Biogeosciences Discuss., 8, C4719–C4728, 2011
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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Use and uncertainty evaluation of a process-based model for assessing the methane budgets of global terrestrial ecosystems" *by* A. Ito and M. Inatomi

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Dear reviewers and editor:

We appreciate your helpful comments on our manuscript submitted to Biogeosciences. The previous manuscript was revised on the basis of open discussion including comments from the reviewers. Major points of this revision are as follows: (1) A number of recent publications were included on the basis of the comment from reviewer-#2: Riley et al. (2010, Biogeosci.), Wania et al. (2010, Geosci. Model Dev.), Ringeval et al. (2010, GBC), Petrescu et al. (2010, GBC), Neef et al. (2010, GBC), Bloom et al. (2010, New Phytol.), Martinson et al. (2010, Nat. Geosci.), Kai et al. (2011, Nature),

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Patra et al. (2011, Atm. Chem Phys.), Dlugokencky et al. (2011, Phil. Trans. Roy. Soc.), Bastviken et al. (2011, Science), Spahni et al. (2011, Biogeosci.), Hodson et al. (2011, GRL), and Lassey et al. (2011, Tellus). Thus, we tried to catch up with the latest achievements in the research field. (2) We re-considered the number of combinations of the estimated specific CH4 fluxes from 576 to 786 by adding one more paddy filed emission value estimated by the Cao's scheme. As a result, total terrestrial budget and figures of frequency distribution (Figure 3) were revised. (3) We added discussion on the uncertainty estimated by the model on the basis of the comments from reviewer-#2. In general, we agree that the opinion that the present analysis underestimated the true range of estimation uncertainty. Therefore, we explicitly stated this point as a limitation of this study, and suggested that further analyses including parameter uncertainty are required.

For each of the general and specific comments, we reply as shown by the following. We hope this revision is satisfactory for acceptance.

Reviewer #2 Comment: In this study A. Ito and M. Inatomi present a comprehensive review of established process parametrisations for terrestrial methane emissions. They revisit parametrisations especially for methane emissions in wetlands and apply them as schemes in the VISIT ecosystem model. They find CH4 emissions within the range of previous estimates.

Reply: Thank you for your understanding. Although this study is not latest for every aspect of this actively growing research area, we still believe that it brings new findings and some integrated view using our model.

Comment: Their calculations are very reasonable and presented in a well organised way. However, their findings are not groundbreaking and do not give new insights or constraints for the present day methane budget. I thus suggest to strentghten the review character of the paper by including most recent findings from similar studies, that have been conducted especially over the last two years. I thus would support a

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publication in Biogeosciences after a revision. Several studies have assessed global methane emissions from terrestrial ecosystems in a similar way, using either dynamic vegetation models, ecosystem models, observations or a combination of both. In the following I list a selection of additional references for a potential comparison with model results for the individual ecosystems: peatlands, i.e. bogs and fens (Wania et al., 2010, Spahni et al., 2011), inundated wetlands (Ringeval et al., 2010, Bloom et al., Science, 2010, Spahni et al., 2011, Hodson et al., 2011, Riley et al., 2011), saturated and non-saturated soil emissions (Bloom et al., Science, 2010, Ringeval et al., 2010, Spahni et al., 2011, Riley et al., 2011), rice paddy emissions (Spahni et al., 2011),upland soil uptake (Spahni et al., 2011), lakes, rivers and reservoirs (Bastviken et al., 2011). Of course there are even more studies that could be added.

Reply: I agree to cite more recent literature. As mentioned as a major point, we added a number of recent (i.e., published in 2010 and 2011) papers, both modeling and observational ones, in our manuscript. Most of literature suggested by reviwer-#2 and several recent reviews and reports were cited in the revised manuscript.

Comment: Although these studies use similar or sometimes exactly the same parametrisations of the emission/uptake processes, the global net CH4 flux densities to the atmosphere are different in space and time (season, year). However, their total global emissions per year are very close to each other independent of their setup and parametrisations used. This implies that all studies somehow scale to the same global CH4 emissions in order to be compatible with the atmospheric CH4 budget inferred from top-down. I would thus argue that the different parameterisations (for wetland emissions at least), are not independet of each other. Thus my main critics is that total CH4 emission uncertainty and variablity from bottom-up process based estimates are greatly underestimated and arguable larger than +/- 18.9 Tg/yr as inferred for the calculated source total by Ito and Inatomi in this paper. Reply: We believe that two schemes of wetland CH4 emission (i.e., Walter-Heimann scheme and Cao et al. scheme) were operated independently, at least in the present study. We note that the range of estimation actually spanned from min. 262 to max. 359 Tg CH4 yr–1 (cf. Figure 3); standard deviation, ± 20 Tg CH4 yr–1, is a metric of dispersion but does not represent the possible range. However, we agree that this study underestimate the true range of uncertainty, because (1) uncertainty in parameters (e.g., Q10 of methane production) was not fully addresses in this study, and (2) uncertainty in prognostic estimation of wetland area was not included in the present study. If these uncertainties were included, the range of terrestrial CH4 budget should be enlarged. Evaluation of these factors is, however, beyond the scope of this paper and will remain for our next study. This limitation is explicitly discussed in the revised manuscript.

Comment: The authors correctly point out that the upscaling from natural ecosystem CH4 emissions from point based measurements is difficult, as it can vary with ecosystem and area, e.g. area and location of inundated wetlands. However, there is little discussion on these important uncertainties and how they affect the outome of the estimated total CH4 emission range. More detailed questions regarding these point are listed within the specific points below.

Reply: This is an important point, and therefore we added further discussion on the difficulty in upscaling of fluxes.

Specific points Comment: (1) p7034,l24: Please mention atmospheric water vapour in this context.

Reply: We revised as "CH4 is the second-most-important GHG except vapor…".

Comment: (2) p7036,l1: Please add also some other newer models from the list above.

Reply: We listed here, "Petrescu et al., 2010; Ringeval et al., 2010; Tian et al., 2010; Wania et al., 2010; Riley et al., 2011".

Comment: (3) p7037,l26: So for CH4 the model is evaluated for one deciduous broadleave forest? Or have other sites been used to test CH4 fluxes? How well are other ecosystem represented for their annual CH4 flux, like inundated wetlands, peat-

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lands, rice paddies? What about emission from lakes, are they included or validated? It is eligible to use straight forward model simulations, but more information on which CH4 flux were validated and which not would be very helpful.

Reply: Actually, we evaluated our model at a paddy field in Japan (Inatomi and Ito, unpublished data) and natural wetlands in West Siberia (Sasakawa et al., Atm. Chem. Phys. Discuss. 10, 27759-27776, 2010). Although these are ongoing unpublished results, we only briefly mentioned them in the revised manuscript.

Comment: (4) p7038,l20: Was FP, the proportion of the decomposed organic carbon transformed into CH4, kept constant at 0.47 over space, time and ecosystems for the simulations?

Reply: We checked the model code and confirmed that we used a constant value. The manuscript was revised to explicitly mention this point.

Comment: (5) p7040,l15: labelling for titles of CH4 uptake schemes seems to be irregular, "2.2" or "2.2.1"? Reply: We corrected labeling of section numbers. Thank you for this comment.

Comment: (6) p7044,l1: The authors are right, there has been a big controversy regarding aerobic emissions from plants. But recent estimates have come down considerably, e.g. see Bloom et al., New Phytologist, 2010 suggest total sources of 0.2 to 1.0 Tg/yr. Another estimate you can find in an online reply to a comment by F. Keppler in Spahni et al., 2011 in this Journal: http://www.biogeosciences-discuss.net/8/221/2011/bgd-8-221-2011-discussion.html How does the aerobic plant emission parametrisation compare to these two upscalings?

Reply: We guess that there still remains a wide range of uncertainty in the present evaluation of plant CH4 emission. We confirmed that several studies such as Bloom et al. (2010) presented lower flux values, while several additional sources (e.g., tank bromeliads; Martinson et al., 2010, Nature Geoscience) are being discovered. Be-

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cause it is difficult to cover the full range of possibility to date, we focused on the range caused by different scaling-up methods. We agree that it is an important task to fund a consistent value and clarify the range of uncertainty in the plant CH4 emission.

Comment: (7) p7045,l10: This is a very interesting approach for the ruminant livestock Ch4 emission estimate. Do animal density somehow correlate with model based pasture productivity, like e.g. grass NPP?

Reply: Not yet. We simply used livestock density derived from the FAO dataset. We need further data, such as physiology and management of livestock, to refine this estimation.

Comment: (8) p7045,l25: "focused"

Reply: Revised as suggested.

Comment: (9) p7046,l18: Which parameters were varied, if at all, in the two schemes for the 576 simulatons?

Reply: In this revision, we got 768 different combinations: 3 wetland sources $\times$ 4 paddy field sources $\times$ 2 fire sources $\times$ 2 plant sources $\times$ 2 livestock sources $\times$ 2 termite sources $\times$ 4 upland soil sinks (see Table 1).

Comment: (10) p7046,l20: Ringeval et al. 2010 showed that total annual CH4 emissions must be considered as a non-linear combination of wetland area and CH4 flux density. Thus does the wetland area vary from year to year? The Prigent data is available for the years 1993-2000. How was this data set combined with the wetland and lake data set by Lehner and Döll? Are lakes and rivers included? A study by Bastviken et al., 2011 shows that lakes might make up a big part of the methane budget of up to 103 Tg/yr. How does that fit within the VISIT estimate?

Reply: The present study, unfortunately, did not fully address the effect of interannual variability of wetland extent, because we primarily focused on long-term properties (e.g, means and trends) in the terrestrial CH4 budget during the period 1901–2009.

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Clearly, to investigate the interannual variability, we should include this factor by using appropriate datasets or prognostic wetland extent schemes. We acknowledge that this is a limitation of the study and discussed this point in the revised manuscript. Also, this study did not emissions from freshwater systems such as rivers and lakes (Bastviken et al., 2011). This source may be added to the present terrestrial fluxes, when comparing with top-down (i.e., atmospheric observation and inversion) estimates.

Comment: (11) p7046,l26: Since the 1980 CH4 emissions from rice paddies are estimated to have gone down, even with increasing rice paddy area and rice production. The decline in rice CH4 emissions is explained by an increased use of ferilizer (see e.g. Kai et al., 2011). Is this considered in the VISIT estimate?

Reply: In the present study, fertilizer input did not affect the magnitude of CH4 emission from paddy fields, and then our result may be somewhat inconsistent with that by Kai et al. (2011). Inclusion of agricultural management into the biogeochemical schemes used in this study is beyond the scope of this study, and the present study reflected only the expansion of paddy fields in the last decades. This limitation should be solved by adopting sophisticated cropland schemes (e.g., DNDC-paddy, Fumoto et al., 2008).

Commet: How were the areas of Monfreda et al. seperated from the inundation data set by Prigent et al. ? Is there a overlap?

Reply: We assumed that the inundation area by Prigent et al. includes both paddy field (by Monfreda et al., 2008) and natural wetland, because this dataset was developed using satellite data. Therefore, some overlapping could occur.

Comment: (12) p7047,l5: "Kirschbaum" instead of "Kirchbaum"

Reply: Revsied as suggested.

Comment: (13) p7048,l7: Here it is mentioned: "We expected that the distribution of the total budget produced by these simulations would reveal the range of estimation uncertainties caused by variability in the base data and evaluation schemes." As outlined in my general point I think the uncertainty is greatly underestimated. This assumptions certainly needs more justification. How were the 576 combinations achieved? Is each combination equally probable?

Reply: We regarded the 768 (in this revision) different combinations with equal probability, because it is difficult to weight specific fluxes on the basis of certain criteria (confidence level etc.). We agree that the present study did not capture the full range of estimation uncertainty; e.g., uncertainties in parameter values and prognostic inundation estimation were not included. In this revision, we explicitly discussed this limitation in the present analysis.

Comment: (14) p707,Figure 7: Is the global CH4 emission pattern roughly compatible with the atmospheric CH4 concentration gradient? How does it compare to other budgets constrained by satellite or inversions (Bloom et al, Science, 2010, Spahni et al., 2011)?

Reply: Our model outputs were provided to the TransCom-CH4, i.e., intercomparison of atmospheric transport models (e.g., Patra et al., 2011, Atm. Chem. Phys. Discuss.), as an extra land flux data. Through this activity, we would like to assess consistency and disagreement with atmospheric observations in a quantitative manner. Although Figure 7 does not include anthropogenic sources, we guess that the latitudinal-seasonal pattern of terrestrial CH4 exchange may substantially contribute to the latitudinal gradient in atmospheric CH4 concentration.

Comment: (15) p707,Figure 8: There is hardly a trend in CH4 emissions from wetlands over the last century. Are CH4 emissions affected by a CO2 fertilisation effect?

Reply: Both wetland emission schemes (Walter-Heimann and Cao) considered net primary production (NPP), which was affected by the CO2 fertilization effect in the model. In the present simulation, the estimated wetland CH4 emission has a weak increasing trend ($\sim$ +0.2 Tg CH4 yr–1). Therefore, increases in paddy field and livestock emissions were more important in the temporal trend estimated in this study.

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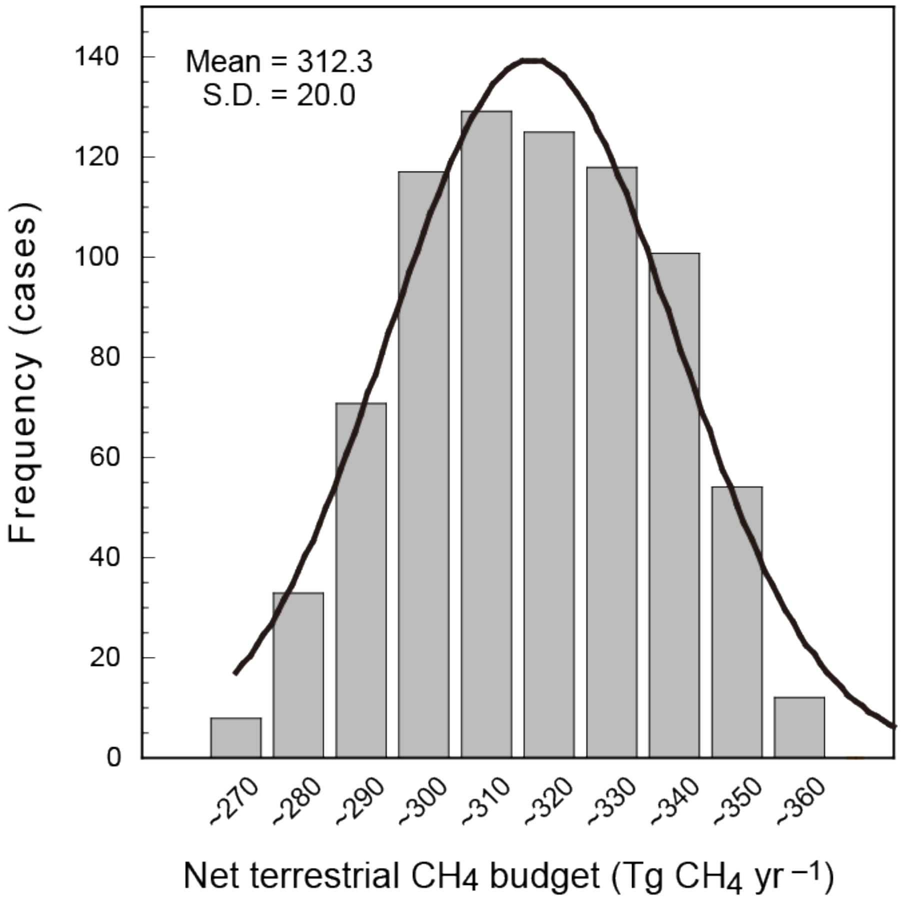

Fig. 1. Revised Figure 3