Response to Referee #1, 28.2.2017 by Thum et al.

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 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.

We thank the referee for this encouraging feedback and hope we're able to provide improved version of the manuscript based on these recommendations.

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 (0.5 mW m-2 sr-1 nm-1 as mentioned on P9 L4) and doesn't scale with signal, this technique will remove more points in fallwinter-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.

We thank the referee for this insight and agree. We have thus redone the analysis with the inclusion of 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?

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

We agree with the referee that this is one focus of the manuscript, but it has not received enough focus in the earlier version. We therefore added discussion in this topic, which is shown later. There we propose using temperature related changes to the base rates of the biochemical parameters. Here the SIF observations can be used as a valuable evaluation tool for large scale estimates.

In the end, it would be desirable to have process-based description for the cold acclimation processes to properly describe the seasonal cycle of boreal forests, as this would also enhance our skill to predict changes of the carbon cycle in future climate. However, at the moment there is lack of observations to parameterize a large scale model in this respect. The parameters related to the amount of active reaction centers and sustained non-photochemical quenching are steps to this direction, but they would need parameterization in order to be useful in large scale models.

Additionally, also other processes play part. It has been suggested, that slow recovery of Rubisco has influence in spring recovery (Monson et al., 2005). It might not be possible to include all the factors in models, but combining observations and modelling efforts at different scales will hopefully reveal, which processes are most important to be included.

The spring recovery of the forests to its full summertime capacity is a gradual process (Bergh et al., 1998), that can be tracked with several environmental and biological variables (Thum et al., 2009). Air temperature is quite good proxy to be used (e.g. Thum et al., 2009), but the averaged temperature indices might benefit from inclusion of delay due to night frosts (Thum et al., 2017), that might even reverse spring recovery (Ensminger et al., 2004).

Large-scale observations can be very useful, since earlier studies (e.g. Walther et al., 2016) have shown that the temperature sensitivities differ between different regions. This study is a first step in doing that work with more extensive remote sensing data available soon. However, also more data-based approaches are valuable (e.g. Luus et al., 2017, Walther 2016) as they are increasing our understanding of the carbon cycle.

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.

In earlier version the MONI-PAM results were not discussed in detail, as they've been published also elsewhere (Porcar-Castell, 2011; Kolari et al., 2014), but of course they haven't been shown in this context and therefore there is reason to further elaborate then. In the earlier version they were discussed in the discussion, but we moved these points to the Results section and added some more detail, now mentioning photoprotection of the needles and the possible differences in the electron transport rate between observation and simulation.

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.

We added some results, including two points: i) the ability of observed SIF to capture seasonal cycle of photosynthesis at site scale, ii) the goodness of simulated SIF values vs. observations at regional scale.

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.

The font size in the figure legend was increased to make it easier to read. The text was corrected, so that it states that the fluorescence yield is the modelled yield (shown in solid red in the figure). The caption was corrected to say that GPP is in panel B.

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).

We remade the figure like suggested.

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

Corrected.

P6L12: *is used in

Corrected.

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.

We sincerely apologize for the confusion and would like to explain how this happened. We briefly introduced the properties of GOME-2 in Sect. 2.3.2, where we added "at the equator" to clarify that the actual overpass time depends on the region under investigation. The wording to express the time for one revolution might have been inappropriate. We clarified this issue by rephrasing the sentence to: "..., while one revolution takes 100 minutes." In Sect. 2.2.3 we added "local solar time" in order to prevent any misunderstanding.

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 Day 280. F' decrease is also much more gradual and doesn't reach its minimum until January.

This is right. We have now corrected this part in the text.

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?

Apologies for having a mistake in the subscripts, making the message of the paragraph very unclear. The important message here was connected to only fluorescence yield, that in spring it is the fluorescence yield that is holding back the increase in SIF.

In low light conditions in general, the photosynthetic yield and fluorescence yield are anti-correlated. Actually, with increasing light levels, the fraction of incoming energy used for fluorescence increases and the fraction used for photosynthesis decreases (van der Tol et al., 2009). This is because when photosystem II absorbs light and primary quinone acceptor of PSII Q_A has accepted an electron, it cannot accept another electron before it has passed on the first electron to the subsequent electron carrier. Thus, the proportion of closed reaction centres lead to a reduction in efficiency of photochemistry and increase in fluorescence (Maxwell and Johnson, 2000). We don't have any data that shows different behavior for this in spring. We are sorry for the confusion, due to the wrong subscripts.

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

Indeed, it is a good idea to concentrate on this result more deeply, as it is one of the main results of the study. For now we added uncertainties of the slopes in the table 3 and calculated the averages and standard deviations of the different cases. These are now also mentioned in the abstract.

Note, that due to the different fitting algorithm the slopes might differ slightly from the earlier values. Also, due to the inclusion of negative observed SIF values in the analysis, the slopes are not now systematically lower in the model.

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

Yes, it depends on which correlations you're looking at in Table 2. Here we were trying to refer to correlation between modelled GPP and modelled SIF, which is at least 0.92 for other sites and 0.83 for FI-Ken. To clarify this, we added "with each other" to the text.

P12L10: provide reference for peat effect on drought

We decided to replace word "peat" as "humus" as we consider it to be more appropriate term in this context and added reference.

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

Although there are indeed (minor) seasonal variations in the local solar time of GOME-2 overpasses (slightly earlier during NDJ; 10:15 during winter, 10:45 during summer), we do not expect a dramatic influence, because of the senescent vegetation during this period. After including also negative values (as suggested by referee), we obtain SIF values close to zero as it can be anticipated and removed the concerned sentence accordingly. However, the morning overpass of GOME-2 leads to challenging measurement conditions (inclined solar angles) during the winter (mentioned in P14L1).

Inclined solar angles lead to longer photon path lengths, in which case rotational Raman scattering could fill in solar Fraunhofer lines. This might interfere with the SIF retrieval, which relies on the in-filling of Fraunhofer lines. We included these measurements anyways to be able to present a continuous time series (again: we observe SIF values close to zero during winter).

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

We added the following text here:

"*The photosynthesis of forests is often modelled using constant temperature response for the biochemical model parameters V_{max} and J_{max} throughout the year. However, studies have revealed that this assumption does not hold for ecosystems with strong seasonal cycles, but causes overestimation of CO_2 fluxes in transition periods. Kolari et al. (2014) found seasonally varying values for leaf level for those parameters from leaf level observations at FI-Hyy. Ueyama et al. (2016) found seasonally varying biochemical model values at four different black spruce forests in Alaska in a model inversion study at eddy covariance sites. In an earlier study using inversion at boreal coniferous forests (Thum et al., 2008), it was found that three forests at northern boreal zone (FI-Hyy, FI-Sod and FI-Ken) had temporal evolution in the biochemical parameters, but a site located on temperate boreal (Norunda, Sweden) did not.*

Leaf level studies have used temperature acclimation for the changes of biochemical parameters (Wang et al., 1996). Similar results have been obtained for site level results at FI-Sod, where dark acclimated chlorophyll fluorescence observations have been used in combination with eddy covariance observations to disentangle the effect of changing maximum photosynthetic capacity (Thum et al., 2017).

The changes taking place in the needles of conifer forests in winter are numerous to protect the needles in challenging environmental conditions. E.g. the light harvesting complexes are aggregated (Porcar-Castell, 2011) and the xanthophyll cycle enables photoprotection (Ensminger et al., 2004). Some of these processes can be in future be included in a large scale model, as adding changes to the parameters in the ChlF model discussed below, but as changes in the boreal spring happen at quite fast pace and those can be tracked with several different environmental and biological variables (Thum et al., 2009), for large scale applications a temperature related changing of the biochemical parameters might be next step forward and remotely sensed SIF observations provide a very useful evaluation tool in this context."

P15L4: add condition "assuming a homogeneous landscape"

We guess that this was meant for page 16… We made the addition there.

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