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Interactive comment on "Chemistry-climate model SOCOL: a validation of the present-day climatology" *by* T. Egorova et al.

Anonymous Referee #3

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Review of "Chemistry-climate model SOCOL: a validation of the present-day climatology" by Egorova, Rozanov, Zubov, Manzini, Schmutz and Peter

GENERAL COMMENTS

Firstly I would like to congratulate the authors on the development of a CCM that can run on a PC with excellent wall clock performance and that furthermore they are making the model available to the international community. This is truly admirable.

The introduction of the paper is too long. I have suggested below some material that I think could be removed from the introduction.

SPECIFIC COMMENTS

Page 1, line 20: When in the abstract you say 'observed link between the tropical stratospheric structure and the strength of the polar vortex' are you referring specifically to strength of the Brewer-Dobson circulation. If not, I'm a little confused. If so, why not just say so.

Page 2, line 7: There are no modelling tools that are able to represent all known atmospheric physical and chemical processes and their interactions and yet people still make model runs to predict the future evolution of the atmosphere. The models clearly don't need to incorporate all known mechanisms; just the ones that matter. Also you refer to physical and chemical processes but say nothing of dynamical processes.

Page 2, line 11: I would suggest rather than saying 'atmosphere and chemistry coupled models' say 'general circulation models (GCMs) to which interactive chemistry has be added'. You can then probably delete the sentence 'Each of these models ... their evolution in time'.

Page 2, line 18: At the end of sentence '... rather different results' I would suggest again referring to Austin et al. 2003.

Page 2, line 19: I thought it was a delay to Antarctic ozone recovery not Arctic ozone recovery that the GISS model suggested? I may be wrong though but please just check.

Page 2, line 24: Many readers of this paper are not going to be overly familiar with CCMs. Less informed readers may wonder why you can't just do day-to-day comparisons between CCM output and observations. I think that it would be very valuable to have just one or two sentences in here that outline the fact the CCMs produce 'realistic' rather than 'real' atmospheric states and that therefore validation can only be in terms of the models ability to reproduce (1) the climatological mean state of the atmosphere (2) any trends with respect to that climatology (3) an accurate representation of observed atmospheric variability. You have these three points listed there but a sentence or two describing why validation needs to be done in this way would be valuable. Also why are ensemble runs required to do a climatological comparison with reality? Why can't you just use one run? Perhaps you need to have the material from page 3 line 10 up here to clarify this.

Page 2, line 31: The three types of model validation that you list here are not process-oriented model validation approaches. Therefore you cannot say 'This process-oriented model validation ...'. Where are the processes that you are validating?

Page 3, line 1: It is not clear to me what you're getting at here. When you say 'mostly because our knowledge of atmospheric climatology and processes is incomplete' do you mean that e.g. you can't compare a model climatology of total column ozone over the 1990s with an observed climatology because our knowledge of the 10 year ozone climatology is incomplete? If that is what you mean I would strongly disagree. And why do we need to have any knowledge of atmospheric processes to do a climatological comparison of a CCM with observations?

Page 3, line 13: With regard to 'or even from a particular reality (namely the one assumed by planet Earth), should be interpreted with caution'. That may be going a bit overboard. The CCM is tailored to model the atmosphere of Earth.

Page 3, line 21: I think that this over emphasis on the use of assimilated data products for CCM validation is misleading and overly complicated (as you say 'the various assimilation schemes and applied models differ substantially'). There are a variety of long-term observations of the real atmosphere that can be used for such validation e.g. Struthers, H., K. Kreher, J. Austin, R. Schofield, G.E. Bodeker, P.V. Johnston, H. Shiona, and A. Thomas, Past and future simulations of NO2 from a coupled chemistry-climate model in comparison with observations, Atmospheric Chemistry and Physics, 4, 2227-2239, 2004. If you're restricting yourself to the use of e.g. 3D-var assimilated fields, to some extent this reduces to a model-model intercomparison. Can we at least have some acknowledgment that there are measurements out there of the real atmosphere (the one in which we live) that can be used to validate CCMs.

Page 3, line 31 to Page 4, line 13: I would recommend deleting this paragraph from the introduction. The introduction is already too long and I don't think that many readers will be terribly interested in the details of the Garmisch meeting (those that were there will already know this and those that weren't are unlikely to be interested), and the creation of the table. Including this sort of discussion in your introduction will also cause your paper to date quickly. People reading your paper in 10 years time won't care about the Garmisch meeting etc.

Page 4, line 10: I don't like the use of the word 'response'. It suggests that stratospheric ozone and temperature are driven by changes in the strength of the vortex whereas I could argue that ozone depletion causes local cooling which steepens the meridional temperature gradient which strengthens the vortex i.e. it is the vortex strength that responds to the ozone and temperature changes.

Page 4, line 11: What's the point of introducing the Arctic Oscillation here? This sentence leaves the reader hanging somewhat.

Page 4, line 21: Its not true that there is only one reference data set available at the moment. There are numerous climatologies of observations against which your model can be compared. It is also not clear to me why when using URAP you can only compare the climatologies 'without statistical significance analysis'. URAP data sets include standard deviations on the mean values and by calculating the standard deviations on the CCM mean values you should be able to do a climatological intercomparison that does include statistical significance analysis.

Page 6, line 14: Can you please provide a citation that describes the iterative Newton-Raphson scheme.

Page 6, line 15: I think that this acceleration technique for solving a sparse system of linear algebraic equations is excellent and I just wonder why it isn't used routinely by other modelling groups who are currently spending large sums of money on CCMs runs. Maybe they will after your paper is published?

Page 7, line 21: I am guessing that your model has two types of 'water'. Tropospheric water as produced by the GCM and stratospheric water which is not related to the tropospheric water. Is that correct? If so, perhaps you should say something about this.

Page 7, line 27: I think that its remarkable that you can do a 10 year simulation in 40 days using a PC. With 10 PCs you could do a 10 x 130 year ensemble run in just under 1.5 years. Someone should do this for the next WMO/UNEP ozone assessment.

Page 8, line 7: Regarding the source gases for the model at the lower boundary - there is one thing I would like to know about this (and perhaps other readers would too) and that is: presumably CFC-11 and CFC-12 concentrations at the lower boundary are specified otherwise you wouldn't get the radiative forcing right. Now are Cly concentrations also specified or are they derived from the CFC-11 and CFC-12 concentrations? The reason I ask is that climate change is likely to accelerate the Brewer-Dobson circulation which would result in shorter lifetimes for CFCs in the upper stratosphere and therefore less time to photolyze to produce Cly. So this process could lower Cly levels. Would your model capture this process? To what extent do other CCMs capture this process and how much of the inter model variability, described earlier in your paper, might this explain?

Page 9, line 1: But surely trends in the different data sets which comprise this 64 years of observations would invalidate this approach? Presumably the boundary conditions driving the model (SSTs and trace gas concentrations at the lower boundary) are with respect to real world time. There will be trends in these factors. Or are you actually using your 40 x 1 year ensemble runs for 1995 conditions for this? If that's true shouldn't you just be comparing your model results with the observations in a few years around 1995 (I agree that the comparison shouldn't be done just for 1995). This is not very clear to me. I'm really concerned that trends in the observational data sets and going to bias your climatology.

Page 9, line 6: You've already expanded the URAP acronym on page 4, you don't need to do it again here.

Page 9, line 11: Did you take the total column ozone data from the TOVS instruments onboard Nimbus 7, Meteor 3, Adeos and Earth Probe, or from the TOMS instruments onboard these satellites? The TOMS data are much better quality than the TOVS data. Why didn't you use the TOMS data rather? And why did you select the 1993-2002 period specifically. Is that because you wanted it to be close to 1995 (the conditions for which your model was run) but include as much data as possible? Again I would argue that there are significant trends in the ozone data from 1993-2002 that would result in your ozone climatology not being representative for 1995. Comparing climatologies that are not generated over the same time periods always worries me and I would like to see more attention paid to this in this paper.

Figures 1 to 4: Is it necessary to have four figures? Can't you just have two - each figure showing the SOCOL zonal mean fields, the URAP means fields and then their difference i.e. 2 columns and 3 rows of panels, or as you have done in Figure 6. Don't the bottom panels of Figure 1 and the top panels of Figure 3 just show the same data (although they don't appear to and I'm not sure why)?

Figure 5: why show two repeating years which are exactly the same? You are showing twice as much data as you need to show. This is true for a number of the following figures.

Figure 6: The two bottom panels need more contours and better contour labeling.

Page 12, line 24: A couple of years ago we also found a potential problem with the URAP data. In particular we had obtained the URAP data from two different sources e.g.: 1) http://code916.gsfc.nasa.gov/Public/Analysis/UARS/urap/o3_base_extra_haloe_mls_theta-eqlat.html 2) http://www.sparc.sunysb.edu/html/uars/haloe+mls_o3/haloe+mls_o3.html and when we compared the data files, which should have been the same, there was

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a 1 month offset between the data files, particularly obvious in the austral winter months May, June and July. June from URAP looked like July from the SPARC site. We emailed Bill Randel about this but the problem was never resolved. This may be a pointer to the problem that you are experiencing with the URAP data.

Page 13, line 5: And in turn the H2O mixing ratio at the entry level depends on the temperature of the tropical tropopause layer (TTL) right? or at least the coldest temperature in the TTL (see Gettelman, A., W.J. Randel, F. Wu, and S.T. Massie, Transport of water vapor in the tropical tropopause layer, Geophysical Research Letters, 29 (1), 10.1029/2001GL013818, 2002.)

Page 14, line 21: Its not clear to me which total column ozone data you are comparing the model against. Is it just TOVS data that you are using or some composite of TOMS and TOVS? If it is a composite of different data sets, how were they combined and how were inter-satellite offsets and drifts removed?

Figure 14: Why are the differences between SOCOL and observations not also shown as was done in the other figures?

Page 14, line 32: Is this difference of 8 DU really significant given the accurate of the TOVS data against which you are comparing SOCOL? What is the accuracy of the TOVS data?

Page 15, line 15: The imprint of the Arctic Oscillation on what?

Page 15, line 23: It is not clear from your description whether temporal dependence in the boundary conditions (SSTs, GHG concentrations etc.) was included in this transient model run.

Page 17, line 2: But you are calculating photolysis rates below 250nm though right?

Figures 15 to 18: some indication needs to be given on whether these differences between the positive and negative phase of the AO are statistically significant. For example are the 0.5% or 1.0% changes in ozone between the positive and negative phases of the AO really significant. And while on the ozone point, how where other factors affecting ozone removed e.g. QBO. If when making up your positive and negative AO phase composites, one of the subsets included more westerly QBO phases than the other, this could seriously bias your results. You need to give a more detailed account of how you avoided other geophysical drivers from causing differences in your composite subsets since these other drivers could cause much bigger changes in ozone than those expected from a change in sign of the AO. This could also account for some of the model/measurement disagreement that you are seeing.

GRAMMAR AND TYPOGRAPHICAL CORRECTIONS

I understand that the author's first language may not be English and as a result the grammar in the paper occasionally falls short of an acceptable standard. While this may be irrelevant for assessment of the research presented, it often distracts the reader from the good quality of the science. I would suggest that the authors find someone to thoroughly proof read the paper to correct all grammatical errors. Some of the more glaring errors (but certainly not all) are listed below.

Page 1, line 23: replace 'as it as been predicted by' with 'as predicted by'.

Page 2, line 4: replace 'reacting to the different' with 'reacting to different'.

Page 2, line 29: replace 'the model's ability of simulating' with 'the model's ability to simulate'.

Page 3, line 1: replace 'the consideration' with 'consideration'.

Page 3, line 14: delete the comma.

Page 4, line 16: please expand the 'PMOD/WRC' acronym.

Page 5, line 6: replace 'described in details' with 'described in detail'.

Page 6, line 32: replace 'in vertical direction' with 'in the vertical direction'.

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Page 9, line 27: replace ’maximum locates at higher altitudes’ with ’maximum is located at higher altitudes’.

Page 11, line 7: replace ’summer hemisphere’ with ’the summer hemisphere’.

Page 11, line 13: replace ’seasonal variation’ with ’the seasonal variation’.

Page 12, line 6: please list the photochemical lifetime for methane.

Page 12, line 31: replace ’model overestimates mixing ratio of H2O compare to URAP data’ with ’model overestimates the mixing ratio of H2O compared to the URAP data’.

Figure 13 caption: replace ’difference in percents’ with ’difference in percent’.

Page 18, line 5: replace ’Despite of’ with ’Despite’.

Concerning the Butchart and Austin 1998 reference, I am sure the journal volume is 55 and not 35.

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