Interactive comment on "Carbon stocks and fluxes in the high latitudes: Using site-level data to evaluate Earth system models" *by* Sarah Chadburn et al.

Anonymous Referee #2

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General Comments

This paper describes a study in which 3 land surface models are assessed against site-level evaluation data on some of the key indicators of carbon dynamics in high-latitude terrestrial ecosystems. The analysis and comparison of models were conducted such that the study identified particular, problematic issues for these models in capturing carbon cycling and related mechanisms important in simulating arctic ecosystem processes. From these, the authors recommend three issues as priority areas for model improvement in representing high-latitude components of earth system models. The paper presents a set of analyses that were well-designed to evaluate key processes in

these models. The paper is well-written and organized in such a way to give the audience a straightforward and easily understandable read of the methods and results of the study. A strength of this paper is the authors' interpretation and synthesis of the results into three, clearly outlined recommendations for model improvement. While none of these are necessarily "game changers" in the way we think about the problem, the results of this study will be of interest and use to those members of the modeling community who are working to build or improve their simulation of high-latitude ecosystem processes. Some additional thoughts on the motivation, objectives and hypotheses or expectations of this study might help define the scope of inference for this paper. But, the authors do take good care not to overreach on the conclusions, and appropriately offer several caveats of the analysis and remaining uncertainties that are not addressed with this study.

Specific Comments

While the study is laid out well in the paper, I do wonder if some re-structuring and/or additional information could strengthen the emphasis on the key ecosystem processes being studied here, i.e. the 6 or so pieces serving as subsections of the results. More text on background and motivation could be added to the introduction for each, including more references for previous studies that have explored these issues. Perhaps it'd be useful in the introduction to add a sentence on each issue that frames them as questions or hypotheses as a preview of what the reader can expect to see in the analysis and results. Next, the authors might consider moving the 'Methods' / 'Evaluation data' more up front, ahead of the model and site descriptions. This way, the descriptions could then focus on the model and site information most directly relevant to the specific evaluations conducted here. One example, as pointed out in a previous review comment, is that PFT information is not included for JSBACH – and for the other two apparently in the end are effectively just cold C3 grasses? Here's how this might look: rather than subsections for each model, it would read "here we evaluated model A (ref), B (ref) and C (ref). For process X, model A & B are similar in that they do Y, but

model C is different because it does Z". Again this puts the emphasis on the process and organizing it this way allows the reader to keep track of and more directly compare the models across the key evaluations presented in the results. Another potential addition to consider, at the beginning of the methods perhaps, is a short but explicit definition / overview of the scope of inference of the study. The scope has three angles, which follow the subsequent subsections of the methods, i.e. (1) indicators (evaluations included in this analysis, e.g. snow, ALT, soil C, CO_2 fluxes, etc.), (2) processes (collectively included / compared among these 3 particular models), and (3) geography (climate, vegetation, permafrost etc. conditions across the 5 sites). Line 418: the suggestions above may help to expand on this statement about what / how C dynamics are intrinsically linked to the physical state of the system... 420-1: consider adding a sentence or two to point out the important, relevant results of the three previous studies referenced here. Did those results provide direction / motivation for this study? 469: the partitioning of CO_2 vs CH_4 is appropriately mentioned as a key issue here, but it is not evaluated or discussed elsewhere; do these models even simulate CH_4 fluxes? 503: there is "very little carbon" – is that true when aggregated over the whole profile? Perhaps a 6th panel could be added to show / compare the total stocks simulated? 514-515: this implies that vegetation C via GPP is the only input, but would orchidee and jsbach also have inputs from thawed permafrost C that should be considered in the turnover rate?

Technical Corrections

I did not find any obvious technical errors in the text, but there are a couple of issues with the figures like inconsistent and/or missing labels for the site names in the mean and spread panels, lacking an explanation of what the error bars represent in Figure 4, and lacking explanation of what the dotted lines refer to in Figures 4 and 5.