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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variability in hindcasts of atmospheric chemistry: the role of meteorology" *by* P. Hess and N. Mahowald

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The authors wish to thank the reviewer for their time and effort in reviewing this paper and for their helpful comments.

Below are the referee's comments and our responses. Responses are proceeded and followed by a '*'

- The paper analyses two hindcast methodologies, and seems to suggest that there are certain advantages of forcing the climate model CAM with SST. There are some indirect statements on better comparison with observations, but really in the paper there is hardly any solid evidence that this is the case. I think either the authors should include this evidence in the paper, or leave out unsubstantiated statements.

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Interactive Discussion
Discussion Paper

* The paper points out that the long-term trends from the NCEP simulation and the CAM simulation are not the same for a number of variables (Table 6). In particular, the CAM simulation has a significant change in temperature and precipitable water vapor between 1979 and 1999 while the NCEP simulation does not. Clearly there can be little dispute about the observed temperature trend during this period. Ross and Elliot (2001) show that water vapor has increased at most stations in the N.H. between 1973 and 1995 and Trenberth et al. has shown clear increases between 1988 (mistakenly written as 1998 in the text) and 2003. Thus we believe an increase in precipitable water between 1979 and 1999 is consistent with observations. We show in the paper that trends in a number of chemical variables are related to meteorological variables, in particular temperature and water vapor. Thus we believe that the CAM simulation offers some advantage over NCEP if one is looking at global changes related to temperature and water vapor. We believe the evidence for this is good, is included in the paper and is substantiated. We have included the reference to Ross and Elliot (2001) in the revised version of this paper.*

- Atmospheric chemistry focuses on gas phase only- possible feedback between aerosol and gas phase are not discussed.

The referee is certainly correct that the feedbacks between aerosol and gas phase chemistry are obviously very important. However, in this paper we have simplified the problem to only examine the gas phase chemistry. It is outside the scope of this study to add an additional aerosol component. We point out this limitation in the revised version.

The paper focusses on monthly and annual averages, an analysis of extremes would have been very useful.

We agree with this comment. However, this is also outside the scope of this initial work.

Maps for figure 1,2,3,4 are missing colorbars: hard to check the numbers.

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Interactive Comment

Fixed

Simulations are called SNCEP and SCAM; perhaps you don't want to call your latter simulation like this :)?

Replaced with NCEPC and CAMC.

Abstract: p. 3864 l 1 I find the reference to two 40 years datasets and subsequently using only 20 years is a bit confusing. Also in the paper you give not a lot of evidence that the discontinuity is really artificial. Why not just saying: we use the last 20 years of the 40 years re-analysis ... etc. Also it is only strictly true for the SNCEP re-analysis, and not for the SCAM.

We wish to emphasize that even though 40-year datasets are available, they do have artifacts which make them difficult to interpret. We do show the entire 40-year times series. We slightly modify the sentence in the abstract to read: "Analysis of these simulations focuses on the period 1979-1999, due to meteorological discontinuities in the NCEP reanalysis during the 1970s." This seems clear to us.

l. 10 CAM3 and trends; see above. Were in the paper is this analysis made? It is really tricky to compare trends with some cited values in the literature, since often different periods and methods are used for calculations

We have changed this to read: "The meteorology using CAM3 captures observed trends in temperature and water vapor; the simulation using NCEP meteorology does not". We agree that a comparison of trends is tricky. However, we believe studies on the observed trends in temperature and precipitable water are complete enough to generalize to the 1979-1999 period. See discussion above.

l. 13 J-NO2 as proxy for cloudiness? Why not just analyse cloudiness?

J-NO2 is used for a proxy for cloudiness because: 1) it effectively integrates in a chemically relevant manner over all cloud types 2) To save storage space in these long simulations we did not save all the relevant cloud variables.

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l 20: Global analysis?

Yes. Clarified.

p. 3865 l. 13 OH variability can not be observed- it can be derived.

OK, we took reference to interannual OH variability out.

l. 21 interannual variability for me means several years (15-20); interannual differences?

Thank you, differences is better.

l. 26-28 don't understand this sentence: variability comparable to trend?

I have clarified this sentence to read detrended variability.

p. 3488 l. 17 observed repeated twice

Thank you.

p. 3488 l. 3 as I understand it these numbes are NCEP model derived trends?

These numbers are the observed trends from work by Trenberth et al, and global temperature records (e.g., the IPCC).

p. 3488 l.7 there are several 40 years re-analysis products, would be good to mention them, and motivate why NCEP was chosen.

We have listed the ECMWF and DAO products in the revised text. We did not have the resources to run these simulations using all the reanalysis products. We rather arbitrarily chose to use NCEP.

p. 3488 l 17 It would be good to give a short overview of currently applied methods to use re-analysis products. Motivation can also be that often future climate simulations are based on SST forcing.

*We are not sure about what the referee means by "currently applied methods". We

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did include the point that GCMs must be used in future simulations.*

p. 3489 l. 10 the expeted dependency on meteorological variability of trace gas constituients.

Corrected.

p. 3489 l.22 "the paper is organized as follows": double paragraph.

Corrected.

p. 3490 l.10 I guess you mean chemical schemes are the same (chemistry can change).

Corrected.

p. 3490 l. 16 would that make a difference for the statements made here? I can imagine that convection or lightnign would depend on the spacing of vertical levels.

We have noted in the text that: "This may have subtle impacts on the simulations, including the simulation of convection, lightning NOx emissions, and boundary layer transport."

p. 3490 l.24 tell better what surface fluxes are prescribed.

We have changed the text to read: "The moisture is prognostic in the model, except for the surface latent heat flux which is input from the driving meteorological fields"

p. 3490 l. 26 perhaps here "give" away already that there are very small variations on LNOx (??)

We prefer to discuss this point later.

p. 3491 l. 2 this is of course a weakness; since multi annual feedbacks through stratospheric ozone (influx; O3 photolysis) are important

*We agree that interannual feedbacks through stratospheric ozone are important. I am

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not sure I would call this a weakness as this paper, does not try to include all processes which might be important.*

p. 3492 l. 9 overhead cloudiness, why not analyse it directly

See discussion above

p. 3493 l.20 Discussion of Figure 1 is rather poor, there are some qualitative statements on the possible reasons but I can not conclude from this what is finally making the difference between SCAM and SNCEP. Does Fig 1 proof that "the upward an poleward orientation of this minimum suggest this feature involves the quasi isentropic transport by mid-latitude eddies"?

This figure is intended to provide an initial qualitative overview of some of the differences between the simulations. We hypothesize explanations for several features but have not investigated in detail. We use the word "suggest" to show that we do not have a proof. Further research is needed to understand the difference in these simulations in detail. However, this is outside the scope of the current paper.

p 3493 l. 23 and O3 Interannual Variability have a pronounced ..

corrected

p. 3494 l.1 Suggested title: Midtropospheric interannual variability at 500 hPa

We have changed to: Surface and Midtropospheric Interannual Variability

p. 3494 l.10 unsubstantiated statement

This statement is prefaced with "may" which suggests a plausible, but by no means proven explanation.

p. 3994 section 4.2 why was 500 mb chosen? Figure 1; shows quite some variation of the maxima with height..

*Figure 1 We wanted to show latitude-longitude cross sections at the surface and in

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the middle troposphere.*

p. 3495 orthogonol=>orthogonal

corrected

p. 3495 I think the use of EOF analysis and the expected outcome can be better explained (there may be a lot of people not so familiar with it).

*We have added the sentence: EOFs are used to separate the space and time variability of a variable into a number of orthogonal modes. There is considerable literature on it and for a better understanding the reader will need to consult the literature.

Explain better what is meant with Gaussian weights. You always use the full model domain or rather a cut off at high latitudes?

We have eliminated the reference to Gaussian weights and now simply state the fields are area weighted. We use the full model domain.

p. 3495 l 26 explanation?

We have modified the sentence to read: "The North Atlantic Oscillation (or the related Arctic Oscillation) is a dominant mode of N.H. extratropical meteorological variability characterized by north-south fluctuations in atmospheric pressure within the Atlantic Basin"

p. 3496 It would interesting to see in Figure 5 also the HNO3 trends; since HNO3 is the only one which correlates with NAO

We agree, however, it is impossible to present everything and thus ended up selecting some of the other variables.

Similarly JNO2 trend starts decreasing in the mid 97s (before ENSO); while Table 1 indicates the highest correlation 0.84 between JNO2 and ENSO. Consistent?

The correlation is based on more than one ENSO event.

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p. 3497 l. 9 OH and CH4

Corrected.

p. 3498 l. 12 precursor emissions

We prefer to leave it just as "emissions" as many of the relevant emissions are not really precursors to OH.

l. 13 10 % variability- indeed derived from inversions of Methylchloroform. Much of the discussion and controversions around OH trends and variability is associated with the uncertainty of MCF emissions (see Bousquet): if you would assume larger uncertainty in MCF the variability of derived OH goes down.

We prefer not to enter into the discussion on the uncertainty of MCF emissions. I believe the discussion is correct as it stands.

p. 3498 Would be good to have in Table 3 gives also serate NH, and SH analysis, like in T. 5

The split between tropical and global is consistent with Figure 5. We prefer to leave as is.

p. 3500 l.10 trends of what?

The text states: "Trends in the globally averaged variables in SNCEP and SCAM are given in Table 6. The global long-term trends are significantly different between the two simulations". "trends" in line 9 refers to the variables given in Table 6.

p. 3500 l.12 temperature trend of 1 % per year seems impossible. What is meant here?

The line should read 0.1% C per year. .01 degrees/year [Table 6]/ 12 degrees [global average] = ˜ .1%/year.

p. 3500 l. 26 how much variation in NOx emissions, and does it matter for anything else in the simulation?

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Table 2 shows Lightning NOx emissions are within a factor of 2 with considerable more variation for NCEP. These emissions can play a relatively large role in upper tropospheric chemistry.

p. 3501 l. 17 suggest not talk about interdecal =>interannual

Actually, we do mean interdecadal variability here; large long timescale oscillations are evident in the timeseries.

Table 6 section 5.4 it is not very clear to me, what period you are discussing here? 40 years of 20 years. Anyhow, how can the temperate change of 0.011 degrees C/yr (40 years 0.44 be reconciled with the 0.6 mentioned earlier); or if you discuss only 20 years even worse?

All analysis is based on last 20 years of the simulations as stated in the paper. The 0.6 degree temperature change mentioned earlier is since 1960 (as stated in the text). A 0.44 degree change taken from drawing a linear fit through a timeseries is not too different from the ball-park figure of 0.6 given in the introduction.

The earlier discussion of percentage changes is also confusing.

I am not sure what is confusing here? The percentage change is simply the absolute change per year divided by the mean.

Section 5.4 Mention consistently all uncerainty ranges of trends for better comparison with other studies.

We have now included the uncertainty range in the precipitable water.

p. 3505 the analysis of sensitivity to climate variables is interesting, I would actually expect larger deviations between OH and climate variables, since it is assumed that there is no feedback between O3 and Q and other variables.

Yes, I would not have been that surprised to see larger differences.

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p. 3509 l. 26 climate models constrained by SST may do be better. You are probably right, but I think the real proof (observations) can be done better.

I think the referee is suggesting that we compared with local observations. For simplicity, we have kept the analysis on the global scale which simplifies interpretation. Even on the large scale we show that NCEP reanalysis does not capture some of the observed trends with global chemical ramifications. Other meteorological analysis might give different results. I think we have shown that rather conclusively that in some cases simulations driven by observed SSTs might do better than meteorological analysis.

Some furhter minor points:

3492 l. 7 modifies water vapor pressure l.21 Wilks 2007=>2006

Corrected.

p. 3501 l 6 0.26 %=>0.24+/- 0.06 % year-1 S1154

Corrected.

p. 3517 Can you explain better was is menat with 252 samples and 21 samples?

There are 252 monthly samples for ENSO and time correlations, and 21 annual samples for the NAO correlation.

p. 3522 Table 6 change with year? per year? some units have a time unit,others not. Please check?

We simply report the trend. The interpretation is that on average CO, for example, changes by -3.24E-05 ppbv in one year, the rain rate, for example, changes by 1.4e-03 mm/day in one year.

p. 3526 Legenda Table 10: units are not clear change per K; or percentage change per percentage change in T

*The caption reads: "Percent change in variable with percent change in temperature.";

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We should probably state that percent change in temperature is measured in K.*

p. 3527 figure 1 is this yearly or monthly? If monthly which month. Figure 1-4 explain colors.

It is annual. The colorbar got clipped accidently. We will make sure it is included in a revised version.

p. 3532 Fig q-r ppm=>ppb?

Will correct.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 3485, 2009.

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