

Interactive comment on “An exploration of ozone changes and their radiative forcing prior to the chlorofluorocarbon era” by D. T. Shindell and G. Faluvegi

Anonymous Referee #1

Received and published: 10 October 2002

Review report manuscript No. ACP2002-49, by D.T. Shindel and G. Faluvegi

General comment I find the subject original and of great interest. It gives insight into the relative changes in total ozone of the troposphere and stratosphere over a larger time period than up to now has been considered. The importance of good quality data sets is obviously crucial. The limited amount of available data should in principle not prevent this new original approach, unless the overall uncertainty would be much too large to make any quantitative conclusion. The assumptions made, including different scaling, gives room for discussion whether the overall uncertainty would indeed be too large, despite the justified authors comments in the manuscript that the confidence of the estimates is low.

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Nevertheless, this exploratory study is acceptable for publication in ACP, after the items below have been addressed.

Minor comments

Section 2, last paragraph You mention two other sparse ozone data sets, but do not describe them here as you do with the other data sets. I think it would be better to explain in this section what these data sets contain.

Section 3, third paragraph Uncertainty in ozone trends from water vapor is the largest term, but is based only on 20 km altitude observations, since here water vapor has the largest impact on ozone. However, what about the upper (and to a lesser extend the middle) stratosphere? What would be the effect by using the same uncertainty for the whole stratosphere?

The effect of temperature decrease on ozone is relatively small, and shows a negative value. Several other studies show ozone increase due to cooling, suggesting that the active gas phase chemistry regimes (mainly the tropical region) dominate the overall response on ozone, despite enhanced polar ozone loss. Could you comment on these opposite results and if this may have consequences for the use of the parameterized chemistry scheme in your model?

This also implies that transport effects are crucial for the chemical sensitivity, which affects for example the ozone change due to water vapor changes on a global scale. Given the coarse resolution of your model, could you comment how the GCM represents the average turnover time in the stratosphere and what effect it has on the chemical sensitivity in your long-term integrations? In this respect there might be an inconsistency in your model integrations by prescribing water vapor fields and neglecting circulation changes.

Section 5 Climate impacts and conclusions

In the second paragraph you state that the calculated tropospheric forcing is similar to

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that calculated in other models. In the third paragraph you state that the tropospheric ozone forcing is much larger than other estimates. This is somewhat confusing. Could you re-write these paragraphs to make the distinction more clear?

I would further suggest a separate section about climate impact, and perhaps change the conclusions into Summary and Conclusions to make it more readable.

Appendix Could you make a clear overview in one Table of NH and SH stratospheric and tropospheric column changes calculated by the model and observed (1957-1975)? For example, it is not very clear to derive the calculated (northern hemispheric) tropospheric ozone increase in the manuscript: (-8.3 + 7.7 or -7.2 + 7.7 or -8.3 + 8.2 or -7.2 + 8.2?) However, independent from which numbers are taken, the sum seems significantly lower than those given in Table 4. Does this imply that the calculated radiative tropospheric forcing, which seems already high, may even be underestimated? I may misinterpret these results, but it would help if you summarize this more clearly.

Bram Bregman, De Bilt, 10 October, 2002

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 1371, 2002.

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