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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantitative performance metrics for stratospheric-resolving chemistry-climate models" *by* D. W. Waugh and V. Eyring

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The intent of the paper by Waugh and Eyring is to provide quantitative measures of performance (grades) of CCMs. Such measures will be very valuable for the assessment of CCM results, however I think, the proposed method can be improved in several ways.

First, I agree with the comment by V. Grewe that it should be clarified what grade can be considered an optimal model performance. It must reproduce the observations within the natural variability. This could be done by comparing one model year of a so-called time slice experiment as pseudo observations with the other years.

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Second, the choice of diagnostics and their corresponding weights w_j used to calculate a single performance index is of course subjective to a certain extent. The choice of diagnostics and weights should however depend on what the models are aiming to predict. In this study it is said that the models aim to predict the future of stratospheric ozone.

The authors use the model grades to show a model-weighted prediction of ozone. Waugh and Eyring point out, that their choice of the individual weights w_j (based on the transport grades) is for illustrative purposes only. However, for the illustration of an ozone prediction into the future it is necessary to suggest a method, how the individual weights w_j should be best defined. Including such a method would provide an important guidance also how to construct weights w_j that are designed for the prediction of any other diagnostics. It would be desirable for ozone predictions to include with a high weighting w_j diagnostic tests that show the models skill to simulate ozone besides the measures of dynamics. For example, polar Cl_y is one precursor of polar ozone loss that is mentioned in the paper. A correct simulation of Cl_y alone does not tell whether the chemistry is implemented correctly. The most obvious parameter for a diagnostic test would be ozone itself. If the aim of a model prediction is a statement about ozone recovery in the future, the most weighted diagnostic test must be the models' ability to reproduce past ozone time series.

Waugh and Eyring refrain from using an ozone based diagnostic test. But in figure 8, minimum Antarctic ozone, NH mid-latitude total ozone anomalies and their model-grade weighted mean prediction are shown. In a recent paper by Müller et al. (Atmos. Chem. Phys., 8, 251-264, 2008), it was suggested that polar minimum ozone column should no longer be used when analyzing polar ozone loss, because in the Arctic, the minimum ozone occurs frequently outside the vortex. The latter issue is not relevant for the current Antarctic conditions of strong ozone loss. However, it might become important for future polar ozone levels, when there will be less chlorine and less ozone loss. I suggest to add an ozone based diagnostic as proposed by Müller et al. or if this

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is not easily producable a quantity that is based on daily averages (e.g. like the average daily ozone mass deficit shown in fig 6-14a of the WMO 2006 ozone assessment).

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