Interactive comment on "Large carbon cycle sensitivities to climate across a permafrost thaw gradient in subarctic Sweden" *by* Kuang-Yu Chang et al.

Anonymous Referee #2

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General comments: This study applied the ecosys model to predict soil thaw dynamics, NEE, and CH4 fluxes across a permafrost thaw gradient encompassing palsa, bog, and fen at a subarctic peatland. The authors also investigated impacts of potential climate bias on the simulated active layer depth (ALD), NEE, and CH4 fluxes. My major concern is that this manuscript is lacking of clearness for methods and explanation/discussion for some results. While the authors cited a lot of ecosys related references, it is not clear how this model simulate ALD, water table, NEE, and CH4 fluxes (i.e. lacking of the model structure/principle/processes related to these variables) and how the authors set different parameters for palsa, bog, and fen to simulate these variables for different land types (i.e. lacking of description for setting of some parameters;

please check specific points). I therefore suggest adding these contents. In addition, I noticed some indigestible results, but did not find explanations/discussions related to these results (check specific comments). I suggest adding explanations/discussions for these results. Specific comments: Line 151: ALD should be defined at the first place of 'active layer depth'. Line 151: For clarity, I suggest changing '35 cm' to '35 cm below the peat surface'. Lines 240 to 241: Did you mean that you used the climate data from 1901 to 2010 for model initialization? If so, which year's results were used for analysis? Line 268: How about the values for bog? Lines 264 to 274: How about the vegetation parameters for these three land types? Lines 296 to 298: In this sentence, did you want to say inter-annual variability of GSWP3 temperature is smaller in summer. If so, I suggest adding information of the inter-annual variability, in addition to information of underestimation. Lines 336 to 337: This is not accurate; I noticed some points of net CO2 emission during summer from the figure 4. Line 345: What is the meaning of 'different subsites' in this and other places? Different chambers for a peatland type? Lines 351 to 356: I noticed consistent over-predictions of net CO2 uptake for bog. Could you please provide some explanations? Lines 372 to 375: It is not clear for me how the authors simulated different water table for different land types without considering lateral water transport. Was the simulated different WT driven by different ET among the types since they had the same rainfall? In addition, why this is 'a particular issue' for bog considering that fen receives a large amount of water from a lack (Line 153)? Line 390: The 'weaker CH4 emission variability measured across subsites' is confusing. Line 399: I cannot catch this sentence. The model can produce hourly/daily results, so it is easily to calculate seasonal cumulative NEE directly using the simulations. Why you calculate it based on the seasonality identified in another paper? Lines 427 to 429: Could you please explain why the simulated ALDs at palsa and fen under cold and wet conditions are shallower than that under cold conditions? This seems not consistent with the comparisons between wet and control. Lines 451 to 452: Why the fen showed weak CO2 emissions under cold and wet conditions and net CO2 uptake under cold conditions, due to reduced NPP and/or increased soil res-

piration under wetter conditions? I cannot understand the large impacts of wet on CO2 emissions at fen given that WT is close to or above ground surface for this site (Figure 5). Lines 483 to 487: Could you explain the simulated negative impacts of wet on CH4 emissions at bog and fen; in particular for the cold and wet scenario, why the CH4 emissions was simulated close to zero?

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