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Comment

Interactive comment on “Reassessment of causes of ozone column variability following the eruption of Mount Pinatubo using a nudged CCM” by P. Telford et al.

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I have structured this in the form of pasting the comments of the referees (and leaving them italicized) and responding directly below them.

REF 1: GENERAL COMMENTS

This is an interesting paper that uses a nudged chemistry-climate model to attribute the sources of changes in total column ozone in response to the Mt. Pinatubo volcanic eruption. There is only one aspect of the paper that leaves me a little uneasy and that is the calculation and interpretation of the delta $O3_{dyn}$ metric. On line 16 of pg 5430 it is stated that delta $O3_{dyn}$ is calculated by subtracting the chemical induced ozone loss

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from the 'observed ozone record ($\delta O3_{obs}$)' where $\delta O3_{obs}$ is the detrended and deseasonalized total column ozone. It would certainly be instructive in Figures 2, 3 and 4 to see $\delta O3_{obs}$. It is clear that any QBO signal in the observed total column ozone is not removed since only a mean annual cycle and linear trend are subtracted from the observations to generate $\delta O3_{obs}$ (bottom of pg 5249). The chemical ozone loss on the other hand has no QBO signal since that cancels out between runs A and B. So $\delta O3_{dyn}$ is not the dynamical response of ozone to the eruption; it is the dynamical response added to all other geophysical sources of variability in the original observed ozone time series (QBO, ENSO, solar cycle, noise).

For me, this confounds the interpretation of the results. Why not use a regression model to remove the mean annual cycle, the trend (or EESC induced change in ozone), the QBO, the solar cycle, and ENSO from the total column ozone observations? That would leave a $\delta O3_{obs}$ that is the observed change in total column ozone as a result of the Pinatubo eruption alone (plus some noise). Then, when you subtract the chemical ozone loss, you would have the true dynamically induced ozone loss induced by the additional sulfate aerosols alone. That would make much more sense to me. Right now when you plot the $\delta O3_{dyn}$, it displays non-zero values before the eruption and this is very confusing. You end up coming to the conclusion, as stated in your abstract, that 'the remaining variability is dominated by the QBO'. But this is already well known and it doesn't say anything about how Pinatubo affected the dynamics. You could then also make a much firmer conclusion than what you currently have as the last sentence of the paper.

We have rephrased to rectify any misunderstanding about our approach. The dynamical variability was never intended to be attributed solely to the eruption. This technique, or any regression technique, is not capable of extracting the signal from the volcano as this is entwined with other variability. For example when we obtain the variability caused by the QBO this includes effects of the volcano as the QBO itself was impacted by the eruption. To us there doesn't seem to be any simple way of separating these.

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Compared to the other sources of variability any remaining direct variability appears small, too small to be determined using regression techniques. The best way of determining the direct dynamical impact would seem to be by running free running models as is the case of the study of Dameris et al. This is however difficult at present with imperfect representations of both the QBO and the dynamical response to volcanoes in most models.

A simple check of this technique is to, by eye, take the difference between the black line and the red line in Figs 2-4. In general this seems very noisy with no clear signal (with the exception of Fig 4a with very low ozone in 1993 and 1995).

The dominance of the QBO signal allows us to gain confidence that the model chemistry is good. However it also says something about the dynamical effects of the eruption, in that their direct impact (i.e. excluding the impact on the QBO) is a lot smaller than that of the QBO, on a global scale at least.

The CCM simulations are good and are likely to be very instructive. If you could put some further thought into how to do the attribution of Pinatubo induced changes in ozone to chemistry and dynamics, I think that the paper would be much improved. It's really close but by dragging known non-volcano induced dynamical variability in ozone (specifically the QBO) into your delta $O3_{dyn}$ metric, you are muddying the waters. Specifically, if you could quantify the difference in the true aerosol induced change to the effect of dynamics on ozone between the northern and southern hemispheres, that would be interesting - then again, that wouldn't actually explain WHY the hemispheres show different dynamical responses.

We agree it would be very interesting. However the system seems to be complicated enough that this is decidedly non trivial. As we mentioned above the main dynamical effect of Pinatubo may well be its influence on the QBO. As the main way of extracting the QBO signal is to use the observed winds, so removing the QBO may well be removing much of the dynamical impact of Pinatubo! To avoid having to consider these

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dynamical entanglements we study all dynamical changes. We have clarified the text to indicate that the dynamical factors are not solely due to Pinatubo.

REF. 1: SPECIFIC COMMENTS

Page 5424, line 6: Were those record lows in extra-polar ozone observed in both hemispheres or only in the northern mid-latitudes? I think that the lowest ozone over southern midlatitudes was observed in 1997.

There were record lows observed in the average extra-polar zone. It is true that these lows are dominated by the northern mid-latitudes (as can be seen in Fig. 4 or in Bodeker et al). We have relabeled extra-polar 'global' to try and clarify that this is an average rather than referring to specific regions.

Page 5425, line 3: I think that a more revealing paper here is that by Gleason, J.F.; Bhartia, P.K.; Herman, J.R.; McPeters, R.D.; Newman, P.A.; Stolarski, R.S.; Flynn, L.; Labow, G.; Larko, D.; Seftor, C.; Wellemeyer, C.; Komhyr, W.D.; Miller, A.J.; Planet, W. (1993). Record low global ozone in 1992. Science 260: 523-526. That was earlier than the others and showed clearly the large reductions in ozone over northern midlatitudes; more so than over southern midlatitudes.

This reference has been included.

Page 5426, line 2: Longitudinal patterns in what?

This refers to longitudinal patterns in the ozone column. This has been clarified

Page 5426, line 15: I was confused by this phrase 'surface aerosol density'. What exactly is that? Usually people refer to the aerosol surface area density (m^2/m^3) i.e. the aerosol SAD. You seem to have created some sort of hybrid phrase here that doesn't make sense to me and seems to be at odds with what is used elsewhere in the literature.

This is a typographical error. This should have read aerosol surface density as you

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suggest.

Page 5429, line 2: Replace ERA-40 re-analysis data' with 'ERA-40 reanalyses'.

This could be changed. The terminology was adopted for consistency with Telford et al 2008 (as specified in the review process for that paper). There does not seem to be any definitive naming scheme.

Page 5429, line 21: It wasn't clear to me what you meant by 'simpler variability'.

We have removed any ambiguity from this by describing the variability explicitly.

Page 5429, line 22: Why not just calculate the mean annual cycle, subtract that from the time series, and then subtract the linear trend? That would be far easier. That said, if you go with my suggestion above of removing all of the variability except for the effects of Pinatubo and the unforced variability, that will all get subtracted anyway.

This would be one way of removing the annual cycle. However we feel that our simple approach is doing a reasonable job and any changes would be too small to justify making a change.

Page 5431, line 2: Figure 1 doesn't show 'the global average ozone column in the data and the model runs'. It shows the anomalies away from a mean value.

Again this is a typographical error, which has been corrected.

Page 5431, line 5: I think you need to provide some details on exactly how high this bias is.

The bias is around 10

It would be instructive to have the observations included in Figure 2, 3 and 4.

We have augmented Figure 1 to include the latitudinal dependence of the observations and the best guess run. We hope this addresses this issue.

Page 5433, line 6: It wasn't clear to me what you meant by 'importance in the global

residual'.

This was poorly phrased and has been rephrased.

Page 5433, line 16: You state that 'After removing the variability associated with the QBO we still see low ozone at the time of the Pinatubo eruption'. But I don't see that really clearly in Figure 3. You talk about a 5 DU reduction but it's not clear to me what I should be looking at in Figure 3 to see that 5 DU. Then everything in the rest of that paragraph seems a little confusing.

This refers to a line obtained by subtracting the contribution of the QBO (the red line) from the dynamical variability (black line). This shows a 5 DU difference at the time of Pinatubo. We decided not to include this line as it made the figure more difficult to interpret. The text has been modified to make clear where this number comes from.

Page 5434, line 15: But I thought that the depletion was quite different in the two hemispheres as described later in that paragraph.

The observations show considerable differences between the hemispheres. However this study (and others see Stolarski 2006, Fleming 2007 etc.) show that the modeled chemical depletion caused by the increased SAD is similar in the northern and southern hemispheres. It is the dynamically driven low ozone that differs greatly between the northern and southern hemispheres. This can be seen in Fig. 4 where the chemical depletion (blue line) is similar in both plots whereas the 'dynamical variability' (black line) differs greatly.

The figure legends are confusing. The blue line should be labeled ' δO_3^{chem} ' and the black line should be labeled ' δO_3^{dyn} '. Then in the figure caption say that blue is Run A minus Run B and black is detrended and deseasonalized observations minus blue.

This has been changed.

REF. 1: TYPOGRAPHICAL AND GRAMMATICAL ERRORS

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Page 5425, line 25: replace 'concentration' with 'concentrations'. Page 5430, line 21: replace 'firstly' with 'first'. Page 5431, line 8: replace 'data is' with 'data are' and please make the same correction elsewhere in the paper.

All changed

REF 2: GENERAL COMMENTS

Telford et al. utilised a nudged version of the new UKCA climate-composition model to study chemical and dynamical aspects of the Pinatubo eruption impact on total ozone. They performed two 10-year simulations nudged by the ERA-40 analyses with ("Best Guess") and without ("Background") time-varying observed aerosol surface area and one 10-year free-running simulation with the time-varying observed aerosol ("Free"). By comparing/subtracting the detrended, deasonalised, 6-month smoothed anomalies of the various runs and the TOMS/SBUV total ozone they remove the chemical signal of the eruption and attempt to isolate the dynamical effects that they ultimately attribute, predominantly, to QBO changes. I welcome the nudged set-up of the UKCA model and the related experiments to illuminate aspects of the Pinatubo impact on stratospheric ozone and I find the work a useful contribution towards that direction. The paper is very well written and structured and the main results and the graphs are presented clearly. I feel there are four major issues, i) unsatisfactory model validation, ii) questionable isolation of dynamical effects, iii) too much emphasis on the dynamical attribution to QBO and iv) lack of further dynamical diagnostics . If these issues, together with some minor ones are dealt with by the authors in some degree, then I will be very happy to see their paper published in ACP.

REF 2: SPECIFIC COMMENTS (MAJOR)

1) The validation of only the model global ozone column is not enough. Since, apart from globally, various latitudinal bands are analysed, a better picture is needed of the performance of the modeled ozone changes. The stratospheric version of UKCA has

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a comprehensive chemistry and I would very much like to see how "realistically" the experiments (with the nudging towards the assimilated meteorology) produce the total ozone inter-annual variability. Ideally I would like to see comparison of zonal mean Lat vs. Time total ozone anomalies for at least the Best Guess run and the observations without any data processing and with the same colour scale. Alternatively, time-series of the tropical, northern and southern hemisphere mid-latitude anomalies for the Best Guess run and TOMS/SBUV should be shown.

We agree that a latitudinally resolved ozone column is interesting and we augment Fig.1 to include this for the data and the Best Guess Run in addition to the global average.

2) The presented methodology cannot isolate completely the dynamical effects. Subtracting the Background from the Best Guess certainly isolates the chemical signal arising from the volcanic eruption but NOT other chemical signals so when this residual is subtracted from the observations the remainder is not pure dynamics. For example, another process (which depends on both chemistry and dynamics) is the polar stratospheric cloud (PSC) ozone loss exported from the Polar Regions to the middle latitudes every spring. This process is not negligible and every year depletes about 10-20 DU in the N.H. and it appears to peak every 2-3 years in the 1990s (for example, as seen in Fig. 6e of Harris et al. 2007 or in Fig. 4 of Hadjinicolaou et al. 1997). This should be somehow acknowledged, at least, in the analysis (see also point 4)

We acknowledge that export of ozone-depleted air contributes to the variability in both the introduction and the results section. However we do note that our dynamical effect in the northern mid-latitudes peaks early in the year, unlike the PSC effect in Harris et al. which peaks in the summer.

We agree that our definition of 'dynamics' is slightly arbitrary, with for instance changes in chemical processes caused by temperature changes labeled as being 'dynamically induced' and we now acknowledge this more clearly in the text. We chose to adopt

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this label to avoid having to use more clumsy and confusing ones, but acknowledge in the text that the 'dynamical' label includes several factors.

3) The temporal evolution of the "dynamical" effects (Obs. minus (BG-B)) curve has, in all plots, an evident biennial periodicity and the regression to the QBO proxy clearly relates this oscillation to the derived dynamical impacts (although the choice of the 6-month smoothing might have optimised this agreement?). But this is nothing new or explicitly related to the possible eruption impacts as the QBO/dynamical correlation in the plots also appears before 1991 and long after 1991-93. (The QBO has always been the primary explanatory variable of choice for studies of ozone inter-annual variability and trends). Also for the middle latitudes, the inexact timing (in the S.H.) and the partial magnitude explanation (in the N.H.) of the QBO variations compared to the dynamical ozone ones suggest that other mechanisms are also in play.

The fact that the QBO dominates is itself interesting suggesting that the direct dynamical effects are much smaller than this source of variation. We agree that we don't provide mechanisms for the changes in variability due to the QBO in mid-latitudes. We include the changes to PSCs as suggested by you as one other possible cause of variability. It is beyond the scope of this work to determine these mechanisms, but we do agree that it would make interesting future work.

*4) More could have been done in diagnosing other dynamical processes that influence lower stratospheric/total ozone, such as the strength of the polar night jet or the vertical EP-flux which controls the winter-time ozone build-up, or, the product VPSC*EP-Flux (VPSC=volume PSC). These proxies are importantly related to the PSC-induced polar ozone loss and the dilution to middle latitudes (see Harris et al. (2008) and the references therein). For the ERA-40 analyses the EP-Flux and VPSC proxies are available online from the /atmospheric_circulations/projects/candidoz/ep_flux_data link at: www.awi.de/en/research/research_divisions/climate_science*

We agree that changes in dynamics are interesting. However such dynamical studies

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seem to be less appropriate in a nudged format and would be more appropriate for studies using reanalysis data or free running models (see Morgenstern et al 2009 for one such study using UKCA).

REF 2: SPECIFIC COMMENTS (MINOR)

1) page 5426, line 19 and page 5434, line 2: By reading these lines it appears that Solomon et al. (1996) "argued for an important dynamical contribution to ozone variability". The authors are of course free to infer that, by looking at the 50

Thank you this has been rephrased to reflect the original opinions expressed those papers.

2) Page 5427, line 10: The introduction and the related literature review is very good and it will be complete if the authors add that the effect of the Pinatubo eruption has also been studied with thorough statistical analyses based on observations in the context of long-term total ozone trends and it was found to be important only in the N.H. middle latitudes (Mader et al., 2007; Wohltmann et al., 2007)

Done

3) Page 5428, line 17: It would be nice for the reader to acquire a basic knowledge of the related to this study aspects of the model without having to go through the cited model work. The authors need to add here 2-3 lines saying a few words about the comprehensiveness of the stratospheric chemistry used as well as the feedbacks allowed among radiation, chemistry and dynamics.

A few lines have been added. The feedbacks have been excluded, as they are not important in the nudged model. A reader interested in the model itself is free to read the description in Morgenstern et al 2009.

4) Page 5429, line 2: In the same spirit as in the previous comment, the authors need to add another 2-3 lines reminding the vertical range (mainly 15-45 km?) and the "strength" of the nudging.

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Done.

5) page 5429, line 7: In section 2.1, the model set-up is not complete if basic elements of this work's experiments are not mentioned, like initial conditions, source gases used, chlorine loading, solar forcing. The exclusion of the last two are mentioned in page 5432, line 11, but they need to be clarified at this section too.

Done

6) Page 5431, line: 16: The ability of Run C to capture the observed ozone of 1994-95 is as good as in 1991 so any comment about agreement of the free running simulation with particular years does not add anything to the validation. On the other hand, the modeled decrease after Pinatubo by Run C is noteworthy and should remain.

This was included to describe the fact that the free running run is capable of producing years with low ozone. We have added a line noting that in other years (1993, 1997) it does not, which we believe relates to the incorrect periodicity of the QBO rather than any features of the chemistry.

7) Page 5435, Conclusions: More quantitative information of the relative importance of volcanic chemistry and dynamics can be given (not only in absolute, DU), for example," the global ozone reduction in 1993 can be ascribed by 2/3 to aerosol chemistry and 1/3 in dynamics". The same for the regional assessment.

We didn't want to make any such definitive statement to mislead people. For instance the 2/3:1/3 ratio is only one moment in time and at another time or place this will change. We would rather present the whole picture in the Figures and let the reader take the appropriate figure from the time and place they are interested in.

REF 2: TECHNICAL CORRECTIONS 1) page 5426, line 26: Replace "... forced by ECMWF..." with "...forced by UKMO..." 2) Page 5426, line 28: Replace "... in studies of recent..." with "... modeling studies of recent : : ." 3) page 5430, line 3: Replace "...chemistry run B...", with "... chemistry from the volcano run B" 4) page 5430, line 7:

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Replace "... without nudging..." with "...without nudging but SAD"

All done

REF 3

This paper uses a nudged CCM, satellite column ozone data, and regression modeling with a QBO proxy to attribute causes of the column ozone changes after the Pinatubo eruption. In fact, the role of the nudged CCM is quite small: It is really only used to derive the signature of chemical ozone depletion due to enhanced aerosol loading, but accurate calculation of this term is essential for the later attribution of other contributions. This paper gives hardly any results of the model runs and so it is not possible to judge how well the model is doing in terms of the minor species which lead to the ozone depletion. The only comparison shown is for column ozone. The authors do state that their calculated ozone depletion agrees with values calculated by other 2D and 3D models.

We have added a couple of lines giving a basic description of the chemistry, though we refrain from providing exhaustive details as this has been done in great detail by Morgenstern et al, GMD (2009). The agreement described is with results obtained using regression modeling of observations.

The later analysis is then based on subtracting the model calculated O3 (column) loss from observations and using a regression model with a QBO proxy. This is the part of the study which can claim to be novel, I think. The conclusion that dynamics played a role in the low ozone in the NH midlatitudes (but not the SH) is not new. This paper argues for a link between the NH dynamical effect and 'changes in the QBO' but no strong justification is made. It is a shame that the CCM cannot be used (maybe with the free running model and its different QBO period) to investigate and support the conclusions of the coupling of the QBO and volcanic signal.

As you state we feel that the combination of nudged CCM results and observations

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is novel. Previous regression analyses have assumed shapes of the ozone depletion and fitted to them. We make no such assumptions, obtaining our results from our model. The good agreement is interesting, providing confidence in the model and in our understanding of the chemical changes after Pinatubo.

Abstract: Line 14. The chemical depletion cannot be observed - it can only be diagnosed from the overall column reduction using some tool (e.g. a model). You should be clear if you mean your calculated chemical loss agrees with other previous estimates, or if you really are comparing your model column with observations (chemical depletion and dynamics).

We agree that the chemical depletion was diagnosed rather than observed. This has been changed in the text. Our calculation produces results that can be most easily compared with previous estimates using regression techniques on observations (Fleming, Stolarski). Interestingly we obtain closer agreement with the observed changes than the modeled changes in these cases.

Section 2.1: The model details are brief. Is the sulfate SAD only used in the heterogeneous chemical rate calculations, or is it also used in the model's radiation scheme? The text is not clear. I don't understand the point of the sentence 'The optical depth is prescribed...'. Please explain where that comes in.

The sulfate SAD from D. Considine is used only in the heterogeneous chemistry scheme. The model radiation scheme uses a more crudely binned data set from Sato et al. In the nudged runs this is not important as the nudging terms dominate over the details of the radiative heating. It does have some effect on the free running model. We agree that using two different data sets for chemistry and radiation is not ideal and they will be standardized in the future. For the nudged model runs, which are central to the paper, the heating terms are not important.

A small point: Is it really true to say the model vertical wind is a prognostic variable? I realise the model calculates it but is it not just diagnosed from the horizontal winds (and

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other variables which are stepped forward in time) at each model step? Are you trying to make the point the vertical wind is consistent with the nudged horizontal winds?

The new dynamics UM is a non-hydrostatic model and so the vertical wind is indeed a prognostic variable.

Section 2.3: Again, the details of the model runs are brief. What is the chlorine and bromine loading used for the runs? It seems that this is kept constant (based on text elsewhere) but please say at what values.

The Cl loading was 3.4 ppbv and the Br loading was 17.5 pptv, taken from Morgenstern et al 2009. These have been included in an expanded model description.

Section 3.1: The model runs in Figure 1 do appear to show good agreement with the data but to the eye this agreement is dominated by the model simply having the correct annual cycle. Can you add another panel to figure one which removes the annual cycle and therefore shows a clearer picture of the modeled long-term variability (e.g. anomaly from the monthly mean). Please give a number for the 'slight high bias'.

The slight high bias is around 10%, is mainly in the UTLS region and has been included. The modification of Figure 1 to include the latitudinal plots makes this clearer.

Section 3.2: Comment: The black curve in Figure 2 (obs - chemical signal) does appear to fit a QBO variation, which I take is a key result. This provides the authors with evidence that the model chemical signal is correct.

We agree that this is one of the conclusions we draw from Figure 2. However we feel that our method of calculating the ozone loss from chemistry complements those from regression methods, as we make no prior assumptions about the shape of the distribution.

Section 3.3 and 3.4: The comparisons of the black and red curves in Figs 3 and 4 do show significant differences in the tropics and NH, which leads to the conclusions on the role of ENSO and dynamics. However, how confident can you be that the spatial

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variation of the modelled chemical signal is realistic?

Other referees have expressed interest in the latitudinal performance of the model. To increase confidence that this performs well we have modified Figure 1 to show latitude time plots in addition to the single line.

Section 4: The paper ends with the sentence 'We speculate that this low ozone arises from qualitative differences in the QBO after the eruption'. This comment comes out of earlier mentions of this but in this final summary section please be clear what you are suggesting.

The changes in the QBO after the eruption are well documented, with 'phase locking' producing a qualitative change. Our speculation relates to the effect that this has on ozone column, noting that the correlation between our QBO proxy and our ozone column in the extra-tropics changes at this time. Unfortunately the data is insufficient and our technique unable to make any definite link between the changes in the QBO and the ozone column. However we feel that the effect is an interesting one for future study. We have clarified this in the summary. We have also changed the final sentence of the conclusions to be less ambiguous.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5423, 2009.

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