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Interactive comment on "Modelling effects of seasonal variation in water table depth on net ecosystem CO_2 exchange of a tropical peatland" *by* M. Mezbahuddin et al.

Anonymous Referee #1

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This study used the ecosys model to simulate carbon and nutrient cycling in a tropical peatland in Indonesia. The model successfully simulated CO2 fluxes over several years, as compared to eddy covariance measurements from the site. Fluxes were suppressed during periods of very deep and very shallow water table, relative to intermediate water table.

This study applies a sophisticated ecosystem model to a globally important ecosystem that is underrepresented in the scientific literature. Their insights into water table control on tropical peatland carbon cycling are useful and well supported. The complex interactions being studied are described clearly. However, there are some issues with the organization and interpretations of the results that should be addressed before

publication.

1. The authors assert several times that the gap-filling process introduced bias to average EC-based flux values, as an explanation for model bias. However, there does not seem to be much justification for this assertion beyond the fact that it is a convenient explanation for model bias relative to EC values. At the least, the authors should describe the gap-filling method in more detail rather than referring to another manuscript, since the results and potential for bias of the gap-filling method are stated to be important to the results of this study. Hirano et al 2007 conducted gap-filling using look-up tables created every three months and incorporating soil moisture and temperature, so these values would have incorporated changing ecosystem conditions such as water table effects, as long as they were not occurring at faster time scales than the time scale of the gap-filling method. If the authors believe the gap-filling introduced bias to EC values, I recommend that they compare the EC values with an alternate gap-filling method or otherwise attempt to assess the bias in a systematic way. The authors could refer to Richardson and Hollinger (2007) for estimates of uncertainty resulting from gap-filling, and to Desai et al (2008) and Moffat et al (2007) for comparisons of multiple flux partitioning and gap filling techniques and their potential to introduce bias and random error. Overall, the EC measurements were a major part of the study and should be more fully described in the methods section.

Desai, A. R., Richardson, A. D., Moffat, A., Kattge, J., Hollinger, D. Y., Alan Barr, et al. (2008). Cross-site evaluation of eddy covariance GPP and RE decomposition techniques. Agricultural and Forest Meteorology, 148, 821–838.

Moffat, A., Papale, D., Reichstein, M., Hollinger, D., Richardson, A. D., Barr, A. G., et al. (2007). Comprehensive comparison of gap-filling techniques for eddy covariance net carbon fluxes. Agricultural and Forest Meteorology, 147(3-4), 209–232. doi:10.1016/j.agrformet.2007.08.011

Richardson, A. D., and D. Y. Hollinger (2007), A method to estimate the additional

uncertainty in gap-filled NEE resulting from long gaps in the CO2 flux record, Agric. For. Meteorol., 147(3–4), 199–208, doi:10.1016/j.agrformet.2007.06.004

2. Modeled values are compared to eddy-covariance values throughout the study, but there is no presentation or discussion of the uncertainty in EC values. This is especially important in Fig. 5, where it is impossible to tell whether the differences in EC fluxes between hydroperiods are significant or not. If possible, the authors should estimate uncertainty in EC values and show error bars on those values, especially in Fig. 5.

3. The Discussion section presents several new sets of model results (see specific comments below). The manuscript would be easier to follow if these were presented in the Results section.

4. Fig. 6 shows a systematic positive bias in both GPP and Re in the model relative to EC values. This bias is not explicitly discussed in the manuscript, even though it could have important implications for the accuracy of the model.

Specific comments:

Abstract: It would be helpful to state the meaning of positive and negative NEP explicitly

The abstract only talks about how well the model reproduces the measured effects. It would help to include some more information about the scientific results.

Introduction:

13354, Line 25: Define WTD – this is defined in the abstract, but should be defined in the main text as well

13355, line 5: By what factor does energy yield from aerobic respiration exceed that from alternate electron acceptors?

13357, line 25: "co-existed" is a strange word choice. Maybe replace with "co-dominated"

13358, line 10: The ecosys model is first introduced here, described as "the hourly time step model ecosys". The model should be introduced more clearly as a "process-based ecosystem model" (as used below in line 17)

13368, line 6: i.e., not e.g.

Line 7-12: This paragraph uses the word "influxes", but based on the figure being discussed, but figure 5 is showing net fluxes, and has not been decomposed into influxes and effluxes. The text states that CO2 influx was suppressed in both shallow water table and deep water table time periods but was higher during intermediate water table. This does appear true in 2002. In 2004 the shallow and intermediate measurements don't appear very different, and in 2003 and 2005 none of the measured periods are separated very much between the three hydroperiods. Without any information about uncertainty, it is impossible to tell whether there is a significant separation in measured values in any year. So, while there is a clear pattern in modeled values, it is not accurate to say that the same pattern was "also apparent in EC measured CO2 influxes". The second paragraph of section 3.3 is mostly interpretation of the results and presents hypotheses for mismatches between modeled and measured values. This type of text should be in the discussion.

13369: Line 12-21: This paragraph discusses Fig. 6, but makes no mention of the clear positive bias in simulated GPP and Re.

Section 3.4: Why is there a discussion of bias in NEP but no mention of the bias in GPP and Re? The content starting with "This can be explained by . . ." should be in the discussion section. This explanation is also problematic. The authors believe the gap-filled EC fluxes to be biased, but only back this up by stating that they are biased relative to the model. Are the gap-filled values biased relative to non-gap-filled measurements? It is dangerous to assume that the model is more "correct" than the measurements, especially since the model has significant bias based on Fig. 6. It is difficult to assess the importance of EC bias when there is no way to visualize uncertainty in EC values.

Section 4.3: This section introduces a significant amount of new model results, that should be in the Results section.

Section 4.4: Hirano et al 2007 conducted gap-filling using look-up tables created every three months and incorporating soil moisture and temperature. The "complex WTD effects and biological processes" would have been reflected in the measured data that gap-filling relationships were based on, so this is not a suitable explanation for model bias.

Line 24-27: This is the first mention of methane in the paper. These values should be included in the results section, or omitted from the manuscript since they do not appear to be integrated into the rest of the paper.

Section 4.5: This is a separate model experiment that should be presented in the results section rather than introduced in the discussion section.

Section 4.6: This section includes numerous model results that were not reported in the Results section. Move those to the results section.

Section 5: 13377, line 26: It is not accurate to say that "ecosys required sophisticated coupling . . ." The ecosys results were not compared to a model without these sophisticated couplings, so the authors cannot really state with certainty whether or not such complexity was required in order to simulate GPP and Re patterns successfully. Perhaps a simpler model could have done just as well. However, the authors could accurately claim that the sophistication of the model gave them more insight into complex processes than a simpler model could have.

Interactive comment on Biogeosciences Discuss., 10, 13353, 2013.