

Reply to Reviewer #1

(Referee comment on "Biases in estimated vegetation indices from observations under cloudy conditions" by K. Wolf et al. (egusphere-2025-2082), <https://doi.org/10.5194/egusphere-2025-2082-RC1>, 2025)

We would like to thank the Reviewer for taking the time to review the manuscript and for providing comments that helped us improve it. Below, we respond to the Reviewer's comments. For clarity, the Reviewer's comments are in **bold** and the changes to the manuscript are *in italics*. Please note that some additional changes have also been made to improve the writing and style.

Dear Dr. Wolf and co-authors,

Thank you for developing this framework to address the cloud influence on ground reflectance measurements. Indeed, in the remote sensing community, our field measurements are highly dependent on the illumination conditions, frequently altered by clouds. I have several minor remarks for your consideration.

In eq. 1, 4, 6 what do you mean by the 'sr' argument? π is already assumed to be in steradian (sr) units, cancelling steradian in the upwelling radiance I .

While in some publications π is considered to have the unit "sr", we disagree in the point that this is always the case. In our case, π and its unit result from integrating over all solid angles of the hemisphere and assuming an isotropic / Lambertian surface. Thus there is no "sr" related to π . Therefore, we kept the notation of "sr" in equations 1, 4, and 6.

A bit on the same line, multiplication by π suggests that the surface reflects homogeneously in all directions, Lambertian reflectance. How big would you expect the influence of the directionality of the actual surface to be on the reflectance value?

The Reviewer is right that we have to be more precise with the definition of reflectivity. The paragraph to define the reflectivity has been updated as follows:

"All remotely sensed VIs rely on the spectral reflectivity $\rho(\lambda)$. In its most general form, $\rho(\lambda)$ is defined as the ratio of the surface-reflected radiance from within an infinite solid angle to the incoming radiance from within an infinite solid angle (Nicodemus, 1977; Martonchik et al., 2000; Schaepman-Strub et al., 2006). The amount of radiation reflected into a given solid angle is defined by the surface-specific bidirectional reflectance distribution function (BRDF, Schaepman-Strub et al., 2006). Furthermore, the surface reflected radiation depends on topography (Matsushita et al., 2007) and on changes in illumination conditions determined by solar zenith angle, aerosol particles, and clouds (Singh and Frazier, 2018). Definitions of $\rho(\lambda)$ based on first principles are given by Nicodemus (1977) and Schaepman-Strub et al. (2006). For brevity and simplicity, we restrict the definition of $\rho(\lambda)$, assuming idealized conditions of pure diffuse illumination and Lambertian reflection at the surface, which results in: [...]"

Under natural conditions, incoming radiation is neither purely diffuse nor purely direct. In such cases, the deviation from a Lambertian surface is expressed by the hemispherical-directional reflectance function (HDRF). SCOPE2.0 internally considers for the HDRF, since we prescribe the direct and diffuse irradiance obtained from libRadtran, and specify the solar zenith and azimuth angles, as well as the observation zenith and azimuth angles. This allows to obtain radiances that would be observed under field conditions, demonstrating the advantage of coupling SCOPE2.0 with libRadtran.

To make this more clear, we added the following sentence to section 2.2.2 Vegetation radiative transfer model SCOPE2.0:

"The angular dependence of $I^l(\lambda)$ is considered for by the actual illumination and observation geometries, the direct and diffuse $F^l(\lambda)$, and the internal calculation of the reflectivity in SCOPE2.0. "

Figure A1 in the Appendix of the submitted manuscript shows the normalized hemispherical-directional reflectance factor R_{HDRF} at 550 nm wavelength. The plot displays R_{HDRF} for different illumination conditions, which are specified by the cloud optical thickness τ and two solar zenith angles θ_i , and resulting direct to diffuse ratios f_{dir} .

The polar plots and particularly the cross-sections show that the actual surface reflectivity deviates strongly from the isotropic assumption. In the general case, the amount of radiation that is reflected in a specific direction depends on the angles of the incoming irradiance, as it is given in equations A1 and A2 in the Appendix. The dependence on viewing geometry is pronounced along the principal plane, which is defined along the 0°-180° azimuth line, with the Sun positioned at 0°. For the example given in Fig. A1 a-d with solar zenith angle θ_i of 25° and $\tau=0$, a change in the viewing direction from nadir ($\theta_r=0^\circ$) towards the hot spot at 25°, observed reflectivity would almost double. With increasing values of τ the pronounced effect of the hot spot vanishes but the effects towards large viewing angles become more pronounced (see Fig. A1 d). Even greater difference in between nadir observation and more slant observation geometries appear with increasing values of θ_i (see Fig. A1 e-h). Also other azimuth directions off the principle plane show a strong dependence on the viewing geometry (see Fig A1 right most column)

On the SCOPE model (section 2.2.2 and Table 2).

- **First of all, it is unclear why SCOPE was chosen instead of SAIL or INFORM. The latter is more suitable for pine forest (L163) simulations, as it explicitly has the concept of trunks and branches in it. SCOPE has energy balance, thermal domain, photosynthesis and chlorophyll fluorescence that other models do not have. Using it as a tool for a single reflectance simulation is overkill. Nonetheless, your choice.**

Thank you for providing your model suggestions. However, we are aware that several models for vegetation radiative transfer exist.

To our knowledge, an equivalent model to SCOPE2.0 would be, for example, PROSAIL, which combines PROSPECT (for leaf optical properties) and SAIL (for radiative transfer in the canopy). We chose SCOPE2.0 over other models because it provides an accessible way

to couple it with libRadtran, without the need to make fundamental changes in the code base of both models. Another factor is the ability of SCOPE2.0 to provide simulations in the thermal wavelength range, a potential topic that we are interested in future investigations. Using SCOPE2.0, now for the visible and near-infrared, and later for the thermal infrared, allows us to use the same or similar model framework for both wavelength ranges with only minor modifications.

We also acknowledge that INFORM may be better suited for an erectophile leaf angle distribution, but since we also simulated spherical and planophile leaf angle distribution, SCOPE2.0 appears to be a reasonable choice to us.

- **Why was only half of the important SCOPE input parameters chosen? The BSM model, for example, has brightness, two shape parameters and moisture content, but only B is shown in Table 2. In any case, those parameters are set to their default values so it is also not clear why highlighting them at all.**

In the companion paper by Wolf et al. (2025a)

[<https://bg.copernicus.org/articles/22/2909/2025/bg-22-2909-2025.html>], we presented a sensitivity study of selected SCOPE parameters relevant to canopy optical properties within the visible and near-infrared wavelength range. In the sensitivity study, the selected variables have been varied around their default values and the effects on the surface reflectivity and canopy albedo have been determined. Based on the relevance of these factors, ordered by their magnitude, they were selected to be included in Table 2 in the current manuscript.

While some of the parameter default values have been modified to represent forest, others were kept constant, like the BSM parameter. The default value of the BSM model is listed because it was found to be an influential parameter in surface reflectivity. To better clarify the selection of parameters and the values, we added the following sentences to section "2.2.2 Vegetation radiative transfer model SCOPE2.0".

"Table 2 provides an overview of the selected parameters for the vegetation RT simulations. The parameters were selected based on their relevance to surface reflectivity within the visible and near-infrared wavelength ranges. Their individual relevance was estimated in a sensitivity study by Wolf et al. (2025a)."

- **Finally, could you please be more explicit about which SCOPE output was integrated with the libRadtran output and how? I am a bit confused because L152 says "As an initial guess of the surface albedo in libRadtran, the "mixed-forest" albedo was taken from the IGBP data base." According to my understanding, it was sufficient to take some spectral forest reflectance for the exercise instead of running an RTM.**

The Reviewer is right that we were not very clear about the iterative nature of the model coupling.

The introductory subsection "2.2 Radiative transfer simulations" was modified, now including a rephrased sentence, which reads as follows:

*"Furthermore, radiation interactions may occur between the surface and the cloud, which can be accounted for by **iterative** coupling of the RT models of the atmosphere and vegetation (Wolf et al., 2025a). In the present paper we use the same model coupling setup*

introduced and described by Wolf et al. (2025a)".

To clarify the confusion about the initial guess of surface albedo:

When the atmosphere radiative transfer (RT) model libRadtran is run for the first time, it is initialized with a first guess for surface albedo, which is taken from the IGBP database. After that, SCOPE2.0 is run with the provided downward irradiance from libRadtran. In the second iteration, the initial guess is replaced by the surface albedo that is based on the upward irradiance provided by SCOPE2.0 and the downward irradiance provided by libRadtran. Two iterations were found to be sufficient for the simulated cases.

To emphasize that the IGBP is only used in the initial run and is later replaced by the albedo determined from the coupled atmosphere-vegetation model, the following sentence in section "2.2.1 Atmospheric radiative transfer model libRadtran", was modified:

"The iteration process was first started by running libRadtran, with an initial guess for the surface albedo. The "mixed-forest" albedo was taken from the IGBP database (Loveland and Belward, 1997). After one iteration cycle, the surface albedo determined during the iterative model coupling process was used (Wolf et al., 2025a)."

We hope that this answers the Reviewer's question. However, we do refrain from providing a more detailed description of the model coupling and implementation in the submitted manuscript because the coupling is described in depth in the companion paper by Wolf et al 2025.

Figure 2b. Please, add a legend.

Figure 2a and b share the same legend. To clarify this, the following sentence has been added to the figure caption.

"Panels (a) and (b) share the same legend."

L187 – “Wolf et al. (2024) have shown the influence of clouds on direct and diffuse $F^\downarrow(\lambda)$, the associated effects on $F^\uparrow(\lambda)$,” please, write exactly what the influence was. I guess more clouds – more diffuse radiation.

Yes, this is correct, Wolf et al. (2024) showed, that clouds increase the diffuse radiation. An increase in cloud optical thickness τ leads to a decrease in direct irradiance $F^\downarrow_{dir}(\lambda)$. However, the response of the diffuse irradiance $F^\downarrow_{dif}(\lambda)$ depends on τ . For values of τ below 4 to 6, the diffuse irradiance first increases, and then decreases as τ increases further. The total amount of $F^\downarrow(\lambda)$, direct plus diffuse $F^\downarrow(\lambda)$, decreases as τ increases, with an increasing fraction of diffuse radiation.

Furthermore, an increase in the diffuse fraction reduces the influence of changes in the solar zenith angle on the upward irradiance. Lastly, the presences of clouds shifts the weighting of the incoming radiation towards shorter wavelengths, as clouds primarily absorb radiation at longer wavelengths. The paragraph in section 3.1 has been rephrased as follows:

"Wolf et al. (2025a) have shown the influence of clouds on direct and diffuse $F^\downarrow(\lambda)$, the effects on $F^\uparrow(\lambda)$, and the resulting albedo effects over vegetated areas using coupled atmosphere-vegetation radiative transfer models. An increase in τ leads to a decrease in $F^\downarrow_{dir}(\lambda)$, while the response of $F^\downarrow_{dif}(\lambda)$ depends on τ . For values of τ less than 4 to 6, $F^\downarrow_{dif}(\lambda)$ first increases and then decreases as τ increases further. The total $F^\downarrow(\lambda)$ and $f_{dir}(\lambda)$ both continuously decrease as τ increases. In addition, $F^\uparrow(\lambda)$ became less sensitive to changes in θ . Lastly, the presence of clouds modulates the

incoming radiation spectrally by shifting the incoming radiation towards shorter wavelengths, as clouds primarily scatter radiation at shorter wavelength and absorb radiation at longer wavelengths. Wolf et al. (2025a) also showed that radiative interactions between the canopy and the cloud base increase $F^{\uparrow}_{dif}(\lambda)$ and albedo compared to cloud-free conditions. The present paper focuses on the related effects on $I'(\lambda)$ and $\rho(\lambda)$."

Figure 3. What do grey areas show? Sentinel-2 bands?

"The gray marked areas highlight the Sentinel-2 bands B2, B4, B8, B8a, and B11." The very same sentence was added to the figure captions of Fig. 2 and Fig. 3.

Figures 3 and 4 captions. Please, note, you are working with synthetic (modelled) data.

Remove the term "measured" reflectance; do not mislead the readers.

To be more clear, we added the word "synthetic" to all instances, where we refereed to "measurements" to make clear that we refer to the simulated measurements. The sentence in caption of Fig 3 was changed to: *"... and constant cloud optical thickness during calibration and the actual synthetic measurements ..."*

Figure 5. Please, check the location of symbols inside the heatmaps Whereas for NDVI (circle) λ_1 and λ_2 are matching the expected NIR and RED, NDWI1240 is definitely far from $\lambda_1=1240$ nm. Furthermore, the symbol in Figures 5c and 5d around $\lambda_1=900$ nm, $\lambda_2=1600$ nm is unclear (or absent from the legend).

Thank you for pointing this out. The second Reviewer had a similar comment. The figure has been revised, now with the correct position of the markers and the completed legend. The color-style has been adjusted to improve the legibility of the markers.

Reply to Reviewer #2

(Referee comment on "Biases in estimated vegetation indices from observations under cloudy conditions" by K. Wolf et al. (egusphere-2025-2082), <https://doi.org/10.5194/egusphere-2025-2082-RC2>, 2025)

We would like to thank the Reviewer for taking the time to review the manuscript and for providing comments that helped us improve it. Below, we respond to the Reviewer's comments. For clarity, the Reviewer's comments are in **bold** and the changes to the manuscript are *in italics*. Please note that some additional changes have also been made to improve the writing and style.

This manuscript reports on the effect of clouds on the derivation of vegetation indices from remote sensing platforms in a radiative transfer modelling approach. Basically it is a sensitivity study on how clouds contribute to the incoming radiation and what effect it might have on the calibration conditions for deriving the reflectance values and consequently on the computation of the various VI with Sentinel 2 bands as example for different solar zenith angles.

Overall, it is an interesting and relevant study since it brings our attention to the basics of using reflectance values and its impact on derived vegetation indices. It is important that we do not forget the basics of RS data. The preprint is very well written and well-documented, with nice figures.

Thank you for the generally positive feedback.

To my opinion, the authors should elaborate a bit more on the consequences of the effect on biases in values of VI's. They briefly touch the impact on LAI derived from EVI, but the authors should add some paragraph how it could impact remotely sensed derived biophysical properties such as GPP, green water fluxes etc with proper referencing. This would make the paper even more relevant. Perhaps, indicating some of the effects as percentages can increase the visibility.

We added a subsection titled "3.4 Implication of biases in vegetation indices on estimated biophysical properties" in accordance with the Reviewer's suggestion. The added subsection discusses the impact of biases in the enhanced vegetation index (EVI) and the normalized differential vegetation index (NDVI) on the estimation of gross primary production, fresh and dry biomass, vegetation water content, and leaf area index. We added relevant citations in which the correlations between EVI, NDVI, and the biophysical properties are derived. Due to the length of the subsection, we would like to direct the Reviewer to the track changes file.

In lines 117, 306 and others, the authors directly link NDVI to vegetation health. I would refrain from that, since NDVI essentially says something about the "greenness" of the

vegetation, but not necessarily on its health. It can be used for vegetation health, but it is not synonymous for health.

We considered the Reviewer's comment and rewrote all instances where the NDVI was directly linked with "vegetation health". The sentences were rephrased as follows:

"The greatest variability is found for $\theta = 25^\circ$, where NDVI increases from 0.87 to 0.91 with increasing τ , which could be interpreted as an overestimation of vegetation health."

"For $\tau_{cal} = 0$ an extreme increase of τ_{mea} from 0 to 40 results in a decrease in NDVI from 0.87 to 0.84, which could be interpreted as an underestimation of vegetation health"

"In both cases, the inferred ground-truth vegetation health would be overestimated."

Furthered, I only have few minor/textual comments

Normally, numbers less than 10 are written as text; So less than one; equals one, etc

We followed the suggestion of the Reviewer and scanned the text for these mistakes. However, during previous submissions to Copernicus journals, phrases like "...ranges from 0 to 1..." were accepted by copy-editing and typesetting. Therefore we kept this stile of writing. Similarly, we ket instance such as "...smaller than 1", since those were also accepted by Copernicus copy-editing and type-setting. The Copernicus guidelines say: "For items other than units of time or measure, use words for cardinal numbers less than 10; use numerals for 10 and above (e.g. three flasks, seven trees, 6 m, 9 d, 10 desks)."

If one uses for instance 0.29, also use 0.20 and not 0.2 (see L338, but also elsewhere): always use the same amount of decimals for the same property

We acknowledge this comment. However, we did not find a specific guideline that says that all values have to use the same number of digits. The SI-guidelines say that trailing zeros should be used when one wants to imply a certain precision of, e.g., a measuring device. During previous submissions to Copernicus journals, trailing zeros have been removed. Since there are no clear rules, we would like to leaf it to typesetting and copy-editing of the journal.

I do not know the policy of the journal, but normally the cited references in the text are first ordered chronologically and then alphabetically

We agree with the Reviewer that citations should be in chronological order. This was an error on our part, and we have corrected the order of the citations.

Abstract: add more on possible consequences;

In line with the Reviewer's first comment, we added the following sentence to the abstract:

"Other estimates of biophysical properties derived from EVI, such as gross primary product, fresh and dry biomass, or vegetation water content, are similarly affected."

Captions Fig 1. Replace “relate” with “connect”

We followed the Reviewer's suggestion.

L30: specify what tau is under the text of Eq 1; see lines 45-47; this should come earlier in the text

The Reviewer is right and the definition of the cloud optical thickness is given earlier in the text.

"The cloud optical thickness $\tau(\lambda)$ is a measure of the extinction of radiation for a vertical path through the cloud, serving as vertical coordinate."

L61: “attempts”? This is not a proper use of the word here;

The word "attempts" has been replaced with "allows." The sentence now reads:

"Using a RP enables transfer calibration."

L67: In general, it is a “transfer function” rather than a factor, although used as a factor;

To be more precise, we followed the suggestion of the Reviewer and replaced "transfer factor" with "transfer function".

L76: Should be “requires frequent calibrations of the transfer function”;

We partially adopted the Reviewer's suggestion and modified the sentence as follows: "...requires frequent RP overflights to obtain updated transfer functions."

L217: remove “the“ before Appendix A;

"The" has been removed.

L224: “valueS”;

The typo has been corrected.

L270-271: “ARE close to zero”

The sentence has been corrected.

Figure 5: Why is the symbol of NDII missing in the upper right panel?

The second Reviewer had a similar comment in this regard. Figure 5 has now been revised with the markers in the correct position and a completed legend, and the intensity of the color scale has been adjusted to enhance the legibility of the markers.

L281, 324 and other lines: I would refrain of using the term “exemplary”; it echoes a bit as “exemplary behavior or punishment”; Perhaps use “illustrates”?

Based on the Reviewer's suggestion, all instances of "exemplary" have been rephrased.

L345: Should be “The effect of changes in fdir”;

The text has been modified as suggested.

L378-379: “... was between 0 and 40,” and “.. ranged ...”; Remove “were covered”:

The text has been modified as suggested.

L406: remove comma after NDWI

As suggested, the comma was removed.

L407: twice increase, increasing;

The second instance of "twice" was removed.