

# ***Interactive comment on “Advances in representing interactive methane in ModelE2-YIBs (version 1.1)” by Kandice L. Harper et al.***

## **Anonymous Referee #2**

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### **Overview**

This paper describes advances to the NASA GISS ModelE2-Yale Interactive terrestrial Biosphere global chemistry-climate model (ModelE2-YIBs) and its use to optimise natural methane emission sources for the year 2005, through comparison of modelled and observed surface atmospheric methane concentrations. These emission inventories and the overall model performance are then assessed against atmospheric column methane measurements from the SCIAMACHY satellite instrument and ozone sonde measurements.

The Global Methane cycle continues to be a topic of much current interest. Methane is policy-relevant; it has the second largest radiative forcing after carbon dioxide and methane mitigation is an attractive option in achieving the warming targets of the Paris

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Climate Agreement. The cited synthesis papers of Kirschke et al. [2013] and Saunio et al. [2016] both conclude that there is still significant uncertainty in the magnitude, temporal trends and spatial distributions of the different methane sources and sinks. The papers also highlight a significant gap between the total methane emissions derived from aggregating bottom-up, largely process-based estimates and the top-down estimates derived from atmospheric measurements.

With the uncertainty in the methane emission source terms, many chemistry-climate and Earth System models prescribe the surface atmospheric methane concentrations. The use of an interactive methane scheme (i.e. driven with surface methane emission and removal processes) is welcome, while technically challenging. Methane is relatively long-lived in terms of tropospheric chemistry, making it both sensitive to and affecting the hydroxyl radical concentrations. Thus successful modelling of methane needs a robust description of OH; a 1% change in OH concentration is equivalent to  $\sim 5 \text{ Tg CH}_4 \text{ yr}^{-1}$  change in methane emissions.

This is a limited study in that emission inventories and the model evaluation are for a single year (2005) and the derived inventories may be specific to the ModelE2-YIBs. It would have been more interesting to consider the inventories and model performance over a longer period (e.g., 2000-2014) covering both the period of near-zero growth between 2000 and 2006 and the renewed growth from 2007 onwards.

Although the paper falls in the remit of the journal, there are a number of key issues that need to be addressed before it can be considered for publication.

### Specific Comments

Model development: The present model builds on the cited work of (1) Shindell et al. [2013], who described an interactive methane (and ozone) chemistry scheme in the GISS E2 chemistry-climate model, albeit for emissions from 2005 onwards, and (2) Yue and Unger [2015], who developed YIBS v1.0, a dynamic vegetation model for carbon-cycle studies, which also includes ozone-induced vegetation damage and

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biogenic VOC emissions. The main model development appears to be coupling of the GISS E2 chemistry-climate and YIBS models.

The title of the paper gives the impression that significant advances have been in the representation of an interactive methane chemistry scheme in ModelE2-YIBs. In which case, I would have had many comments about comparing with other OH concentration datasets, using other atmospheric tracers (CO, CO<sub>2</sub>) to constrain specific methane sources. In reality, it seems more limited. The only description of the model developments made in this paper (page 4, line 15) is in relation to the earlier work by Shindell et al. [2013] "this study updates the natural non-wetland methane fluxes; focuses on steady-state methane; applies a different anthropogenic emissions inventory; includes a new land surface model with interactive computation of isoprene and monoterpene emissions; and applies observed ocean boundary conditions". Although these are to some extent secondary to the primarily objective of the interactive methane scheme, there should nonetheless be some discussion as to how these have improved the overall model performance (compared to for example the GISS E2 chemistry-climate model), either in the main paper or as supplementary information. As an example, we are presented with global annual biogenic VOC emission estimates (Table 1, page 8), with no discussion as to how these compare to previous or other estimates (e.g., see Figure 10 in Sindelarova et al., 2014).

Wetland methane emissions: On page 10, line 19 and Table 2, a total of 140 Tg CH<sub>4</sub> yr<sup>-1</sup> is derived for the global mean emissions from wetlands for the year 2005. Effectively, this is a residual term after specifying all the other methane emission sources.

Earlier in the paper (page 4, line 5), the authors state "The model-measurement comparison was used to refine the spatial and temporal distribution of methane emissions from wetlands. The second and third steps were repeated, applying the newly optimized wetland emissions to ModelE2-YIBs, until strong model-measurement agree-

ment was achieved". Later on the same page (line 12), "Using ModelE2, Shindell et al. [2013] previously used a similar procedure of modifying the wetland methane source to achieve a modeled methane concentration that is in line with present-day observations, noting that the accuracy of the magnitude of the wetland flux that is derived in this way depends on whether the other prescribed fluxes have been accurately assigned. (Relative to the Shindell et al. [2013] study, this study updates the natural non-wetland methane fluxes ....".

As far as I can tell, there is no further discussion of this optimisation process, what is involved, what is meant by strong agreement and hence how the the emission total of 140 Tg CH<sub>4</sub> yr<sup>-1</sup> is derived. The implication is that the wetland methane emissions are taken from or as used in Shindell et al. [2013]. This should be clarified and the text amended. I note that this optimised wetland emission dataset is provided in the Supplementary Information, as an annual dataset.

For sure, the total is within the range of current estimates (Saunois et al. [2016] is an update of and effectively supersedes Kirschke et al. [2013]). As someone who both derives and uses methane wetland emission datasets, the single annual dataset provided is of little value. We know that wetland methane emissions vary seasonally. I would like to see more information about the dataset, e.g., temporal trends (both seasonally and inter-annually), how do the regional totals compare to those in Table 4 of Saunois et al. [2016]? The wetland model intercomparison of Melton et al. [2013, cited paper] summarised the then state-of-the-art in wetland modelling and the large uncertainty in modelled wetland area and wetland methane emissions. To remove one of the largest areas of uncertainty (in wetland area), the wetland models contributing to the synthesis paper of Saunois et al. [2016] all used the same prescribed spatially and time-varying wetland product (SWAMPS), described in the follow-on paper of Poulter et al. [2017]. How do the wetland areas compare with SWAMPS?

Soil uptake: Similar comments can be made about the lack of information on

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the soil uptake of methane. The spatial and temporal distributions given in Fung et al. [1991] are used (Page 7, line 25) and a total uptake of 60 Tg CH<sub>4</sub> yr<sup>-1</sup> is then derived (Table 2, page 9), which appears to influence the derived wetland methane emission estimates (page 10, line 19). This is then compared with and found to be higher than recent estimates (Page 10, lines 27-29). Could not the biome-specific measurements in the cited paper by Dutaur and Verchot not be used to create a new global methane uptake driven with relevant parameters from the land-surface model? More information is needed on how the total was derived.

Emission Maps: It would have been useful to include maps of the various methane emission sources (and any seasonal cycles) either in the paper or in the Supplementary Information to help interpret Figures 1, 3 and 4.

Model performance against observations: As presented, the model performance appears impressive, with differences of ~ 1-2% between the modelled and observed methane concentrations (both surface and column). The OH field is considered to be realistic as it gives atmospheric methane lifetimes in agreement with other estimates. That said, there are issues.

The seasonal cycle at surface high latitude southern hemisphere sites is underestimated (Figure 3, pages 14-16). Is this because of the temporal and spatial assumptions made in the natural methane sources? (as well as the cited underestimation of the austral summertime chemical loss). The model fails to capture the annual cycle at a few locations, notably Pallas-Sammaltunturi in Finland; Barrow in Alaska, USA; and Ulaan Uul in Mongolia. This is ascribed to local influences. From Hayman et al. [2014], a similar study using the UK HadGEM2 chemistry-climate model with an interactive methane scheme, it is likely that the Barrow and Pallas-Sammaltunturi sites are (over)influenced by wetland emissions and the Ulaan Uul by other sources.

The model performance is slightly worse against the mean SCIAMACHY atmospheric

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methane column mixing ratios (XCH<sub>4</sub>) (page 16, section 4.2). In their comparison, Hayman et al. (2014) also found that the HadGEM2 chemistry-climate model underestimated the observed SCIAMACHY XCH<sub>4</sub>, because the modelled CH<sub>4</sub> concentration fell off too rapidly with altitude (note the model configuration used a tropospheric chemistry scheme with an additional first-order loss process for methane to represent stratospheric methane chemistry, unlike the case here). It might not be a chemical problem (of sources and sinks) but potentially atmospheric dynamics and transport. This could be tested using other satellite CH<sub>4</sub> products which are more sensitive to the upper troposphere and lower stratosphere (e.g., TES, IASI).

VOC speciation (page 5, line 8): The chemical scheme has explicit representations of methane, isoprene, and formaldehyde, "while other hydrocarbons are represented using a lumped scheme (Houweling et al., 1998) that is based on the Carbon Bond Mechanism-4 (Gery et al., 1989) and the Regional Atmospheric Chemistry Model (Stockwell et al., 1997)". No information is given on how the non-methane VOC emissions are attributed to the model VOCs. Presumably, the existing NMVOC speciation is used.

Initialisation and Runtime: Nothing is said about how the model runs were initialised nor about runtime and platforms.

### **Code availability**

The source code for ModelE2-YIBs (version 1.1) is available on request to the authors.

Included in the Supplementary Information as a zipped file are the natural methane emission and methane soil sink datasets, as gridded annual averages. I see little value in these as currently provided as several of the sources (e.g., wetlands) have strong seasonal variations. The EXCEL spreadsheet format should be converted into a non-proprietary format, e.g., flat text (such as comma separated variable) or more

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usefully netCDF or similar.

## Technical comments

Page 2, Line 27: "principle sink" should be "principal sink"

## References

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