Biogeosciences Discuss., 6, C2889–C2896, 2009
www.biogeosciences-discuss.net/6/C2889/2009/

Biogeosciences Discussions

Interactive comment on "An integrated model of soil-canopy spectral radiance observations, photosynthesis, fluorescence, temperature and energy balance" *by* C. van der Tol et al.

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Received and published: 8 November 2009

Response to reviewers C. van der Tol, W. Verhoef, A. Verhoef, J. Timmermans and Z. Su

The main comment of Reviewer 1 is that a greater effort should be undertaken to explain SCOPE application areas, supported by implementation examples.

We agree with this comment and believe that we could do more to improve this part of the paper. In a revised version of the paper we include a section with four application areas, including examples. We also revised section 3. In the new section 3, we present the output of SCOPE, and compare the output to actual measurements as a

first validation of the model (spectra, brightness temperature and latent heat flux).

Reviewer 2 has a number of specific comments and highlighted some occasions where our paper needs clarification and improvement.

In the remainder of this document we would like to respond to the specific comments of both reviewers one by one, and indicate how we handled them.

Specific comments of Reviewer 1:

1. 'The abstract is short and lacks important information. The presence of a photosynthesis module should be mentioned. The evaluation section is very vague and the statement "Model simulations ..." is in my opinion not sufficiently supported by the presented results (the paper would greatly benefit by a more thorough validation effort). The abstract should also focus more on highlighting the potential application areas of SCOPE. ' We agree with this point of critique. We have revised the abstract such that (1) photosynthesis and fluorescence are mentioned, (2) the results of the model evaluation are presented, and (3) potential application areas are mentioned.

2. 'The introduction describes some of the interesting application areas of the model (page 6028) but little effort is done to describe/demonstrate how to use SCOPE for these applications (there could be a section in the discussion part).' We have included a new section, '4. Applications of SCOPE' where we discuss applications of the model.

3. Page 6028 L21: "A validation of the model ....". The paper would greatly improve its relevance if a validation section were to be included. We agree. We have included evaluations against field data for the most important output variables of the model in the revised paper: spectra (Fig 7), surface temperature (Fig 8, left), and the fluxes of latent and sensible heat (Fig 8, right). Because of the length of the paper, we could not validate all aspects. We will do so in a following paper.

4. 'Effort should be made to clarify the model structure description (page 6030) and the interaction between the various modules. It may help if the associated fig. 1 was

improved (it is currently a little confusing). Fig. 1 also contains many commas (",") in odd places that need to be removed. The iteration sequence should also be displayed in Fig. 1. ' We have revised Fig. 1. We believe that the new figure shows the sequence of the calculations and the parts that are involved in the iteration more clearly. We have also modified the corresponding section in the text accordingly.

5. 'Section 2.2: The need to setup and run MODTRAN independently of SCOPE before SCOPE can be run is a definite drawback of the scheme. As described in the text, this is not a trivial task. I would have preferred that the MODTRAN (or any alternative atmospheric radiative transfer model) computations (equations 6 and 7) were an integral part of SCOPE in order to minimize the complexity of setting up SCOPE for a new user.' MODTRAN is indeed not integrated into SCOPE. One of the reasons why we did not integrate MODTRAN is that it is a rather computationally demanding model. Another reason is that we would like SCOPE to be able to run for any input spectrum, or even spectrally integrated input. The need to run MODTRAN before using SCOPE is indeed a drawback. For this reason we would like to supply the user of SCOPE with a library of MODTRAN spectra for various weather conditions and solar angles to select from. We mention this aspect in the revised paper (section 'Conclusions').

6. 'Page 6036 L3: The model needs to include a wide spectrum of soil reflectance spectra in order to be useful for the list of potential application areas listed in the introduction. ' Indeed, spectral reflectance of the soil is needed. These spectra can be found in spectral databases. In the future, we would like to provide a spectral database or a model to compute soil spectra. We mention this aspect in the revised paper (section 'Conclusions').

7. 'Page 6036 L11: I see a strength in the detailed leaf to canopy scaling technique that considers sunlit and shaded leaves at 60 leaf layers, 13 leaf inclination angles and 36 leaf azimuth angles. It would be interesting to see how that may affect the energy balance computations compared to a more simple scheme. ' This seems an interesting application as well. In the past multi-layered models have been compared to big-leaf

models (e.g. De Pury and Farquhar, 1997; Wang and Leuning, 1998), but SCOPE also includes leaf inclinations.

8. Page 6043 L9: Please mention that the photosynthesis module distinguishes between C3 and C4 crops. We revised this according to your suggestion.

9. Page 6044 L6: The linking of Prospect parameters (such as leaf chlorophyll) to parameters of the biochemical model (e.g. Vmax) is an excellent idea and a useful way to constrain the parameter space. This is something that needs to be pursued in future studies. Thank you for your suggestion. We hope that this will eventually lead to better estimates of photosynthesis.

10. Page 6050 L10: Jacquemoud et al. 2000 only reports validation/comparison of canopy reflectance spectra within the shortwave region (400 - 2500 nm). Additional references should be given for validating turbulent heat fluxes. Indeed. We rephrased the sentence as: 'Most of the individual components of the model have been validated before, such as the optical radiative transfer model (Jacquemoud et al., 2000) and the leaf physiological model (Von Caemmerer and Baker, 2007)'. We realize that this statement is still insufficient, and for this reason we have done validation of several components of the model (Figs 7 and 8).

11. Fig. 4 is difficult if not impossible to grasp. The caption should also note what each line represents (sunlit, shaded, weighted average temperature) and which line is what exactly? And indicate on the y-axis the direction of increasing/decreasing depth. We have added a legend and labeled tick marks on the axes for clarification.

12. Page 6053: I would suggest adding a discussion section here explaining potential SCOPE application areas, benefits of using SCOPE for these task and how to setup/implement SCOPE (examples would be nice). Highlight and demonstrate what kind of applications SCOPE would be particularly useful for. We have added a section to discuss the potential applications of SCOPE in much more detail.

13. Page 6054 L1-L4: A good and informative statement that should also appear in some form in the abstract. Revised according to your suggestion (see point 1).

Technical corrections: 1. Page 6028 L26 to page 6028 L3 is a word by word repeat of the abstract which is not appropriate. Indeed. We have revised the abstract (see point 1).

2. Page 6051: The parameter values chosen for the simulation runs are a little odd (e.g. why use a LAI value of 3.22 and not simply 3?) The reason for the odd parameter values is that we have taken the values from a real field experiment, in order to be sure to have realistic data as input. In the revised text, we explain this and give a reference to the literature where we collected the data from. We have excluded a full description of the database because this would be too lengthy, but we hope to use the database in the near future for further validation.

3. Table 2: N in the PROSPECT model is a leaf structure variable (leaf mesophyll structure) and is not directly related to leaf thickness. This is indeed correct. We revised it.

4. Fig. 5: Azimuth angles should be indicated on the plots. In the revised figure, we indicate azimuth angles in italic. We modified the figure caption accordingly.

5. Fig. 6: The line types are difficult to distinguish especially in the top plot. The caption contains a few typing errors ("a all direct..", "..is remains unchanged") We corrected the errors and we revised the figure. We removed one of the scenario's which was not very informative, and we plotted the differences with the reference scenario. In this way the differences between the scenarios are clearer and the lines can more easily be distinguished.

6. Page 6059 L18: "Kg=0.4" should be K=0.4 Corrected; we now use Kr for Von Kármán's constant throughout the paper, because there was a conflict in symbol use: K and ïĄń are already reserved for extinction coefficients.

7. Page 606 L17: Could list an appropriate reference here (rs as a function of soil moisture) We have added a reference here.

8. Eq. B9-B10: Wrong symbol used for von Karman's constant (should be K) Corrected (see also 6).

9. Page 6064 L20: Change Modi-fication to Modification. Corrected

Anonymous Referee #2 General comments

Detailed comments Page 6026, line 4: . . . radiation, energy and carbon balance. . . We revised the abstract, including the sentence of line 4, according to your the suggestions and those of reviewer 1.

Page 6027, line 4: . . . by inverting a radiative transfer model on satellite-derived hyperspectral reflectance data. We revised the phrase according to your suggestion

Page 6028, line 4: . . .(although, unlike CUPID, a site water balance is not calculated at present, thus requiring direct information on soil water content). We agree it is clearer in this way and revised the phrase according to your suggestion

Page 6028, line 9: . . . of water, carbon and energy fluxes. . . Revised.

Page 6029, line 19: . . . outgoing radiation spectrum Indeed, there was a mistake here! Incidence has been replaced by outgoing according to your suggestion.

Page 6031, line 1: It would appear as if not only spatial heterogeneity (resulting from both incomplete canopy cover and crown shape; e.g. Huemmrich 2001, also based on the SAIL model structure) but also leaf clumping in twigs and branches are not included in the description. Is this correct? Yes this is correct. We added a phrase explicitly stating so.

Inclusion of leaf clumping (Smolander and Stenberg 2003) could improve the representation of canopy energy transfer in coniferous canopies in future model applications. Clumping plays a role in forest canopies, but it is indeed not included in the present version of the model. In the revised paper we mention that this is a potential future development (at the end of section 3.2).

Page 6036, line 11: Why is this considered only for sunlit leaves? A vertical temperature gradient could also be expected for shaded leaves. Indeed, a vertical gradient also occurs for shaded leaves, and the model simulates this. The original sentence was confusing. We replaced ‘sunlit leaves’ by ‘all leaves’.

Page 6042, line 20: Are computations included in the ‘Leaf biochemistry’ block also included in the iterative procedure? Stomatal conductance (as derived from the Farquhar-Cowan model) has a strong effect on latent heat fluxes, and therefore on leaf energy balance. They are indeed included in the iteration. The calculation of aerodynamic resistance is included too. We have now added a sentence: ‘The aerodynamic and stomatal resistances are included in the iteration, since atmospheric stability and biochemical processes are affected by leaf temperatures.’

Page 6043, line 14: An explanation of the rationale for the simulation of leaf and canopy fluorescence should perhaps be included in the Introduction. Good suggestion. We have included it in the revised paper. It is also in a new section on potential applications of SCOPE.

Page 6044, line 14: Please give a reference for the response function of photosynthetic parameters to temperature. Several algorithms have been proposed in the literature. We give a reference to the method that we used in the revised paper.

Page 6041, line 18: Please note that slightly different algorithms should be used for hypo- and amphystomatous leaves (see Nikolov et al. 1995; Guilioni et al. 2008), as sensible and latent heat exchange take place from different surfaces in the two cases. Indeed, we implicitly assumed abaxial or hypostomateous leaves (one-sided evaporating). In the revised document, we indicate how to handle isolateral leaves, following the literature that you suggested.

Page 6049, line 1: Where are these ‘fluorescence amplification factors’ coming from? Are they based on the assumption of an ‘a priori value of chlorophyll fluorescence (as a fraction of absorbed PAR) in low light conditions’ or on the more realistic model of the interactions between fluorescence and photosynthesis recently proposed by the Authors themselves (van der Tol et al. 2008)? They are from the paper of Van der Tol et al. In the revised paper, we refer to this paper there to avoid confusion.

References De Pury, D.G.G. and Farquhar, G.D., 1997. Simple scaling of photosynthesis from leaves to canopies without the errors of big-leaf models. Plant, Cell and Environment 20, pp. 537–557

Y. -P. Wang, R. Leuning, A two-leaf model for canopy conductance, photosynthesis and partitioning of available energy I:: Model description and comparison with a multi-layered model, Agricultural and Forest Meteorology 91 (1-2), 89-111, DOI: 10.1016/S0168-1923(98)00061-6, 1998

Interactive comment on Biogeosciences Discuss., 6, 6025, 2009.