Anonymous Referee #1

General comments

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 gapfilling 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

Reply: A new section (Sect. 2.2.2) is now included in the manuscript describing EC methodology, flux partitioning, gap-filling and uncertainty estimations in sufficient details. The reasons for systematic divergence between modelled vs. gap-filled CO_2 fluxes are now discussed in details (Fig. 1) (Sects.3.1 and 4.2). Gap-filling techniques and uncertainty estimations were, however, parts of the objectives of previous studies (e.g. Hirano et al., 2007, 2009, 2012). The main focus of this study was to simulate the seasonal and interannual trends of WTD effects on NEP and not to examine the adequacy of a particular gap-filling technique. Inclusions of comparisons between different gap-filling methods may obscure the main focus of this study and hence are not done. However, that can be a focus of a separate study.

Hirano, T., Segah, H., Harada, T., Limin, S., June, T., Hirata, R., and Osaki, M.: Carbon dioxide balance of a tropical peat swamp forest in Kalimantan, Indonesia, Glob. Change Biol., 13, 412-425, doi: 10.1111/j.1365-2486.2006.01301.x, 2007.

Hirano, T., Jauhiainen, J., Inoue, T., and Takahashi, H.: Controls on the carbon balance of tropical peatlands, Ecosystems, 12, 873-887, doi: 10.1007/s10021-008-9209-1, 2009.

Hirano, T., Segah, H., Kusin, K., Limin, S., Takahashi, H., and Osaki, M.: Effects of disturbances on the carbon balance of tropical peat swamp forests, Glob. Change Biol., 18, 3410-3422, doi: 10.1111/j.1365-2486.2012.02793.x, 2012.

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.

Reply: Estimation of random errors due to EC measurements was not possible due to the lack of multiple tower measurements. However, algorithms developed by using multiple tower fluxes over forested site with similar flux magnitudes were used to estimate random errors due to EC measurements (Table 2). Error bars are now added to the graphs of seasonal (Fig. 6) and diurnal (Fig. 7) NEP indicating whether or not the difference in mean NEP between two hydroperiods are greater than the variation in NEP within each of those hydroperiod. Single factor ANOVA (Fig. 6) (Sect. 2.2.6) was also performed to test the significance of WTD effects on NEP throughout the study period.

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.

Reply: Those new sets of model results in the discussion are now moved to the result section (Sect. 3.5).

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.

Reply: The systematic divergence between modelled and EC-derived GPP and R_e are now examined in detail (Sect. 4.2). Some of the causes are also discussed in the Specific Comments.

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.

Reply: NEP sign conventions are now included in the abstract. Also major scientific findings in the study are now added to the abstract (line 26-32).

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

Reply: Defined.

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

Reply: Differences in energy yields related to reduction of oxygen vs. alternate electron acceptors varies with different alternate electro acceptors. For instance, microbial energy yields from reduction of O_2 vs. that from reduction of dissolved organic carbon (DOC) are 37.5 vs. 4.4 kJ g C^{-1} (line 638).

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

Reply: Replaced.

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)

Reply: Done.

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

Reply: Corrected.

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".

Reply: 'Influxes' are now replaced with 'downward fluxes' and 'effluxes' with 'upward fluxes' or 'nighttime fluxes'. Error bars are now included in the figure showing the separation of diurnal net CO_2 fluxes between different WTD hydroperiods (Fig. 7). The agreement and disagreement between modelled vs. EC-gap filled diurnal trends of WTD effects on NEP are now reported in detail (Sect. 3.3). WTD effects on R_e are now further discussed from the differences of nighttime EC-gap filled CO_2 fluxes among the hydroperiods (Sect. 3.3). WTD effects on GPP are now discussed from the differences between the magnitudes of WTD effects on daytime net CO_2 fluxes and that on nighttime net CO_2 fluxes (Sect. 3.3).

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.

Reply: They are now moved to discussion section (Sect. 4.2).

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 gapfilled 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.

Reply: Modelled R_e was systematically larger than EC-derived R_e predominantly in the rainy seasons (November-April) which was consistent throughout the study period (Fig. 1) (Table 2). However, modelled CO_2 fluxes for the same hydroperiods had better agreement with EC-measured fluxes (Fig. 1) (Table 2). Despite similar measured vs. modelled soil water contents and air-filled porosities, gap-filled CO_2 effluxes were much smaller than the modelled CO_2 effluxes during the rainy season (Fig. 1).

Systematic uncertainties embedded in EC methodology were also thought to contribute to larger modelled vs. EC-derived monthly and annual R_e estimates. Nighttime EC NEP decreased with u (friction velocity) in our study site indicating the dependence of nighttime CO_2 flux measurements on above-canopy turbulent mixing. However, biological production of CO_2 by plant and microbial respiration was independent of u* in the model. Thus low u* threshold can induce substantial underestimation of EC-derived R_e estimates particularly in tropical rainforest ecosystems which are not in the case of the modelled R_e estimates.*

Larger modelled vs. gap-filled R_e contributed to larger modelled vs. gap-filled annual GPP (Fig. 8) (Table 3). In EC datasets, GPP was derived from extrapolated daytime R_e (Sect. 2.2.2) and hence smaller gap-filled vs. modelled nighttime R_e would cause smaller EC-derived GPP. Unlike EC-derived R_e that is used to calculate EC-derived GPP, modelled R_e was driven by modelled GPP through fixed C products and through root exudates and litterfall. A further cause of smaller EC-derived vs. modelled GPP could have been the incomplete (~80%) energy balance closure in EC measurements vs. complete energy balance closure in the model, which would reduce EC-derived ET and also possibly GPP (Table 3).

All of these above mentioned sources of larger modelled vs. EC-derived R_e and GPP estimates were related to EC methodology and gap-filling. These larger modelled vs. EC-derived R_e and GPP aggregates, however, could not be resolved in our modelling since, unlike EC datasets, every single mole of CO_2 that was modelled from fundamental ecosystem processes was counted in the modelled C budget (Please see Sect. 4.2 for further details).

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

Reply: Now moved to result section (Sect. 3.5).

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.

Reply: A more detailed explanation for divergence in modelled vs. observed annual trend of WTD effects on NEP are now included (Sect. 4.2.2).

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.

Reply: Methane is now removed from the manuscript.

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

Reply: Now moved to result section (Sect. 3.5).

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

Reply: Now moved to result section (Sect. 3.5).

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.

Reply: Rephrased (line 844-851).