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Interactive comment on "Patterns and controls of inter-annual variability in the terrestrial Carbon budget" *by* Barbara Marcolla et al.

N. Krakauer (Referee)

nirkrakauer@gmail.com

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This work compares interannual variability (IAV) in land NEE (and its GPP and respiration components) for 1982-2011 between eddy covariance flux tower data, a global empirical upscaling of this data (MPI-MTE), and an inversion based on a small number of station CO_2 time series.

1 Major comments

1. All three datasets are of dubious accuracy in representing interannual variability. The annual totals computed from eddy covariance sum much larger fluxes of opposing signs with likely systematic biases, especially in nighttime. The empirical

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upscaling was found to have relatively weak performance in representing inter-annual variability in a synthetic data experiment (without even accounting for any measurement or representativeness error in the training set) reported by Jung et al. (2009), for which the absence of soil moisture as a predictor is given by them as one reason. The inversion estimate, as the authors point out, is dominated at sub-continental scales by the (reasonable) prior assumption that variability scales with modeled NPP, and it probably contains little actual information from the CO_2 time series at those scales. Could it makes sense to run the inversion with a more 'flat' prior, or a prior based on the MPI-MTE IAV, to get different IAV estimates?

2. Figure 4 shows the dependence of median(?) IAV on resolution for the two gridded products. I wonder if something like this could be done with the available Fluxnet stations as well, for example with the help of a variogram (mean covariance of deseasonalized NEE time series as a function of inter-station distance). This could help in deciding whether the lower IAV in the gridded products compared to Fluxnet is only because of the difference in spatial scale or is more intrinsic.

3. I didn't see any analysis of to what extent the IAV between the three products is actually in phase (i.e. the correlation of the deseasonalized NEE time series between the datasets). It would probably be relevant to show this.

4. Also, forest inventories and crop yield statistics provide more reliable direct measurements of (at least above-ground) NPP and its IAV in many countries, potentially with rather good spatial coverage. Would there be any way to compare these to the IAV in the data sets reported here?

In summary, this is a valuable exercise, but I would like to see fuller discussion of the uncertainties, limitations, and potential checks and improvements noted above.

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2 Minor points

1. The element “carbon” is not capitalized (title and line 287).
2. Line 25: no comma before “that”
3. Figure 1c: It would be good to show the station network on the map.
4. “Anomalies” sounds strange as a description of the IAV residuals from linear trend shown in Figure 5 and discussed in the text. Perhaps there is a better term.
5. The Jung et al. (2009) citation should be to the final paper, not the discussion paper.
6. Formatting in the bibliography needs to be fixed, e.g. for Morgenstern et al. (2004) and others.

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