issues (as stated on page S4200). Patterns and shapes may look reasonable for some seasons (solstice for example), but this is different from the circulation strength (quantitative). In fact Austin is critical in this respect, given his following up paper in ACP on uncertainties in CCM calculations. The author mentions Gauss et al. (2005) as another example of detailed validation of E39/C. However, there is only one validation figure (figure 1b) with the Fortuin and Kelder (1998) climatology, which is not a detailed validation. Moreover, even in this figure EC39/C shows about 20% off close to the maximum ozone production region.

For determining the chemical ozone production the air parcel lifetimes in the region of interest is vital. When accumulation this quantity, this parameter becomes very critical, since errors in the residence times will accumulate as well. Thus integrated chemical ozone production in localized is very sensitive to errors in the dynamic residence times. This sensitivity separates the issue from for example ozone variability in total ozone as well as for ozone profile comparisons (which are instantaneous and buffered). The author is well aware that the strongest ozone production region is right in the region where E39/C has its upper boundary. This fact strongly suspects the quality of the dynamic residence times, as the author agrees, and thus to the quality of the variable of concern in this manuscript, namely integrated chemical ozone production (and loss).

I'm not disqualifying the E39/C results, but to my opinion the arguments require an additional calculation with a model that at least includes the most important chemical ozone production region. The manuscript title is too ambitious to ignore this recommendation. Further, the papers mentioned as examples for detailed model validation do not convince me; they show significant differences with MAECHAM and observations or do not contain validation at all. I therefore remain to my previous recommendation not to

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publish the manuscript in its current form and encourage the author to perform a similar calculation with an newer version of ECHAM, just as in Steinbrecht et al. (2005).

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9641, 2005.

**ACPD**

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