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Atmospheric Chemistry and Physics Discussions

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

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Response to Referee #2

Thanks to the referee for the detailed and helpful review. We address all the points raised below and in our revised manuscript.

Since we wrote the ACPD manuscript, a new SCN-B2b simulation of UMUKCA-UCAM has become available. We therefore now include the REF-B1, REF-B2 and SCN-B2b simulations from UMUKCA-UCAM in our analysis. An SCN-B2c simulation of LMDZrepro has also now become available, so we also include this simulation in our analysis. Inclusion of the additional simulations from UMUKCA-UCAM has had only small effects on most of our results, with the exception of the detection analysis applied to

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total ozone trends, for which we now find no inconsistency between the simulated and observed response to greenhouse gas changes. We have revised the text accordingly.

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 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!

Thanks to the reviewer for these comments. We certainly do not intend to obscure anything using the attribution analysis, but rather to quantitatively test for consistency in simulated and observed responses, and to look for evidence in observations of a significant stratospheric response to greenhouse gas change.

ACPD
10, C9023–C9032, 2010

We have revised the paper to make it clearer how the GHG response is defined. GHGs refers mainly to the greenhouse gases carbon dioxide, methane and nitrous oxide. In response to this comment we now list these gases in the first sentence of the second paragraph of the Data and Models section. We also now list these gases in the caption to Table 1. This and other details of the forcings used in the CCMVal simulations are given in Chapter 2 of the CCMVal report to which we refer at the beginning of the Data and Models section.

We define the GHG response using the SCN-B2b simulation. Of the nine SCN-B2b simulations we now consider, four include the radiative effects of ODSs (3 CMAM simulations and UMUKCA-UCAM), and five do not (CCSRNIES, LMDZrepro, MRI, SOCOL, and WACCM). Thus the GHG response also includes roughly half of the radiative response to ODSs. In response to this comment, we compared the lower stratospheric temperature response in the two sets of simulations, and while the simulations including the radiative effects of ODSs do tend to warm more in this region, as expected (Forster and Joshi, 2005), this difference is not generally statistically significant. This is of course not an ideal situation: it would have been better if the CCMVal models had included a completely consistent set of forcings in all their simulations. But, as with all other studies using this output, we are constrained to use the simulations already carried out. We have added some text discussing this point:

The three CMAM simulations and the UMUKCA-UCAM simulation included the radiative effects of changes in ODSs in the SCN-B2b simulations, while the other models excluded them. A comparison of the lower stratospheric temperature trends in the SCN-B2b simulations with and without the radiative effects of ODSs indicated that simulations including these effects tended to warm more in the lower latitudes compared to those excluding them (Forster and Joshi, 2005), though the differences were not generally statistically significant. The GHG response thus includes roughly half the radiative response to ODSs, with the other half included with the calculated response to ODSs themselves.

ACPD

10, C9023–C9032, 2010

Interactive Comment

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.

We have changed this sentence to make it clear that Shine et al. (2003) and the other studies cited investigated the effects of ozone changes on lower stratospheric temperature, not the effects of ODS changes. In the other place where Shine et al. (2003) is cited, we now make clear that that study used simulations with prescribed ozone changes.

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).

Our focus here was on chemistry-climate model simulations of the response to greenhouse gas changes alone, of which these studies are the first that we are aware of. However, the reviewer is correct that the CO_2 effect on ozone has been known for a long time – we have now added a citation to Haigh and Pyle (1982) as an example of an earlier study showing this effect in a two-dimensional model.

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

We have added some more discussion on this point:

Higher in the stratosphere, greenhouse gases have played a more important role (e.g. Shine et al., 2003; Jonsson et al., 2009). There is a difference between partitioning the temperature trend into ozone-induced and GHG-induced changes, and partitioning it into ODS-induced and GHG-induced changes (Shepherd and Jonsson, 2008; Jonsson

ACPD
10, C9023–C9032, 2010

Interactive Comment

et al., 2009). The latter approach, which we follow here, leads to a smaller temperature change being attributed to greenhouse gases, because the GHG-cooling-induced increase in ozone concentration cancels out part of the cooling due to the GHGs themselves (Shepherd and Jonsson, 2008).

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.

We have considerably modified this section in response to this and other comments. We need to strike a balance here between making the section easy to read, and including the model specific information which is requested in the first comment.

The first sentence of section 2 already describes the main experiments considered (REF-B1, REF-B2 and SCN-B2b).

We now say 'a single ensemble member' instead of 'an ensemble size of one'. 'Ensemble' is the standard term to denote a set of simulations differing only in their initial conditions, therefore we prefer to retain this term.

As requested, we now summarise how the responses were derived first, and then give model-specific information and more detail on each of the responses. We have removed the summary from the end of the section concerned.

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

We require an estimate of internal variability here. We subtract the ensemble mean to remove the forced response. However, doing so also removes some of the internal variability. If we have N independent stationary timeseries each with variance V, and

ACPD

10, C9023–C9032, 2010

Interactive Comment

we calculate the mean of them, it will have expected variance V/N. If we subtract the ensemble mean from each timeseries, each will have expected variance $V - V/N = (N-1)V/N$. Thus, in order to match the variance to that of the original timeseries we would have to multiply each by $\sqrt{N/(N-1)}$. We already cite another paper which applied the same correction (Stone et al., 2007). We prefer not to go into a lot of detail on this in the manuscript, but to motivate this variance-inflation we have added the sentence:

The variance-inflation step is necessary because subtracting an ensemble mean removes some of the internal variability as well as the forced response.

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.

We chose 3-yr means in order to average over as much internal variability as possible, without obscuring the volcanic response. The QBO is not treated consistently across the models we use (four prescribe it, one has an internally generated QBO, and two have no QBO, as noted in the caption to Table 1), therefore we do not particularly want to retain QBO-related variability in the analysis. 3 is a factor of 27 (the number of years in our analysis) and is not too large to resolve the volcanic response. In response to a question raised by referee 1 we have now repeated the total ozone and MSU temperature attribution analysis using 2-yr mean and 4-yr means (padding the final years with missing data). We get very consistent attribution results using 2-yr means, and broadly consistent results using 4-yr means. The fact that we get consistent results using 2-yr mean is now reported in the caption to Figure 3.

P17349, l16: "forced" should be omitted.

Suggested change made.

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

ACPD
10, C9023–C9032, 2010

For this comparison we compare raw monthly observations with raw simulated monthly anomalies, so no inflation of variance is necessary. In the caption to Figure 2 we already state:

Variances are calculated over the 1979-2005 period without detrending, and simulated variances are taken from the ALL (REF-B1) simulations.

We have now inserted ‘unfiltered’ before ‘ALL’ to clarify that no filtering is done here.

We have removed the statement referred to saying that the variance is close to one.

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

We think the referee might have missed the fact that we are regressing onto the responses to the forcings, not the forcings themselves (which is stated here). We already include a detailed description of how the responses to the three sets of forcings (GHG, ODS, NAT) are calculated in the second and third paragraphs of the Data and Models section.

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

As noted in the opening paragraph of this response, when we included the newly available UMUKCA-UCAM simulations, this discrepancy was no longer apparent.

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

We have now added some additional text to the Data and Models section describing the SCN-B2c simulations.

To check the linearity assumption, we also repeated our attribution analysis for lower stratospheric temperature and total column ozone using the fixed greenhouse gas SCN-B2c simulations (Eyring et al., 2010), available for all models but UMUKCA-UCAM, to derive the ODS response directly. These simulations generally have fixed carbon dioxide, methane and nitrous oxide in the chemistry and radiation schemes, with the exception of CMAM which allows these gases to vary in the chemistry scheme (Plummer et al., 2010). ODSs are time-varying in the chemistry scheme in all cases, and in the radiation scheme in all models execept CMAM. These differences in the treatment of the gases in CMAM are the main reason that we focus our analysis on the SCN-B2b simulations, but nonetheless these simulations represent a useful check on our results. When using these simulations, we evaluate the NAT response by subtracting the SCN-B2b and SCN-B2c simulations from the REF-B1 simulations.

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)?

Using additional newly available model output, we no longer find an inconsistency in the simulated and observed GHG response in the attribution analysis, so this sentence has been removed. We have however retained the paragraph discussing the comparison of zonal mean trends in the ALL simulations with observed stratospheric column ozone trends. We do also report that the sensitivity of the GHG attribution results to excluding the UMUKCA-UCAM model.

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?

If the response to a forcing is detected in the observations, this means that its regression coefficient is inconsistent with zero. If the regression coefficient is consistent with one, this means that the magnitudes of the simulated and observed responses are consistent (i.e. there is no evidence of inconsistency). Thus there is no conflict between these two statements. We have now added some text to clarify what is meant:

(the GHG regression coefficient was consistent with both one and zero)

ACPD
10, C9023–C9032, 2010

We have also added some text to the third paragraph of the results section to clarify the definitions of ‘detection’ and ‘attribution’, partly in response to a request from Referee 1.

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

Thanks to the reviewer for this question. We previously stated that Steinbrecht et al. used CCMVal-2 simulations, but this was incorrect – they used CCMVal-1 simulations. This has been corrected. They used REF-B1 simulations over the historical period considered here – we state this already.

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

The reviewer is correct that the response patterns are not independent - the correlation between the two is -0.84. This is larger than, for example, the correlation between the ODS and GHG responses in total ozone of -0.64. We have therefore added to the text the phrase:

‘and lack of statistical independence between the ODS and GHG responses’

to convey the fact that this may be a contributing factor to the lack of separate detection of ODS and GHG.

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

We did already state which models include an assimilated QBO in the previous paragraph – but in response to this comment, we now repeat this information here.

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

Changed as requested.

ACPD
10, C9023–C9032, 2010

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.

The figure shows trends in units of dDU per 27 yr and K per 27 yr. This is now stated in the caption.

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ACPD

10, C9023–C9032, 2010

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