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Interactive comment on "An assessment of natural methane fluxes simulated by the CLASS-CTEM model" *by* Vivek K. et al.

Vivek K. et al.

vivek.arora@canada.ca

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We thank both reviewers for taking the time to review our manuscript and their positive comments. We propose below how we plan to address their comments in revising our manuscript. In the following reviewers comments are indicated in italic font and our response is indicated in bold font.

Reviewer 1

This is a well-written and interesting model description and evaluation of CLASS-CTEM. The application of the 1-box model is helpful as a benchmark. I have just a couple of minor comments for helping improve the manuscript.

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Discussion paper

1. Please mention how the inland water flux is or is not included in the CLASS-CTEM wetland emissions estimate, referencing Bastviken, and Thornton GRL where appropriate.

The current CLASS-CTEM modelling framework used in this study does not represent inland lakes. The land mask used by the model is binary with each grid cell either a land or an ocean/water cell. The model is, however, capable of representing inland lakes and in near future this functionality will be represented. We will make this aspect clear in revising our manuscript and also mention the Bastviken and Thornton references.

2. A couple of sentences on how the 1-box model equilibrium is calculated is needed to understand how 708 ppb was estimated. Is there a spinup for this, how many years etc...

No, this process doesn't require any spin up just the application of equations (4) and (5) with dB/dt set to zero. We will explicitly show the calculation of equilibrium 1850 atmospheric methane concentration when revising our manuscript.

3. Does EDGAR 'agricultural soils' include rice cultivation ?

Yes they do. The reason this category was included is because we do not model methane emissions from rice paddies explicitly. We will make this clear in revised manuscript.

4. Main criticism is that the parameter and forcing uncertainties are not explored for CLASS-CTEM. These would affect absolute estimates as well as interannual variability and trends of estimates of CH_4. Some references to ancillary CLASS-CTEM studies on uncertainties would be helpful to address this shortcoming.

This is a valid criticism of any modelling exercise. The CTEM (i.e. biogeochemical) part of the CLASS-CTEM model has more than 100 model parameters alone.

While several aspects of the model have been evaluated in the past at point, regional, and global scales a comprehensive sensitivity analysis is beyond the scope of this study. We will, however, include a discussion of uncertainties related to parameters that determine wetland extent and methane emissions from wetland and fire including those that affect the trend of wetland methane emissions.

Reviewer 2

The manuscript deals with an assessment of natural methane emission from wetlands and fire and soil uptake simulated by a one box model. The paper contains some interesting material, is reasonably well written and is generally well referenced. But the number of figures and tables is too high, some figures should be moved to the Supplement. In summary, the manuscript might be published after major revision.

Thank you for your comments. We will reduce the number of figures and following your suggestion move some material to an appendix or supplementary information.

Specific comments Line 41 - : : :reasonably well with observation based estimates. Could you give any estimations for errors and obtained methane emissions in the Abstract.

We will mention the relevant primary numbers for atmospheric methane concentration and wetland methane emissions in the abstract when revising our manuscript.

Line 58-80 – Remove. Contents is not typical for research article.

Thank you for noticing. Yes, of course, we will delete the Contents. These were put in for ease of the reviewers.

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Discussion paper

Line 88, 91,94 etc – Remove brackets at [CO_2].

The word CO_2 or CH_4 in square brackets is meant to represent atmospheric CO_2 or CH_4 concentration. We will check with the copy editing office to ensure if it is acceptable to use this format.

Line 98 – Give a reference to a paper where CH_4 concentration values were obtained.

We will include the Prather et al. (2012) reference for the methane concentration values when revising our manuscript.

Line 100 – Global warming potential usually is calculated over a specific time interval. According to IPCC 2013 CH_4 GWP is 7.6 – 72 times higher than CO_2 GWP, and other GHG (N_2O, CF_4, HFC-134a) have higher GWP than CH_4.

Thank you for providing this information. We will include this information and the IPCC reference when revising our manuscript.

Line 199 – CLASS model calculates the energy and water balance according to four PFT. None of them is related to wetlands. Is it correct to use upland vegetation types to simulate wetlands?

Thank you for raising this interesting question. It is correct that CLASS-CTEM model does not include any wetlands specific vegetation types. However, as argued in section 2.1.1.2 at large spatial scales the dominant controls on wetland methane emissions are temperature, wetland extent, and available substrate for decomposition. At large spatial scales both net primary productivity and heterotrophic respiration are determined by climate. This is the reason why models like the Miami model which only use mean annual temperature and precipitation are able to reasonably reproduce the spatial distribution of net primary productivity at the global scale. For the same reason, the CLASS-CTEM model is able to simulate spatial distribution of methane emissions reasonably as long as the spatial distribution of net primary productivity and heterotrophic respiration is

reasonable. The corollary of this is that the model will not be able to reproduce methane emissions at a point scale for wetlands and peatlands as reliably as it does at large spatial scales. We will include additional discussion around this aspect when revising our manuscript.

Line 243 -256 – Conception of seasonal changes in wetland extent needs more explanation. Wetland is a land area that is saturated with water (seasonally or permanently). According to figure 14a there is no wetlands in WSL in winter. It is not true. Seasonally frozen and covered by snow wetlands are still act as a source of methane (Langer et al., Biogeosciences, 12, 977-990, 2015 ; Korkiakoski et al, Biogeosciences, 14, 1947–1967, 2017). Details about wetland fraction computation and Table 1 can be moved to Supplementary.

While it is true that wetlands are capable of producing small methane emissions during the winter when they are frozen it is still unclear how much do these emissions contribute to the annual total. As seen in Figure 14 most models and even inversion-based estimates show very little emissions during the winter season when wetlands are frozen. We have some discussion around this aspect on page 37 of the existing manuscript and we will expand it more. We will also move details about wetland fraction computation and Table 1 to an appendix or supplementary information.

Line 296 – Function regulating emission with soil moisture change should be evaluated in details. Melton et al. (2015) describe basic and alternative types of dependences of CO2 heterotrophic respiration from soil moisture. Please give some proofs of applicability this kind of model for CH4 emission.

The basic premise of the dependence of heterotrophic respiration on soil moisture is that heterotrophic respiration is reduced at both too high and too low soil moistures. It is in between these extremes that heterotrophic respiration is at its highest. When soils are saturated and water table is above the surface

then respiration is reduced due to lack of available oxygen for the microbes. The modeling approach that we have used further reduces heterotrophic respiration calculated by the CO2 module of the model by multiplying it with 0.45 to account for this decrease due to anaerobic respiration. In the absence of evaluation of methane emissions at a point scale we can only use this conceptual basis to justify the applicability of our approach. We realize that this is a limitation and we will make a note of this when revising our manuscript.

Line 670-675 – Changes in methane emission from rice paddies due to land use changes and rapid agricultural development in East Asia should be mentioned.

Yes it is true that increasing methane emissions from rice paddies and agricultural development in East Asia have contributed to increase in atmospheric methane concentration. In our modeling study, this increase comes from the specification of anthropogenic methane emissions from the RCP and EDGAR data sets through the agricultural emissions category. Note that the model does not simulate emissions from rice paddies and so their specification is important. We will make this point clear when revising our manuscript.

Figures 1,2,7,8 can be abandoned or moved to Supplementary.

Figures 5,9,15 (also 6,10) can be joined in to two charts.

We will combine figures and move them to an appendix or supplementary information as suggested to reduce the total number of figures when revising our manuscript.

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