

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.

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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

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to fully understand the methodology 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!

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.

A couple of relatively minor points:

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

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

I hope these comments are useful!

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