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Interactive comment on "The origin of ozone" *by* V. Grewe

Anonymous Referee #1

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Major comments

I find the subject quite fascinating, but the main problem I have is the applied modelling tool. The model contains a top at 10 hPa, meaning an upper model layer between 20 - 0 hPa. As a result the dynamical variability within this layer is strongly filtered, affecting the dynamical turnover time. In principle, this may not necessarily be a problem for the mean state depending on the applied boundary conditions, but it will certainly be a problem if production and loss rates are determined for different tracers for which the air parcel residence times are crucial. With other words, the boundary conditions mask the mean state of the tracers and compensate for model errors to create "realistic" distributions. However, errors in the turnover time accumulate when determining quantities like for example the amount of ozone production. Ozone is in any case a problem because the production rates become very important within the model top layer. I em-

phasize that the physics within this layer in principle contain large wave damping which is inevitable. Hence, it cannot be avoided that the dynamical turnover times have subsequent errors. Obviously, as is no surprise, the Brewer-Dobson circulation is strongly affected, since it extends well beyond 10 hPa. I therefore do not agree with the statement in the discussion section (lines 6-90). A little further in this section you correctly state some of the shortcomings mentioned above, followed by the statement that the turnover times are realistically represented, referring to section 4. However, in section 4 the summertime decline of total ozone is discussed with observations is from Fioletov and Shepherd (2003). This addresses the photochemical relaxation rather than the complete stratospheric turnover times. The discussion on page 9653, lines 16-21: if the transport is too fast, it is not straightforward to conclude that the model calculates an upper limit of the contribution to the extra-tropics. It also implies that the residence time in the chemical production region is too short.

This shortcoming is a fundamental one based on physical arguments and I regret that I'm forced recommending not to publish this manuscript in its current version.

However, I do like the model study and I advise the author to tackle this problem by performing a similar exercise with ECHAM5 or a similar climate model covering both the active ozone chemistry regions and the Brewer Dobson circulation more properly and resubmit the work. This extension will create confidence and it will give information on the robustness of the results.

Minor comments

The author states that ozone is chemically controlled between 10 - 30 hPa. I'm not sure if this is true. The time scales for chemical production is of the order of 10-100 days, and the chemical destruction 25-100 days, which is substantially long. I believe the Brasseur and Solomon (1986) have been updated considerably in the mean time.

I'm also not sure about the statement that between 100- 60 hPa ozone is equally dominated by chemistry and transport in the tropics. The time scales for the ozone loss

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rates are very long (a few years), so that transport completely dominates, while the production time scales or of the order or 200 days. But for 100 hPa this will mean dominated by transport too.

Page 9651, Lines 13-14 Additionally and perhaps even more important, in the tropics there is no significant influx from the stratosphere.

Technical:

Abstract: tropopause? I guess you mean troposphere.

Figure 7: I don't see a black line in Figure7. What is "OBS-DJF"?

I further recommend to enlarge especially figure 4 (bur also figure 5), since the labels are too small. I advise to use other colors and thicker lines in figure 5.

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