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

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Received and published: 26 January 2017

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 CO₂ 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
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 CO₂
time series at those scales. Could it make 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 grid-
ded 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 covari-
ance 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 com-
pared 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 mea-
surements of (at least above-ground) NPP and its IAV in many countries, po-
tentially 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.
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.