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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 authors are correct in noting that the Goudriaan radiative transfer model (GOU) does not explicitly account for the scattering of direct beam radiation in the plant canopy. The model infers the scattering of direct beam radiation as the difference between "total" direct beam radiation at cumulative leaf area index x (this includes scattered beam radiation) and direct beam radiation (excluding scattering). The authors claim that this calculation of scattered beam radiation is incorrect, and devise an alternative model. The revised model of the authors (BF) explicitly calculates the forward and backward scattering of direct beam radiation. The authors state that their BF model is improved compared with GOU and should be used by the vegetation modeling community. My overarching concern is that if one is to claim that a particular model

is wrong, then there must be a high burden of proof that the model is indeed wrong, particularly when that model is widely used. Instead, what I see in this manuscript is a very cursory discussion of a quite complicated and involved topic (i.e., radiative transfer through plant canopies). If the GOU model is deficient, then the authors are obliged to inform the readers why it is wrong. Why does the implicit scattering in the GOU model fail to replicate the observations in the test devised by the authors? And why does the BF model match the observations?

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).

3. The authors are correct in noting that the GOU model has been widely used, in part due to is simplicity 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 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.

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 configured 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.

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 trans-

fer 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?

4. The authors should note that their radiative transfer equations give fluxes 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 authors 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.

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.

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?

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.

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 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.

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.

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.

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.

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

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