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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Attribution of stratospheric ozone trends to chemistry and transport: a modelling study" *by* G. Kiesewetter et al.

Anonymous Referee #5

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This paper addresses the topic of understanding changes in stratospheric ozone over the past 3 decades. The authors use a 3D model to attribute the changes to different causes. This is an important issue and highly relevant for ACP. Over the past decade or so (as cited in the paper) a number of studies have attempted to quantify various factors which contribute to ozone changes. The remaining challenge is to improve this quantification.

This paper contains interesting experiments, which could make a useful contribution to this issue, but I do not feel the paper merits publication as it is. I think that this paper is not clear enough about which processes are in the different runs (and therefore what

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Interactive Discussion

Discussion Paper

are the key driving factors for the ozone changes) and the paper is too long. There is also alot of mention of agreeing with past studies, without real emphasis on what is new.

Specific comments

- Details of Linoz scheme. Please give details of the chemistry used in the model which constructs the Linoz coefficients. Depending on this, there are a number of issues which need to be addressed. I presume this Linoz model *does* include mid-latitude heterogeneous chemistry (e.g. $N_2O_5 + H_2O$). If not then the whole balance of NOx/ClOx in the lower stratosphere will be wrong (and I don't see how you can look at the chemical trend). If Linoz does include het. chem. then the runs which change the Linoz coefficients cannot be described as 'gas-phase chemistry'. (Please also say what Linoz does in the polar region - are there any inconsistencies with adding in the additional polar ozone loss term?). You also need to say what changes between 1978, 2000 and 2010. Obviously Cl and Br source gases, but also N_2O and CH_4? In that case you need to be careful when analyzing these runs that you don't ascribe differences from Linoz to just 'halogens'. I noted that you use 'ODS' - N_2O would qualify as that but CH_4 is not so clear. In any case most people would take ODS to be halogens. Use of the word recovery would open a debate if the runs were not just changing halogen source gases. Finally, I assume that aerosols (if they are in the Linoz model) are constant between these dates (presumably at background levels)?

- Chemical ozone loss following volcanic eruptions. In the past many studies have looked at the role of heterogeneous activation on enhanced aerosol has played on mid-latitude ozone loss. This study really ignores this issue. Reference is made to the dynamical effect of aerosols but not the chemical effect. The implication is that mid-latitude aerosol chemistry is not needed to explain the ozone changes? You need to be clear on this.

- Generally the paper is well written. However, it is long (i.e. alot of text is often used

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to describe figures or refer to other parts of the paper) and there are a number of awkwardly constructed sentences. The native English-speaking co-author can ensure these are tidied up.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17491, 2010.

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