Geosci. Model Dev. Discuss., 4, C1043–C1048, 2011
www.geosci-model-dev-discuss.net/4/C1043/2011/

Geoscientific
Model Development
Discussions

Interactive comment on "CAM-chem: description and evaluation of interactive atmospheric chemistry in CESM" *by* J.-F. Lamarque et al.

Anonymous Referee #5

Received and published: 10 November 2011

CAM-chem: description and evaluation of interactive atmospheric chemistry in CESM

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The paper gives an overview of the atmospheric chemistry component of the Community Atmosphere Model (CAM-Chem). The paper consist of two parts: (i) an overview of the chemical schemes and the chemistry-specific parameterisations and (ii) a comprehensive evaluation of the performance of CAM-Chem. For the evaluation CAM-CHem is compared against ozone-sondes, ozone-total columns retrievals, air-craft-observations from INTEX A & B and global surface CO and aerosol measurements. The evaluation is carried out for three different model configuration, which use either on-line simulated meteorological data or off-line data from the GEOS-5 and MERRA

meteorological analyses.

General Comments

The paper is a well-written documentation of the current status of CAM-Chem and provides a good general evaluation of the simulation results. The evaluation assures the reader that CAM-Chem simulations are in-line with result from other global state-of-the-art chemistry models.

The paper remains a bit too vague on the impact of different meteorological data set on the chemistry results, which is clearly one of the main points of the study since the authors present the results for the three different meteorological data sets. The differences between these data sets need to be pointed out more clearly in order to better understand the differences in the chemistry. The reader wants to know how do the off-line data sets (MERRA and GEOS-5) differ from the on-line meteorology and how big the differences are between the two of-line data sets. Referring to earlier papers on climate simulation with CAM (Lamarque et al. 2008, Lamarque and Solomon, 2010, Neale et al. 2011) in section 7 is not sufficient to answer these questions. It would beneficial to discuss the actual differences in temperature and humidity (perhaps a plot complementing Fig. 3.) Cloudiness and lightning activity might also be interesting to look at. Showing derived transport "diagnostics" such as Rn cross-sections would demonstrate the differences in the large-scale transport and convective activity.

Besides having a coarser vertical resolution, the off-line run applies a different wet deposition scheme (Neu et al., 2011), simulates stratospheric chemistry and is carried out for an earlier period (1991-2000) than the off-line runs. These additional differences should be better included in the discussion.

An extended conclusion on the pros and cons of off-line vs. on-line meteorological data, which refers to the literature and discusses also more technical aspects such computational cost, mass-conservation and artificial mixing etc., would improve the paper.

Figures 3, 4, 5, 10 and 11 should be enhanced because it is very difficult to distinguish the individual lines and labels. Figure captions should refer to the labels and colours used in the graphs.

I recommend publication in ACP of this highly-welcome and interesting paper after revisions.

Specific comments

Spell out CESM in title and consider changing it to "Description and evaluation of CAM-Chem: interactive . . ..)

P 2201, line 18: This is not clear. Other CTM also conserve tracer mass, the CAM-Chem advection does not completely conserve mass otherwise there would be no need for a family advection (see section 3.5)

P 2201, l18: add reference for CMT

P 2202, l12: typical model resolution, top height, coordinate system etc. should be mentioned in this section.

P 2203, l1: delete "a"

P 2204, l2: What is the sign of the precipitation biases

P 2205, l3: spell out LAI or delete

P 2205, l15: clarify meaning of Xi

P 2207, l5: The description of the "Neu" and "Horowitz" schemes should be complemented by a discussion on their impact on concentration fields. According to Neu et al. (2011) there is an impact on tropospheric ozone chemistry when using a improved approach for cloud overlap and scavenging in ice particles. It would be interesting to know if this can be confirmed with the presented CAM-chem simulations.

P 2207, l10 : Why is the scaled annual total a range and not one single number?

Provide reference for the choice of value which is a below the most common value of 5 Tg/y. Is the inter-annual variability of lightning activity accounted for?

P 2208, l18: Give exact number for UV limit of photolysis rate simulation.

P 2208, l20: Is the prognostic ozone below 40 km used in the photolysis scheme?

P 2210, l18-25: This amount of detail might be too much, consider shortening paragraph.

P 2210, l28: Give more background on the technical aspects of the MERRA and GEOS-5 data such as original resolution, assimilated observations or model versions.

P 2211, l15-20: Since the impact of chemical solvers is not discussed, this paragraph could be omitted.

P2213, l3: Why only 26 levels. How do the 26 level relate to the 56 levels.

P2213, l8: Please motivate the choice of the simulation period.

P2214, l3-6: This amount of technical detail could be shortened.

P2214, l24: correct typo "Not"

P2215, l3-5: These reference prove the validity of the CAM result but do not help to explain the differences between the off and on-line data sets used in this study.

P 2215, l23: Vertical resolution might be one reasons but meteorological fields may also differ between on-line and off-line. Further, the on-line run uses a stratospheric chemistry which may lead to biases independent of the dynamical features. Chemical aspects (OH) may also play a role.

P2216, l10: Why is MERRA more prone to more stratospheric mixing than GEOS-5.

P2216, l25:. Dry deposition might be also important for biases in surface ozone.

P2217, l6: The on-line flux of 410 Tg/a is at the very low end of published values. How can the low on-line flux be reconciled with the finding of a positive tropospheric ozone bias of the on-line run (p2215 line 23) , which was attributed to problems in strat-trop exchange. Likewise - the good agreement in ozone life time.

P2217, l18:. It would be interesting to see how “unrealistic” the on-line run actually is.

P2218, l25: It is not clear from section 2 that CAM and MOZART-4 use the same PBL parameterisations apart from convection. If PBL ventilation plays a role diffusion scheme and vertical layer depth may be further candidates to explain the differences. Again, dry deposition could be different.

P 2219, l13: Please also mention the CO overestimation in the NH subtropics. This could be a problem in emissions or convective transport.

P 2221, l1: Figures 11 and 12 are not discussed. The differences between HALOE and ACE seemed to be very large. Please explain .

P2221, l18: Please add “surface” before aerosol

P2222, l7: Also consider the relatively coarse horizontal resolution (ca. 200 km) of the model. Peak concentrations are not likely to be captured.

P2222, l23:“performs equally well” – this is a very general statement. Please point also to the differences.

P2222, l27: "separate" i.e. different chemical mechanism have not been discussed in the paper. In the on-line run a stratospheric chemistry scheme was added.

P 2241, caption Table 4: Please clarify units, T, k, r and γ

P 2258, caption Fig.3: Please mention averaging period, make labels more readable, mention colours in caption text.

P 2261, caption Fig 4: mention ozone in caption.

P 2272, capture Fig 9a : should not refer to 9a, change figure order

Interactive comment on Geosci. Model Dev. Discuss., 4, 2199, 2011.