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***Interactive comment on* “Seasonal Variability of Tropical Wetland CH₄ emissions: the role of the methanogen-available carbon pool” by A. A. Bloom et al.**

A. A. Bloom et al.

a.a.bloom@sms.ed.ac.uk

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We thank the referees for having provided thorough feedback and for their suggested corrections. Below we have addressed each individual comment from all referees (referee comments are shown in italics).

Referee 1

General Comments: This paper proposes a simple model of anaerobic decomposition substrate as an important controller of the seasonality of Tropical regional-scale wetland CH₄ emissions. The authors are able to use this hypothesized control to explain the 1-3 month lag between peak inundation and atmospheric CH₄ concentrations over

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the Tropics. Substrate availability as a primary control on methane emissions is a viable hypothesis, but I feel the paper would be enhanced by a more thorough analysis and discussion of alternative controllers on the seasonality of net CH₄ emissions to the atmosphere. It seems possible that the decay constant the authors infer could be explained by other processes with comparable temporal variability.

A few examples to consider as alternative hypotheses:

1) Growth of macrophytic mats in many flooded rivers contribute to seasonality in CH₄ fluxes, both through exudation and active transport in tissues (Melack et al. 2004).

In the revised manuscript we review the seasonality of emergent macrophyte biomass and associated NPP (e.g. Silva et al. 2009) in tropical ecosystems and elaborate on the potential contribution of emergent macrophytes in the annual CH₄ seasonality.

2) Flooding and then stabilization of the wetland area over a month or so could also lead to seasonal variations in pH and redox conditions.

We agree that this is a valid hypothesis. However, quantifying large-scale spatial and temporal variability of pH and redox conditions based on site-level observations is a significant undertaking, and is beyond the scope of this paper. In the revised manuscript we acknowledge this point and provide an overview of the current understanding of seasonal variability of pH and redox conditions.

3) The authors explain in one sentence why they think seasonal variation in oxidation is unlikely, but I think that point needs more substantial treatment. Seasonal variations in methanotrophy could also contribute to the observed time lag. Such a seasonality could come about by a changing competitive environment between aerenchyma transport, methanotrophs, and methanogens.

In the revised manuscript we include a more detailed discussion on the seasonal variability of methanotrophy and its potential effect on wetland CH₄ emission

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seasonality.

4) You hold N_{μ} constant, but it should have some seasonality associated with temperature controls on SOM and litter decomposition, exudation, fine root mortality, etc. You might examine this sensitivity by considering seasonality associated with NPP (for exudation) and temperature for decomposition (in the same way you imposed temperature dependence in equation (2)). You might also describe results from Wania et al. (2010), Zhuang et al. (2004), Riley et al. (2011), and others, who spend a lot of effort describing the suite of processes resulting in net CH_4 emissions, their relative contributions to uncertainty in net emissions, and their impacts on the seasonality of net emissions.

We agree that a more elaborate methanogenesis model should include a temporally variable carbon input N_{μ} . In particular, we acknowledge the reviewer's suggestion: the use of NPP is an appropriate starting point to better define the temporal variability of N_{μ} . However N_{μ} inputs are also seasonally dependent on other C fluxes, namely litter production, which is not expected to seasonally covary with NPP (e.g. Chave et al. 2010, Caldararu et al. 2012). We chose a simple model a constant value for N_{μ} to maintain a simplified approach in understanding the seasonal effects of CH_4 emissions from a temporally variable methanogen-available carbon pool. In our revised manuscript we discuss our choice of N_{μ} , and we overview the temporally variable components associated with N_{μ} .

Specific Comments:

1) The range of Q_{10} values used in regional to global CH_4 models is very large. Please comment on how a much larger value of Q_{10} would impact your inferences about seasonality. You might re-run your simulations with a Q_{10} of 3 to see if that removes some of your inferred seasonality in C_{μ} . Note that at high latitudes, some models use Q_{10} values up to 4 (Zhuang et al. 2004).

We agree that magnitude and seasonality of CH_4 emissions on a global scale are strongly dependent on the Q_{10} value. However, the aim of our top-down pa-

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parameter estimation approach is to optimize multiple parameters simultaneously, including the value of Q_{10} , in our methanogenesis model. To address this comment we will perform a sensitivity analysis to determine the C_{μ} seasonality dependence on Q_{10} .

2) Please give some more details about your uncertainty propagation described in paragraph 15. For example, what are the SCIAMACHY errors, what do they derive from, what is their seasonality? Where did the 16% uncertainty associated with kappa come from? How did you include uncertainty in the non-linear fit? What other uncertainties are you ignoring? For example, error in using GRACE to infer wetland extent? Did you compare to Prigent et al? Can you compare to some of the other inundation datasets (e.g., Melack et al. 2004).

Uncertainties

We aggregated SCIAMACHY observational error at the $3^{\circ} \times 3^{\circ}$ degree grid. Median errors are typically $< 1\%$. There is no significant seasonality of observational errors.

The 16% uncertainty of kappa is derived from a synthetic experiment by Bloom et al. (2010) where the amplitude of the SCIAMACHY sampled GEOS-Chem CH_4 seasonality is compared to the amplitude of the CH_4 wetland emissions. The uncertainty associated with predicting wetland CH_4 flux amplitude from atmospheric column-averaged CH_4 amplitude is 16%.

The uncertainty for each $3^{\circ} \times 3^{\circ}$ degree gridcell is derived by calculating the uncertainty of the linear fit between ΔS and ΔF . The final uncertainty associated with each $3^{\circ} \times 3^{\circ}$ degree gridcell is the propagation of the linear fit uncertainty with κ and a global uncertainty associated with wetland and rice paddy emissions ($\pm 58 \text{ Tg } CH_4 \text{ yr}^{-1}$). We now include a) an overview of SCIAMACHY observational errors and b) A clear description of the derivation of κ .

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Inundated fraction

We have performed a simple basin-wide comparison between the Prigent et al. (2007) seasonality and GRACE, and find that these strongly covary. However these datasets provide limited information on wetland hydrology: while the GRACE data acts as a volumetric constraint, the inundated fraction data is an area constraint. We believe this comparison is beyond the scope of this paper (a more extensive comparison of these datasets has been performed by Papa et al. 2008). However, we now discuss the limitations of GRACE in the revised manuscript.

3) Riley et al. (2011) discussed seasonality in substrate availability, and how that seasonality could be modeled (their 'seasonal inundation factor'). Is there analysis consistent with yours?

Although C_{μ} and substrate availability are not directly comparable, we do find that the wetland water volume seasonality is an important factor in the overall temporal variability of C_{μ} .

Technical Corrections:

We agree on all of the following proposed technical corrections, and we have incorporated these in our revised manuscript.

- 1) Make sure that CH_4 has the "4" as a subscript in all your figures.
- 2) Clarify in Figure 1 that you are referring to 'peak CH_4 emission month'.
- 3) The legend in Figure 4 is unclear, because the colored lines don't correspond to the first line of the description. E.g., it looks like the blue line corresponds to "top-down wetlands & rice by Fung et al". Please correct. Also, 'Top-down' applies to the current study, right?
- 4) Change 'methane' to be ' CH_4 ' in Figure 5 to be consistent with other figures. Clarify

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what 'NH' and 'SH' mean (northern and southern hemisphere, I assume)

Referee 2

This manuscript by Bloom et al. well describes temporal patterns of CH₄ emission from physical conditions (temperature and moisture) and substrate availability. This is a well-written paper, and their ideas are presented clearly. I strongly agree with the reviewer 1 that the authors should explicitly consider alternative hypothesis.

We now include an explicit overview of all proposed alternative hypotheses.

I also suggest them to evaluate their simple model. In previous studies, there are more detailed, more mechanistic models. For example, Walter & Heimann (2000, Global Biogeochemical Cycles) explicitly modeled CH₄ transport including diffusion, ebullition, and vascular transport. They also explicitly simulated CH₄ oxidation in aerobic soil layers. I think the thickness of aerobic soil layers could strongly control CH₄ dynamics in a way that is not treated by this manuscript by Bloom et al. So, please explain why your simple model is appropriate for this study.

We agree that all of the above mechanisms are important factors in defining the overall magnitude and seasonality in process-based models. The scope of this top-down parameter-estimation approach is to identify mechanisms influencing seasonal variability of CH₄ emissions over large spatial scales. Although adding additional processes and model parameters might improve the representation of the observed SCIAMACHY CH₄ variability, our ability to optimize a larger number of model parameters, given the available remote sensing data, would be substantially reduced.

Referee 3

General Comments:

The global methane cycle and the methane emissions from wetlands are topics of much current interest. Wetlands are the largest natural source but there is a significant

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uncertainty associated with the emission estimates. This paper makes an important contribution to this topic.

The paper extends previous work on methane from wetlands by the same authors (Bloom et al., 2010), primarily through the introduction of a time decay of the methanogen-available substrate in the parameterisation of methane release from wetlands. This is used to explain the observation that the maximum in the atmospheric CH₄ column over the Amazon can occur some months prior to the peak in the water table (the measure used to characterise the 'wetland'). The parameterisation is then applied globally and used in the GEOS-CHEM atmospheric chemistry model. This work is highly relevant as many of the leading land surface models (e.g., the US Community Land Model (Riley et al., 2011), the Joint UK Land Environment Simulator (Clark et al., 2011), Orchidee (Ringeval et al., 2010), etc) generally use parameterisations of CH₄ release from wetlands which would scale with the wetland extent/fraction, all other factors being equal.

The paper draws heavily on the earlier work (Bloom et al., 2010) and the assumptions made there. A thorough reading of that paper and its supplementary material is a prerequisite to understand many aspects and implications of this paper. As an example, the conversion constant κ is introduced to relate the grid-square emission flux to the grid-square atmospheric column (page 393-394). This parameter effectively represents the effect of dispersion and other atmospheric processes. There is no information provided in this paper as to how κ or its uncertainty was derived. The authors should address the level of detail provided.

In the revised manuscript we now specify how we performed the derivation of κ and the associated uncertainty of κ . (note: the uncertainty of kappa is explained in the response to referee 1).

I would agree with Reviewer 1 that the authors should explore alternative hypotheses.

We now include an explicit overview of all the proposed alternative hypotheses

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in the revised manuscript.

Specific Comments: The Congo river basin (page 389, line 24) was also highlighted as one of the areas where the earlier parameterisation was less successful (the other being the Amazon considered here). There is however no further discussion of this basin in this paper.

We chose to focus on the Amazon river basin as the Amazon shows the largest lag between peak CH₄ emissions and peak water volume timings. We now elaborate on choosing the Amazon basin as a case study in the revised manuscript.

As indicated above, the parameter κ is used to represent processes occurring in the atmosphere. Later on page 394, the uncertainty σ_{κ} in κ is stated to be $\pm 16\%$. What does this uncertainty represent?

We now include a better explanation of the uncertainty associated with κ (note: the uncertainty of kappa is explained in the response to referee 1)

It is not completely clear whether the parameterisation developed here for the tropics was extended globally or the parameterisation developed by Bloom et al. (2010), amended to account for the temperature cut-off, was used for non-tropical regions.

We perform a single full parameter estimation (including C_μ) on a global scale (section 2.3). We therefore do not use a different parameterisation in boreal regions. We now clarify the description of our single global-scale parameter estimation in our revised manuscript.

The implication of this work is that there is synchronicity of the peaks in the effective water height and the atmospheric methane measurements. The decay constants will be lower (as there is likely to be a temperature control). There would be no decay and time lag.

As outlined by the reviewer, C_μ decay in boreal regions would be small and there would be no observable time lag. We indeed find that extra-tropical C_μ turnover

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rates are significantly lower (typically $\phi < 0.001\text{yr}^{-1}$), and as a result the temporal variability in C_{μ} is also lower.

In the Supplementary Material to the previous paper, a weak correlation was observed between column CH_4 and equivalent water table height in Northern Amazonia, which the authors suggested was due to the effects of the intertropical convergence zone. Is this still seen?

The weak correlation shown in the SOM by Bloom et al. (2010) is due to the lag between CH_4 and GRACE peak values. Since we find that CH_4 peaks consistently ahead of water volume over the Amazon river basin (figure 1), we here advocate that the C_{μ} variability is a stronger hypothesis for this temporal lag.

There is a general lack of site-specific CH_4 flux data in the tropics (Table 3 of Riley et al. (2011) provides a list of the sites used to evaluate CLM). It would however be valuable to see how the parameterisation performs against site-specific measurements (or as suggested by the other reviewers, against more detailed parameterisations).

We strongly agree on the need for an inter-comparison between our approach and more detailed parameterisations. However, by constraining our model using satellite observations we are unable to downscale our parameters to the site-level without the use of additional data constraints. Nonetheless, the parameterisation is suitable for smaller scales: given site-level data such as CH_4 concentrations and equivalent water height, CH_4 fluxes and parameter values can be estimated at site-level by using this approach.

The paper by Bousquet et al. (2011) is not cited. In this paper, two top-down emission estimates derived using atmospheric inversion methods were compared with a bottom-up estimate from the Orchidee model. The top-down approach gave a global CH_4 source from wetlands of 165 Tg CH_4 per annum. For completeness, there is a recent paper in this journal (Ito and Inatomi, 2012) which also looks at global CH_4 emission estimates and their uncertainties.

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We thank the reviewer for highlighting this oversight. In the revised paper we describe the relevant findings associated with the suggested papers.

Technical comments:

We agree on all of the following suggested technical corrections.

Page 393, What does F in equation (4) represent? Presumably, by analogy with S , this is the emission flux variability after the interannual trend has been removed.

Page, 394, line 4: the 'e.g Bloom et al' is not really an example of the methodology but is based on it. This should be replaced with 'as discussed' or 'see Bloom et al.'

Page 398, line 21: the acronym ACTM (=atmospheric chemistry transport model) needs to be defined.

Page 405, Figure 1 caption: Timing (day of year) should be replaced with Timing (month of year) as the upper set of figures use 'month of year'.

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