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***Interactive comment on “Spatial and temporal patterns of CH<sub>4</sub> and N<sub>2</sub>O fluxes in terrestrial ecosystems of North America during 1979–2008: application of a global biogeochemistry model” by H. Tian et al.***

**H. Tian et al.**

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Comments Dear Editor: The manuscript “Spatial and temporal patterns of CH<sub>4</sub> and N<sub>2</sub>O fluxes in terrestrial ecosystems of North America...” by Tian et al., appears to be a novel attempt to model the daily fluxes of these two key greenhouse gases at the continental scale. While the model formulation and basic results appear sound, some critical gaps are evident in the details of the solution method and in adequate explanation of the results. These concerns relate directly to the MS evaluation criteria provided by BGD, namely: Are substantial conclusions reached? and Is the description of ex-

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periments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? I have summarized these “Substantive issues” below, and suggested a number of minor changes following these. My feeling is that the paper will not be intelligible to most readers until all or most of the substantive issues are addressed.

[Response: Thanks for the suggestions. We have reorganized the manuscript, added more data for validation section, and expanded the discussion section.]

### Substantive issues and suggested changes

Comments 1. Equations (1)–(13), which describe the CH<sub>4</sub> module, are not as clear as they could be, e.g., for someone who might like to adopt the same parameterization or check the model results for themselves. My impression is that eq. (1) is to be taken as the fundamental equation for the net CH<sub>4</sub> flux on the lhs. Aside from some inconsistent notation (see specific comments below), it seems sensible. (As a general point, I would suggest that the authors make the necessary changes to the notation so that all fluxes are defined as surface, not volume fluxes. This is more appropriate for their model, which has essentially no structure in the vertical, and will definitely clarify the presentation). In the subsequent equations, I find expressions for all the rhs quantities in equation (1) except Foxidtrans, which is not mentioned anywhere else in the paper. My guess is that it is the same as “CH<sub>4</sub>oxidtrans”, defined in eq. (5), but it would be good if the authors could clear this up by eliminating one of the symbols.

[Response: Thanks for the suggestions. We have reorganized the equations, and clarified the notation. Also the surface fluxes were used in the model description. Both “CH<sub>4</sub>oxidtrans” and “Foxidtrans” have been replaced with Ftrans, oxid.]

Comments Another source of confusion is equation (3), which relates the time rate of change of CH<sub>4</sub> concentration in water to many of the quantities also appearing in eq. (1). Is it a prognostic equation for [CH<sub>4</sub>], or merely meant as illustrative? Since [CH<sub>4</sub>] also appears in equations (6)–(10), I assume eq. (3) is more fundamental. But

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this would be made much clearer by writing (3) as  $d[\text{CH}_4] / dt = f([\text{CH}_4]) = \text{CH}_4\text{prod}$  so that readers understand that the authors are solving a time-dependent ODE as part of the procedure. After equation (6),  $[\text{CH}_4]$  is defined as “the concentration of  $\text{CH}_4$  in soil/water”; do the authors mean “soil water”? If so, they should define this quantity consistently throughout the paper.

[Response: Thanks for the comments. The eq. (3) has been revised, and the  $[\text{CH}_4]$  has been consistently defined as soil water  $\text{CH}_4$  concentration throughout the paper.]

Comments 2. This leads directly to a second concern. What is the solution procedure, exactly? The initialization is discussed in Sec. 2.2, but not the actual method of solving the equations of Sec. 2.1. (Here I’m assuming that eq. [3] really is a prognostic eq. for  $[\text{CH}_4]$ ). Some questions I would ask are, e.g.: What is the order of solution for the various equations, and where are initial values inserted? At what point in the procedure are the rhs fluxes substituted into eq. (1) to calculate the net  $\text{CH}_4$  flux? At what point is eq. (3) solved, and what time-stepping procedure is used?

[Response: The solution procedure starts from eq. (3) (now eq. (2) in the revised manuscript); the environmental control will be first calculated, and then the major processes will be calculated, finally, the eq. (3) will be solved. All the calculated is on a daily time step.]

Comments 3. Most empirical studies of  $\text{CH}_4$  and  $\text{N}_2\text{O}$  have probed the influence of temperature and/or precipitation on the temporal behavior of surface fluxes. Knowledge of how the DLEM responds to these fundamental climate forcings is quite important, in my view. Although some correlation plots appear near the end of the paper, I would suggest this discussion be moved into the model verification section 2.4. Specifically, I suggest the authors expand Fig. 4 to show time series of soil temperature and precipitation at each of the sites shown, in addition to the fluxes. This could be followed by the present Fig. 9 and the corresponding text in the present Sec. 4.3, which I think appears too late in the paper. Seeing these results earlier would, I think, give readers

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more confidence in the model performance and subsequent results.

[Response: We agree that the influence of temperature and/or precipitation on the temporal behavior of surface fluxes would be critically important. Therefore, we have added the soil temperature and precipitation for each validation sites, and expanded the discussion for this section. However, we would argue to keep the Fig. 9 at the end of manuscript. The reasons are: (1) the figure 9 shows the continental-level simulation results, which were used to show the continental climate effects on CH<sub>4</sub> and N<sub>2</sub>O fluxes. It might not reflect the real climate control on CH<sub>4</sub> and N<sub>2</sub>O production and consumption at site-level as they substantially vary among biomes, and are significantly affected by environmental factors.]

Comments 4. Specific comments on the text: p. 2833, line 22: "...soils in Canada and Alaska emitted...". Are the authors referring to wetland soils here? As soils tend to be a sink of methane unless saturated, I think this sentence needs correction.

[Response: In the page 2833, line 22, the soils in Canada and Alaska mean the soils over the entire region. Thanks for the comments, we have revised it.]

Comments p. 2842, line 10: It would be helpful to see line graphs of the environmental functions defined in eqs. (11)–(13), especially since they differ from previous work.

[Response: The line graphs of the environmental functions defined in eqs (11)-(13) have been shown in figure 3.]

Comments p. 2843, line 10: In eq. (13), what is  $W$ ? Is it the same as "vwc"? If so, the same symbol should be used for both.

[Response:  $W$  is soil water content, it has been replaced with "vwc" throughout the manuscript.]

Comments p. 2845, eq. (16): The relation between "WFPS" appearing here and "vwc" in the previous equations needs to be clarified. My understanding is that the two quantities are related via the soil porosity, so perhaps only one quantity needs to

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appear in the equations. Please clarify this point.

[Response: It has been revised, thanks.]

Comments p. 2846, Sec. 2.2: How are ice-covered land surfaces handled in the model?

[Response: It is assumed that the ice-covered land is not capable of producing or taking up CH<sub>4</sub> and N<sub>2</sub>O, and thus was not considered in present simulation.]

Comments p. 2847, line 6: Mention is made of “...long-term mean climate during 1979-2008.” Is this the NARR data? If so, then this should be stated explicitly. The mention of NARR on the previous page is made without specifying the exact period used.

[Response: The long-term mean climate during 1979-2008 is the NARR data. We have provided detailed information for this dataset.]

Comments p. 2847, line 25: I had difficulty understanding the procedure as described here until end of the para. First: “Because the site-level climatic data are not always available, we retrieved the site-level data from our regional dataset for the model simulation.” Does this mean that model-simulated climate (which should not be referred to as a “dataset”) was used in the optimization along with the observed CH<sub>4</sub> & N<sub>2</sub>O fluxes? I would like to see the sites at which this substitution was done indicated in Tables 2 & 3, to help gauge the extent to which this may have biased the derived parameters. Similarly for the next sentence, “We used measurement data of CH<sub>4</sub> and N<sub>2</sub>O fluxes from field sites outside North America if the site-specific data of these fluxes for a specific ecosystem type are not available in North America.” How many sites are in this category? Again, these need to be clearly identified in the tables.

[Response: Because the site-level climatic data are not always available, we retrieved the input data from our regional dataset. It did mean that portions of NARR climate data was used in the optimization along with the observed CH<sub>4</sub> and N<sub>2</sub>O fluxes. However, we might have other local data, namely soil pH, soil texture, slope etc. Few site-level

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CH<sub>4</sub> and N<sub>2</sub>O observations were from outside of the continental North America. These have been shown in Tables 2 & 3.]

Comments p. 2848, line 13: “We retrieved the site-level, model-driven data from our regional dataset for model run because the input data at these sites were unavailable.” The term “data” appears three times in this sentence, and my sense is that only the last use is appropriate. Again, the authors should reserve the term “data” to refer to “observational data,” and not model results or output. As a result, I have no idea what information this sentence is supposed to convey. Please clarify.

[Response: This sentence has been deleted.]

Comments p. 2848, line 19: The authors mention that their model does not simulate sharp spikes in CH<sub>4</sub> flux seen at one site, and that the cause of these episodes is not well understood. However, their model does simulate exactly this effect at another site (Fig. 4a), despite the fact that these episodes are not seen in the observed time series there. I find this puzzling, since the model runs at a daily time step and these spikes are attributed to shorter time scale behavior later in the paper. The authors should be able to explain the origin of these spikes in their model, which may shed some light on the corresponding observed phenomenon, even if there is disagreement at a particular site.

[Response: Thanks for the comments. We have expanded this discussion in the revised manuscript.]

Comments p. 2849: I don't see a need for the three short, separate sections 3.1-3.3 here. The info conveyed is all closely related, and so should be combined into a single section. Similarly, I think Figure 5 should be eliminated, as the same curves are repeated on Fig. 7. The text can reference this figure instead.

[Response: The sections 3.1, 3.2, and 3.3 convey different points; section 3.1. highlights the temporal variations of fluxes; section 3.2. focuses on spatial distribution of

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fluxes; while section 3.3. highlights country-level results. We deleted figure 5, and referred to figure 7 in the text.]

Comments p. 2850, line 5: The authors state the proportions of CH<sub>4</sub> emissions originating from each country. Can they also provide the proportions of CH<sub>4</sub> sink located in each country?

[Response: The text provides continental total fluxes of CH<sub>4</sub> and N<sub>2</sub>O; so the proportions of CH<sub>4</sub> emissions are the net flux between source and sink. The United States and Canada acted as CH<sub>4</sub> sources, while Mexico acted as a CH<sub>4</sub> sink.]

Comments p. 2850, line 12: What is the reason for the high CH<sub>4</sub> emissions growth rate in Canada relative to the other two countries? In the following section, a similar figure is given for the rate of wetlands emissions increase, so I suspect this is the answer—but the authors might make this explicit. The authors might also check the figures cited for N<sub>2</sub>O growth rates in the three countries, as Fig. 7 seems to show that Mexico had the highest growth (but this is difficult to quantify from the figure).

[Response: Yes, we agree that the wetland contributed to the highest growth rate of CH<sub>4</sub> emission in the Canada. This has been discussed later. However, the Mexico did not have the highest growth of N<sub>2</sub>O; the US had.]

Comments p. 2851, line 25: What is the areal proportion of wetland in North America compared to the global total?

[Response: The wetland in the North America represents in total about 30% of the global wetland (Mitsch and Gosselink 2007). Reference: Mitsch, W. J., and J. G. Gosselink (2007), *Wetlands*, 4th ed., John Wiley & Sons, Inc., New York.]

Comments p. 2853, line 5: The authors offer favorable comparisons of N<sub>2</sub>O emissions with Xu et al., but don't tell us how the latter obtained their estimates. This should be provided. Also, a reference to Fig. 8 is missing here.

[Response: Xu et al's estimate is derived from meta-analysis and an empirical model;

we have shown this in the text. Meanwhile, we have added the reference to Fig. 8.]

Comments p. 2853, line 23: I don't think the comparison with anthropogenic estimates is very meaningful, since the natural/anthropogenic emissions ratio applies to global totals – there is no evidence it applies regionally. I suggest it be removed.

[Response: Thanks, we have deleted that portion.]

Comments p. 2857, Sec. 5: This section, entitled “Conclusions,” actually does not summarize the main qualitative or quantitative conclusions of the work, and merely repeats a few of the points already made in the previous section. As a result, the paper ends abruptly, without the main results and “take-away message” being reiterated. I think the authors would strengthen their paper considerably by reminding readers of the main results of their study (e.g., their derived total CH<sub>4</sub> and N<sub>2</sub>O fluxes).

[Response: We have revised the conclusion section to highlight the major findings of this study.]

Comments Tables 2 and 3: Site locations should be identified by name, not just by lat & lon, especially in light of the mention in the text that several are outside North America. The reader needs to know how many.

[Response: We have revisited our data and added the site name into Tables 2 & 3.]

Comments Table 4 & 5: Certain parameters appear to have been kept fixed for the calibration, or at least restricted to integer values. Why?

[Response: We did keep some parameters fixed. This is because most of them have not been intensively studied; and both the model simulations and field observations concluded the CH<sub>4</sub> and N<sub>2</sub>O are not sensitive to these parameters.]

Comments Table 8: I don't understand the results in the column “Bridgham et al. (2006).” There we see figures for “Arithmetic” and “Geometric...”, which I suppose are means constructed from data. But some explanation really needs to be provided in

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a footnote. Also, in the Bartlett & Harris column, I interpret the cited numbers to be for herbaceous wetland only and thus much larger than in DLEM. Is this correct? If not, then please fix the table layout.

[Response: The results shown in Table 8 are correct. We have provided footnote and some discussion in the revised manuscript.]

Comments Table 9: The last column is headed, “Recalculated from Xu et al. (2008).” As mentioned above, no explanation is given on how Xu et al.’s estimates are obtained—are they model-based? Please clarify.

[Response: We have revised that for clarity.]

Minor comments Comments p. 2833, line 2: Change “super-high compared to” to “much higher than that of”

[Response: It has been revised, thanks.]

Comments p. 2833, lines 4–7: Need to specify a corresponding period for these rates of change. E.g., the rate of increase of CH<sub>4</sub> was nearly zero from 2000 to 2008.

[Response: This sentence has been revised.]

Comments p. 2834, line 26: I am unclear about objective 1). What do the authors mean by “enhance”? Is the model being extended somehow in this paper?

[Response: We have revised this sentence to show the work of development of CH<sub>4</sub> and N<sub>2</sub>O modules.]

Comments p. 2835, line 22: Suggest “attributes” rather than “distributions.”

[Response: It has been revised.]

Comments p. 2836, line 4 and ff.: “the” missing before several nouns in this paragraph, e.g., “plant physiology component”..., etc. [Response: It has been revised.]

Comments p. 2837, line 5: “provide” rather than “provided.”

[Response: It has been revised.]

Comments p. 2837, eq. (1): Use different symbols for fluxes on rhs (volume fluxes, say  $f$ ) and total flux  $F$  on lhs.

[Response: It has been revised.]

Comments p. 2838, eq. (2): How is [DOC] obtained? I don't see it mentioned elsewhere.

[Response: DOC is calculated from three sources: GPP allocation as root exudation, soil decomposition, and litter fall decomposition. We have revised and show this in the revised manuscript.]

Comments p. 2838, eq. (3): Isn't this simply another form of eq. (1)? Again, clearer notation would make this explicit.

[Response: It has been revised, thanks.]

Comments p. 2839, line 19: "...the DLEM assumes that there is no atmospheric CH<sub>4</sub> oxidation when soil organic matter is less than 10 gC/m<sup>2</sup>." What is the justification for this particular choice of threshold value?

[Response: This is based on our extant knowledge that the atmospheric CH<sub>4</sub> oxidation is mainly carried out by soil methanotrophy, and low soil organic matter means lower soil microbial biomass (Conrad, 1996). This is primarily for dune in the desert. The exact number of the threshold needs more field work.]

Comments p. 2839, eq. (4): Is [AtmCH<sub>4</sub>] a constant, and if so, what is its assumed value? I read later (p. 2846) of how global mean, transient CO<sub>2</sub> concentrations are specified, but see no mention of CH<sub>4</sub> there either. In eq. (9), the quantity [AirCH<sub>4</sub>] appears, but it appears to be identical to [AtmCH<sub>4</sub>]. Once again, the authors should decide on a single name and stick to it.

[Response: The [AtmCH<sub>4</sub>] has been used throughout the paper.]

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Comments p. 2840, eq. (5): Here, and in the following equation, why is the rate defined as the minimum of the two rhs quantities? Also the definition of “min” in the following para is unnecessary—it is standard notation (same for “max” on p. 2841).

[Response: We have revised them, thanks.]

Comments p. 2840, eq. (6) and p. 2841, eq. (7): Again there is inconsistency in the units in these equations: the lhs is a rate, but [CH<sub>4</sub>] on the rhs is a concentration.

[Response: It has been corrected.]

Comments p. 2846, line 9: Please specify a reference/location for the NARR dataset.

[Response: We have provided the linkage for NARR dataset for our simulation.]

Comments p. 2847, line 1: “Landsat” not “Landsate.”

[Response: Mistake corrected, thanks.]

Comments p. 2847, line 17: Should read “...same as in other terrestrial biosphere models.”

[Response: It has been revised.]

Comments p. 2848, lines 26–28: Omit “quantitatively point-to-point” and replace “seasonal” with “seasonal spatial.” In last sentence, replace “could” with “can”.

[Response: They have been revised.]

Comments p. 2849, line 19: ‘TgC’ should be ‘TgN’.

[Response: Mistake corrected, thanks.]

Comments Figure 2: the visual quality of this figure is a bit poor. Can it be improved?

[Response: The figure 2 has been improved.]

Comments p. 2855, line 23: should read “...while increasing or decreasing...”

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[Response: It has been revised.]

Comments p. 2855, line 25: I don't see that the last sentence in this section adds any content, so suggest its removal.

[Response: The last sentence has been deleted, thanks.]

Comments p. 2856, line 24: Remove "a research need". In next sentence, "Clearly, improved estimates of parameter uncertainties are needed..." Last line: "Fourth," not "Fourthly," and same for "Fifthly," on following page.

[Response: They have been revised, thanks.]

Comments p. 2856, Sec. 4.4: Is there any reason why the DLEM could not be applied to other major continents? This is an obvious application of interest that might be mentioned.

[Response: DLEM can be applied to other major continents as well as global level. This paper addressed DLEM's application to North America, which is an effort relevant to non-CO2 greenhouse gas synthesis activity in North America.]

Comments p. 2867, line 32: "change" misspelled.

[Response: Mistake corrected, thanks.]

Comments p. 2868, line 29: "Sciences" misspelled.

[Response: Mistake corrected, thanks.]

Comments Table 1, 2nd footnote: Remove the word "other."

[Response: It has been revised, thanks.]

Comments Tables 2, 3, 7 and 8: No parentheses are required around the references.

[Response: It has been revised.]

Comments Figure 3, caption: Change phrase in parentheses to "the year 2000 is

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shown.” [Response: It has been revised.]

Comments Figure 8, caption: I suggest the sentence citing the spatial correlation between the two datasets be removed, and replaced by the text in parentheses. The former has nothing to do with the temporal correlation seen in the Fig., and in any case, it is cited in the text.

[Response: It has been revised, thanks.]

Comments Figure 8, caption: Remove the parentheses. [Response: The parentheses have been removed.]

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