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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Antarctic Ozone Depletion between 1960 and 1980 in Observations and Chemistry-Climate Model Simulations" *by* Ulrike Langematz et al.

Anonymous Referee #1

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General Comment: This paper examines the Antarctic ozone depletion between 1960 and 1980 in both observations and 17 Chemistry Climate Models (CCMs) based on the REF-B1 scenario from CCMVal2. These models derive an anthropogenic depletion from 1960-1989 between 26.4% and 49.8% of the total period (1960-2000). Observations over the same period suggest a higher depletion of 56%. The paper is clearly written, concise, and adds to the scientific understanding of what the return date choice for "full recovery" implies. I recommend this paper be published after minor revisions (see below).

Specific comments.

Page 3 lines 3-4 All the models certainly do show a 1960-1980 depletion (26.4-49.8%),

with approximately six model showing values less than $\sim$ 35%. For these 17 models there was large effort to understand how well these models represented transport, dynamics, and chemistry (i.e., the SPARC Report on the Evaluation of Chemistry-Climate Models, 2010). That is, the range in models is not just due to different dynamical variability. It would be interesting to highlight the models that did better in these process oriented diagnostics in Table 2. This was the approach used in Chapter 2 of the 2010 WMO assessment.

You suggest the the temperature trend in the 1960-1980 period was different in observations relative to most models (Figure 5). Are there any other issues with the models that could explain the lower depletion in this period? E.g., the CCMs used in this study also did not include additional very-short lived bromine (VSL) species. This addition 5-7 pptv of inorganic bromine should contribute to the underestimate the total loss in the 1960-1980 period. It will be interesting if you (not for this paper) redo this analysis for the CCMI models that include this additional VSL bromine source.

Page 3, equation 1. The authors did a very nice job of explaining the approach of determining the degree of halogen-induced ozone for the 1960-1980 period. Question: the temperature anomaly at 100hPa is used in the regression fit to address dynamical variability. What equation (1) does not address is the sensitivity of the ozone chemistry in the model to absolute biases in temperature. It therefore would be very informative to show the lower polar stratosphere absolution temperature evolution similar to Figure 4. E.g., if two models both show a similar representation of ESC (i.e., consistent transport/dynamics) and a similar absolute temperature trend (new figure), and would happen to have a different temperature trend vs ozone trend sensitivity (Figure 5) – I believe this would highlight issues in the chemistry representation between the two models. Generally, it would be nice to comment on how this technique could be used to evaluate model components.

Page 4. The discussion of how the observations are combined are in reasonably detailed. Based on this discussion and use of equation 1 (and 2) this work suggests a

decline of 56.4 +-6.8%. Maybe I missed it, but how did you come up with an uncertainty value for this decline (i.e., +-6.8%)? Also, since the results are for SON mean total ozone polar cap average (e.g., Figure 2) – are you masking the model results for periods that are in the dark with little ozone depletion (e.g., high latitudes in September)? That is, are you treating the model and observations derivation of the 1960-1980 decline in a consistent manner?

Figure 2 caption is missing the prime symbol in “c T”.

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