Reviewer 1 (Joshua Ratcliffe)

I have read the revised manuscript and the detailed response of Alekseychik et al to my earlier comments. I am happy to say that I wholeheartedly approve of the changes that have been made. Alekseychik et al., have provided extra figures in their written response and in the revised manuscript. These figures show their interpretation of the data is correct, even when accounting for the seasonal differences in data coverage across years. In my opinion the manuscript is suitable for publication without any further changes. I look forward to reading further work from Siikaneva in the coming years.

As an aside, I noticed that contrary to what is stated in the response to the reviewers the additional row with the flux means excluding the years with the worst data coverage have not been provided in table 5 (now table 6). I am happy for the authors to have the final decision on this, but I still think this addition would be beneficial and it is likely an oversight to have not included it in the revised manuscript.

Dear Joshua, the authors of this manuscript thank you for the extensive commentary you have provided and all the improvement which resulted from it. Regarding the addition to Table 5 (6), it was an oversight on my side, I simply forgot to add this extra line, but now it has been done.

Thank you,

Pavel Alekseychik et al.

Reviewer 2 (Anonymous)

After completing my review of the earlier manuscript and some thought about the modeling methods currently used for the temporal gap-filling that the reviewers (including me) were critical about, I think that my concerns would be best addressed using a process-based model rather than the statistical models currently used. A process-based model would allow a simpler but also more comprehensive framework for understanding these multiple years of measurements with large data gaps and would improve the rigor of the modeling approach. This would help in calculating the seasonal C balances and the results could be compared to the statistical methods currently used. Multiple years of measurements offer a chance of independent validation of the site level parameterization. Some examples of process-based models could be HIMMELI or DNDC. Particularly if these methods are used, I think it is likely that the authors will be able to revise the manuscript sufficiently to address my earlier criticisms while ultimately simplifying a complex approach. My apologies that I didn't think of this in time for my earlier review.

Dear Reviewer, I thank you for your strive to improve this manuscript and the numerous in-depth remarks you have made. I feel that the manuscript has benefitted a lot from the improved clarification of the modeling method, assessment of the uncertainty and other changes that you had requested.

Having said this, I would like to argue in favor of the present modeling approach and against changing for a process-based model. First, as we all are well aware, process-based modeling requires a substantial amount of time, effort and thinking, which is unavailable for the study at hand; it would amount for a new study of the Siikaneva-2 bog data, which will definitely see the light of day in the future. Second, process-based modeling or comparisons between different modeling approaches is outside the scope of the paper. The current paper was planned as solely an exploratory and descriptive

study of the accumulated EC data. Third, for the stated purposes, the present model seems to be quite adequate, which is now explained on LL. 297-302. The R^2 and model/measured flux ratios seem quite suitable for this study. Apologies for omitting these model goodness criteria in the earlier version. I have also considered the earlier experience of Raivonen et al (2017), indeed a colleague of mine, with the HIMMELI model application to the data from the Siikaneva-1 fen. They found that the model performed well in that it realistically simulated the effect of the primary drivers such as LAI, WTD, T_a and T_p. Nevertheless, note that HIMMELI did not capture the mean level of the fluxes as well, which would be an important feature for gapfilling. I expect that the HIMMELI performance would have be similar, when run to simulate the Siikaneva-2 bog.

Given the above reasons, I would like to argue for leaving the process modeling for a dedicated future study, and retaining the present modeling/gap-filling approach in this manuscript.