

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

Anonymous Referee #2

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This is an interesting study, describing two ways of performing hindcasts. This is a relevant topic, and should be interesting for a large community. However, the paper also has some important limitations that are should be better discussed in the paper. I have also a large number of minor comments.

While I think that the study should definitely be published, the paper would need some improvements in order to be acceptable for ACP.

Major comments:

- The paper analyses two hindcast methodologies, and seems to suggest that there

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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.

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

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

Detailed comments:

in general: Maps for figure 1,2,3,4 are missing colorbars: hard to check the numbers. simulations are called SNCEP and SCAM; perhaps you don't want to call your latter simulation like this :)?

Abstract:

p. 3864 | 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.

I. 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

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

I 20: Global analysis?

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

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

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

p. 3488 l. 17 observed repeated twice

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

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

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.

p. 3489 l. 10 the expected dependency on meteorological variability of trace gas constituents.

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

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

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

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

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

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

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

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p. 3493 I.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"?

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

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

p. 3494 I.10 unsubstantiated statement

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

p. 3495 orthogonal=>orthogonal

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

p. 3495 I 26 explanation?

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

p. 3497 I. 9 OH and CH4

p. 3498

p. 3498 I. 12 precursor emissions

I. 13 10 % variability- indeed derived from inversions of Methylchloroform. Much of the

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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.

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

p. 3500 I.10 trends of what?

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

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

p. 3501 I. 17 suggest not talk about interdecadal =>interannual

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? The earlier discussion of percentage changes is also confusing.

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

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.

p. 3509 I. 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.

Some furhter minor points:

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

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

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

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

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

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

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

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