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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The impact of orbital sampling, monthly averaging and vertical resolution on climate chemistry model evaluation with satellite observations" *by* A. M. Aghedo et al.

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We would like to thank the Anonymous Referee #1 for the comments. We provide our response below each of the comments (now italicized):

General comments
This paper quantifies the uncertainties associated with the technical steps required for using nadir sun-synchronous satellite observations for multi-model evaluation. In particular, the impacts of orbital sampling, monthly averaging of the averaging kernel (AK), and the satellite operators are evaluated with two chemistry climate models (ECHAM5-MOZ and GISS-PUCCINI) and TES observations for ozone, CO,

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atmospheric temperature, and water vapor. It is an interesting paper and the questions brought up here are ignored by many previous studies comparing model outputs with satellite data. The main problem undermining this paper is the overstatement of the results, which could be solved by additional work. Before publishing in ACP, the following questions and comments should be considered and answered.

Specific comments
1. What is the impact of clouds? Nadir infrared satellite observations are inevitably affected by clouds. Especially for those instruments like TES, with sparse sampling footprint, clouds are likely to have large influence over cloudy regions (e.g., ITCZ). As shown in Fig. 2 and 3, TES can at most have four measurements at given grid boxes over tropics for one month, if one or two (or even more) of them are contaminated by clouds (which is probably true for tropics), can the remaining data still capture the monthly mean value? Also, the C-shape profiles should be removed for TES ozone (see user guide). What is the influence from the data screening? Besides the steps covered by the paper, the data screening process is also a required step for making comparisons. It is worth adding it, as the goal of this paper is to quantify the uncertainties of these required steps.

We agree with the reviewer that clouds reduce the sensitivity of the retrieved profiles to the atmospheric state though the 5x8 km footprint increases the number of less cloudy scenes. The reduction in the sensitivity due to clouds is accounted for in the averaging kernels (Kulawik, S. S., J. Worden, A. Eldering, K. Bowman, M. Gunson, G. B. Osterman, L. Zhang, S. Clough, M. W. Shephard, R. Beer, Implementation of cloud retrievals for Tropospheric Emission Spectrometer (TES) atmospheric retrievals: part 1. Description and characterization of errors on trace gas retrievals, J. Geophys. Res., 111, D24204, doi:10.1029/2005JD006733, 2006). When the averaging kernels are applied to model output, it accounts for the limitation in the vertical resolution of

TES due to clouds. We tested this sensitivity in Section 6, and found that for accurate comparison of models with TES observation (and observation from instruments similar to TES), the effect of clouds cannot be ignored, and satellite operators must be used to simulate the retrieval process in the models (as given by Equation 2). For example, if we have 4 cloudy observations in the tropics, then the sensitivity below the clouds would be low, and this is shown in Fig. 6, where we found that ignoring the satellite operators could lead to high biases, especially in the tropical region. We stated this in our conclusion on Page 9723, lines 20 – 24. We did not claim to capture all of the actual atmospheric variability, particularly on a per grid box level, because of TES sampling and vertical resolution. However, we can characterize the impact of this limitation on our analysis, and this is what we have shown in Section 6. TES sampling captured the monthly zonal-mean concentrations of the species, subject to the condition that the observation operators are applied to model output (Page 9723, line 29 – Page 9724, line 1). We now explicitly state in Sections 3, 6 and 7 that that averaging kernels account for limited vertical resolution due to clouds. We have now added a new sub-section 4.2 to discuss the influence of selecting only good data (i.e. data screening). Note that 3 sets of new figures are included.

2. Many conclusions in this paper are exaggerated. Some of them may not be wrong, but cannot be fully supported by the results. For example, p9716, l18-20, the altitude by latitude plots of monthly mean errors (Fig. 4a and 4b) can only tell if the zonal means and zonal distributions are consistent. It does not provide information about horizontal distributions and the variance as well. The authors, however, conclude that ". . .adequately capture the magnitude, the distribution and the variability of ozone and. . .". In order to draw such conclusions, geographical plots of monthly mean biases and plots of variables, such as the standard deviation, are required. The model and observations can have very different variability with similar mean values. In addition, the conclusions about monthly means are generally for zonal monthly means, as the analysis is mainly done with zonal differences on monthly scale. It should be

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expressed explicitly to avoid misleading.

Luo et al, JGR, 107, D15, 4270, doi:10.1029/2001JD000804, 2002 uses the GEOS-Chem model output to demonstrate the capabilities of TES nadir observations, and to provide a guideline for the observation strategy. They showed that the interpolated daily global maps generated from the TES synthetic data (sampled from the original model time series) were comparable to the original daily-mean model output, within a spatial error that is less than 10% in more than 70% of the cases for ozone, and less than 20% for 80-90% of the cases for CO. In the case for our manuscript, we have focused on the comparison on a monthly-mean time scale, rather than the daily mean, in consideration for our objective to provide TES observational data that are useful for monthly to decadal multi-model evaluation. We now provide examples of the comparison of the geographical distributions of the sampled and the original model for ozone at 562 hPa in the GISS-PUCCINI model. The comparison show that on a monthly-mean time scale, averages computed from points sampled along the TES orbit capture the large-scale distributions due to transport pattern. The biases we calculated are less than the errors calculated by Luo et al., 2002 due to our longer time-averages. We showed in Figure 3 that sampled data has larger standard error, which are controlled by the small number of samples. We rewrote p9716, l18-20 to include "zonal-mean" and remove "variability".

3. P9710, l3: How much lightning NOx is emitted annually in the ECHAM5-MOZ model?

The lightning NOx emissions was about 6.7 Tg(N)/yr.

4. p9712, l2: TES only has varying layer thickness for the lowest layer above clouds.

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The rest layers have fixed thickness.

The lowest layers are more of a function of surface orography, rather than clouds. We have modified the sentence to “TES profiles have 67 vertical levels from the surface to 0.01 hPa, with varying layer thickness in the boundary layer following the orography. . .”

5. p9715, l15: with only four measurements for a month, it is even inadequate to capture weekly variations.

This sentence is replaced with “despite its limited number of sampling in a month”.

6. p9717, l8: on zonal mean ozone, carbon monoxide . . .

This is changed to “zonal-mean ozone, carbon monoxide. . .”. Example of geographical distribution is provided. See our response to comment 2 above.

7. p9717, remove equations (12) and (13) and change the text accordingly. These two equations only rewrite the third term of Eq. (11) in different forms and seem redundant.

We disagree that the equations are redundant. Equation 13 is a critical step that is necessary for Eq 14 to be a valid approximation of Eq 11. Moreover, Eq 13 underscores the importance that processes driving the variability of sensitivity need to be uncorrelated with processes driving the variability of the state, e.g., ozone. We kept the equations as they were to improve clarity for the readers.

8. p9718, l10: 1/nG is missing for the second term in Eq. (15).

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Done. Thanks.

9. p9723, l27: the zonal distribution. The synoptic scale variability? No result about variability is shown in the paper, except for Fig. 3.

By synoptic scale variability, we meant “ozone distributions due to large-scale transport pattern”. We now show the geographical distributions, which confirm that TES captures the distributions due to large-scale transport pattern. We replaced the phrase “synoptic scale variability” with “large-scale distribution pattern due to transport”.

10. Fig. 4-6: Are the results shown here the mean of all the months (2005-2008)? Note that TES misses observations for many months.

This is the result of all months in 2005 through 2008. TES only has June 2005 missing. We discussed missing observations in Section 4.2, where the influence of data screening is presented.

Minor corrections

All minor corrections are done. Thanks.

p9706,l16: satellite -> instrument. TES is one of the instruments on Aura satellite. p9707,l23: spatial -> horizontal. Spatial resolution includes both horizontal and vertical directions. p9709,l8: longitude (T42). T42 is used later. It is better to define it here. p9709,l14: includes

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p9710,l9: delete “modelE”
p9712,l12: a priori
p9714,l19: figure...shows or figures...show
p9715,l21: change “ECHAM5-MOZ (and GISS-PUCCINI)” to “two”. L17 already says the model names.
p9716,l11: make the “UTLS” consistent with that on p9719, l12.
p9718,l3: construct
p9719,l7: Fig. 5a and 5b?
p9722,l27-28: 2008 (Table 2). Delete “The summary ... models”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9705, 2011.

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