

Responses to comments of both reviewers are given in italics below:

Reviewer no. 1

The sign of the units reporting depth are inconsistent. Removing the negative sign on the units of water table in figures 2, 6, 9, 10 and 11 would ease comprehension. Particularly in figures 9 and 10, where opposing conventions of reporting depth are used in the same figure.

The sign of all water table axes in the Figures have been changed to positive values.

Additionally, it is ambiguous whether the air temperatures reported in figures 2, 3, 4, 5 and 7 are modeled or measured and whether or not there is any meaningful difference.

All air temperatures were measured and used unchanged in the model.

A typo appears in the abstract, line 23, where it reads “In sensitivity test of the model”, and presumably should be either “In sensitivity testing of the model” or “In sensitivity tests of the model”.

Corrected.

A more substantive concern is raised by the rise in the water table at day 210 in 2006. The observed rise in figure 2q appears to be nearly half a meter, from about 0.7 to about 0.3 meters below the surface. While the modeled rise looks only to be a change of 0.05 meters, from about 0.75 to about 0.70 meters below the surface. This moistening corresponds to an observed reversal of the sign of the CO₂ exchange that appears well represented by the model. Inspection of figure 9 suggests that the CO₂ response in the model was insensitive to water table depth; perhaps that the moistening of the unsaturated column rather than the change in WTD was responsible for the efflux of carbon. If this is the case, it would be valuable to test the model’s response to an accurate change in water table depth.

The modelled change in NEP at this time was driven by surface wetting, as shown in Fig. 7 and in lines 371-373. The modelled changes in NEP to accurately modelled changes in WT are given in Figs. 3, 4 and 5.

Further inspection of figure 2 suggests a consistent overestimate of carbon efflux in the spring and fall that appear correlated with high temperatures. Further comment about this than is found in section 3.2 could help elucidate the behavior of the model; particularly since this behavior was also seen in Sulman et al. (2009) as discussed in section 4.1.3.

I have added further comment about these emission peaks on lines 570 – 578, and a references to a study in which similar emission peaks were measured in another boreal fen.

Also in section 3.2, the modeled wintertime water table depths are explained to be below observed depths in most years due a lower calculated bulk soil water potential in the presence of ice. Should this

be calculated as such? And does the associated wintertime efflux of carbon impact the interpretation of the net carbon balance in tables 2 and 3?

I have clarified the cause of deeper winter WT in the model in lines 317 – 319. The model output for WT depth is calculated from air-filled porosities of the soil layers but does not itself affect gas exchange in the soil. Gas exchange during winter in the model is strongly limited by low temperature and by low air-filled porosity in frozen soils. However small wintertime emissions sustained over several months affect the NECB in Tables 2 and 3, as has been found experimentally.

Reviewer no. 2

As it is indicated in this paper, WTD is one of the key biophysical parameters to determine the biogeochemical processes in wetland ecosystems. Since Ecosys modelled the WTD based on the vertical and lateral flow, is it reasonable to compare the modelled WTD with the measured WTD to show the model's performance as well (in terms of a, b, R², etc, same as the one done for C fluxes)?

This could be done during the ice-free period, but what constitutes a successful test? Acceptable parameters for tests of modelled vs. measured CO₂ fluxes are well established from earlier studies, but such parameters for those of WT depth have not been established. The performance of the model for WT depth can already be assessed from Fig. 2.

P5580 Line 2-4: If all the processes are site-specific, what can we model them in a global scale or even a regional scale?

We can model regionally or globally with site-specific input datasets for weather, soil and external WT depth at regional or global scales. There is a lot of work in progress to develop these datasets. In fact, we are currently involved in a project to model terrestrial productivity of North America, although wetlands are not yet included.

P5580 Line 14-17: It is confusing that greater WTD and shallow/deeper water tables were mentioned in the same sentence. What do you really mean for the greater WTD? Does it refer to shallow or deep WTD? Moreover, what do you mean diurnal CO₂ influxes and effluxes?

Greater WTD means deeper water table. Increases in WTD means deepening water table. Diurnal CO₂ influxes mean daytime CO₂ uptake, diurnal CO₂ effluxes mean nighttime CO₂ emission.

P5586 Line 12: Add in before the soil surface layer.

Done

In Fig.1, what are the dz, dt and Lt?

Definitions of these terms have been added to the legend of Fig. 1.

In Table 1, RMSD was said to be RMSE? It is better to explicitly explain what are RMSE and RMSD here.

A definition of RMSE has been added to the legend of Table 1 to supplement that in the text.

P5591 Line5-15: you have modelled hummock and hollow C fluxes separately with different configuration and parameterization. These modelled C fluxes represent the micro-scale C fluxes. However, how did you compare them to the EC measured C fluxes, representing the spatially-integrated landscape-scale C fluxes? Have you done any scaling-up for this comparison? Please check Wu et al. (2011) in Ecological Modelling, which addressed how to deal with the microtopography, for more details.

C fluxes modelled over the interconnected hummocks and hollows were averaged, based on similar areas of hummocks and hollows observed at the experimental site, as stated in section 2.2.3 and again in section 3.1.

P5592 Line 13-14: Do you have any idea why this happened? Is it possible to associate with the issues on the modelled WTD?

This issue is now discussed in greater detail in lines 571 – 583.

P5592 Line 22-24: Do you mean influx refers to downward flux (i.e. C sink) and efflux refers to upward flux (i.e. C source)? If so, it is better to indicate them here to clarify.

That is correct. I believe these terms are already well defined.

P5592 Line 25: From Fig.3 a and d, I cannot see the weather for the two years are comparable. Can you justify this statement (i.e. comparable weather for year 2002 and 2006)?

There were similar ranges of radiation and temperature in both years during the periods of comparison, as now stated in the text.

Fig. 3,4,5 and 7: It is hard to see which one represents which one for Fig.3/4/5 b,c,e,f and Fig.7 b,c. Can you make some changes so that each presentation in these figures is more distinctive?

I have thickened the lines and changed some colours to make these graphs clearer.

P5593 Line 9-11: Similar comment to the one for P5591 Line 5-15. Here hummock and hollow C fluxes were modelled with different configuration and parameterization separately. These modelled fluxes represent the micro-scale C fluxes. However, they were compared with the EC measured C fluxes, which represent spatially-integrated landscape-scale C fluxes, covering the footprints of the EC tower. So how can you make these two fluxes comparable, although they are at different spatial scales? Have you done any scaling-up work for this comparison? Please check Wu et al (2011) in Ecological Modelling that addressed how to deal with the microtopography for more details.

As mentioned earlier, modelled fluxes were spatially averaged over interconnected hummocks and hollows. Implicit in this comparison with EC fluxes is the assumption that this treatment of microtopography represents the fetch area.

Table 2: How was NEP calculated? Can you indicate what refers to the negative DOC+DIC and NECB? How did you calculate the average from hummock and hollow fluxes? It is good to include the EC measured NEP in this table for clear comparison.

I have added modelled and EC-derived EC NEP to Table 2, and defined positive vs negative NECB and NEP, as well as averages for hummock and hollows.

P5594 Line 26-28: Is it possible to justify this statement (i.e. driven more by variation in MAT than WTD) from model's sensitivity test or by regressing the annual GPP/NPP vs. MAT and WTD? P5595 Line 5: Is it possible to justify this statement (i.e. driven more by variation in WTD than MAT) from model's sensitivity test or by regressing the annual Rh vs. MAT and WTD?

I have added R2 from regressions of NPP and Rh on MAT and WTD from Table 2 to the text in lines 403 – 408 that corroborate these observations.

Fig 9 and 10: It is hard to see the distinction among these three categories. Can you change the graph scheme to make them more clearly to be distinguished?

I have further edited Figs. 9 and 10 to clarify distinctions.

P5597 Line 7: Add than after less and before those in NPP.

Done

Table 3: In this table, it seems that all other variables, except water table, which was shown for hummock, were shown for average over hummock and hollow. To be consistent, why not also show the water table for average over hummock and hollow?

Because the hummock and hollow were interconnected, the WT was level so that WTD in the hollow was 0.075 m closer to the surface than that in the hummock, as explained in sec. 3.3.

Section 4: Please double-check where the past tense should be properly used and where the present tense should be properly used in the Discussion section. For example, in section 4.1.1, it is difficult to follow when past tense was used for most cases.

I had used the present tense in the Discussion when referring to model equations which are of course still in use. However I have changed these to the past tense.