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

Anonymous Referee #3

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Marcolla and co-authors look at three different NEE products, compare the inter-annual variability (IAV) of each, and relate these measurements of IAV back to climate variability and contributions of plant productivity and ecosystem respiration. The study is nice, but incomplete in some ways or confusing in others. These suggestions are intended to strengthen the impact of the paper.

————Broad comments————-

I feel like the paper is missing the bigger take home message I was looking for, to the globally (or Fluxnet) integrated anomalies in NEE match up with 1) each other and 2) anomalies in the land C sink the global carbon project (Le Quéré et al. 2014; these data are available in a downloadable spreadsheet at http://www.globalcarbonproject.org/carbonbudget)

Since the paper is ostensibly about inter-annual variability in the terrestrial C cycle (NEE) what aren't all data products detrended first (these are weak responses anyway caused by different assumptions made with each approach)? Then the authors would be better able to address the IAV (or anomalies) which seem to be the focus of the paper.

What climate or weather data are used in MTE or the Jena inversion. Presumably neither used CRU (temperature) and GPCC (precipitation), as the authors of this paper chose to do? Thus, are analyses of climate drivers on IAV of NEE actually really just comparisons of distinct climate reanalysis products? Also, why not use the CRU precipitation product for consistency with the temperature data being used?

Much of the text in section 3 is heavy on the results with little discussion and interpretation of the key findings. Although some sections do communicate broader statements about the findings (e.g. lines 197-206), similar thoughtful development of ideas should be included throughout this section

Why aren't correlations of IAV with site – level or global-scale climate drivers shown for Fluxnet or Jena inversion products?

I'm unclear what value is communicated by the calculation of CUP and CRP and would suggest removing these analyses from the paper. The finding that temperate and boreal systems have a stronger seasonal cycle in their CO2 drawdown seems obvious from atmospheric CO2 growth curves. Instead, if the purpose of these analysis is to "identify the role of photosynthesis and respiration as sources of IAV_NEE" (line 67), then it seems much more straightforward to just look at the IAV (or anomalies) of GPP and TER from the Fluxnet and MTE products directly. Then they could be correlated with climate drivers too? For example, at high latitudes do GPP and TER show strong temperature sensitivities, with anomalies GPP outpacing TER in warm years? Conversely, are Tropical GPP anomalies largely temperature related too, whereas TER shows less inter annual variability & climate sensitivity?

——————Minor comments & Technical corrections——————-

I'm not used to seeing citations in the abstract. Is that the format for this journal?

I'm used to seeing ecosystem respiration referred to as ER, but maybe the authors are used to using different conventions?

Line 55 This single paragraph is a single sentence consisting of a very long list of NEE estimation approaches. Why not break this into a sentence about each approach and discuss strengths/ weaknesses of each?

Line 67 organization of objectives i), ii), and iii) don't align with the organization of methods and sections 3.1, 3.2 and 3.3. Can the objective reflect the broader layout of the paper?

I'd suggest Line 73 are "LaThuile and 2015" two distinct references?

It's not clear if or how data were re-gridded (e.g. [1] subtracting finer scale RETRO and GFED4 fire fluxes from the Jena inversion, or [2] for temperature and precipitation in Fig. 2).

More broadly, is subtracting for fire fluxes even necessary? Do the 14 observations extrapolated to this global product even 'see' the effect of forest fires? Don't the atmospheric inversion products the global carbon project implicitly see the effects of these fires? If so, why should the be subtracted out here?

Line 80. There are enough abbreviations in the text already. Are these needed too? Their use in lines 210-219 makes the text very hard to follow.

Line 109 Air should not be capitalized.

'Jena inversion' or 'Jena Inversion' should be used consistently throughout the text.

Were any lagged correlations explored to see if climate variability affected NEE in the subsequent season / year?

Standard deviation and IAV are used interchangeably throughout the manuscript, but I think they mean the same thing? If so, just one term should be used for consistency. If they are different, it should be clarified in the text.

Line 133 I have no idea what this means "the difference between the two determination coefficients was computed" or where this analysis is presented (Fig 9)? More broadly, I'm unclear how / why the authors tried to infer something about GPP and TER from the inversion product.

Line 171. Why was IAV normalized using GPP estimates and not NEE, the later giving a real coefficient of variation (CV; grid cell standard deviation NEE / mean grid cell NEE). This should be clarified both in the text and caption. Also, shouldn't grid cell CV be calculated first, and then averaged over each climate bin?

Line 180 & Fig. 3 I am unclear what insight this figure provides to the manuscript and it's sparingly discussed in the text. It's used to justify the CV calculation in Fig. 3 (line 173), but as this is a standard statistical approach I'm not sure it's warranted? As such, should the display item just be removed?

Line 200. It seems like 'trends' in IAV should be driven mainly by environmental presses like atmospheric CO2 concentrations or broad-scale / chronic N deposition inputs. By contrast, climate variability, land use change, and fires should be responsible for 'anomalies' the dataset. Given that the Jena inversion depends strongly on modeled NPP products it's not surprising that is shows stronger 'trends' (see suggestion to detrend data, above). Also, it would be interesting to see if fire fluxes were not backed out of the Jena inversion (again mentioned above) how the magnitude and timing of anomalies from these two data products compared to anomalies in the atmospheric CO2 growth rate. This also could provide a better opportunities for the authors to illustrate the differences between the data products that are currently in the discussion.

Line 296 Carbon should be lowercase

The conclusion is really just a summary of results already presented (and repeated from the abstract). I'd omit this text, or say something more broadly about what we can infer from the study.

Fig. 6 & 8 I know abbreviations for each plant functional type are given in the text, but not using them in the caption or x-axis label bar make this figure hard to understand.

Fig. 6 Aren't there enough observations to include error estimates (or box-wisker plots) for Fluxnet sites?

Fig. 7 Caption and text should use the same (consistent) terminology here. I'm not really clear what is being compared here? How does one calculate a spatial correlation coefficient on two single values (e.g., correlation of IAV$\sim$ mean temperature)?

Fig 7 The use of red-blue color bar on the left plots to show +/- correlation is confusing when on the right panels red-blue shows zonal mean correlations with trends or anomalies?

Fig 8 If this part of the analysis stays in the revised manuscript, I'd suggest the caption should be more descriptive (what are red and green bars).

Fig 9 I really don't understand what this figure is showing. The text & figure caption are not clear. More, the inset showing Western Europe seems strange. If this figure remains in the paper at all, would it make more sense to 1) omit the inset or 2) put it into supplementary material?

Fig 10 I also cannot understand I'm unclear what the color bar signifies (DRˆ2)? Is this the difference between TER/GPP when NEE < 0 during uptake periods and GPP/TER when NEE > 0 for MTE? If so, what does this difference of ratios really less us? I also still unclear how this is translated onto the Jena data?

———-References—————

Le Quéré et al. 2014 Global carbon budget 2013, Earth Syst. Sci. Data, 6: 235–263,

doi:10.5194/essd-6-235-2014.