Biogeosciences Discuss., 12, C3815–C3820, 2015
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Biogeosciences
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# *Interactive comment on* "The ability of atmospheric data to resolve discrepancies in wetland methane estimates over North America" *by* S. M. Miller et al.

**Anonymous Referee #1**

Received and published: 26 July 2015

Review on the manuscript titled 'The ability of atmospheric data to resolve discrepancies in wetland methane estimates over North America', submitted for publication in Biogeosciences by S.M. Miller et al. in May 2015.

The authors present an approach to evaluate bottom-up model estimates of North American methane emissions through atmospheric inverse modeling. In the first section of their study, they conduct synthetic data experiments to demonstrate the capability of the existing atmospheric observation network over the continent to constrain regional CH4 budgets in different seasons, and to make use of the spatial structure of surface emissions within each combination of region and season. These sections reveal that wetland signals can only be expected to be accurately detected by atmospheric observations in the (North-)Eastern part of the North American continent, and best links are obtained during the warm seasons. In the second part, the authors directly couple flux estimates from several bottom-up process models in forward mode to an atmospheric transport and mixing model to generate modelled time series of $CH_4$ mixing ratios that are then compared to the observational data of several towers. Here, they demonstrate (from the atmospheric point of view) that most models fail to deliver plausible spatial flux patterns, have too high overall emission rates and a biased seasonality.

The manuscript is well written and structured overall. Concerning the language, the authors frequently make use of question sentences, often of rhetorical nature, which I personally find quite annoying within the context of a journal article. So I highly recommend rewriting these passages. Particularly these question sentences are often unnecessarily repeated throughout the paper. The structure is clear and concise, though at some sections slightly unbalanced - in some cases (e.g. Section 2.1) you skip over many essential details and refer to existing manuscripts, while in others (e.g. 2.2) you provide many details where you could also have used citations instead.

The science behind the presented results is certainly sound, but particularly under the light of the overall objective posed by the title of the text (resolve discrepancies . . .), the interpretation/discussion is extremely lopsided, and fails to discuss important elements. The authors claim in the conclusions section (p.9357, ll.3ff) that bottom-up and top-down modelers should do a better job in joining forces to arrive at more solid estimates of methane emission budgets, a statement that I fully support (even though I don't think it belongs into a conclusion section in the way it is presented here ..). At the same time, they work out various systematic differences between bottom-up and top-down products within the context of this study, and attribute all the 'blame' to the bottom-up models, without even starting to discuss shortcomings in the inverse modeling approach which also lead to (well known) large uncertainties. Stated a bit provocative, it sounds like the authors' intention is to tell bottom-up modelers that they need to do a much better job, and better ask the top-down crowd how to do things right . . . Moreover, I was disappointed to find that the authors don't really make an effort to explain where such differences might stem from.

Summarising, I think the presented approach to evaluate the performance of different bottom-up models for methane emissions on the regional scale against top-down observations is highly relevant, since it can serve as an independent reference to those flux estimates, and has the potential of pointing out systematic discrepancies that need to be adjusted for. However, a comprehensive interpretation of the observed differences as presented herein needs to include an extensive section that discusses the uncertainties and potential biases that stem from the atmospheric inversion part of the comparison. Also, the authors should make a better effort to explain the differences they found. If these elements have been added to the discussion of the results, and the list of (often minor) details given below has been addressed, I recommend publication of this study.

DETAILED COMMENTS P.9345f, Section 2.1: As mentioned above, I think this is extremely short. Even though the details might be given somewhere else, the reader needs more information to understand what approaches you used in the context of this study.

p.9346, l.16f: You never explain and/or discuss how the low temporal resolution (monthly) of emission fluxes is actually coupled to your mixing ratio observations, which probably have a temporal resolution of 1-3hrs (details also not given in the text)? Do you assume flat temporal trends in emission rates over the course of one month, then a step change to the rates of the next month? If so, you should add a sensitivity study how this low temporal resolution in the bottom-up products affect your inversion results. Didn't you have access to bottom-up products with a higher temporal resolution?

p.9346f.: Section 2.1 is almost of equal length compared to 2.1, even though also here you could refer many of the details to e.g. the Gourdji et al. reference. These are just



BGD
12, C3815–C3820, 2015

minor details, but they make the paper appear unbalanced in parts.

p.9347ff.: the strategy of the synthetic modeling setup needs to be rewritten in some parts. Some details are only given in the last paragraph, which are required earlier to understand the concept. For example, you only mention in the past paragraph that the 16 combinations of regions/seasons are optimised separately. Also, one thing that is not clear to me: in 1000 repeats different combinations of turning the 16 options for regions/seasons are randomly created. If each region/season gets an individual model fit through the BIC approach, why do you need the repeats?

p.9349, ll.25ff: Your assigned scaling factors for EDGAR emissions should be discussed in more detail as sources of uncertainty in the simulated mixing ratio time series! What about the influence of boundary layer height, which is certainly shallower in winter, and might thus exaggerate the influence of ground sources on mixing ratio changes in the atmosphere.

p.9350ff, Section 3, first part: I'm lacking a summarising conclusion/discussion here. To what extent does the ratio of natural to anthropogenic emissions influence the detectability of wetland fluxes? And to what extent is the network configuration responsible?

p.9351f, Section 3, second part: since the patterns displayed in Figs. 3a and 3b are virtually the same, the question arises whether you can truly separate the 2 effects you are looking after. After all, it boils down to the same question: What is the ratio of natural and anthropogenic emissions in a certain region/season, and how well is the observation network designed to capture these signals. I therefore strongly recommend to explain better if the 2 steps of virtual experiments truly provide different answers!

p.9351ff, Section 4.1: I think it is a very important finding that plausible spatial patterns in CH4 emissions from bottom-up models are only based on land cover maps, not on the remotely sensed inundation maps. Here, you provide the only detailed recommen-



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12, C3815–C3820, 2015

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dation to the bottom-up community how their model estimates can be improved! So this definitely deserves a more detailed discussion, and a more prominent place in the conclusions.

p.9354ff, Section 4.2: these results basically indicate that none of the bottom-up models is useful for North American regional simulations ... the summertime emissions seem to be extremely overestimated, so that the resulting seasonal courses in modeled data are opposite of what the observations show. This isn't discussed at all ... ??? I think what definitely needs to be added here is an uncertainty estimate of the background data set, and the scaling factors of the EDGAR emissions. Given the substantial overestimates in summertime emissions by virtually all models, it's hard to imagine how these models could be re-calibrated to match the observations, given that the other modeling components are correct ...

p.9354, l.12f: you need to provide an explanation why you restricted your time series analysis to only a few sites, and why you chose those 4.

p.9355f, Section 4.3: I think it's not enough to base the seasonality analysis on relative flux contributions from each month alone. Since most of the bottom-up models (as shown in Fig.4) have very high flux emissions rates in summertime, it may well be that the shoulder season fluxes are matching the inverse product quite well, while there are simply way off in the peak of the warm season.

p.9356, ll.10ff: It's a plausible explanation that air temperatures are significantly decoupled from the conditions in the soil (where $CH_4$ is produced) for fall, but not for spring ... even if you can show through NARR that soils start thawing in April, this isn't possible without air temperatures that are appropriately high ...

p.9357, ll.3ff: As mentioned already above, this hasn't been discussed earlier, and I don't think this is the proper place to start with this kind of agenda. I agree with the general statement, but if you want to place it in a publication you need to be more constructive. Your results show that there are obviously still large discrepancies between the methane signal that is simulated by WETCHIMP models, and the methane signal as seen from the atmospheric observations. Still, you don't offer any conclusions how information from atmospheric methods might be used to improve the bottom-up models ...

p.9357, 2nd paragraph (ll.8ff): I think this part of the conclusions needs more details. You just list your basic findings, without even attempting to interpret where these differences come from. Also, you seem to assume that any atmospheric inverse modeling product (or the approach to link tower observations to surface fluxes through atmospheric inverse modeling) can be regarded as the 'truth', and all discrepancies with bottom-up products can be attributed to shortcomings in the latter.

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Interactive comment on Biogeosciences Discuss., 12, 9341, 2015.