

Interactive comment on “Simulations of atmospheric methane for Cape Grim, Tasmania, to constrain South East Australian methane emissions” by Z. M. Loh et al.

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We thank the reviewer for their helpful comments. Each comment is addressed below with the original review in italics and our responses in normal font.

... The authors discuss the main differences between the model ratios and the observations, and attribute the discrepancies between the models and observation to the representation of wetland methane. But they do not mention differences between models and other specific mismatches, this leaves questions.

We have assumed that these 'questions' are those detailed below and that in address-

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ing those comments we have answered this more general point.

The manuscript is descriptive with the simulation results with the prescribed emission scenarios. It would have been nice to see additional model experiments to support their implications. From the comparison between the simulation results and observations, the authors conclude that the austral winter wetland methane emissions are overestimated, but suggesting the springtime maximum of wetland emissions. To support the idea, the authors show the satellite measurements (Fraser et al., 2011), but it would be more interesting and supportive to try to assess (at least partially) the sensitivity of seasonality in wetland emission to the simulated residual CH₄ concentrations, as well as the ratio of CH₄ and radon.

While we anticipate that a future study will further investigate our hypothesis of greater springtime methane emissions, we have performed an additional sensitivity experiment with CCAM in which we shifted the CTL fluxes (across the whole globe) later in time by 3 months. Since the only significant seasonality in the CTL fluxes for SE Australia is the wetland component, this gives an initial indication of the likely impact of moving wetland emissions from winter to spring. We find that this gives largest methane to radon ratios in September–November instead of May–August with slightly smaller magnitude (around 12 ppb/Bqm⁻³ instead of around 13 ppb/Bqm⁻³). This is a better fit to the observations although a further one month shift in wetland fluxes would likely give a further improvement and the early winter CH₄/radon ratios remain too high. This may be due to large methane fluxes in February which are just north of our region of interest and in this test get shifted to May. We have rewritten the first paragraph of Section 5 (now split into 4 paragraphs) to include this information (in the third paragraph) and added Figure 8 to show the methane to radon ratio seasonality for this new simulation.

2 Detailed comments

- Section 3.1: The authors state that the maximums of WLBB and EXTRA are in December and January due to biomass burning, as seen Figure 3. I wonder how much

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the annual wetland emission in WLBB and EXTRA and how the seasonality looks like, compared to the wetland fluxes in CTL and CTL_E4 and BB which has annual total of 1.24Tg/yr with high emissions for May to October. How about the CH4 emission from rice cultivation? How significant the rice emission among the total CH4 emissions and in the seasonality? The seasonal variation of wetland (and rice) CH4 emission is one of the key points in this study. These would be helpful to understand the results discussed in the later sections.

For the TransCom-CH4 model intercomparison, the emission scenarios were provided to modellers as the sum of component emissions. For the CTL case only, the individual components were also made available for analysis purposes and it was from this information that we were able to determine that wetland emissions dominated the seasonal cycle of CTL fluxes for SE Australia (while rice emissions are much smaller, approximately 0.13 Tg/y in summer and zero in winter). In theory the wetland component of WLBB should be deducible from WLBB - BB + CTL(wetland component). We have done this but the resulting flux shows a residual biomass burning signal (identifiable by its interannual variability), possibly due to a slightly different scaling being applied when each scenario was created, in order to match a global total flux. This makes it impossible to exactly determine the wetland and rice component fluxes in the WLBB and EXTRA scenarios although they appear to be close to zero for SE Australia or they would not be obscured by the residual biomass burning signal. This examination of the emission scenarios highlighted a couple of mistakes in the description of the emissions in Sec 3.1. These have now been corrected in the text and in Table 1.

- Table 2: The difference between WLBB and EXTRA is only rice CH4 emission. The inter-annual variation periods of wetland emissions are different between WLBB and EXTRA in Table 2? Why?

The period of interannual variation is correct in Table 2. The error was in Table 1 (and the text). The wetland emissions for EXTRA are from the VISIT model (as for rice emissions) and not from Ringeval et al as used in WLBB.

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- Section 3.2: The description on ACCESS is not clear. How different is the run for this study from the run for TransCom-CH4? Why this ACCESS run can be analysed on synoptic variations for statistical purpose, but not for individual events? It would be helpful to give more description of the ACCESS model, and to explain why the two models used in this study.

We have rewritten the paragraph describing the ACCESS model (now paragraph 3 of Sec 3.2) to give more detail about the model simulation and how it compares to the one submitted for TransCom-CH4 (only the horizontal resolution was changed). A sentence has been added to the first paragraph of Sec 3.2 to note why two models have been used (to determine whether the analysis is sensitive to transport model error).

- Section 4: "...exceptionally high methane concentration in January and February... (BB, WLBB and EXTRA)" But it seems inconsistent with Figure 4, where high concentrations in January and February can be seen in BB and WLBB and INV in (a)ACCESS, and BB and INV in (b)CCAM. EXTRA shows the lowest concentrations among the simulations by both model runs.

Sorry for the confusion. The text was correct but the EXTRA and INV lines in Figure 4 were incorrectly labelled. This has been fixed. The figure has also been extended to include 2002 and 2004 to provide some context for the large summer concentrations in 2003.

- Figure 4: BB and EXTRA are in similar colors. It is hard to distinguish.

Agreed. We have redrawn the figure using the same colour scheme as Figure 3.

- Section 4.1: the model experiments with injection levels of fire methane have been conducted. The results are described very briefly, but it is not clear. Does the simulation with fire methane emitted in higher model level give comparable amplitude to the observation?

The sensitivity tests are not directly comparable to the observations since they model

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only one component of the CH₄ flux (biomass burning). Nevertheless, they do illustrate how the amplitude can be reduced substantially when emissions are inserted higher into the atmosphere. An extra paragraph has been added to Sec 4.1 to describe the sensitivity tests in more detail including giving one measure of amplitude for comparison between cases.

- *Section 4.2: the discussion here is mainly regarding the difference of wetland emissions. But in Figure 7, the model runs with CTL_E4 show the difference each other, unlike the other simulations. So I would like to see some explanation on CTL-E4. How different are anthropogenic emissions between EDGAR 3.2 and EDGAR 4.0 for south-eastern Australia? I understand both are annually constant with no seasonal variation. If the difference is only increasing trend (seen in Figure 3b), CTL_E4 simulation results can be expected to be similar to CTL in this study which is focused on the non-baseline events? But the results of CTL_E4 are different from CTL, especially in ACCESS run. Furthermore, the seasonal cycles of both model ratios are not close to the observations, but the springtime ratios are more comparable to the observations than other emission scenarios.*

We have added a paragraph at the end of Section 4.2 to discuss the CTL_E4 case. The difference in winter to summer ratios is smaller with CTL_E4 (despite larger ratios overall). There are differences in the spatial distribution of fluxes between CTL and CTL_E4 and we think this is the most likely explanation for the slightly weakened seasonality. We have looked at the trend in ratio over time between the different cases to see if the increasing emissions from CTL_E4 are supported by the observations, but the large seasonal and interannual variability in CH₄/radon ratio means that we have little confidence in any calculated trend.

- *P.21205: The discussion on Wang and Bentley (2002) is not easy to follow. They suggested the large reduction of CH₄ emission in the region of interest in this study? In Section 3, however, it can read that the CH₄ emission estimated by Wang and Bentley (2002) is comparable or slightly larger than those by Fraser et al. (2011) and EDGAR.*

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This text was confusing because it was not clear that Wang and Bentley provide both an inventory based flux estimate and one based on an inversion (which is much lower than the inventory). We quoted the inventory based flux early in the paper but discussed the inversion based estimate here. We have hopefully removed this confusion by adding a table of flux estimates from different sources (as requested by reviewer 2) (Table 3) in Sec 3.1 and modifying the text in the second last paragraph in Sec 5.

- *p.21205, L24: Please spell out NSW.*

Done.

- *Figure 7, "tracers" should be "emission scenarios"? It is hard to see the bars of standard deviation for January and December. Their presentations need to be improved.*

The figure caption has been changed. The x axis has been extended to make January and December easier to read and the error bars for each curve have been offset slightly to avoid overlap.

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