

# ***Interactive comment on “Do we (need to) care about canopy radiation schemes in DGVMs? An evaluation and assessment study” by A. Loew et al.***

**A. Loew et al.**

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Received and published: 17 December 2013

## **1 General comments of reviewer**

- *The present work is devoted to provide an evaluation of canopy RT schemes of different DGVMs. This work also shows the potential impact of the identified deviations on carbon production. The state of art is well reviewed in particular and present findings will bring new insights on the topic.*

We thank the reviewer for these positive comments regarding the submitted manuscript.

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- *However, I found the form of presentation to be sometimes confusing. The objectives of this manuscript are numerous and not clearly sound. The evaluations of each canopy RT scheme suffer from a lack of solid validation. RAMI4 virtual experiments allows to make evaluations of 3-D RT models according idealized cases. But these experiments do not represent real canopies in a realistic manner and are not compatible with the needs of these DGVMs that have global scale applications. This critical aspect of the paper is pointed out by the authors in the discussion section (4.2). In my eyes, the work is described as an evaluation but it is really more of a comparison and should not be considered a validation (or evaluation). Otherwise, ground measurements and satellite products of FAPAR and GPP should be considered. On the other hand, the discussion about the impact of albedo biases on the radiative forcing is very interesting. My opinion is that the paper should be shortened and the paper should deal with an unique objective (maybe less ambitious).*

We thank the reviewer for this comment, which is addressing the scope of the submitted manuscript. We actually believe that the comment of the reviewer is more related to terminology or definitions, rather than the actual content of the present manuscript. The fact that the reviewer was asking the question about the scope of the manuscript has shown us that we need to further elaborate on a clearer communication of the objectives of the present study.

Let us summarize here briefly how we think the issue raised by the reviewer can be addressed. We would like to start first with, what we think, the paper actually is **not**:

- The paper is **not** an evaluation of global DGVMs
- nor is it aiming for an evaluation of e.g. global faPAR and surface albedo fields (this is subject to model benchmarking activities like e.g. in iLAMB)
- nor is it dealing with impact on long timescales

- nor is it a comprehensive comparison of different canopy RT schemes (this is at the core of RAMI)

For these reasons the paper is also not using any satellite data. The authors have done such large scale evaluations already for various models (e.g. Hagemann et al., 2013, Brovkin et al., 2013, Loew et al., in prep.)

We have the feeling that the second part of the paper title might have led to the perception that the paper is aiming at addressing one or multiple of the above mentioned points and that this is misleading.

**We will therefore revise the title of the paper in a revised version of the manuscript**

We agree with the reviewer, that the present study does not provide an evaluation, rather than a comparison of different DGVM canopy RT schemes under predefined (idealized) conditions.

The paper objectives have been formulated in the submitted manuscript as [...] *Here, we aim at a show case to (i) evaluate the consistency among a number of representative state-of-the-art DGVMs, employing different definitions, assumptions and temporal and spatial scales for the canopy RT formulations, using various reference RAMI4PILPS simulations, (ii) evaluate at which conditions the used canopy RT 10 schemes (and their simplifications) lead to major errors in faPAR and/or albedo in these representative DGVMs, and (iii), importantly, assess the potential implications thereof for net irradiance and carbon productivity estimates [...]*

**We will rewrite these objectives and also the introduction to make more clear that we are dealing rather with a comparison study than with an evaluation study**

Having said this, we believe that we don't see what kind of major changes we should apply to the present manuscript, rather than clarifying the scope more

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clearly, like discussed above. The paper is very specific in its content, as it provides answers to the following questions

- What is the impact of different canopy RT schemes widely used in DGVMs on surface reflected and absorbed solar radiation fluxes
- How far are currently used canopy RT schemes following physical principles and satisfy e.g. energy conservation aspects?
- How good is the performance of these canopy RT schemes for (simplified) and idealized setups of canopy geometries under very well defined conditions?
- What are **potential** consequences of different canopy RT schemes on surface radiation and carbon fluxes?

Again, like we have also discussed in the paper we do not aim to provide any quantification of the **actual** impact of the discussed differences in canopy RT schemes on longterm or global simulations.

However, we think that it is of particular importance to raise awareness in the DGVM development community that it might be important to revise current implementations of canopy DGVM schemes that were developed 20 years ago and raise awareness about **potential implications**.

The reviewer is also arguing that the investigated RAMI test cases don't provide a realistic setup of real canopies. We agree with that statement and had discussed this point already in the manuscript when describing RAMI as well as in the discussions of the manuscript. Actually, the RAMI database contains also much more realistic reference simulations for complex canopies. The problem however is, that **none of the DGVM canopy RT schemes is capable to represent complex canopies**, nor have the currently used canopy RT schemes been designed to represent canopies in a realistic manner (e.g. 3D structure), nor do

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current DGVMs provide required structural information on canopies as prognostic variables so far.

Thus, the RAMI scenes used in the present study actually provide a perfect testbed for assessing the performance of individual canopy RT schemes under very well defined and controlled conditions. We show in the paper, that even for the simplest case (closed canopies = green jelly), some of the models show deviations (in particular for surface albedo). This setup is totally coherent with the assumptions the different canopy RT schemes are based on and should be the first order benchmark for model performance.

What this implies for global scale applications is a different story and beyond the scope of the present study like discussed already. We believe that some of the deficits in canopy RT schemes do probably not play a role in applications in DGVMs as the model parameterizations might have been tuned (e.g. tuning of leaf reflectance and absorption parameters) in a way that the models simulate a reasonable climate. We have discussed this point also in the discussion section. As a consequence, the results are probably “right for the wrong reason”.

**We will therefore think again what parts of the paper can be presented in a more concise way like suggested by the reviewer.**

## 2 Specific comments by reviewer

- *Please could you justify the chose of the selected DGVMs? Would results be still valid using other models ?*

The models chosen are all well established DGVM schemes as used in renowned Earth System Models. The choice of these DGVMs was motivated by the fact that these models represent well typical types of canopy RT schemes like used in DGVMs (parameterized, 1D schemes). To the authors knowledge these are

the most common types of canopy RT schemes used in DGVMs. While there is a rich suite of canopy RT schemes existing (in particular in the Earth Observation community), none of these is typically used in current DGVMs. Comprehensive comparisons of different canopy RT schemes as such is provided e.g. as part of RAMI. As other DGVMs typically implement similar canopy RT schemes like the ones studied here, we believe that our results are transferable also to other models.

**We will clarify this point in section 2.3 in a revised version of the manuscript.**

- *A comparison of the FAPAR (and carbon net assimilation of the leave) from the different DGVMs at different levels within the canopy could be instructive. I would suggest to add for each DGVMs a sensitivity study to the number of levels used? For example, is it possible to have an idea of the performance of JULES with 3 layers?*

The faPAR profile for the different canopy RT schemes is available. We had also performed a sensitivity study regarding the number of layers and had included it in an initial version of the manuscript. We had then however decided to **not** include this information in the paper, as it does not provide too much additional insight and would distract, as the major objective of the study is to use the canopy RT schemes "as is" and not modify them.

**We will provide results of the sensitivity study of the canopy layers as well as the faPAR profiles as part of our responses for a revised manuscript version. We will also think about if it will make sense to include these information as additional (digital) annex to the manuscript.**

- *In equation 12, I do not understand  $LAI=12$ .*

The value of  $\Lambda_{max} = 12$  is an empirical value to obtain a reasonable faPAR profile together with Eq.12. This is like it is implemented in the ORCHIDEE program

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code. One can certainly question if this value is physically useful or not, but the ORCHIDEE developers had decided for that approach. Our results show that this somewhat empirical approach seems to provide nevertheless reasonable faPAR profiles.

Please note also that there was a typo in Eq.12. It needs to be 0.15 instead of 0.5 as a coefficient in the *exp* function in the nominator. This will be corrected in a revised version of the manuscript.

**We will clarify the empirical character of  $\Lambda_{max}$  in a revised version of the manuscript**

- *Equation 13, is it the total surface albedo?*

We are not sure, what the reviewer means by *total* surface albedo. The surface albedo calculated here corresponds to the (broadband) surface albedo of a model grid cell. In that sense it is *total surface albedo*. Note however, that we assume here a homogeneous vegetation cover for all experiments, like described in the manuscript. This means that we don't need to weight the surface albedo contributions in accordance to different PFT fractions, like is typically done in DGVMs which are based on a tiling approach.

- *How multi-scattering effects between the soil and the vegetation layer are taken into account in section 2.1 ?*

Multiple scattering is typically not accounted for in the investigated RT schemes. Scattering effects are only parameterized using the single scattering albedo ( $\omega$ ), which is an oversimplification of the physical process. Multiple scattering is e.g. important in case of bright surfaces, where neglecting multiple scattering might result in a significant underestimation of surface albedo.

**We will explicitly emphasize the fact that multiple scattering effects are not considered in a revised version of the manuscript**

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- *Please could you discuss the performance of the models under diffuse conditions?*

Results for diffuse conditions are already included in the manuscript. These are labeled by *ISO*, which corresponds to complete diffuse conditions. The setup of different illumination conditions is described in section 2.4. Results are then also discussed for direct and diffuse illumination conditions.

- *In my eyes, the snow cases should be discarded from the study.*

Can the reviewer please give a motivation for this statement? We actually think that it is important to include also the snow cases in the study for different reasons: a) snow covered surfaces play a major role in the global surface radiation budget, b) effect on (absolute and relative) surface radiation fluxes can be large especially in periods with a strong change in solar illumination conditions (spring, fall), c) discrepancies in accurately simulate surface albedo of snow covered surfaces will have direct impact on surface radiation budget and temperature and can lead to significant phase shifts e.g. in the start of the vegetation season.

We therefore think that it is important to raise awareness how the simulated surface albedo of snow covered areas might depend on the canopy RT scheme chosen.

- *In Section 3.1.1, I would suggest to discuss the effect of thermal and water stress on plants. During these periods of year, canopy RT models are not so useful. We assumed no stress, either thermal or water (or nutrient for that matter): Based on observed mean  $J_{max}$  and  $V_{cmax}$  for tropical and temperate regions, net photosynthesis was calculated employing limitations by light availability above and within the canopy only at ambient CO<sub>2</sub> concentrations.*

**In the revised manuscript, we will make explicit that it was assumed that no other limitations occurred.**

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- *The authors show a deviation in net photosynthesis rate up to 10molCm-2s-1. Is it realistic? Mali et al 1998 show that Amazonian rain forest has a rate of 8molCm-2s-1.*

The deviations up to 10 $\mu$ molCm-2s-1 occurred at conditions when GPP was calculated to be at 30-35  $\mu$ molm-2s-1. Such deviations of about 25% seem realistic given the settings of our analyses: We would like to emphasize that the calculated net photosynthesis rates apply to instantaneous rates at a given zenith angle and assuming the other limitations are absent. When integrated over a day and when taking other limitations into account, it is very likely that the average rate is (much) less than this 30-35  $\mu$ molm-2s-1. Indeed, when analyzing peak uptake rates (e.g. Fig 9 of Malhi et al. 1998) rates up to 25  $\mu$ mol m-2s-1 are obtained, presumably those peak rates refer to conditions when indeed other limitations were relatively weak. Finally, although the order of magnitude of fluxes is reasonable, we would like to emphasize that our simulations by no means aimed at obtaining fully realistic flux estimates. Instead, we aimed at analyzing the sensitivity of our estimates at particular well-defined conditions.

- *In Section 3.1.2, could you explain why the radiative forcing effect increases if the LAI increases?*

The dependency of the radiative forcing of the LAI is dominated by the changes in the reflectance in the NIR domain. Figure 1 shows the difference of surface albedo ( $\Delta\alpha = \alpha_{WSA} - \alpha_{BSA}$ ) for both, the VIS and NIR domain for different solar zenith angles. An increasing LAI leads basically to an increase in  $\Delta\alpha$  between an LAI of 1 and 2.5. Largest increase is observed in the NIR domain.

Figure 2 shows the dependency of the surface albedo (VIS and NIR) of the leaf area index for the diffuse (solid line) and direct (dashed lines) illumination conditions. In the visible domain, one can observe a slight decrease of the surface albedo (darkening) with increasing LAI. However, this is compensated by an increase in the NIR. For the calculations of  $\Delta\alpha$  the relative differences between the

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diffuse and direct surface albedo are important which are increasing both for VIS and NIR (Figure 1). The stronger increase for the NIR comes basically from the different sensitivities of ISO and the direct cases like can be seen from Figure 2.

**We will briefly describe this behavior in a revised version of the manuscript.**

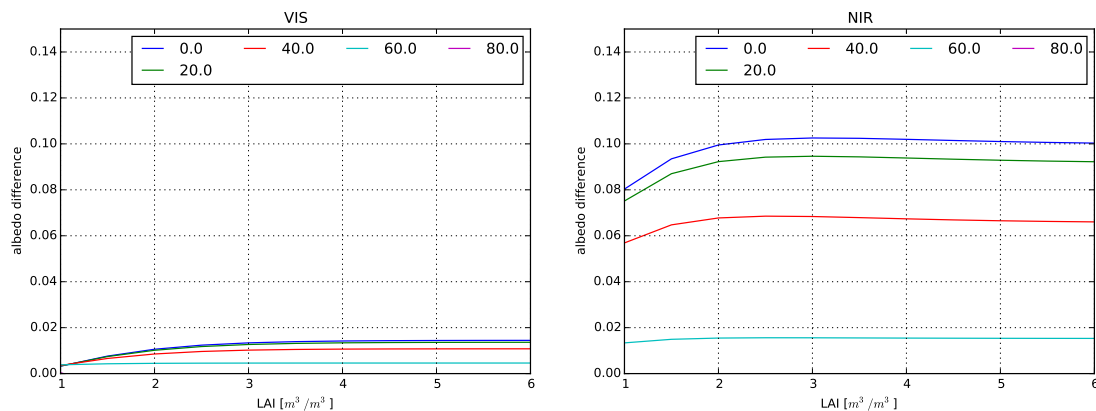
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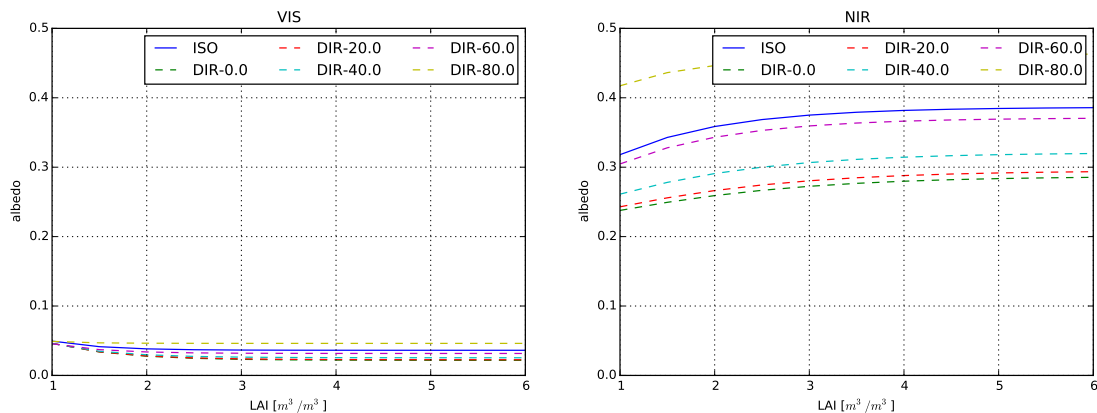
Interactive comment on Biogeosciences Discuss., 10, 16551, 2013.

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**Fig. 1.** Dependency of surface albedo difference  $\Delta \alpha$  on LAI for different sun zenith angles

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**Fig. 2.** Dependency of direct (dashed) and diffuse (solid) surface albedo for different sun zenith angles

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