Atmos. Chem. Phys. Discuss., 5, S1147–S1151, 2005
www.atmos-chem-phys.org/acpd/5/S1147/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term changes and variability in a transient simulation with a chemistry-climate model employing realistic forcing" *by* M. Dameris et al.

Anonymous Referee #1

Received and published: 13 June 2005

This paper is generally well-written and should be of interest to the community. It presents an informative look at some of the results from transient simulations with the E39/C CCM. The paper would benefit from a clearer statement of the emissions changes used to drive the troposphere, namely the lack of changes in the ozone precursors CO and non-methane hydrocarbons. It should also be made clear that there are serious limitations in assessing how deterministic transient behavior is when using a model simulation driven by observed SSTs which themselves may include a substantial non-deterministic signal from internal ocean fluctuations. Finally, the section of the response to the solar cycle requires a proper analysis using regression tech-

niques rather than simply looking at a few years by eye. Detailed comments below address these issues more thoroughly. Subject to these revisions, the paper should be published and will provide a nice addition to the CCM literature.

Abstract: The end of the sentence noting that the 1986 and 1988 Antarctic ozone losses were different should say "smaller than in other years of that decade" rather than "of the respective decades".

The abstract also says "no stratospheric warming was found for at least 6 years". Since this is a model study and all information about what it did should be perfectly well-known, please just give us the number of years rather than "at least 6".

2300, L25: The question of how deterministic the model's behavior is cannot be reliably answered with the current experimental setup using observed SSTs. There is no way to ascertain to what extent the SST patterns over the past 40 years were a deterministic response to external forcing and to what extent they resulted from internal variability. This is an important caveat to the interpretation of the results here, which simply show how the model responds when driven by observed SSTs. A true ensemble simulations with a coupled model would be required to get an idea of how deterministic the ECHAM model's response is. The paper should reflect this.

2301, L26: "gasphase" should be "gas-phase".

2303, L10: The emissions prescribed are given as GHGs, and NOx. This was mentioned earlier in the abstract as well (2298, L9). What about other ozone precursors in the troposphere? The emissions of CO and hydrocarbons should have varied as well, e.g. as in van Aardenne et al, GBC, 2001.

2303, L14: I presume that the monthly mean SST fields were interpolated to finer timescales (e.g. daily) rather than jumping at the first of each month. It would be useful to say that a linear interpolation based on monthly mean values was used, if that's indeed the case.

ACPD

5, S1147–S1151, 2005

2304, L12: I don't understand why this version of the ECHAM model could not calculate its own heating rates in response to volcanic aerosols? The setup used is less than ideal, with rates taken from a different experiment but only covering two years or so and various assumptions for the rest of the time periods. The use of those two Pinatubo years to represent other eruptions also seems odd, as they would not necessarily have caused a similar response (despite the Labitzke reference, which just looks at temperatures. The real world includes unforced variations as well as forcing, so examination of only a few observed eruptions doesn't adequately demonstrate that all eruptions, even if of similar size, show similar responses. This setup is rather unsatisfactory, without and explanation for why it wasn't done properly. Given that the aerosol distributions were put into this run anyway, why not just calculate the heating rates they induce?

2305, L15: The solar flux variations given in Table 2 would be easier to interpret if the difference column was in percentage units. If there is really no variability at all in the longer visible wavelengths, that would not agree with observations which have around 0.1% there. This should be addressed, but maybe it's just a question of changing to percent.

2305, L6: As I pointed out earlier, not only is CO constant, but so are all the other hydrocarbons. It should be noted that this neglecting increases in these gases will cause some underestimation of tropospheric ozone changes (most likely, though isoprene could cause the opposite in some cases), and an overestimate of OH values.

2306, L13: The description here implies to me that the Benkovitz et al geographic distribution was used for all years with an IPCC trend in total emissions imposed. This would neglect all the differences in development between different global regions. For example, the tropical countries have increased emissions much more rapidly in later years, while those in NH mid-latitudes countries have instead slowed. Is this really what was done? If so, this limitation should be pointed out, and it's a substantial one.

2307, L6: The biomass burning NOx emissions are said to increase at 0.3% per year.

ACPD

5, S1147–S1151, 2005

The citation is IPCC 1999, but citing an entire report for a single value makes it nearly impossible to verify this number. Please either give the page within the report or cite the chapters (as IPCC and WMO request) rather than the whole report. I tried to look this up because this number seems quite small to me. Looking at the van Aardenne inventory mentioned above, the rate of increase is more like 1-3% per year, about an order of magnitude larger. Can the authors please comment on this discrepancy?

2312, L15: It's not surprising that the middle tropospheric temperature trends are underestimated in the model, as so many tropospheric ozone precursor trends were not included, nor were black carbon aerosols.

2312, L3: The strengthening of the tropospheric jet is consistent with observations, however the weakening of the westerly zonal flow near the surface at northern latitudes from about 50-60 degrees is not (e.g. Shindell et al., JGR, 2001).

2313, L24: "do" should be "does".

2315, L27: This line seems to say that the model does not include methane oxidation as a source of water vapor. Is this true? Since this is a chemistry-climate model and includes methane, I don't understand why this would not be included? Please explain. Also, this should have been discussed in the chemistry description, which to me implied that methane chemistry was included everywhere in the model.

2316, L20: It's not possible with the experimental setup used here to assess the role of external forcings separately from fluctuating SSTs, as the SSTs are prescribed and run along with the external forcings. This sentence is misleading.

2322, L10: The authors cite WMO 2002 as showing 3% column ozone variation over a solar cycle from Fig 4-5. This is an odd figure, which shows the total ozone variation in the middle panel, and then the residual after removing the solar signal in the bottom panel. However, it never shows the ozone-solar regression itself. Nevertheless, since the values of the total variation are about 3%, and the bottom panel shows a fairly large

ACPD

5, S1147–S1151, 2005

residual left over after the solar portion is removed, it's clear that the solar component must be much smaller, probably around 1%. This value is more consistent with direct analyses as well.

2324, L12: Not only does the solar analysis rely on just a few parameters, there is no attempt to extract a the signal that is correlated with solar variability. The analysis method of looking at a couple solar minimum or maximum years rather than performing a straightforward regression with 10.7 cm flux is far too simplistic, and the results of this section are therefore extremely weak. This should really be done properly or left out, and since it's not difficult to do a regression, I can think of no good reason why this wasn't done.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2297, 2005.