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Interactive comment on "Aviation 2006 NO_x-induced effects on atmospheric ozone and HO_x in Community Earth System Model (CESM)" *by* A. Khodayari et al.

Anonymous Referee #2

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The manuscript by Khodayari et al. reports the results of calculation of the impact of aviation NOx emissions using two versions of the NCAR CAM model using identical chemical mechanisms. The model is based on 2006 aviation emissions. Results are presented for the calculated ozone perturbation, HOx changes, and associated radiative forcings.

General Comments:

This a fairly pedestrian paper comparing results of two similar models. Results from the two models are discussed but the reasons for the differences do not seem to be well thought out. The differences in the two models are presented as due to the different aerosol treatments but the authors do not build a strong case that the other differences between CAM4 and CAM5 are inconsequential for the aviation NOx impact. In the discussion of the detailed chemistry, the authors only discuss the gas phase reactions. There is no detailed discussion of the aerosol processes which might support their thesis. If the purpose of the study is to evaluate the reasons for the differences between the two very similar models, then the authors need additional analyses and perhaps additional model runs.

The description of the comparison of the modeled output for a number of the chemical species with observation at "within the central 50% and 90% of available observations" seems very clumsy. The authors seem to believe this is good agreement. This reviewer is skeptical of that conclusion and not quite sure of what to make of that statement. The authors should consider better ways to present quantitative comparisons with observations. It is generally accepted that all models have some weaknesses and the authors should not be afraid to show the challenges as well as the successes.

Specific Comments:

Page 6166 lines 27- page 6167 lines 1-3. The authors state that the major difference between the CAM4 and CAM5 models was the treatment of aerosol chemistry. This is misleading since CAM5 also used different treatments of convection, dynamics, and radiation. Later in the paper, the authors fail to quantify the importance of the aerosol chemistry for this problem. If CAM5 can be run with the bulk aerosol treatment used in CAM4 and shows a similar result to that of CAM4, their claim would be much more persuasive.

Page 6167 lines 13-15. The NCAR site shows that there are 5 official releases of CAM5 but the authors seem to be using some beta version. The authors should identify which of those releases is closest to the beta version used in this paper and justify why they are not using one of the official releases.

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Page 6169, lines 3-5. The authors should explain why they are still using zonal mean clouds from ISCCP in their radiative forcing calculations when cloud fields are available that would be self consistent with the meteorology in CESM.

Page 6169, lines 10-11. The authors describe the total number of vertical levels but should also state the vertical resolution near the tropopause, which is important for this problem.

Page 6170, lines 3-18. The authors describe their evaluation of modeled ozone versus measurements but do not address a number of critical tests for studying NOx perturbations in the upper troposphere – How good is the background NOx and NOy? Is the NOy partitioning accurate or are some species (e.g., PAN) wrong? How does the modeled HOx precursors in the UT compare with measurements? Just evaluating the background ozone is not enough.

Page 6171, line 21 – the authors state that in some places the "estimated ozone is very accurate" while noting in other places that it is not very good. Can the authors be more quantitative?

Page 6172, lines 7-16. The authors report that in the 4-8 km altitude range (i.e., below the altitudes where the aircraft mostly fly) the simulations fall in the range of 50-90% of the observations. That doesn't sound like particularly good agreement to this reviewer although the authors seem to imply that is is. The fact that the models agree with each other when they assume the same chemistry, the same emissions, and the same boundary values does not reveal anything about the accuracy of the model.

Page 6173, lines 1-20. If the aerosol treatment is the key factor in explaining the differences between the two models, please add those processes to the mechanism discussion.

Page 6174, lines 13-26. Results are presented but not a discussion of the processes that explain the differences. For example, why is CAM5 more efficient at producing

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ozone than CAM4? Is it a transport effect, a radiative effect, or ???

Page 6177, line 16. The authors should clarify that the change in mean ozone column is an increase.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6163, 2014.

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Interactive Comment