

Interactive comment on “Seasonal variation in ecosystem parameters derived from FLUXNET data” by M. Groenendijk et al.

Anonymous Referee #1

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This paper sets out to investigate a range of flux measurement time series using a simple model. Five parameters within the model were varied to optimise model fit to C and water flux observations. The authors then attempted to interpret variability of parameters in space and time according to the dominant vegetation from the various flux sites.

The authors have undertaken a large model optimisation exercise, using data generated by a large number of groups. But overall this is a very disappointing paper and should not be published. It does not contain any novel research, and actually it generates unsupported conclusions. I was hoping to see an analysis of how model parameters varied spatially across PFTs, using an effective and trusted model. But instead we have a new model of little obvious value that is used for a mystifying purpose

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- identifying how parameters change in time – which simply shows up major model failings.

The 5PM model is presented for the first time in this paper. It seems to be a combination of models from Farquhar and Cowan. The Farquhar photosynthesis model is relatively complex, process based and well verified. The components from Cowan, that predict internal CO₂ concentration and thus stomatal conductance, are more theoretical, based on assumptions about optimisation of water use, and largely untested. Other components of the model seem to be missing – there is no radiative transfer scheme to distribute light through the canopy, no plant-water routines to include hydraulic stress, no phenology routines to schedule plant activities. It is never made clear how the model switches between enzyme limited and light limited constraints on photosynthesis. The respiration model is a simple temperature response function, completely decoupled from photosynthesis, and with no separate components for autotrophic and heterotrophic sources.

Overall 5PM is confusing, a mix of complex components, simple components, missing components, and containing no internal feedbacks, such as a carbon mass balance. How does 5PM really differ from the typical gap-filling methods used to fill spaces in the flux data, apart from having a little extra complexity? These well known and frequently published methods use simple, decoupled models to estimate respiration and GPP response functions. The purpose of these gap filling models is clear.

The authors here attempt to use their 5PM model to diagnose ecosystem behaviour by parameter estimation. This is not novel in itself. Knorr & Kattge (GCB 2005) did something very similar, though just for a single site. A critical difference in this study is that the authors fit their parameters sequentially for each week. Parameter estimates thus vary over time. Many people will find this approach problematic, or even wrong, as we generally require that model parameters be constant in time. This constancy must certainly be the case if a model is to be used prognostically. Varying parameters over time implies that the model is missing out some key process. If this process were

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included in the model, then the varying parameters could be substituted by constant parameters. I concur that model parameters should be constant. I can see that data can be used to identify components of the model that are poorly represented, through finding a signal of parameter variability. But I have two caveats to this. Firstly the model must have been constructed with some representation of the key processes that are expected to govern behaviour. Secondly, any observed variation in parameters must be diagnosed and use to improve the model. The authors do not meet my caveats.

The 5PM model is deficient in key areas for the purposes of this analysis, the most important of which are the lack of mass balance and the lack of any phenological component. The 5PM model does not describe canopy light interception, nor its variation in time. A key conclusion of the paper is that “broadleaf forests... have large seasonal variation in... parameters”. This result could have been determined without any model runs at all, from basic knowledge of forest ecology. This result must be an outcome of seasonal variation in leaf area index (LAI) in the monitored (eddy flux) broadleaf forests. 5PM does not include LAI, but the observed data are highly dependent on LAI change, ergo the variable parameter estimation results in a seasonal signal that is an analogue for LAI change. The authors even admit this. But this “missing LAI” problem means that nothing useful has been learned from this exercise. The seasonally varying parameters are not used to improve the model, and so the model has no prognostic value. Perhaps biochemical parameters do change over time (though many studies suggest not), but without accounting for an LAI signal the authors cannot find evidence of such changes. If they had substituted a more complete model for 5PM then this very interesting line of enquiry could have been followed up.

I am very concerned by the conclusions generated by this paper. The authors state that their analysis focuses “on short term processes”. They then conclude from their analyses, in a manner that is not clear at all, that “climate change will have the largest impact on the terrestrial carbon fluxes in boreal regions and for deciduous forests, and less for grasslands and evergreen forests”. How has this conclusion been reached?

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How can a model that lacks mass balance be used to make any such claims about climate change impacts, which necessarily involved complex feedbacks over years and decades? The authors state that the model “can be applied globally”, but I cannot see for what purpose it can be applied – certainly not for prognosis. The finding that C fluxes are not linearly related to climate is a trivial rehashing of basic knowledge.

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