

***Interactive comment on* “The impact of a declining water table on observed carbon fluxes at a northern temperate wetland” by B. N. Sulman et al.**

Anonymous Referee #2

Received and published: 10 March 2009

This paper reports on changes in wetland water table (WT) draw down over a six-year period and the resultant effects on ecosystem carbon dioxide exchange measured using eddy covariance. Comparisons are made with near-by upland forest sites in order to substantiate that water table influence is the driving process at the wetland site. The main conclusion reached is that draw down of the WT affects ecosystem production (GEP) and CO₂ loss (ER) in such a way that the net exchange NEE is not correlated with WT change. On the whole the data they provide support this view. However, I am not convinced that the comparison of annual C exchange and yearly WT are the best way to demonstrate this. The authors need to be careful about how strongly they state their conclusions. Primarily, they have used correlation analysis (e.g., GEP vs WT, etc.). Such results do suggest that a process may be occurring, but statistically

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significant correlations are not proof of 'cause and effect', nor is the lack of a correlation. The wording in the manuscript (e.g., bottom of pg. 8) should be changed. In addition, there are problems with the discussion of the results that need to be addressed. I have additional concerns about the some of the peripheral analyses presented. The paper could be shortened and tightened up before publication. Main points are as follows.

1. The Discussion section of this paper is too long and not very focused. It could be tightened up into a more coherent argument, rather than the current series of sub-sections discussing each results section, which leads to some redundancy. A few points about the discussion in general are: 1) Pg. 12, lines 369-376; this section is rather peripheral to the paper and could be dropped, 2) the following section on ER and GEP is particularly weak, referring to the Cook et al. 2009 citation in preparation for the increase in biomass is not useful, bringing the data into this paper would greatly strengthen the results and interpretation, 3) The speculation in this section about wetland hydrology and carbon feedbacks is too vague, there are many different types of wetlands, and the feedback suggested here would not apply to all wetlands, 4) section 4.4 is confusing, and is not well supported by the annual data presented in most of the figures, what is the explanation for similar WTs in 2006 & 2007 but quite different growing season precipitation?, 5) the WUE discussion is suspect (see below), not helpful and sheds little light on process, 6) 4.6 should be dropped, although tantalizing, the information is too thin to be useful here is such a brief mention, a more thorough analysis is needed to make any real sense of these data. 2. Introduction section (and throughout the paper) the reference to previous wetland literature, especially those studies dealing with drought and WT effects, is rather limited. I would suggest a better summary of this past literature. 3. The site description (especially for Lost Creek, since it is the main site of interest) are weak. More detail is needed. For example, what does the soil profile look like; this is extremely important information given the nature of the subject such as the ecosystem respiration. 4. Pg. 6, transpiration calculation; it is hard to accept that this calculation of ecosystem transpiration

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is accurate. The modeling of the substrate aerodynamic (r_d) and soil resistances (r_s) are unsubstantiated and it is not clear how r_d would reflect that change in the canopy that occurred over time. The purpose of these computations is to derive canopy water use efficiency (WUE). As it forms only a small part of the analysis it could be dropped from the paper. The text does not give an adequate explanation of why WUE should behave this way. Also, it is not clear what a yearly value of WUE represents as WUE is a variable that has distinct seasonal and diurnal trends. Alternatively the authors might use the eddy fluxes of evapotranspiration and GEP to compute an ecosystem WUE, however, its interpretation will need considerable thought as the drivers are changing through time in this study. I suggest the former option. If the WUE piece is kept, the description of the calculation on page 10 should move to the methods section. 5. The linear fit in Fig. 1 is nonsense, it may help to reinforce the author's point that WT's are getting deeper with time, but besides being unsupported by any statistical data, it is inappropriate for this time series. In fact, the data seems to show that the deepest growing season WT occurred in the year 2003, this brings into question the issue of using annual data for most of this analysis and begs the question why 2003 does not stand out against the years 2006 and 2007, which are portrayed as the years with lowest WT. On the whole I think that the conclusions reached in this paper about WT effects on C exchange are likely correct, but are not well demonstrated. 6. The conclusion section is rather redundant; it could be shortened and more concise.

Other points to consider:

7. On page 7 a bit more detail could be provided about these modeling procedures, e.g., how large was the moving window for ER and GEP? 8. Pg. 9 / 10, lines 276-291 – this discussion of the GEP variable is rather wordy and confusing. It could be improved. As well, the symbols for the plot variables should not occur in this text, they should only appear in the figure captions. 9. Pg. 10, lines 309-311 – the claim here is that 2007 had an unusually dry growing season resulting in abnormally low GEP. However, in much of the data you show (based on annual averages) indicates

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2006 had a lower water table. The difference in these facts needs to be clearer, Figure 1 is not detailed enough to determine this difference. The text here refers readers to Sect. 4.4 for an explanation, yet this difference between growing season and the annual averages is still not clear. 10. Pg. 11, line 335 – the authors should be careful to note that the Silvola et al. 1996 study measured soil CO₂ emission , not ecosystem respiration. 11. Pg. 11, lines 357-343 – this discussion of the effects of WT draw down, might also consider that some acclimation of the ecosystem may occur after a period of time, such behaviour has been suggested for arctic tundra (see Oechel et al. 2000, Nature). 12. Fig. 2 could benefit from labeling the points on the plot by year. 13. Figs. 4 and 8, the dots in these should not be joined by lines. IN fig. 4 it is not clear if the WT data are an annual average.

Interactive comment on Biogeosciences Discuss., 6, 2659, 2009.

BGD

6, S398–S401, 2009

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