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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

Interactive comment on "Attribution of observed changes in stratospheric ozone and temperature" *by* N. P. Gillett et al.

Anonymous Referee #2

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This paper attempts to attribute changes in ozone and temperature over the recent past. Forcings considered are: changes in ozone depleting substances (ODSs), greenhouse gas concentrations (GHG), and natural variability (NAT). The study uses some observational and CCMVal-2 model data. The topic is relevant and the paper is suitable for ACP after some revisions detailed below.

As with many attribution papers the results are somehow expected, assuming that our underlying physical understanding wasn't wrong in the first place. Therefore this paper serves the useful purpose of providing confidence in our physical understanding.

Unfortunately, attribution is sometimes also used a little bit like a smoke screen: The reader cannot always be quite sure what is actually attributed. Part of the problem lies in the definition of the model runs used (this is not meant as a criticism; the model

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runs performed within CCMVal-2 were a compromise between what could be achieved with the existing models and what was required for the WMO report); but part of the problem lies also with insufficient information provided by the authors. In particular it never becomes quite clear what the GHG attribution is. Some attempt to describe the process is made, but unfortunately even after carefully reading the paper twice, I am still not quite sure: Does the GHG term just include the anthropogenic CO2 increase, or are other gases (like N2O and CH4) considered as well? As the authors say, ODSs are greenhouse gases as well. Do I understand correctly that the GHG part of the ODSs is neglected? I think it is necessary to precisely define the individual forcings in equation 1 (please see below) and to explain clearer what the models have done! Some of my comments below will hopefully help to guide the authors to areas where I believe more (specific) information is required to clarify their results.

P17344, l6: The Shine et al. study focused mainly on the temperature trend that could be explained with the observed ozone changes versus CO2 increases (and mentioned H2O as well); it did not directly evaluate the impact of ODSs. Misunderstandings might happen, and this distinction should be clearly made throughout the paper.

P17344, p21: The fact that cooling in the upper stratosphere locally increases ozone is not exactly a recent finding only available from the CMAM model (similar is true for the effect of increased upwelling).

P17345, p8: This is exactly the important point and should be elaborated on: “ozone or ODSs”; or “ozone change due to ODSs” (see above).

P17345-17347: The first part of section 2 requires a thorough rewrite. It is confusing and repetitive. The explanation of “ensemble sizes of one” seems unnecessary, the experiments (Ref-B1, etc.) should be summarised shortly, before the differences of experiments are referred to. The authors seem to feel as well that the first part is confusing, and provide a repetitive summary (p17347, l2). Model specific differences should go at the end, to avoid confusing the reader with information overkill.

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P17347, l19: Inflating the variance might require some more explanation, even though it is only a 7% effect.

P17349, l2: I am slightly puzzled by the choice of 3 year means, in particular when thinking about the natural variability (e.g. QBO). Please explain this choice.

P17349, l16: “forced” should be omitted.

P17349, l19: I assume the inflated variance is used? What is “close to one”? Some areas are much higher others slightly lower.

P17350, l2: Please explain the individual forcings – how are they constructed?

P17350, l17-19: This might also indicate an unlucky choice of “forcing term”.

P17350, l21: Please explain the light shaded bars in more detail; SCN-B2c has not been introduced yet.

P17351, l12-16: This statement is very interesting and important, and invites the question why the regions have not been chosen accordingly (I am also slightly puzzled how the stated region relates to the 20deg. regions mentioned in p1749, 27)?

P17352, l16: This sentence is problematic: How can you have a signal consistent in magnitude but not detectable? Presumably a signal has been detected (and is consistent in magnitude), but either the signal or the agreement between the signals (in observations and models) is not significant?

P17353, l3: Please clarify which data (CCMVal experiment) was used by Steinbrecht et al. in their study.

P17353, l17: Do we expect to be able to separate ODS and GHG? They are presumably not independent for the height region covered.

P17354: l7: The methodology should already clearly indicate which models are used when.

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P17361: Figure 1b and 1d should follow the example of Fig.2 and have the latitude as x-axis.

P17363: If Figure 3b is showing a trend the unit should be something over time; if Figure 3b is showing a difference (what the units seem to indicate) it should be clearly stated how the difference was defined.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17341, 2010.

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