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Interactive comment on "A simplified gross primary production and evapotranspiration model for boreal coniferous forests – is a generic calibration sufficient?" *by* F. Minunno et al.

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This paper can be summarized simply: Boreal Coniferous forests in fennoscandia (Scots pine and Norway spruce) do not require highly-tuned site-specific parameter sets when simulated using a simple light-response model developed at one of the sites. In fact, a long data record is as (or even more) important than deriving parameter values from multiple sites. This finding is neither new nor novel. While it is clear that the authors have performed a considerable amount of work, I don't find that this paper delivers enough new scientific finding to merit publication. Most models use a single parameter set for use in simulating a given Plant Functional Type (PFT; boreal

coniferous forest-BCF for short, for example). This is a technique that has been used for decades, and does not break down here. Is this finding worthyof publication? Parameters that have been found to have large influence on BCF simulation converge to values with small uncertainty, while those parameters that do not strongly influence the simulation of GPP and/or ET can fluctuate. This is intuitive, and not new or novel. The analysis is presented in a clinical "here is what happened" manner, with no attention given to what it means with respect to our understanding of the environmental controls on GPP and ET. In section 3.2 there is discussion of the correlation between model parameters (Table 1), with no mention of why the correlation is positive or negative. Physical interpretation is lacking throughout the paper. The paper is clearly written, and I do not find fault with the methods. The problem is, there is nothing new or novel here. For that reason, I must recommend that this paper be rejected for publication.

Reply: Response: It is true that many models, such as Dynamic Global Vegetation Models, have assumed for decades that the concept of plant functional type is valid and can be used for parameterising not only submodels of GPP and ET like here, but in fact the entire dynamics of the PFT and the related vegetation type. However, we don't know of any comprehensive studies where such models have been tested against data to compare site-specific and multi-site parameterisations. In contrast, there has been increasing concern in the literature about the justification of this assumption (e.g. Reichstein et al. 2014), as is also reflected in the comments on this manuscript by Reviewer #1 and #2. For example, Reichstein et al. (2014) suggested that a set of individual plant traits should be taken as a basis, instead of plant functional types that may consist of species with some shared and some separate traits. In fact, they used a simple version of the LUE model as an example of observed wide variability of parameters within a plant functional type (their figure 2). Another recent paper studied the possibility to improve the PFT-specific parameterisations of the MODIS GPP algorithm (Chen et al. 2014 Remote Sensing), finding that indeed an improvement could be achieved. The goodness of fit of that model to the data used was lower than that in the present study, however, they did not analyse whether site-specific parameterisation would have improved the results. Kasurinen et al. (2014, GCB) on the other hand applied the Penman-Monteith approach to analyse the flux of latent heat from different ecosystems, finding significant within-PFT variability. They found, for example, that the parameterisation for boreal spruce and pine needed to be separated. Because of the above, we believe that our result is not self evident. This also seems to be the opinion of the other two reviewers. On the contrary, they are curious as to why this should be, given that the recent literature seems to be pointing to the opposite direction. We can think of several possibilities which are discussed below. The eddy measurement as such has a tendency to average over sites, because the footprint area is larger than the actual central site used for model parameterisation. The forest landscape in Finland and Sweden is quite heterogeneous, such that the footprint area could cover different soil types and species. We believe that this could explain some but certainly not all of the similarity of the sites included here. Particularly, the similarity between the upland and wetland sites does not seem expected. A more thorough analysis of the footprint area and the contribution of the surroundings would be required to asses this quantitatively, which we believe is beyond the scope of this study. It is also possible that the environmental responses of GPP and ET are genuinely relatively independent of site and species, once the fAPAR of the stand is known. The latter carries information about stand structure which, on the other hand, is known to be strongly dependent on both site and species (Medlyn et al. 2005, Duursma et al. 2009) and may also develop dynamically in a site and species specific manner, which was not considered here. This independence has also been found in some previous studies, especially in the case of GPP (Duursma et al. 2009, Medlyn et al. 2005), although the studies cited above found the opposite (Chen et al. 2014, Kasurinen et al. 2014, Reichstein et al. 2014). A lack of independence of site could also be caused by model inadequacy (rather than simplicity), where the parameters are needed to compensate for model inaccuracy. A typical cause for this in many GPP studies could be a sub-optimal representation of the seasonal cycle (Reichstein et al. 2014, Chen et al. 2014). On the other hand, increasing model complexity would also probably increase the need to make the description

more species and site specific (such as in Kasurinen et al. 2014 in comparison with the present manuscript). The question is then, how much of the variability could be explained with a generic model, and how large a gain could be obtained by increasing complexity. We have found in previous studies that even if parameters vary between sites as a result of site-specific fitting, the processes can be (almost) equally well represented by a generic parameterisation, because there are mutual correlations between the parameters (Mäkelä et al. 2008 GCB). This could of course be interpreted as opposite errors canceling out each other. Importantly, it could also signify a tendency of ecosystems to evolve towards essentially similar environmental responses, albeit through different structural and functional adaptations (e.g. Valladares et al. 2002). In this study, we conclude that a large percentage of the variability in GPP and ET can be explained using a generic parameterisation. Part of the remaining variability could likely be explained by increasing the complexity of the model. If this part is as low as it seems to be here, then certainly there seems to be room for the use of a simple approach also. Having said all this, we do appreciate that the role of the complexities and differences between sites and species may become more pronounced if the environment changes. The need of complexity in such cases could be studied, for example, by investigating how far a complex model can be reduced in such a situation. We have added these points to the Discussion.

Specific comments: Page 5106, line 8: MCMC is not defined

Reply: MCMC has been defined and the algorithm used has been specified.

Figure 6 is too small to see.

Reply: It should be bigger with the right orientation of the page.

Figure 7, the different shades in the error decomposition are not defined.

Reply: They are defined below the plots.

Tables 4 and 5 are probably superfluous, as description of their results is given in the discussion.

Reply: Since they give a better overview of the results we decided to keep them.

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