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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ozone sensitivity to varying greenhouse gases and ozone-depleting substances in CCMI simulations" *by* Olaf Morgenstern et al.

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(In the below, our responses are in bold.)

This paper outlines a series of CCMI simulations carried out by several chemistry-climate models. The effects of CH_4, N_2O, equivalent Cl, and equivalent CO_2 on O_3 are presented in the profile, total column, and at the surface. The paper is clearly written but the repetitive organization and lack of new insights make it a slow read. More significantly, there is little attempt to explain the underlying causes of model differences presented. It is hypothesized in several places that different stratospheric transport and dynamical responses between the models are the cause of most of the differences.

However, this is not diagnosed and the reader is left wondering what to conclude from this study (see below). An evaluation of the dynamical feedbacks between the models would help immensely. Some detailed exploration of the cause of peculiar behaviour for some of the largest model outliers would also greatly help the paper. Even some speculative remarks about the causes of specific outliers would add value to the paper. I cannot recommend publication of this paper without at least some attempt to explain the differences between the models.

We thank the reviewer for these thoughtful comments. The "repetitive organization" was deliberate; the idea is to apply the same methodology to the four different forcings. The purpose of the paper is partly to inform the model PIs about how their models compare to others; hence the encyclopaedic approach. Completely diagnosing where the differences in model behaviour come from is beyond the scope of the paper. We are however now presenting an analysis of how age-of-air responds to the different forcings. Age is a much easier diagnostic than ozone because it only responds to transport. For CH_4 and Cl^{eq}, there are some qualitative inconsistencies in the responses which require further in-depth investigation.

General Comments

As stated above the lack of an attempt to explain the discrepancies between the models diminishes this paper. Given that representatives of all the modeling groups are co-authors of the paper, they should diagnose some select outliers in the simulations.

In at least one case, this has happened. We now present a "fixed-CO_2" simulation produced by CESM1-WACCM. In this model, the original analysis had indicated that CESM1-WACCM does not exhibit decreasing tropical total-column ozone in response to increasing CO_2^{eq}. However, this analysis had been based on the fGHG simulations, with the effects of changes in CH_4 and N_2O added on subsequently. The new simulation shows that CESM1-WACCM does indeed have

decreasing TCO in the tropics in response to increasing CO_2, but the response is weaker than in most other models. The discrepancy between the two results points at a limitation of the linear analysis conducted here.

As the paper presently stands, one must conclude one (or more?) of the following possibilities:

- Our chemical/dynamical understanding is incomplete (except in the middle stratosphere).

  Our impression is that it is more our understanding of dynamics, as reflected in the model formulations, that is to blame. The relatively consistent response e.g. to N_2O and Cl^{eq} suggests that chemistry appears to be relatively well understood and simulated consistently.

- These models include significant differences in their treatment of the chemistry, which induce different responses on ozone from the source gases.

  To some extent that may be the case, but fundamentally the consistent response of ozone in the middle stratosphere, where ozone is dominated by gas-phase chemistry, does confirm that chemistry appears to be relatively consistent across the models. That would not be a surprise, given that kinetics information is well established and available, as are methods to integrate the kinetics equations.

- There are errors in some of the models.

  That cannot be ruled out, based on the new analysis of age-of-air. The analysis shows qualitative differences in behaviour within the CCMI ensemble that might indicate model formulation errors.

- Dynamical variability is larger than the chemical effect of the source gas changes on ozone.

  Our analysis finds a lot of statistically significant signals, so we think dynamical variability is unlikely to blame here. Also the consistent responses e.g. of the ACCESS-CCM and NIWA-UKCA models (two largely identical models producing different dynamical variations) suggest that dynamical variability does not dominate the results, at least not in these cases.

- Differences in dynamical feedbacks are larger than the chemical signal on ozone.

  A more complete discussion of this is needed in the paper. The paper only mentions the last possibility with no analysis to support it. The relatively small regions that are eliminated by being outside the 95% confidence interval suggest that pure dynamical variability is not the cause of the differences (or at least that such dynamical variability is auto-correlated on the timescale of a few years or longer and thus is included in the forced response). This is a bit surprising and so I'm curious how you computed the significance regions. Including an assessment of the dynamical feedbacks between the models is needed to support the assertion that these feedbacks are the likely cause of the model differences. The differences in the chemistry could be isolated by comparing results of simulations nudged to reanalysis output but this is likely beyond the scope of this paper unless such calculations exist in the CCMI archive.

  Qualitative and quantitative differences in dynamical feedbacks indeed exist; there now is a new section highlighting this for the age-of-air diagnostic. Significance is calculated using a standard approach, see text. The trend calculation indeed assumes that the remainder ϵ in the regression analysis (equation 1) consists of "white noise", so autocorrelation can be assumed 0. We have tested this assumption and have mostly found this to be the case, with some notable exceptions which may point to limitations in the linear regression conducted here. Unfortunately the nudged simulations in CCMI-1 (which do exist for some of the models) are of limited use here because these simulations do not explore the sensitivities to varying

long-lived gaseous forcings, and also because they are too short (only covering 1980-2010) and follow different scenarios. A comparison is of course possible, but we agree this needs to be the subject of a separate study.

Other Comments

1. It would be useful to include the profile plots in density units in the supplement (i.e., convert to DU/km per source gas change or another similar unit). Then one could clearly see where the column changes are coming from.

   Done. We now include four such plots in the supplement.

2. Include somewhere the formulas used to compute the significance values used on the figures. This should include the assumptions made in arriving at the formulas. This could be in a methods section, appendix, or supplement.

   This is now spelt out in detail in the appendix.

3. Section 3.4: I wonder if it would be better to use CO_2 directly instead of equivalent CO_2 since CO_2 dominates the radiative effects of these gases in the stratosphere (as you note in lines 94-107).

   We have tried this alternative and generally find no substantial difference. We maintain that CO_2^e is a more useful measure to use here because the SEN-C2-fGHG experiment is defined in terms of keeping all non-ODS GHGs constant, not just CO_2. Therefore CO_2^e reflects more accurately what that does to radiative forcing. This is of course still a simplification because various subsets of the gases making up the RCP scenarios are actually considered in the models' radiation schemes, and also models use or do not use lumping to account for gases not modelled explicitly. The impact of these considerations on the results presented here is small, though.

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