

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.

Anonymous Referee #3

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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 worthy

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of 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.

Specific comments:
• Page 5106, line 8: MCMC is not defined
• Figure 6 is too small to see.
• Figure 7, the different shades in the error decomposition are not defined.
• Tables 4 and 5 are probably superfluous, as description of their results is given in the discussion.

Interactive comment on Geosci. Model Dev. Discuss., 8, 5089, 2015.

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