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Biogeosciences Discussions

Interactive comment on "McGill Wetland Model: evaluation of a peatland carbon simulatordeveloped for global assessments" *by* et al.

et al.

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1. We have looked at the Zhuang et al. (2006, GRL) and do not really understand what the reviewer is specifically requesting. This paper has no model evaluation in it – it examines the relative importance through simulation of climate change and carbon feedbacks for northern landscapes, with and without the inclusion of fire. The MWM model does have a lot of similarities with Frolking et al. (2002) as PCARS formed the basis for the general structure of MWM. In fact, the work presented in this paper was motivated to bring PCARS into a format that was compatible with the general model structure of the Canadian Terrestrial Ecosystem Model (see Arora 2003 etc) and therefore, at some point in time in the future peatland might be included in climate simulations. We think we are clear on this in the introduction of the paper as seen in

the quote below.

"In this paper we develop a model, based on the general Peatland Carbon Model (PCARS: Frolking et al., 2002), but that has the same general structural and functional components as the Canadian Terrestrial Ecosystem Model (CTEM: Arora 2003, Arora and Boer 2005a & b, 2006), the terrestrial C model developed for inclusion in the Canadian Centre for Climate and Model Analysis (CCCma) coupled general circulation model."

In Frolking et al (2002) there is not a rigorous analysis of model performance against direct measurements as presented in the present paper. We outline clearly the method of evaluation and apply those statistics directly. We do a similar analysis to Frolking when we present scatter plots similar to Figures 5b and 8c.

However, as requested at another point in the review we will add a new table which is the correlation between simulated and observed annual GPP, ER and NEP (see new Table 2).

2. There is a tremendous amount of information in Table 2 and 4. We have looked at making bar graphs out of these tables and find the bar graphs more confusing than the original tables. We believe to convey the same amount of information in the two tables we would need at least four bars graphs (Table 2) and the bars would be bunched together for eight years plus the eight year mean. For Table 4 this would add at least another 3 bar graphs. Finally, we believe that the reader benefits from having the actual values of the results as they can do their own analysis of the model performance and sensitivity as this reviewer has done. If we had used bars graphs reviewer 2 (and McGuire the another reviewer) would not have been able to do their own analysis that lead to the very constructive comment on the performance of the model for the annual values. We have addressed this by doing the Spearman's correlation analysis as suggested by this reviewer and will include this in a revision.

3. Most of the values in Table 1 are taken from the literature from studies done on many

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different northern peatlands (e.g. Letts et al. 2000 is a broad survey of hydraulic properties from over 30 or more studies) or laboratory studies (e.g. Williams and Flanagan 1998). There are only two parameter sets that are derived from Mer Bleue. The Vcmax25 for the shrubs was determined from a study done on Mer Bleue as no other studies describing these parameters exist in the literature. The initial biomass estimates also come from Mer Bleue. This is required to initialize the model. However, the biomass at Mer Bleue is within the range of that found on northern shrub dominated bogs. The comparison on the biomass observations at Mer Bleue with those reported in the literature can be found in Moore et al. (2002).

Response to several of the specific comments.

1. We have done the Spearman's correlation analysis on the annual values of GPP, ER and NEP and will insert the following sentences in a revision (excellent suggestion):

"We did a Spearman's correlation analysis between simulated and observed annual GEP, ER and NEP. The results, all significant with a $p > 0.10$, are 0.67, 0.63, and 0.57 respectively. As expected the coefficients are higher for GPP and ER than NEP, as any errors in GPP and ER are propagated in NEP"

2. We will add the reference to Willmott's index of agreement in the revision. This should have been in the original manuscript and we were remiss to not include it. The reviewer is quite correct to ask what the index of agreement is. In Comer et al. (2000), which we have cited at another location in the paper, we present the statistical formula for this index – we cite Comer here now to allow the reader to see how the Willmott index is derived, but this is also shown in far more detail in the original reference of Willmott 1985 - this has been added to the reference list of the revision.

Willmott, C. J., 1985. Statistics for the evaluation and comparion of models. Journal of Geophysical Research, 90:8995-9005.

Interactive comment on Biogeosciences Discuss., 5, 1689, 2008.