

Interactive comment on “Modelling sun-induced fluorescence and photosynthesis with a land surface model at local and regional scales in northern Europe” by Tea Thum et al.

Anonymous Referee #1

Received and published: 3 January 2017

The manuscript by Thum et al examines the use of SIF to predict GPP in coniferous forests in southern and northern Finland. The authors implement a SIF module in the JSBACH biosphere model and evaluate seasonal and spatially variability against SIF and GPP measurements at leaf, canopy, and ecosystem scale, with focus on spring and autumn transition seasons. A key innovation is the use of active leaf level fluorescence data to understand the seasonal relationship of photochemistry and fluorescence and evaluate model performance. Although many uncertainties exist in the model simulations and in understanding dependencies on environmental vs biochemical effects, the authors show good correlation of observed and simulated variables, providing some confidence for future testing and evaluation, and paving the path for

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future efforts to scale between leaf and canopy/ecosystem levels. In general, I found this paper interesting and innovative, but it was hard to read at times, and the objective weren't clearly established making results and discussion hard to follow. I recommend a more careful analysis of satellite observations and some general clarifying throughout, but I expect this to be an important study with a few substantial revisions.

Major Comments

My main concern is biasing of GOME-2 time series by filtering of negative SIF values. These data are part of the noise needed in averaging to produce a smoothly varying signal. Because the noise is fixed ($\sim 0.5 \text{ mW m}^{-2} \text{ sr}^{-1} \text{ nm}^{-1}$ as mentioned on P9 L4) and doesn't scale with signal, this technique will remove more points in fall-winter-spring when errors are large compared to signal, leading to positive cold season biases, early spring GPP onset, and underestimated seasonal amplitude. This should explain why observed SIF doesn't reach zero level (P13 L5) and why the authors find an opposite phase an opposite phase relationship of GPP and SIF in spring at FI-Hyy in active data (photochemical yield synchronized with SIF) compared to passive data where SIF precedes GPP. I recommend reanalyzing GOME-2 results with negative values.

The authors show that model GPP is systematically early in coniferous forests compared to ground and satellite data, a finding that is consistent with previous studies of cold limited ecosystems. I was hoping the authors could take better advantage of the multiscale observations and new model capabilities to provide explanations at biochemical and environmental levels, especially since the challenge of understanding the spring transition is listed as a motivation for the study. Some speculation is provided in the discussion (e.g., frost) but not much detail and no mention in the conclusions. I think this is an important enough result and application of new methods as to warrant further discussion. I would like to see the authors discuss what is needed to improve model representation of the spring transition. What would be the effect of seasonal PSII and thermal dissipation? Growing degree days, cold temperature days, and/or frozen soils?

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What important environmental controls are included/missing in the Farquhar model?

I am also interested in further elaboration of results in autumn at FI-Hyy, in particular, why F' and photochemical yield are strongly delayed relative to GPP in autumn but synchronized in spring.

Minor Comments

Abstract and Conclusions – Mostly a discussion of methods and no mention of new results. I suggest discussing at least one new and interesting result from your study. Something about spring or autumn photosynthesis, or using leaf level measurements with satellite data, or comparing model simulations to active and passive fluorescence data.

Figure 1 - Figure legend is difficult to read and it's not clear from figure or caption what is being plotted in panel (A) – legend appears to suggest fluorescence yield as solid red but text refers to photochemical yield. GPP is not shown in panel (A) as stated in caption – please correct.

Figure 3 – color scheme is confusing especially with multiple variables on 1 plot. keep observations in black and models in color like in figure 1. Use same line styles for same variables (solid for GPP, dashed for SIF).

P5L10: *an indication of the fraction of electrons in the leaf that follow the ChIF pathway

P6L12: *is used in

P7L23 & P9L2: Confusion about overpass time. Here it is stated as 10:30 am but as 9:30 am in Section 2.3.2. Please clarify or correct. Also clarify what it means for the satellite overpass time to last for 100 minutes.

P10L18: I don't see the simultaneous decrease in observed GPP with F' . GPP is already declining on Day 200 while F' appears steady until \sim Day 280. F' decrease is also much more gradual and doesn't reach its minimum until January.Åã

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P10L25: please elaborate what is meant here - are you suggesting that in low light conditions of spring, most of the absorbed radiation goes into photochemistry thus reducing that available to fluorescence? Å

P12L1: quantify "reasonably similar" - within 10% of observations? 5%? Å Regression is slightly lower on average in model

P12L8: FI-Sod has lower correlations than FI-Ken.

P12L10: provide reference for peat effect on drought

P13L25: what is the magnitude and direction of the seasonal drift in GOME-2 overpass, and what is the likely influence?

P14L13: please explain what a static temperature response is, the effect on early GPP, and how this could be corrected in the model

P15L4: add condition "assuming a homogeneous landscape"

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-514, 2016.

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