Please find our replies in italic below specific comments and questions.

Interactive comment on "Deciphering the components of regional net ecosystem fluxes following a bottom-up approach for the Iberian Peninsula" by N. Carvalhais et al.

A. Friend (Referee)

adf10@cam.ac.uk

Received and published: 3 August 2010

This paper is based on the excellent premise of testing the impact(s) of initial carbon pool sizes on carbon dynamics. Initial conditions are one of the greatest areas of uncertainty in interpreting flux measurements. This study shows that inter-annual variability (IAV) and trends are found to be dominated by the effects of initial conditions. The work goes on to claim that removal of these allows the forced signals to be identified, and a methodology is proposed. It is notable that trends are then mainly driven by trends in fAPAR - light absorption is the controlling variable.

The greatest problem I have with the paper is the following. This may just be my inability to fully understand the metholodogy employed, which is not explained clearly to me, but I would like the authors to respond. I see that it is possible to remove the effect of recovery from the variance and trajectories of NEP using the method proposed, but as we do not know the initial conditions in any of the observations, what does this tell us that we do not know from assuming initial steady state? In other words, calculation of NPPD removes most of the sensitivity to initial conditions, but what does this tell us that NPPeq does not? I would like to see the authors clarify this point (if it is correct, then the whole basis for the paper is without foundation). Further, it is stated that "A direct implication of these results is the ability of the approach to detect climate and phenology induced trends that are independent of the initial carbon pools." Surely, really, this nice analysis shows that assuming equilibrium conditions, and then calculating the anomalous behaviour due to the drivers, is as good? And, that is what is already mostly done? Please can the authors clarify? I am sure I cannot be right as the authors would not have gone to the bother of this work if I am!

Since steady state is a proposed modeled initial condition, it is dependent on the model drivers used to reach equilibrium. It is not seldom that approaches different from the one used here are followed; for example, using the climate data observed in the first years of the simulations as the spin-up datasets (instead of considering the mean of the simulation period as the spin-up drivers, as done here), or cycling through an earlier decadal set of data followed by transient model runs. Using any of these approaches would lead to different initial carbon pools estimates to the ones computed here, which would imperatively yield different trends in ecosystem fluxes (such as is the case of $\eta \neq 1$ simulations). The observation of very similar patterns in the spatial trends of NEP^D and NEP_{eq} is a particularity of considering the spin-up climate equal to the mean of the period of the actual simulations (1982-2006). The current approach is important given the different designs possible for the spin-up model runs and generally applicable.

In order to improve the clarity of the whole methodology employed, and following the proposed graphical approach, we propose the introduction of Figure 1 as the methodological workflow of the current manuscript.

Overall, I found the presentation rather difficult to follow, as well as being repetitive. I had to read the paper a number of times before I was able to grasp its essence. This led to the above questions. A simple graphical analysis could be used to explain the methodology, and show how it is useful for determining both IAV and trends in real cases.

We followed these guidelines to build the flowchart presented in Figure 1. We hope that including such figure will improve the clarity of the manuscript in addition to the changes done in the text (we refer to the reply to Prof. Ronald Neilson for a detailed description of all these changes).

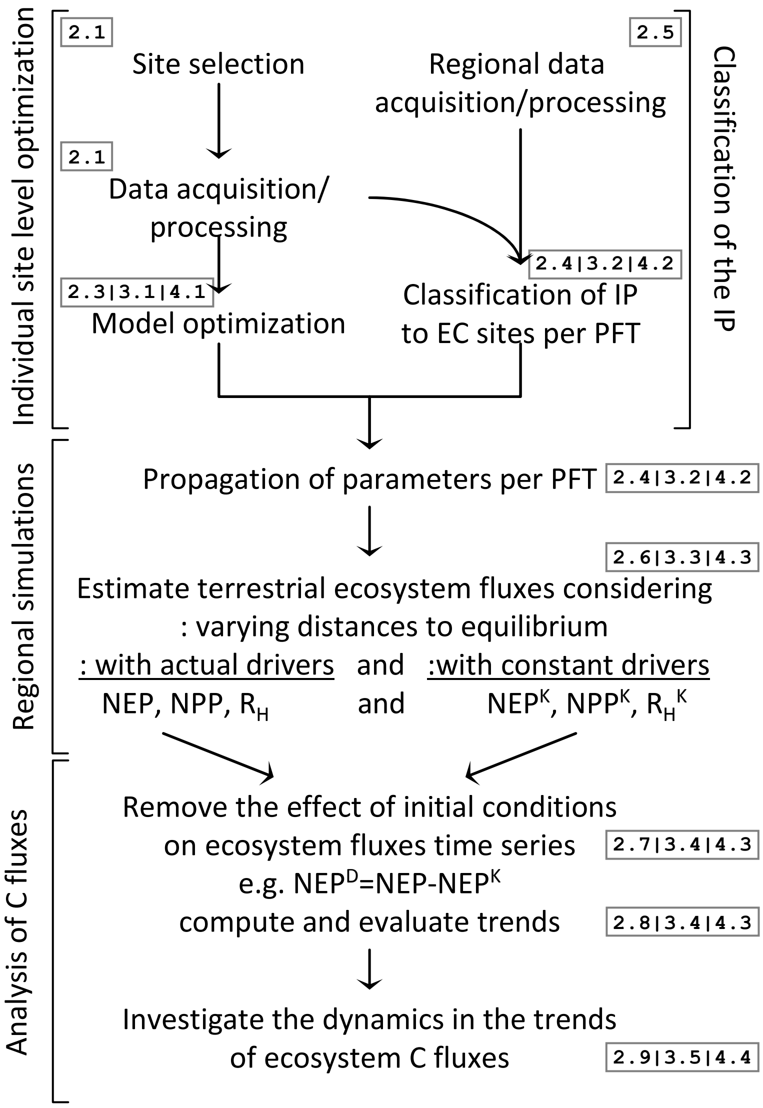

Figure 1 – Workflow of the current analysis: from site level observations to regional dynamics of ecosystem C fluxes. The model-data integration followed at site level yields optimized parameter vectors used for regional simulations. The propagation of parameter vectors is dependent on the classification results of each IP gridcell according to site level characteristics, which support the sensitivity analysis to the initial conditions. Ultimately, the evaluation of trends in ecosystem fluxes quasi-independently from initial conditions allows the decomposition of modeled ecosystem fluxes into component fluxes and investigating the underlying dynamics of NEP trends. The numbers in gray boxes indicate the sections of the manuscript focusing on the different aspects of each step: materials and methods (2.x); results (3.x); and discussion (4.x).

A couple of relatively minor points:

Should add "e.g." to a number of the citations in the Introduction.

We added "e.g." where we found convenient to do so.

Topt does not seem correct - can it really be that low in grasses? I doubt it very much.

The low T_{opt} values found for grasses also called our attention ("4.1 CASA model optimization"). "The low T_{opt} values found for grasses are border line or below the 10ºC to 25ºC range for C3 and 30ºC to 40ºC for C4 plants, although some C3 species are quite active at 5ºC (Breymeyer 1980). Most of these sites are C3, with the exception of PT-Mi2 which is a C3/C4 mix. Site level records show the occurrence of high gross primary production (95th percentile) at temperatures below 10ºC. The coherence between observed climate and the optimized values is supported by previous observations of higher photosynthetic rates at low temperatures in C3 plants native to cool growing seasons (Bunce, 2008; Berry and Bjorkman, 1980). In Mediterranean climates higher photosynthetic activity of grasses (mostly C3) can be observed during seasonally cooler periods when plants grow due to higher moisture availability (e.g. winter or spring). This is consistent with the comparison between T_{opt} of grasses and the mean monthly temperatures for the Iberian Peninsula, where the confidence bounds of T_{opt} include the monthly temperatures observed during colder months. However, from a model-data integration perspective both model drivers as well as model structure can influence the actual values of parameters." (This text was added to the discussion section 4.1.).

I hope these comments are useful!

Andrew D. Friend