

Interactive comment on “A fertile peatland forest does not constitute a major greenhouse gas sink” by A. Meyer et al.

Anonymous Referee #2

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The manuscript presents a year worth of data on CO₂, CH₄ and N₂O fluxes in a drained afforested peatland. The explicit distinction between productivity and carbon sequestration, and the consideration of non-CO₂ greenhouse gases makes this a valuable contribution to the ecosystem ecology literature. It addresses an important topic, using a range of techniques, and sheds new light to the feedbacks that operate in wetland ecosystems. The paper is clearly written and mostly well referenced. However, before being able to recommend it for publication, I would ask the authors to address the following.

1. By reporting only highly synthesized and integrated data (only annual values of fluxes) with abbreviated methodology section, it is not possible for the reader to evaluate the rigor of different data processing steps. While most of the methodology may

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have been described in earlier publications, the key points of all methods should be mentioned here, too. 2. The use of different techniques to cross-validate one another is potentially a great strength of this study. However, with limited methodological detail and overly sweeping generalizations, the mismatch between the two methodological approaches is currently not adequately analyzed and explained. The potential for mismatch between biometric, chamber and eddy covariance measurements is well documented. For example, Barford et al. (2001, Science) and Gough et al. (2008, AFM) are two classic studies discussing the reasons behind the phenomenon. The current study did not mention the main source of such differences - interannual variability in belowground allocation. It should perhaps be added to the list of potential causes of the mismatch. I recommend that a section in Discussion be explicitly dedicated to evaluating the potential sources of error and uncertainty. Right now this is cursorily done in sections 4.2.1. and 4.2.2., but not in sufficient rigor and in a systematic manner. Furthermore, some assumptions are treated as a fact, with no uncertainties or errors considered. Other components are assigned an arbitrary uncertainty level. At the very least, these values should be justified. As a preferred option, all assumptions should be critically evaluated, the performance of all gapfilling and interpolation models should be evaluated and presented. In the end, the authors probably want to be able to say that both approaches are consistent, given the uncertainty of measurements and gapfilling. Right now, this cannot be done, because the errors are not evaluated in a systematic manner. Additional and editorial comments are included in the amended PDF document.

Please also note the supplement to this comment:
<http://www.biogeosciences-discuss.net/10/C1717/2013/bgd-10-C1717-2013-supplement.pdf>

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