Reviewer #1

The authors' revisions to the manuscript (MS) in response to previous comments are adequate. My recommendation is for the MS to be accepted after minor revisions are completed. These revisions should cover two points :

1. The authors could add more text/discussion (page 19) on the role of C3 plants on dead tissue production and decomposition as those sites with C3 % > 0 clearly have different C capacity (and estimated uncertainty). Are the authors arguing that plant species composition's role on C storage capacity and saturation is "negligible"?

We appreciate this comment and have included a new paragraph focused on the potential impacts of C3 abundance on C cycling dynamics (L 423-435). In brief, we did find that the two sites with the highest C3 abundance had faster turnover rates of aboveground plant tissue and 'fast' SOM pools. However, this didn't result in particularly low C residence times due to slower root turnover rates at one site (HPG) and low sensitivity of C turnover to temperature and soil moisture (CPER).

2. I understand that soil CO2 flux data (soil respiration) were collected and assimilated but (a) the references to the measurement method are missing and (b) the limitations of the method and the role of the uncertainty of the collected data are not discussed. Soil respiration CO2 fluxes are very variable (temporally and spatially) but they are, also, a large flux of the C cycle at the ecosystem scale. This means that the uncertainty of these measured data and how it is treated in the assimilation process are too important to not discuss. For example, it would be good to see time series of measured soil respiration (with error bars) per site as well as some text on how the uncertainty around soil respiration CO2 (and other assimilated variable; in fact) is integrated into the MCMC process i.e. what metric of model forecasting skill is used in the assimilation process and how does it allow for measured data uncertainty to be considered?

We created a new supplemental figure (Fig. A9) with all the raw surface CO2 flux data used to drive the data assimilation process, along with the average uncertainty associated with cross-plot estimates. We also included text in the discussion concerning this uncertainty (L506-510).

Overall, I believe the authors have made a good job with revisions. With some additional minor revisions the MS would be more complete. Finally, I believe that the authors could refer to the relevant literature more when building their case for data-model integration. It is not necessary to do so but it can make the article a better read. The number and quality of studies on ecosystem C cycling quantification through data assimilation has been increasing since ~2010. Fusing model predictions with data measured at ground level and obtained via UAV/airplane/satellite-born instruments improves the robustness and the spatial/temporal resolution of C cycling estimates.

We appreciate this suggestion and now have expanded our discussion of data-model fusion into the introduction L85-90.

Reviewer #2

The manuscript is much improved after its response to reviewer comments.

We appreciate the kind words.

There is much greater clarity thanks to less reliance on study specific variables and the addition of a new Table 1. However, the same issue remains when referring to the model parameters. I suggest that giving a description on first use in each major section, i.e. don't rely on the reader having read all of the methods to find the definition of mscut, as the paper is still quite dense. Similar issues can be found in the discussion.

We incorporated this suggestion two ways. First, we now have updated the Table 1 with our terms to include the model parameters. Second, we define model parameters as they come up in the results and then again in the discussion.

The Discussion has been much improved with its more expansive engagement with the literature.

Great to hear!

Specific comments (line numbers refer to the track changes version):

L165: "(aboveground biomass R2=0.99, belowground NPP R2 165 =0.94)." I think you should include some rmse and/or bias information here too.

Done

L171: "but it also requires higher temporal resolution of data to be successful." This is a little simplistic. The information content / requirements for DA to be successful depends on multiple factors including, combination of data types, their uncertainty, the specific model in addition to data quantity. I think a more generalist statement on data richness would be more accurate.

We now incorporate a more inclusive statement about data needs for data-model fusion to be successful.

Figure 2 - this section of the analysis assumes a linear association between MAP, MAT and residence time. I suggest that some non-linear, threshold or bounding behaviour are also possible not best illustrated with this plot. A scatter plot would be more informative.

We now include the raw relationships in Figure 2. Additionally, we tested goodness of fit between log-linear and linear models for each relationship and the linear models always produced the best fit.

Figure 4 - I think this figure should go to the SI and be replaced with one which focuses more closely on the sensitive parameters. Doing so should improve the readability of the figure.

Done. The old figure 4 is now Figure A10, and we focus on four parameters in the new Figure 4 – c5, c6, mscut, and Q10. We think this greatly improved the readability of this figure as the reviewer suggested.