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Interactive comment on “Methane and carbon dioxide fluxes and their regional scalability for the European Arctic wetlands during the MAMM project in summer 2012” by S. J. O’Shea et al.

Anonymous Referee #1

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O’Shea et al. investigate northern Fennoscandian wetland methane (and to a lesser degree CO₂) flux using a combination of chamber, eddy covariance and aircraft measurements and compare these against two large-scale models. The authors suggest that wetland area is underrepresented in the models, which results in an underestimation of flux magnitude.

Aspects of the analysis are interesting, but the investigation is carried out at too many spatial scales, from chamber fluxes (on the order of centimeters and seconds) to model runs on the scale of decades and hundreds of kilometers. These multiple scale mismatches mostly confuse the message and obfuscate any simple apples-to-apples type

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comparisons. It follows then that much of the analysis is what I often call a 'bunch of stuff' (analyses that are not fully related to one another) with the authors questioning their own approach for example on page 8471 line 9. An on page 8474 line 18.

The analysis was carried out competently in many regards, but in my opinion it would help the reader if the focus was simplified to describe the aircraft measurements in more detail and leave the chamber, eddy covariance, and modeling work - most of which comes from other analyses - to a comparison in the discussion. Right now the paper is trying to do three things, instead of one thing well.

Specific comments: 'The Fennoscandian wetlands' in the abstract probably doesn't encompass their entire extent. (Reading the paper confirms that this is the case.)

Page 8458 line 9 is vague: please describe these feedbacks.

The parenthetical comment on page 8458 line 17 is a bit distracting, and is highly quantitative. How are these approaches poorly constrained? What critical uncertainties remain? Use this as a motivation for the present research; at the moment it sounds like a poorly justified and vague swipe at previous efforts.

The statement at the bottom of the page regarding methane consumption is well-put. Note also the recent manuscript by Parmentier et al. <http://www.biogeosciences.net/8/1267/2011/bg-8-1267-2011.html>

On page 8459, Land surface models run at far more resolutions than just 0.5 degrees.

Far too many abbreviations in section 2.1. It's just as easy to write - and easier to read - 'whole air samples' rather than WAS.

In section 2.2, why was the Webb et al. correction applied to the closed path IRGA-measured fluxes? It needn't be, unless it was decided that the tube length is too short to fully attenuate the effects of pressure and temperature fluctuations. In this case, often a partial Webb et al. filter is applied.

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I question the use of asymptotic fits to the chamber data, unless the model is to better-fit the linear portion of the concentration/time curve. Saturating concentration/time curves often indicate that the effects of the chamber are obscuring the ability to measure flux. Please describe this section in more detail.

I know that the purpose of this manuscript is to test an upscaling approach and not explore different model formulations, but in equation 1 I would doubt that a carbon substrate parameter is needed. Methane efflux is probably not ever carbon limited in these systems. Also, is there any reason to believe that the default parameter set should be changed to better-fit observations?

Page 8464 line 24 and onward sounds like an advertisement for this model rather than a succinct and technical description of its capabilities. How is the canopy representation sophisticated and canopy conductance realistic?

Further regarding the model assumptions, CH₄ flux is controlled by diffusion, advection (e.g. through plant aerenchyma), and ebullition by bubbles. What was done to account for the (potential) impacts of the other two transport processes?

I found the error propagation approach to be sound. It assuaged many detailed concerns that admittedly would be a bit tough to measure, like PBL height and entrainment dynamics.

In section 3.3, the eddy covariance footprint depends on measurement height, sensible heat flux, and wind statistics. Sometimes it is in the 100 m to 1000 m range. What is the representative footprint dimension of the study site during the campaign?

It is interesting to show that the models dramatically underestimate methane flux, but 30+ years of data (that don't even encompass the measurement domain) is unnecessary to do so. The interesting part of this analysis is that wetland extent appears to be underestimated, but the authors don't attempt to quantify by how much except to run the models under the assumption that all pixels are wetland, which is only partially

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realistic (all pixels almost certainly contain some wetland).

In Figure 3, don't use red and green at the same time unless necessary. Here it is unnecessary.

A higher quality Figure 6 is needed.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8455, 2014.

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