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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying uncertainty in projections of stratospheric ozone over the 21st century" *by* A. J. Charlton-Perez et al.

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Response to Reviewer 1: Quantifying uncertainty in projections of stratospheric ozone over the 21st century. A. J. Charlton-Perez, E. Hawkins, V. Eyring, I. Cionni, G.E. Bodeker, D. E. Kinnison, H. Akiyoshi, S. M. Frith, R. Garcia, A. Gettelman, J. F. Lamarque, T. Nakamura, S. Pawson, Y. Yamashita, S. Bekki, P. Braesicke, M. Chipperfield, S. Dhomse, M. Marchand, E. Mancini, O. Morgenstern, G. Pitari, D. Plummer, J.A. Pyle, E. Rozanov, J. Scinocca, K. Shibata, T.G. Shepherd, W. Tian, and D.W. Waugh

We thank the reviewer for their helpful comments, which we address as follows:

- We have added a line to page five to describe CAM3.5

- In this instance we have confused the reviewer. Our intention here is to indicate variation in estimates of the internal variability which is dominated by the smaller area and averaging period. The reviewer is correct to point out that internal variability is larger at the poles, but if we instead averaged for three month periods and to 50 N/S then our estimates of the true internal variability would be more stable even in a very variable region. We have tried to slightly reword the sentence in question to make it clearer.

  On the second point, we do not have a simple hypothesis for why the internal variability might be higher during this period. However, since the section is designed to discuss the validity of our constant internal variability approximation then we do not think it appropriate to investigate the cause further.

- Added a comment to this effect.

- The reviewer is correct that the black line shows the total uncertainty (i.e. that from all three sources) but this line also shows standard deviation so the caption is correct. Section 3.1 describes this in detail.

- Thankyou for pointing this out, we made a comment to address this point.

- Our reference to Oman is the problem here, we simply wanted to point to the previous work on this topic but ended up confusing the issue. To avoid this conclusion we omit the reference. This has the added advantage that the next paragraph flows more naturally.

- The sentence has been removed in response to the above comment.

- We were unable to obtain the reference.

- Thanks to the reviewer for pointing out our error here. We have redrafted this section to link the model uncertainties in the Arctic and Antarctic more clearly.

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Our hypothesis that ODS reductions are responsible for the decrease in Antarctic model uncertainty could still hold if an additional growth of uncertainty related to model GHG sensitivity becomes important around 2050 (rather like what we imagine in the Arctic).

- The reviewer misunderstands our point here, we are simply trying to point out that where model uncertainty is large, this may increase the variability of our estimate of scenario uncertainty since it relies on single model in the case of CAM3.5. On reflection the description of the relationship between individual models and the REF-B2 multi-model mean is confusing and so we remove this line.

- We agree with the reviewer that the language used in this section is rather loose and we should clarify it. In a related study, Hawkins and Sutton state that, 'To assess the significance of the S/N, we consider the null hypothesis that the signal is zero. This null hypothesis can be rejected in favour of the hypothesis that the signal is non-zero at around the 8% significance level for a S/N of 2, using a t test.'

  This is the case for 9 degrees of freedom (i.e. a decadal mean) as also shown in our figure 4. It therefore makes sense for us to delete the comments about robustness and statistical significance and simply use the word detectable.

- We agree that this was confusing, we simply mean total uncertainty.

- The fractional uncertainties in this figure are slightly different from those which could be estimated directly from Fig. 2 (although of course are related). As we describe in the text, we imagine that in practice the return date will be calculated 'post-hoc'. In this case, because the internal variability in ozone has some year-to-year auto-correlation it is possible that linked year-to-year variations could result in uncertainty in the return date estimate. Therefore, it is wise to simply try to replicate the procedure of actually determining the post-hoc return date. In

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simple terms, it is not enough to simply imagine projecting the wedges in Fig. 2 on to the zero line to get an estimate of the fractional uncertainty in return date.

- We agree, and have added some text on p23, line 16 to clarify this.

Typographical corrections

- Fixed
- Fixed
- Caption simplified
- Fixed

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