Atmos. Chem. Phys. Discuss., 5, S554–S565, 2005
www.atmos-chem-phys.org/acpd/5/S554/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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

T. Egorova et al.

Received and published: 22 April 2005

Answer to Anonymous Referee #3

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."

We thank reviewer#3 for his appreciation of our efforts to make the model available for community. His comments and one request for our model tool we got already encourage us to keep a brief description of the model technical aspects, even though reviewer#1 advised to eliminate this section.

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

We agree with this and will try to follow reviewer#3 advise.

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.”

We have added this to the last sentence of the Abstract.

“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.”

We have changed this sentence accordingly.

“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’.”

We have changed this sentence accordingly.

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

We have changed this sentence accordingly.

“Page 2, line 19: I thought it was a delay to Antarctic ozone recovery not Arctic ozone

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Interactive Comment

recovery that the GISS model suggested? I may be wrong though but please just check."

Shindell et al., (1998) predicted a delay to ozone recovery over the both Arctic and Antarctic.

"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."

We have changed this paragraph according to the reviewer suggestions .

"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?"

We have eliminated this sentence.

"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. "

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Of course it is possible to compare the model results with 10-year total column ozone climatology, but it is not enough to detect and attribute model deficiencies, because our knowledge of atmospheric variability is incomplete. A good example of this is early splitting of the southern polar vortex in 2002. If we got this feature in the model before 2002 we would probably consider this as a model deficiency and try to find what model component should be improved to avoid such situation. However, after 2002 it became clear that this feature can occur in the real atmosphere. We think that it is not the last surprise from the nature.

“And why do we need to have any knowledge of atmospheric processes to do a climatological comparison of a CCM with observations?”

The knowledge of atmospheric processes is necessary to find out how the model should be improved to eliminate the model deficiency detected from the comparison of the CCM climatology 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.”

Maybe it is a little bit exaggerated, but we would like to emphasize that our knowledge about the nature is limited, simply because from the observations we can get the information about only one particular realization of the stochastic system.

“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 assimi-

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lated 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."

We have also added a sentence about other possibilities.

"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."

We have substantially shortened this paragraph.

"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."

We have reformulated this sentence.

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

We have reformulated this sentence.

"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

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deviations on the CCM mean values you should be able to do a climatological inter-comparison that does include statistical significance analysis."

We are not aware of alternative official reference data set comprising of zonal and monthly mean latitude-altitude cross sections of atmospheric species. We have calculated significance of the model deviations from the observation data using standard deviation from URAP.

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

We have added requested citation.

"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?"

No comments.

"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."

In the model we have one type of "water". The water cycle in the troposphere and lower stratosphere is treated in the GCM part of the model, while the water vapor chemistry and transport on the stratosphere and mesosphere is treated in chemistry-transport part of the model. Unique water vapor field is transferred from GCM to CTM and back at every step. We have added additional explanation to the text.

"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."

We agree that some useful simulations can be carried out with the model. That is why we are interested in active collaboration with other groups.

“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?”

Cly concentrations are derived from CFC-11 and CFC-12, therefore the level of Cly depends on the atmospheric state. Therefore, our model would potentially capture this process. Any CCM with not prescribed Cly should also capture this process.

“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.”

We are actually using for comparison 40-year steady-state run for 1995 conditions. We cannot use for comparison just a few years of observations around 1995, because they are not representative for the long-term climate variability. The main goal of the paper is to define the most substantial biases in the model, the magnitude of these biases is rather large and exceeds usually the magnitude of the trends in the atmosphere. As you can judge from Figure 4 the model biases exceed 5K, while the observed trend is up to 1.8 K/decade. Moreover, the trend is partially compensated because we consider several years before and after 1995. However, following the reviewer advise we have change the time span for the observation data and used only the data for the last decade of 20th century. We agree that for more accurate validation it would be better to use transient simulations and we have plans to validate the model using the results from our transient runs, which are now in progress. It would be interesting to compare the results of different validation schemes.

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

We have eliminated URAP expansion.

"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."

We have change the data set. Now, we are using merged TOMS/SBUV data set covering 1991-2000, which should be more representative for comparison with the simulated climatology.

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“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)?”

The bottom panels of Figure 1 and the top panels of Figure 3 are not the same data. In Figure 1 the observations are from URAP, while in Figure 3 the observations are from composite data set. The composite of the observation data has top level at 1 hPa, therefore in Figure 1 and 2 we decided to illustrate the model performance in the mesosphere.

“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.”

We are showing the climatology repeated twice, not “one year repeated twice” . It has been done to show the transition periods more clearly. If we show only one year starting from January then the transition from December to January will be not clearly visible.

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

We have changed two bottom panels in Figure 6.

“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_thetaeqlat.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 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."

Thanks for your advise. Your experience is an additional argument to use several independent data set for the detection of model deficiencies.

"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.)"

We have reformulated this paragraph.

"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?"

We are using merged TOMS/SBUV data set covering 1991-2000.

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

We have changed Figure 14. Now it is Figure 15.

"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?"

The difference is not significant. We have change this sentence.

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

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We have changed this paragraph.

“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.”

We did not analyze transient run here, we still use the results of steady-state run.

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

Yes, the photolysis rates are calculated for the spectral range 120-800 nm.

“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.”

We have changed these figures adding the statistical significance of the obtained signal. From Figure 16 (now 17) it is clearly seen that the difference of the zonal wind in the tropical stratosphere is rather small, which means that the results are not biased with respect to QBO.

“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."

The text of the paper was screened by native English speakers and several mistakes (including all mistakes pointed by the reviewer#3) have been corrected.

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