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Geoscientific
Model Development
Discussions

Interactive comment on "Efficient modeling of sun/shade canopy radiation dynamics explicitly accounting for scattering" *by* P. Bodin and O. Franklin

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The main concern of the reviewer seems to be that we, in his view, have not sufficiently proved that our model is better than the GOU model and that the importance of our modifications is overstated.

Because the GOU model is widely used due to its efficiency an improvement to this model without increasing computational time we see as an important finding. Below we now also show on how important changes can be in relation to GPP.

No argument by the reviewer has been made stating that our formulation of the scattering process should conceptually and mathematically be less sound than the original GOU. In addition, comparison to data shows a better fit and thus our modifications are very likely to be an improvement. The second question is how large the improvement is. The reviewers' comments were of great value in helping us test this potential importance more conclusively. Our examples show that the difference may be up to 6% for simulated GPP, a number which is not negligible.

The comments made by the reviewer are commented on below. The comments are not dealt with in the order they were given by the reviewer but rather in an order that follows how we performed the new analysis as suggested by the reviewer.

3. The authors are correct in noting that the GOU model has been widely used, in part due to is implicity and ease of calculation. If the authors believe that the model is invalid and that their revision is better, then they must be held to a high scientific standard. They must thoroughly investigate their BF model, the standard GOU model, and compare the models in exacting tests with each other, with other radiative transfer models, and with observations. I do not see such rigorous and exacting testing.

Instead, the model testing is quite vague. a. The authors compare the GOU and BF models against data published by Baldocchi et al. (1985). The data are the mean daily vertical profile of downward diffuse radiation through the canopy, both measured and calculated with Norman's model. The authors extract the data from Figure 5 of Baldocchi et al and compare their simulations with this data. My concern, here, is with the authors's GOU and BF simulations and their comparison with the Norman simulation reported by Baldocchi et al. The authors's simulations appear to be for the broadband (PAR and NIR) rather than distinguishing between these two wavebands. I infer this because the authors use reflectivity (0.30) and transmissivity (0.22) reported

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by Baldocchi et al for broadband. How would the GOU and BF simulations differ if the authors had distinguished between PAR and NIR? The Norman simulation reported in Baldocchi et al that the authors compare to did in fact distinguish these two wavebands.

First of all, we do not believe that the GOU model is generally invalid. On the contrary, we find it a very useful model for our own research purposes and only suggest a way of improving it further. Evidenced by theoretical arguments and the published data available for testing, we believe that our modifications are conceptually sound and results in an improvement compared to the original GOU model.

In Baldocchi et al (1985) the only measured profiles of scattered radiation available are in the broadband spectra, so it is only for this that we can compare against measurements. For all other spectras the global radiation (both direct and diffuse radiation) is used which is dominated by the beam flux and thus of little use for our purposes.

However, we can still compare the profiles of simulated scattered radiation between the BF and GOU models using the scattering parameters for NIR and PAR found in Baldocchi et al. (1985). Using these we find that the difference between the models is larger in the PAR spectra and smaller in the NIR spectra (supplementary figure).

b. Furthermore, the details of the GOU and BF simulations are not clear. On page 1799 (line 16-17), the authors state "In the GOU model σ was set to equal t in order to only account for downwelling scattered radiation." Yet on lines 20-21 of the same page they state that "For the BF model leaf reflectance (r) was set to 0.30 (Fig. 5: Baldocchi et al., 1985) and in the GOU model σ to the sum of r and t." It is not clear from this what parameters were used in the GOU simulation or even if the GOU simulation was *conïňĄgured in a manner comparable to the BF simulation. The caption to Figure 2 also states that the GOU simulation used "scattering equal to the sum of transmittance (t=0.22) and reflectance (r =0.30)." The parameters used in both models must be clarified.*

Here, some clarification obviously is needed. In the comparison against measured data (Fig. 3 in the ms), only downward scattering is taken into account. Therefore we set scattering to be equal to transmittance in the GOU model in order to only account for the downward flux.

In the comparison between the BF model and the GOU model (Fig. 2) we plot the individual fluxes of upward and downward scattering (for the BF model) as well as the sum of these two fluxes. The total scattering in the BF model is then compared to the total scattering in the GOU model. Therefore we need to use scattering=transmittance+reflectance when comparing against the total scattered flux of the BF model.

c. I would be more convinced if the authors reported additional simulations and described the theoretical derivation of their equations to show why the two models differ.

And then the authors should compare their simulations with Norman's radiative transfer model to help differentiate the GOU and BF models. Instead, the authors take the Norman results presented in Figure 5 of Baldocchi et al without any context and do not perform their own simulations. Have the authors really compared their model to the benchmark model of Norman if they do not use the Norman model in any simulations?

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In our simulation we use the same parameter values for scattering and transmittance and the same value for incoming radiation as used in the simulations using the Norman model. We also assume a spherical leaf-angle distribution, as does the Norman model. Therefore there is no need to implement this model rather than using the results found in Baldocchi et al. (1985).

My major comments are as follows: 1. I am left wondering how important the difference between the GOU and BF models really is. The authors motivate this study by noting the necessary distinction in light absorption between sunlit and shaded leaves, the importance of diffuse radiation for shaded leaves, and that this affects GPP. However, the authors never show whether the difference between the GOU and BF models matters for GPP. In contrast, the Spitters (1986) paper, which the authors cite as the reference for the GOU model, does in fact show the sensitivity of GPP to radiative transfer of direct beam and diffuse radiation. In the Bodin and Franklin manuscript, the authors demonstrate a difference between their BF model and the GOU model, claim that this is very important for GPP and therefore vegetation models, but never demonstrate that it is important.

2. Additionally, the authors do not show results for sunlit and shaded leaves. The study is motivated by the need to distinguish light absorption between sunlit and shaded leaves, especially as determined by scattering of direct beam radiation. But the authors never show profiles of light absorption by sunlit and shaded leaves simulated by the GOU or BF models or by their benchmark model of Norman (1979).

4. The authors should note that their radiative transfer equations give ïňĆuxes per unit ground area. Fluxes per unit leaf area are needed to (i) distinguish between sunlit and shaded leaves and to (ii) calculate photosynthesis. Neither of these are discussed in the paper (as mentioned previously), but need to be included in the study. So the au-

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thors will have to also show the equations per unit leaf area for sunlit leaves and shaded leaves. Spitters (1986) provides fluxes both per unit ground area and per unit leaf area.

The claim that a correct representation of diffuse and direct radiation is important for correctly simulating GPP is well documented in the literature as shown by the references quoted in our manuscript. We do not however, as the reviewer correctly comments, show the effect of our modifications on simulated GPP. This effect will depend on a whole range of factors independent on the fraction of absorbed irradiance (e.g. nitrogen profiles, the amount of incoming global radiation, and photosynthesis model parameters) and any statement on how large the effect will be on GPP will dependent on these factors. Nevertheless we try here to give examples on how big this difference can be. The calculations of absorbed irradiance are based on Anten et al. (1997) (see supplement).

Using the same parameter values as reported in Baldocchi et al. (1985) for PAR we get no difference in absorbed radiation for sunlit leaves (as expected), whereas there is a large difference in absorbed radiation for shaded leaves (not shown), with the largest difference at the top of the canopy (above LAI=2).

Photosynthesis was then simulated for a range of incoming radiation (50-800 W m-2) and a solar declination angle between 45° and 80° and the same LAI (5.5) and value for fDif as in Baldocchi et al. (1985). A simple non-rectangular hyperbola function for GPP (Thornley, 2002) was used (see supplement).

The difference in simulated GPP (for LAI=5.5) between the GOU and BF model varies between 0.1 and 3.1%. Below 150 W m-2 the difference is >1.8%. At the top of the canopy (LAI<=2) where the difference between the modeled profiles is largest the

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difference is between 1.2 and 6.2% and for an irradiance below 150 W m-2 >4.4%.So for a low irradiance and a sparse canopy the difference will be detectable.

5. It would help readers who are not experts in radiative transfer theory (and Goudriaan's model) to have further discussion of the derivation of these equations. Two things stand out. a. page 1796, line 22: "The direct radiation on sunlit leaves is assumed to be equal at all canopy depths. . .". An uniformed reader might think this is a big assumption, when in fact it is a simple outcome of Beer's law. Also this applies to the mean irradiance of sunlit leaves per unit sunlit leaf area.

This indeed follows Lambert-Beer's law and simply means that all sunlit leaves experience the same level of beam irradiance. In reality this is only partly true due to the penumbra effect (e.g. Stenberg et al. 1998), an effect which we chose to neglect in this model.

b. page 1797, equation 5: This is the equation that the authors are critical of. The authors must explain in more detail how it is derived. What, in its derivation, is wrong and how does the BF model improve upon this equation?

This is, as correctly noted by the reviewer the crucial equation in our manuscript. The way Goudriaan (1977) simulates scattered irradiance is as the difference between "total direct" and beam irradiance. The former being an aggregate flux containing both direct and diffuse irradiance. This combined flux is somewhat artificial as absorption differs strongly between diffuse and direct radiation. The more logical way as we see it is to first calculate the amount of transmitted and reflected irradiance formed at each canopy layer and to follow these fluxes downwards or upwards following the same rule as for incoming diffuse irradiance. Further, an observed (hump shaped)

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profile of downward scattering as the one reported in Baldocchi et al. (1985) can never be reproduced by the GOU model as both diffuse and scattered irradiance follow Lambert-Beer's law with the only difference in the absorption of the "total direct radiation"-flux and beam radiation being a difference in the extinction coefficient (eq 2b and 2c in Anten, 1997).

6. There are several instances where the authors use statements that while not factually incorrect are very misleading and overstate the significance of their model. These include: a. page 1794, lines 4-6: "To include this effect in vegetation models, the plant canopy must be separated into sunlit and shaded leaves, for example using an explicit 3-dimensional ray tracing model." The authors are correct to note the distinction between sunlit and shaded leaves, but they imply that explicit 3-D ray tracing models are needed to account for sunlit and shaded leaves. The inference is that 3-D ray tracing models are impractical for global vegetation models, but the new model devised by the authors is not and therefore it solves a major problem. This is not true. In fact, the authors compare their BF model to the Norman (1979) model, which is not an explicit 3-D ray tracing model and does account for sunlit and shaded leaves.

Some clarification is needed here. We do not wish to imply that ray tracing models are needed to take into account the separation into sunlit and shaded leaves. We only wanted to point at the usefulness of simpler schemes such as the BF and GOU models or indeed the models by Norman (1979) and Sellers (1985) which we are aware have been used in LSMs.

b. page 1794, line 10: ". . . which however does not explicitly account for radiation scattering." This sentence implies that the GOU model is incorrect because it does not account for radiation scattering, which is itself not a correct statement. Yes, the GOU

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model does not explicitly account for scattering of direct beam radiation, but it does implicitly account for this scattering. Moreover, the model does account for scattering of diffuse radiation.

This was the point we were trying to make, that our model takes into account scattering explicitly as opposed to implicitly.

c. page 1796, lines 1-2: ". . .in contrast to previous comparable models (e.g. Norman, 1979, 1980; Sellers, 1985) we do this without sacrificing the analytical solvability." Here, the authors suggest that their solution is preferable to other models (Norman, Sellers) because it correctly accounts for direct beam scattering and can be solved analytically. The comparison with Norman (1979) is valid, because that requires a multi-layer canopy and scattering is solved iteratively. The comparison with Sellers (1985) is not correct, because that model does account for direct beam scattering and is solved analytically.

This is a fair comment. We did however perform a quick test using the Sellers model and found the GOU model to be about 1000 times faster than the Sellers model. Also, compared to the GOU and BF model it is fair to say that the Sellers model is relatively complex and fairly intractable.

d. page 1801, lines 11-12: "However, it is notable that this model still represents state of the art in this field despite its shortcomings." The authors have not satisfactorily demonstrated the shortcomings of the GOU model.

We have demonstrated conceptually that our model is more realistic for the scattered radiation, and demonstrated mathematically and practically that the predictions differ

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between the models. Thus, this strongly suggests that our model provides a significant improvement. The reviewer does not give any reason why the GOU model should be better than the modifications suggested in the manuscript and there is no reason to have that baseline assumption.

e. Page 1801, lines 21-23: "Given the importance of canopy radiation modeling, our qualitative as well as quantitative improvement of a tractable analytical canopy radiation model can be used to improve a wide range of plant, vegetation and ecosystem models." This is a gross overstatement of the significance of the results shown in the manuscript. In no way have the authors demonstrated that their BF model improves plant, vegetation, or ecosystem models.

We do not incorporate our radiation scheme in a LSM, and thus do not test that the model in any way improves the simulation of say NEE compared to Fluxnet data or other benchmark tests normally used for land surface models. This would be an interesting future test to be made. We do however, especially after the additional simulations suggested by the reviewer, show that the inclusion of this model could have a significant effect on simulating these fluxes, at least at a regional level.

7. Figure 2: The caption is inconsistent with the legend.

This will be corrected in the revised manuscript.

New references:

Franklin, O., Ågren, G.I.: Leaf senescence and resorption as mechanisms of maximizing photosynthetic production during canopy development at N limitation. Funct. Ecol. 16, 727-733. 2002.

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Reich, P.B., Kloeppel, B., Ellsworth, D.S., Walters, M.B.: Different photosynthesis-nitrogen relations in deciduous hardwood and evergreen coniferous tree species. Oecologia 104, 24-30. 1995.

Stenberg, P.: Implications of shoot structure on the rate of photosynthesis at different levels in a coniferous canopy using a model incorporating grouping and penumbra. Funct. Ecol. 12, 82–91. 1998.

Thornley, J.H.M.: Instantaneous canopy photosynthesis: analytical expressions for sun and shade leaves based on exponential light decay down the canopy and an acclimated non-rectangular hyperbola for leaf photosynthesis. Ann. Bot. 89, 451–458. 2002.

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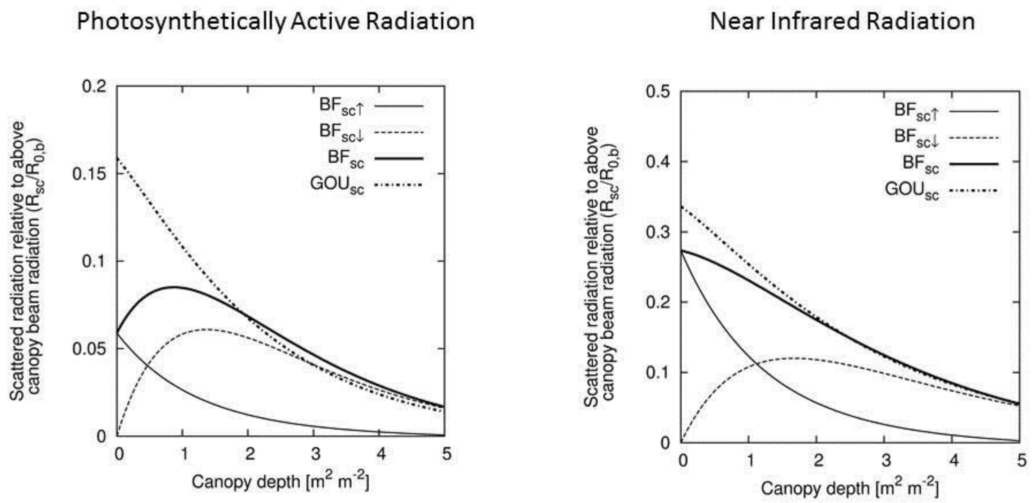

Figure S1. The modeled profiles of scattered radiation (R_{sc}) relative incoming beam radiation ($R_{0,b}$) for the respective models. For the BF model scattering is divided into upwelling ($BF_{sc\uparrow}$: dashed line) and downwelling scattered radiation ($BF_{sc\downarrow}$: thin solid line). Simulations are made both in the Photosynthetically Active Radiation (left) and in the Near Infrared Radiation spectra (right).

Fig. 1.

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Equations for modeling absorbed radiation by sunlit and shaded leaves

Absorbed radiation for shaded leaves in the BF model is calculated as:

$$R_{sh,a}(L) = (1 - A_{sl}(L))\left[\frac{k_d}{\sqrt{1-\sigma}}R_d(L) + \frac{k_d}{\sqrt{1-r}}R_{sc\uparrow}(L) + \frac{k_d}{\sqrt{1-t}}R_{sc\downarrow}(L)\right]$$

and for the GOU model:

$$R_{sh,a}(L) = (1 - A_{sl}(L))\left[\frac{k_d}{\sqrt{1-\sigma}}(R_d(L) + R_{sc}(L))\right]$$

Absorbed radiation by sunlit leaves can for the BF model be calculated as:

$$R_{sl,a}(L) = A_{sl}(L)\left[\frac{k_d}{\sqrt{1-\sigma}}R_d(L) + \frac{k_d}{\sqrt{1-r}}R_{sc\uparrow}(L) + \frac{k_d}{\sqrt{1-t}}R_{sc\downarrow}(L) + k_b R_{b,0}\right]$$

and for the GOU model:

$$R_{sl,a}(L) = A_{sl}(L)\left[\frac{k_d}{\sqrt{1-\sigma}}(R_d(L) + R_{sc}(L)) + k_b R_{b,0}\right]$$

Fig. 2.

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<u>Equations for modeling photosynthesis</u>

Photosynthesis was then simulated for a range of incoming radiation (50-800 W m^{-2}) and a solar declination angle between 45° and 80° and the same LAI (5.5) and value for fDif as in Baldocchi et al. (1985). A simple non-rectangular hyperbola function for GPP (Thornley, 2002) was used:

$$P(L) = \frac{P_m + \Phi R_a(L) - \sqrt{(P_{m+}\Phi R_a(L))^2 - 4\Theta P_m \Phi R_a(L)}}{2\Theta}$$

Where Ra is absorbed irradiance, ϕ is the quantum yield (µg J^{-1}), Pmax is the light saturated gross photosynthetic rate (µg C s^{-1}) and Θ is the convexity of the relationship between P and Ra.

P_{max} is calculated as:

$$P_m = (n_a - n_{min})$$

Where n_a is nitrogen content per leaf area, n_{min} is the leaf nitrogen content for which P_{max} is 0, and a is the slope of the P_{max}-n_a relationship.

In this test we assume ϕ to be 2.73 µg J^{-1}; Θ to be 0.75 and n_{min} 0.4 g N m^{-2} (Franklin and Ågren, 2002). Leaf nitrogen content (n_a) is assumed to be 2.3 and a is assumed to be 65.7 using values for *Q. rubra* (Reich et al. 1995). For simplicity a uniform nitrogen distribution in the canopy is assumed.

Fig. 3.

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