Atmos. Chem. Phys. Discuss., 9, C176–C180, 2009
www.atmos-chem-phys-discuss.net/9/C176/2009/


Atmospheric Chemistry and Physics Discussions



# *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.

**Anonymous Referee #2**

Received and published: 12 April 2009

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.

Specific Comments

Major issues

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 modelled ozone changes. The stratospheric version of UKCA has 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.

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 strato-



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spheric 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).

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.

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

Minor issues/suggestions

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% discrepancy

of the chemically-driven 2D-model ozone changes compared to the observed, dynamics must have played a role too. But the Solomon et al. 1996 paper, as also emphasized by its title, actually championed the "role of aerosol variations in anthropogenic ozone depletion at northern middle latitudes", did not investigate any dynamical role (apart from a one-paragraph literature review) and certainly did not argue in favour of the dynamical effects.

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)

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.

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.

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, buy they need to be clarified at this section too.

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 modelled decrease after Pinatubo by Run C is noteworthy and should remain.



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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.

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 "... modelling studies of recent . . ."

3) page 5430, line 3: Replace “...chemistry run B...”, with “... chemistry from the volcano run B”

4) page 5430, line 7: Replace “... without nudging...” with “...without nudging but SAD”

References

Harris, N. R. P., Kyro, E., Staehelin, J., Brunner, D., Andersen, S.-B., Godin-Beekmann, S., Dhomse, S., Hadjinicolaou, P., Hansen, G., Isaksen, I., Jrrar, A., Karpetchko, A., Kivi, R., Knudsen, B., Krizan, P., Lastovicka, J., Maeder, J., Orsolini, Y., Pyle, J. A., Rex, M., Vanicek, K., Weber, M., Wohltmann, I., Zanis, P., and Zerefos, C.: Ozone trends at northern mid- and high latitudes: a European perspective, Ann. Geophys., 26, 1207-1220, 2008.

Mader, J. A., Staehelin, J., Brunner, D., Stahel, W. A., Wohltmann, I., and Peter, T.: Statistical modelling of total ozone: Selection of appropriate explanatory variables, J. Geophys. Res., 112, D11108, doi:10.1029/2006JD007694, 2007.

Wohltmann, I., Lehmann, R., Rex, M., Brunner, D., and Mader, J.: A process-oriented regression model for column ozone, J. Geophys. Res., 112, D12304, doi:10.1029/2006JD007573, 2007.



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