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Biogeosciences Discussions

Interactive comment on "Spatial and temporal patterns of CH_4 and N_2O fluxes in terrestrial ecosystems of North America during 1979–2008: application of a global biogeochemistry model" *by* H. Tian et al.

Anonymous Referee #1

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Dear Editor:

The manuscript "Spatial and temporal patterns of CH_4 and N_2O 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:

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Interactive Discussion

Discussion Paper

Are substantial conclusions reached?

and

Is the description of experiments 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.

Substantive issues and suggested changes

1. Equations (1)–(13), which describe the CH_4 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_4 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 $F_{\text{oxidtrans}}$, which is not mentioned anywhere else in the paper. My guess is that it is the same as "$CH_{4\text{oxidtrans}}$", defined in eq. (5), but it would be good if the authors could clear this up by eliminating one of the symbols.

Another source of confusion is equation (3), which relates the time rate of change of CH_4 concentration in water to many of the quantities also appearing in eq. (1). Is

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it a prognostic equation for $[CH_4]$, or merely meant as illustrative? Since $[CH_4]$ also appears in equations (6)–(10), I assume eq. (3) is more fundamental. But this would be made much clearer by writing (3) as

$$\frac{d[\mathsf{CH}_4]}{dt} = f([\mathsf{CH}_4]) = \mathsf{CH}_{4\mathsf{prod}} - \ldots$$

so that readers understand that the authors are solving a time-dependent ODE as part of the procedure. After equation (6), $[CH_4]$ is defined as "the concentration of CH_4 in soil/water"; do the authors mean "soil water"? If so, they should define this quantity consistently throughout the paper.

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 $[CH_4]$). 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 CH_4 flux? At what point is eq. (3) solved, and what time-stepping procedure is used?

3. Most empirical studies of CH_4 and N_2O have probed the influence of temperature and/or precipitation on the temporal behaviour 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 more confidence in the model performance and subsequent results.

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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.

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

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.

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 appear in the equations. Please clarify this point.

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

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.

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_4 & N_2O fluxes? I would like to see the sites at which this substitution was done indicated in Tables 2 & 3, to help

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gauge the extent to which this may have biased the derived parameters. Similarly for the next sentence, “We used measurement data of CH_4 and N_2O 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.

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.

p. 2848, line 19: The authors mention that their model does not simulate sharp spikes in CH_4 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 timestep and these spikes are attributed to shorter time scale behaviour 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.

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.

p. 2850, line 5: The authors state the proportions of CH_4 emissions originating from each country. Can they also provide the proportions of CH_4 sink located in each country?

p. 2850, line 12: What is the reason for the high CH_4 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_2O 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).

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

p. 2853, line 5: The authors offer favourable comparisons of N_2O 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.

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.

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_4 and N_2O fluxes).

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.

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

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 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.

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.

Minor comments

p. 2833, line 2: Change "super-high compared to" to "much higher than that of"

p. 2833, lines 4–7: Need to specify a corresponding period for these rates of change. E.g., the rate of increase of CH_4 was nearly zero from 2000 to 2008.

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?

p. 2835, line 22: Suggest "attributes" rather than "distributions."

p. 2836, line 4 and ff.: "the" missing before several nouns in this paragraph, e.g., "plant physiology component"..., etc.

p. 2837, line 5: "provide" rather than "provided."

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

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

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

p. 2839, line 19: "...the DLEM assumes that there is no atmospheric CH_4 oxidation when soil organic matter is less than 10 gC/m2." What is the justification for this particular choice of threshold value?

p. 2839, eq. (4): Is [AtmCH_4] a constant, and if so, what is its assumed value? I read later (p. 2846) of how global mean, transient CO_2 concentrations are specified, but see no mention of CH_4 there either. In eq. (9), the quantity [AirCH_4] appears, but it appears to be identical to [AtmCH_4]. Once again, the authors should decide on a single name and stick to it.

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).

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_4] on the rhs is a concentration.

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

p. 2847, line 1: "Landsat" not "Landsate."

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

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

p. 2849, line 19: 'TgC' should be 'TgN'. Figure 2: the visual quality of this figure is a bit poor. Can it be improved? *p. 2855, line 23:* should read "...while increasing or decreasing..."

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

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.

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.

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

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

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

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

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

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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.

Figure 8, caption: Remove the parentheses.

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