Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-19-AC2, 2020

Geoscientific Model Development Discussions

Open Access

Interactive comment on "FLiES-SIF ver. 1.0: Three-dimensional radiative transfer model for estimating solar induced fluorescence" *by* Yuma Sakai et al.

Yuma Sakai et al.

ysakai@jamstec.go.jp

Received and published: 3 July 2020

Dear Reviewer #2,

Thank you very much for your comments on our manuscript and your sincere efforts in constructing a decision report. The comments and suggestions made by you have been very useful in improving our manuscript. We have revised the manuscript following careful consideration of your comments. In the revised manuscript, rewritten and additional sentences are indicated in red and blue, respectively. We hope the revised manuscript is now suitable for publication in Geoscientific Model Development. We look forward to your favorable consideration. Our responses to your individual com-

Printer-friendly version

Discussion paper

ments and questions are given below.

Very sincerely,

Yuma Sakai

[Comments] This manuscript presents the FLiES-SIF ver. 1.0 model, a 3-D radiative transfer model for SIF, and provides sensitivity analyses of the model to LAI, VZA, and fluorescence yield. Despite the recent advances in remote sensing of SIF, the impact of 3-D canopy structure on SIF signal is not well understood. Thus, the radiative transfer model for 3-D canopy and the sensitivity analysis presented here is of interest to the SIF community and has the potential to improve the estimation of SIF from satellite platforms. Overall, the efforts to simulate SIF in 3-D canopy are important. However, the manuscript needs to be improved to make it a better contribution to the community. Below listed the major concerns, followed by some detailed comments. [Comments]1/ The lack of leaf physiology. This model does not have a leaf fluorescence module that is based on key parameters that control the SIF emission. For example, Vcmax or Chl. This is particularly important for the estimation of SIFyield, as the relationship between SIFyield and APAR depends on Vcmax and Chl. Without the leaf physiology component, the model has a limited use for the correction of satellite SIF data. While running 3-D radiative transfer model can be computationally expensive, our computers have also advanced quite significantly in the past 10 years since the first publication of FLiES (Kobayashi and Iwabuchi 2008). Adding a photosynthesis module to the model will put this model to a higher level. If adding a photosynthesis module is difficult at this point, I would at least ask the authors run a much more extensive simulation in SCOPE to estimate the potential uncertainties in Figure 7. [Response] In accordance with comment 3 by this reviewer, we have substantially improved Sect. 2. The FLiES-SIF model does indeed have a leaf physiology module, although our leaf-scale module itself is not a new model and is based on two existing models (those of Tol et al. (2014) and Farquhar et al. (1980)). This is why we did not include a detailed description of leaf physiology and fluorescence in the original paper. In the revised manuscript, we have added a description of how we incorporated the leaf physiology models in Sects. 2.1.3 (Simulation flow) and 2.1.5 (Computation of leaf level fluorescence yield). As described in these subsections, we created a look-up table of the SIF yield φf under various environmental conditions and leaf traits (such as maximum carboxylation capacity, Vcmax). In the FLiES framework, there is a module that computes the interrelations among the energy balance (leaf temperature), stomata, and photosynthesis based on the CANOAK model (Bakdocchi and Harkey, 1995). However, this would entail a greater computational load and require further input variables. Thus, in the current FLiES-SIF, we used the following assumptions to obtain reasonable photosynthesis simulation results. First, the leaf temperature was assumed to be the same as the surface air temperature. This is usually acceptable, except in very dry conditions where the stomata are almost closed in daytime. The other assumption concerns the stomata modeling. The FLiES-SIF module does not explicitly use the stomata model. Rather, the consequences of the stomata activity, i.e., down-regulation of intercellular partial CO2 pressure (ipCO2), were modeled as a function of the vapor pressure deficit (VPD). (Sect. 2.1.3 on page 5 and Sect. 2.1.5 on page 6)

[Comments]2/ The lack of details for the readers to evaluate and (potentially) reproduce. Information on the following processes / models / parameters are needed: âĂć How were the SCOPE runs conducted? What are the parameters used in the SCOPE run? âĂÍ [Response] Thank you for your comments. We added the information of leaf physiology module of FLiES-SIF in improved model description section (especially, Section 2.1) âĂć In Section 3.4., the authors mentioned that they used the Farquhar model to obtain data on the âĂÍphotosynthetic rate. How exactly was it done? âĂÍ [Response] Thank you for your comments. I apologize for a confusion. We used the Farquhar model to obtain the tentative photosynthetic yield to derive the phi_f. This has been corrected in the revised manuscript. (Lines 564-566 on page 19). âĂć Section 2.2.2., how was fs determined? Where was the data source that gave the full SIF spectra? âĂÍ [Response] The leaf module of FluorMOD was used to determine fs and the leaf-level SIF spectra. We derived the leaf SIF spectra information from the FluorMODleaf model (Zarco-Tejada et al., 2006; PedroÌĄs et al., 2010). The calculated leaf-level spectral SIF radiance variations were then normalized to determine the fraction of SIF at wavelength λ, fs (mW m$-$2 sr$-$1), with respect to the broadband SIF (W m$-$2). That is, we only used the fraction of the spectral composition from the FluorMODleaf model. The radiance was then determined from APAR and φf, which varies with environmental conditions and leaf traits such as the maximum carboxylation capacity, Vcmax, used in the photosynthesis model. We have added the above description in Sect. 2.1.5. (Lines 182–188 on page 6)

[Comments] 3/ Overall structure of section 2. It would make the readers' job easier if there is an overarching paragraph and a diagram (not Figure 2) showing each component of the model, and how they are interconnected. For example, provide the description of canopy representation and some basic assumptions (e.g., turbid media) at the beginning of the section, as this information is essential for readers to understand some of the equations. This section in the current form reads like that each subsection is disconnected. âĂÍ [Response] Section 2 has been substantially revised and improved. As suggested, we have added a new subsection 2.1 (General outline of FLiES-SIF), in which we summarize the overall framework of the FLiES modeling. Newly added Fig. 1(a) shows each radiative transfer component and how they are related. In the previous manuscript, the canopy representation was described in Sect. 2.4. This description has been moved to subsection 2.1.2 (Canopy structure represented by FLiES-SIF). The basic assumptions made for the crown volumes (e.g., turbid media, clumping, and leaf area density distributions) are also described in this section. We have also added a flowchart illustrating the simulation process and the major input variables used in the model (Fig. 1(b)). This describes how some basic information (forest structures and leaf physiology) is derived from the input variables and how the FLiES-SIF model proceeds. (Pages 3–6)

[Comments]4/ The unit for SIF. Whenever SIF from a specific wavelength is simulated, it should be in the unit for spectral radiance, which is mw/m2/sr/nm. Check figures like

Figure 9. âĂÍ [Response] Thank you for this comment. This has been corrected in the revised manuscript. (Figs. 11, 14, 15, and 20)

[Comments]5/ The benefit of 3-D modeling. Just for the benefit for the readers, can you provide some sensitivity analysis of SIF simulations to different canopy structures? This is perhaps one of the key novelties compared with 1-D models. âĂÍ [Response] Thank you for this suggestion. We have added simulations to different canopy structures and a comparison of 1D and 3D modeling in subsections 3.4 and 3.5, and the figures therein. (Pages 18-19)

General comments:âĂÍ [Comments]1/ Please provide continuous line numbers, instead of numbers every five lines.âĂÍ [Response] Thank you for your comment. However, we have compiled the manuscript using the LaTeX package supplied by Copernicus Publications. Thus, we cannot change the line number format.

[Comments]2/ L3: have revealed instead of have been revealedâĂÍ [Response]Thank you for pointing out this mistake. This has been corrected. (Line 3 on page 1)

[Comments]3/ L9-10: “due to the lack of complexity” should describe 1-D models, not the 3-D models. âĂÍ [Response]Thank you for pointing out this mistake. This has been corrected. (Lines 10–11 on page 1)

[Comments]4/ L11: the→a.âĂÍ [Response] Thank you for pointing out this mistake. This has been corrected. (Line 12 on page 1)

[Comments]5/ Line 33: Frankenberg et al. 2011 used GOSAT, not GOSAT 1&2. [Response] Thank you for pointing out this mistake. This has been corrected. (Line 37 on page 2)

[Comments]6/ Line 49: “fluorescence signals enhanced by ...”. Be more specific, is it total fluorescence signal, or the fluorescence signal observed by the sensor? The former is weakened by reabsorption during multiple scattering. [Response] Thank you for pointing out this omission. This refers to the fluorescence signals observed by sensors, as now specified in the manuscript. (Line 53 on page 2)

[Comments]7/ L52: the causality – this word here is confusing. [Response] Thank you for pointing out this error. The phrase "causality of directional canopy SIF" was inappropriate. We have changed this to read "mechanism of anisotropic light interactions such as scattering and absorption in plant canopies."(Line 57 on page 2)

[Comments]8/ L58: DART-SIF is not the only available 3D model. There is at least also FluorFlight and FluorWPS. [Response] Thank you for pointing out this error. We have investigated these models and have added the appropriate references. (Line 63 on page 3)

[Comments] 9/ L83: top of canopy instead of atmosphere? [Response]. Thank you for pointing out this mistake. This has been corrected. (Line 190 on page 7)

[Comments] 10/ L96: this sentence needs rewording. If the canopy is sparse, we would expect less attenuation. Do you mean more attenuation by the trunk? [Response] Thank you for this comment. We meant to refer to "transmitted PAR" rather than "attenuated." We have replaced "attenuated" with "transmitted." (Line 204 on page 7)

[Comments] 11/ L99: forcing leaves to absorb all the photons does not make much biological sense here. Even for tropical forest, fPAR is 0.99 not 1. Please clarify. [Response]. This is a variance reduction technique for the Monte Carlo ray tracing proposed in this study. The proposed method, as noted by the reviewer, artificially enhances fapar in the Monte Carlo simulation. However, the simulated SIF radiance is later scaled by the actual PAR (APARc) (please see the new Fig. 1(b) in the revised manuscript). By applying this scaling, the simulated SIF will not be biased, even if we force all photons to hit leaves.

[Comments] 12/ L123: what does it mean by "negligibly small"? Please quantify. [Response] Thank you for your comment. The phrase "negligibly small" was inappropriate.

Printer-friendly version

Discussion paper

We have changed this to read “when the hotspot effect is not considered.” (Lines 231–232 on page 8)

[Comments] 13/ L128: dΩLis redundant as you have dθLdφL [Response] Thank you for this comment. To retain consistency in the parameter definitions, we have removed dθLdφL. (Equation (9) on page 8)

[Comments] 14/ L141: should be G(ΩL)+G(Ωj)? [Response] Thank you for pointing out this mistake. This has been corrected. (Equation (10) on page 9)

[Comments] 15/ L156: The integration should probably be for exp($-\tau$s(θ,φ))|ΩÂůΩL|sinθdθdφ, please check this equation. [Response] Thank you for this comment. Indeed, you are correct. We have modified the equation accordingly. (Equation (12) on page 9)

[Comments] 16/ L185: Write down this equation as some readers may not have access to the original paper. [Response] The description is not straightforward. This equation is the same as Eq. (16) in Kobayashi and Iwabuhi (2008). We have rephrased this sentence as follows: “E quation (15) is exactly the same as the multiple scatterins in the shortwave radiative transfer (Kobayashi and Iwabuchi, 2008).” (Lines 295-296)

[Comments] 17/ L251: Why is this limited to sunlit condition? Seems a spherical integration is also needed for shaded condition. [Response] This is correct. The spherical integration is necessary for both sunlit and shaded leaves. In the revised manuscript, we have removed the words “For the sunlit leaf condition.” (Line 347 on page 12)

[Comments] 18/ L253: Is there a test on how well this method performs? It seems to me large zenith angles are underrepresented. How about a simulation test: Do a more precise numerical integration (e.g., average of 50 directions) and compare the result with the result from their proposed method (average of the five selected angles). [Response] We have added the results of an accuracy assessment of this 5-angle approximation by comparing the reliable 10-degree samplings (9 zenith angles $\times$ 36 azimuth angles = 324 angle samplings). When the attenuation functions were computed by these two angle sampling approaches at 10000 randomly selected positions in the forest landscapes used in the sensitivity analysis described in Sect. 3, the mean absolute error of this approximation was 14.6% (N = 10000). We have added a description of the accuracy of this 5-angle assumption in Sect. 2.5 C. "We tested the performance of this 5-angle assumption by comparing with 10-degree interval samplings (9 zenith and 36 azimuth angles = 324 angle sampilngs). When the attenuation functions were computed by these two angle samplings at 104 randomly selected positions in the forest landscapes used in the sensitivity analysis in section 3, the mean absolute error of this approximation was 14.6 % (N = 10000)." (Lines 340–343 on page 12) Note that there was an error in the angle information in the previous manuscript ((0°, 0°), (45°, 0°), (45°, 90°), (45°, 180°), and (45°, 270°).). The correct zenith angles are (0°, 0°), (60°, 0°), (60°, 90°), (60°, 180°), and (60°, 270°). In the revised manuscript, this error has been corrected. (Line 339-340) [Comments] 19/ L282: the fluorescence quantum efficiency of 0.04 seems to be too high. SCOPE used to have it as 0.02 and has to change it to 0.01 because the simulated SIF values were too high when using 0.02. [Response] Thank you for this suggestion. As you stated, the fluorescence quantum efficiency of 0.04 was too high and not suitable. We have rerun FluorMODleaf with F = 0.01 and saved all updated values. Usually, the F value is linearly related to the leaf SIF radiance if all other parameters remain unchanged. As noted in Sect. 2.1.5 of the revised manuscript, we only used the leaf-level spectral SIF radiance to determine the fraction of spectral contributions. Thus, this change does not influence the subsequent sensitivity studies in Sect. 3. (Page 13)

[Comments]20/ L301: "shows the" instead of "shows that the" [Response] Thank you for this comment. This has been corrected. (Line 417 on page 15)

[Comments]21/ L338: APARapp instead of APARc?âĂÍ [Response] Thank you for pointing this out. This has been corrected. (Line 454 on page 16)

[Comments]22/ L363: the index for the equation is missing [Response] Thank you for pointing this out. This has been corrected. (Line 480 on page 17)

[Comments]23/ L372: in instead of inn [Response] Thank you for pointing out this mistake. This has been corrected. (Line 489 on page 17)

[Comments]24/ L430: “the proposed model can ....” It has the potential but it cannot do what is stated in this sentence as of now because the lack of leaf physiology. [Response] Thank you for this comment. As stated in our improved Sect. 2, the proposed model does in fact include a leaf physiology module.

[Comments]25/ Figure 17: The sequence of upper and lower panels in the caption is not consistent with the figure: “Upper and lower figures indicate SZA dependency (LAI = 3.0) and LAI dependency (SZA=20°), respectively [Response] Thank you for pointing out this mistake. This has been corrected. (Figure 20 on page 44)

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-19, 2020.