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**Biogeosciences Discussions**

# *Interactive comment on* "Attribution of spatial and temporal variations in terrestrial methane flux over North America" *by* X. F. Xu et al.

**Anonymous Referee #1**

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This study investigates the terrestrial CH4 surface flux of North America over the last 30 year and the factors contributing to its estimated increase, using a dynamic ecosystem model. The model allows a comprehensive assessment of the influence of what are believed to be the most important drivers, either separately or in combination. The outcome provides important insight into the continental-scale CH4 flux and its response to climate change, which, in my opinion, provides sufficient justification for publication. My main criticism, however, concerns the readability. In its current form, to appreciate the contents, the reader has to make substantial effort trying to understand what is meant exactly. Below are several suggestions for improvement. In addition, I advice a native American coauthor to have a careful look at the formulations. Else I have some concerns regarding the treatment of uncertainties and the design of the experiments, which – in my opinion – call for further revisions.



UNCERTAINTY

Reading the abstract one could easily get a false impression of the general level of uncertainty associated with the type of estimates that are presented in this manuscript. The number of significant digits suggests the overall emissions are accurate to better than 1 per mil, and the factorial attribution is at the 1 percent level. Another example is section 3.3 where uncertainties are listed that refer strictly (if I understand correctly) to the linear regression of model generated time series and ignore the substantial uncertainty of the model itself. Those who look more careful find out that the authors make an effort to assess uncertainties. Table 4 lists comparisons with measurements and models and section 4 discusses factors that contribute to the overall uncertainty. What is lacking, however, is the connection between that information and the estimates that are presented in the abstract and the conclusions. When I look at Table 4 I find it very disturbing that under 'N input, wetlands' three numbers are listed (this study and others) that are statistically extremely implausible. The only thing that is mentioned in the text is that the sensitivity to N deposition depends on the level of N limitation. To me it says that the numbers can actually not be compared, which raises the question what the purpose is of their comparison in Table 4.

EXPERIMENTAL DESIGN

In section 2.3 it is explained that a 3000 year spin-up is needed, supposedly to avoid that initial conditions determine the tendency of the flux in the analyzed period rather than changes in driving factors in that period. In the end it turns out that climate change has largest impact on the flux tendencies. Why, in that case, was the average climatology for the spin-up period constructed from the long-term mean climate for the analyzed period? If I understand correctly it means that the model is equilibrated under conditions (a warmed climate), to test the impact of the same conditions (a warmed climate). If, with this in mind, I look at figure 7 I start wondering whether the negative impact of climate in the first part of the simulation is caused by the fact that initial climate (mean of 1979-2008) represents more warming than the first part of the sim-

ulation. In this respect, the definition of the baseline is crucial. If I understand right, the accumulated baseline flux for the period 1979-2008 is calculated by assuming that the 1979 flux remained constant for the analyzed period. However, if the conditions in the model were kept constant at the 1979 level during the analyzed period, the fluxes would probably still experience a trend. In a worst case, one could think of the initial conditions explaining most of what happens after. Why is the baseline defined as the 1979 flux and not the result of a simulation that recycles the 1979 conditions? These issues should be clarified.

THE ROLE OF CLIMATE

It is unfortunate that there is a single factor that influences the simulations much (climate change), and a few others that have a relatively minor impact. I'm not surprised that the combined effect is not much different from the sum of the components (given realistic overall uncertainties), because climate change dominates the outcome anyway. It would have been more interesting to understand how exactly climate change influences the fluxes, which suggests another factorial analysis, targeting various aspects of climate change. I understand that this would probably be an unfair requirement. Nevertheless the paper would become much more interesting if some attempts are made to better understand how climate change affects the fluxes.

TECHNICAL CORRECTIONS

Abstract line 10: Break the sentence before 'all'

Page 5385, line 16: 'reality' instead of 'real reality'

Page 5385, line 19: None of the identified factors address transport.

Page 5385, line 25: 'Enhance flux by stimulating emissions'. Because an emission is a flux this doesn't make sense.

Page 5389, line 24: Would wetlands count as water bodies that are excluded? I suppose not, but what does count under water body here?



Page 5389, line 26: A reference to NARR is missing.

Page 5390, line 7: A reference to HYDE is missing.

Section 3.3 and section 3.5: It is almost impossible not to get lost in the numbers while reading these sections. Numbers that are in tables need not be repeated in the text (except maybe a selection of a few particularly relevant ones). More examples of this can be found in other sections.

Page 5393, line 18: ppb.h instead of ppb h-1 Page 5401, line 4: Shorten the formulation.

Table 1: There is no need to repeat the entries of the table in the caption. Somehow this table should inform the reader that the numbers are derived by regression analysis. A 'Changing trend' would be the second derivative of a parameter, whereas the first derivative is meant (or just 'trend').

Table 2: Which year or period do the numbers refer to?

Figure 3: The O3 pollution map looks strange to me. I would have expected the densely populated regions to stand out, instead of e.g the Rocky Mountains. The caption should provide information on the threshold that was used for N-fertilization. PPB.H instead of PPB/H.

Figure 4: This figure doesn't convey much information. The panels are virtually the same. I propose to use one figure for the total flux and plot the others relative to it in order to highlight differences.

Figure 6: The figure does not fit into the page.

Figure 8: There is no reference in the text to this figure.

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Interactive comment on Biogeosciences Discuss., 7, 5383, 2010.