Replies to Reviewer 2

The reviewer comments are in magenta, the replies to the comment are in black.

This manuscript presents a comprehensive effort to implement sun-induced chlorophyll fluorescence (SIF) into the terrestrial biosphere model QUINCY and to evaluate simulated SIF against both tower-based and satellite (TROPOMI) observations at multiple evergreen conifer forest sites. The topic is timely and relevant, as SIF has become an important observational constraint for photosynthesis and gross primary productivity (GPP), yet remains insufficiently represented in many terrestrial biosphere models. The integration of mechanistic radiative transfer and non-photochemical quenching (NPQ) processes within QUINCY represents a valuable step forward. The manuscript is generally well structured and clearly written, and the use of multiple independent observational data sets strengthens the evaluation. However, the novelty of the approach relative to previous SIF-enabled modelling studies is not always clearly articulated, and several methodological aspects require further clarification to ensure reproducibility. In addition, the interpretation of the results could be expanded to better highlight the ecological and physiological implications of the findings. Overall, I find the study promising and suitable for publication after moderate revisions addressing the comments below.

We thank the reviewer for the positive comments on this manuscript and especially for pointing in a very constructive manner to issues where improvements are needed. In the revised manuscript we will pay more attention to the novelty of this work in relation to previous work and will clarify the presentation of the methodology as well as bring in ecological and physiological insights based on our results.

Major Comments

1. Novelty and Positioning within Existing Literature

The manuscript would benefit from a clearer statement of what is new compared to existing approaches that simulate SIF within terrestrial biosphere or land surface models. While the introduction provides a good overview of the importance of SIF, it remains unclear how the present implementation in QUINCY advances beyond previous studies (e.g., SCOPE-based or simplified SIF schemes). I recommend adding a short paragraph explicitly outlining the novel aspects of this work and its main added value.

We thank the reviewer for the insightful comment. In the current work we think the aspects related to novelty would be:

-We have used different radiative transfer schemes for SIF within one model, thus providing a comparison of only these schemes instead of comparing different models (e.g. some studies compare the modelling results of their model to SCOPE results, such as Li et al., 2022)

-Many other studies concentrate only on the far-red region SIF, as most of the current satellite products are located in this wavelength region. In this study we evaluated the model performance for both the red and far-red regions, which can be considered a way to prepare for the FLEX mission. One could argue that photosystems I and II contribute differently to these wavelength regions of the SIF emission and may exhibit distinct dynamics.(e.g. Porcar-Castell et al., 2021). Studying whether the model performs worse in the other wavelength region in terms of the dynamics could reveal incapability of the model to include relevant processes. Moreover, pronounced differences in biases of the model estimates in the other wavelength region could reveal difficulties in modelling of the radiative transfer of SIF.

-Even though the L2SM model has been published earlier (Quaife, 2025) and used in model study earlier (Knorr et al., 2025, using another leaf level model for SIF), no detailed model evaluation of its performance has been yet shown in the scientific literature.

-Our study includes sites in two different continents. The temperature sensitivity of GPP has been found to be different in the boreal forests of these two different continents (Muccio et al., 2025) and in our study we can assess whether the temperature dependent processes, such as sustained non-photochemical quenching are different at sites located in different continents. The study by Chen et al. (2024) also included a similar set-up concerning sites, but there the third site was located in South Korea.

-Two of the tested radiative transfer approaches include attenuation of the SIF signal in the leaf. A comparison of the magnitudes of these approaches with an approach that omits this process will aid in evaluating its importance.

We will write a short paragraph to the introduction that makes clearer the novelty aspects of the current work.

2. Description of the SIF Implementation

The description of the SIF module in the Methods section is relatively high level. For a modelling study, additional technical detail is needed. For example, the authors could consider providing more justification for the chosen parameter values and indicating whether they are site-specific, plant functional type-specific, or globally fixed. You can also add typical parameter value ranges in Table 2. You can also consider adding a schematic diagram summarizing the SIF calculation within the QUINCY framework. These additions would significantly improve transparency and reproducibility.

We thank the reviewer for this very good improvement suggestion. We will write more clearly how we chose the parameter values and how they were applied in this study (they were all global in our case, as we were only working with one plant functional type, but unfortunately this was not made clear in the first version of the manuscript).

Many of the items in Table 2 are actually variables (apologies for the unclear caption, which we have now clarified). Adding a schematic diagram is a good idea and also suggested by the other reviewer. We will add conceptual figures clarifying the calculation of the SIF.

3. Model Evaluation and Metrics

The comparison with tower-based and TROPOMI SIF is a strong aspect of the study. However, the evaluation would benefit from a more consistent quantitative assessment.

Please report standard performance metrics (e.g., RMSE, bias, correlation coefficient) consistently across all sites and modelling configurations, for example, Figures 2-5. In addition, the scale mismatch between tower measurements and satellite pixels should be discussed more explicitly, particularly in the interpretation of model–TROPOMI comparisons.

We will add a more consistent quantitative assessment of the TROPOMI SIF comparison. We will add standard performance metrics in the figure 2. The metrics will be added in a common table for all the model configurations at the three sites. We will also add a table that will show the metrics and the improvement caused by the implementation of the sustained NPQ with the L2SM radiative transfer approach at all sites (shown in Figure 5 for CA-Obs site).

We will also add discussion on the scale mismatch between the tower measurements and satellite pixels. We will additionally add a figure for the TROPOMI-model comparison at FI-Sod.

4. Interpretation of Results

The Results section focuses mainly on model performance, but the Discussion could be strengthened by deeper interpretation of the findings. For example, what do the results imply about the seasonal regulation of photosynthesis in evergreen conifer forests? How does the inclusion of NPQ affect the relationship between SIF and GPP across seasons? Are the differences among sites indicative of meaningful ecological or climatic controls? Expanding on these points would broaden the relevance of the study.

We thank the reviewer for these insightful remarks that are worth addressing in the discussion.

The fact that the same "state of acclimation" parameterization in slowing the development of spring recovery in GPP across the three sites is successful shows that the GPP would be well enough constrained with the same parameterization at these sites.

Using the same parameterization for sustained non-photochemical quenching did not show to be at first that successful at FI-Sod. However, the overestimation of simulated absorbed PAR might be partly contributing to this. We added in the figure showing the APAR estimation from the FloX box also an APAR estimation based on above- and below canopy PAR sensors. These observations started on June 18th 2021, so unfortunately these will not help to untangle the model mismatch taking place in early June. However, they will help to illustrate how the impact of the footprint mismatch contributes to the model performance (similarly to what was seen in the diurnal cycle plot in Fig. S8d of simulated and observed SIF at FI-Sod). Adding the plot for the TROPOSIF vs. simulations will help to assess the sustained NPQ formulation at the FI-Sod site better.

We will do some additional analysis, such as looking into the temperature response of chlorophyll fluorescence yield at the sites with and without the sustained NPQ formulation (similarly to what has been done by Kim et al., 2021) and also look into the SIF/GPP ratio across seasons as well as a function of temperature (as done by Chen J. et al., 2022 and Chen, R. et al., 2025). These analyses will help us to address the interesting points that the reviewer was raising here and we will discuss their results in depth.

5. Uncertainties and Limitations

Please discuss the main sources of uncertainty in both the model and the observations (e.g., uncertainty in tower-based SIF retrievals, satellite noise, parameter uncertainty). A clearer discussion of model limitations would help place the results in context.

In the first version of the manuscript we had some discussion on the uncertainty of the observations, but since it did not have its own subsection, it was not that clear. We have now made a subsection for the observational errors and expanded that to account for the noise in the satellite observations.

We will also add a section for the model uncertainties and also add a clearer discussion of model limitations.

Minor Comments:

1. Table 1 could be improved by adding time period information of SIF measurements.

Thanks, we have now added this information to Table 1.

2. Ensure that all acronyms are defined at first use. For example, TBMs were defined multiple times in L35, L39 and L145.

We apologize for sloppiness with this issue. We will pay attention to correct this in the revised manuscript version.

3. Improve figure readability (font sizes, legend clarity) and consider expanding figure captions. For example, the font sizes of Figure 4 and 6 are too small.

Thanks for this remark. We'll work on all the figures to make them more readable and expanding the figure captions.

4. Minor language edits are recommended to improve clarity and conciseness in a few sections.

Thanks, we'll go through the whole manuscript and aim for clarity and conciseness in the language.

References

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Evergreen Needleleaf Forests," in IEEE Transactions on Geoscience and Remote Sensing, vol. 63, pp. 1-11, 2025, Art no. 4423011, doi: 10.1109/TGRS.2025.3620306.

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