

Interactive comment on “

**Quantifying the uncertainty in estimates of
surface- atmosphere fluxes through joint
evaluation of the SEBS and SCOPE models” by
J. Timmermans et al.**

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The paper presents an evaluation of some of the parameterizations used in the model SEBS to estimate heat fluxes for regions with tall vegetation. The exercise is conducted by using observational data from a whole growth season of a maize crop. A more elab-

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orated model called SCOPE is also run with the observational data to simulate (1) the remote sensing inputs needed by SEBS, (2) some of the intermediate variables needed to estimate the heat fluxes, and (3) the heat fluxes themselves. Once the SEBS parameterizations are improved, the SEBS and SCOPE heat fluxes are closer (compared with the original SEBS fluxes), suggesting that the improved parameterizations result in a better modeling of the fluxes by SEBS.

The study is of obvious interest, as it highlights present limitations in a well known model used to derive heat fluxes, and it proposes newer parameterizations to produce more accurate fluxes. The paper is also of interest in illustrating how the methodology of coupling of SCOPE and SEBS allows to identify some of the modeling issues.

Aspects of the paper that may be improved are:

(1) The study is comparing modeled fluxes (SEBS) with modeled fluxes (SCOPE). This is a legitimate exercise, but the paper may need also some more discussion around the SCOPE fluxes. For instance, the paper comment on issues regarding the ground fluxes simulated by SCOPE, but there is not much discussion regarding the validity of the SCOPE latent and sensible fluxes. This may be of importance, as a large suit of flux related data is measured at the field, but unfortunately not the key latent and sensible heat fluxes.

(2) The paper would benefit from some re-writing to clearly identify what exact in situ measurements are used by SCOPE, which outputs from SCOPE are used as inputs to SEBS, and what outputs from SEBS are compared with SCOPE outputs and/or in situ measurements. This could either be added to the text in Section 3 or incorporated in Figure 1. In my opinion, the text requires a large effort from the reader in order to follow how the data/models have been used in the study.

(3) Once SCOPE simulations are used to identify and correct SEBS issues, the paper would be more attractive to the reader if a comparison of SEBS with in situ measured fluxes at some specific locations could also be shown. The paper suggests that the

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original SEBS parameterizations for this type of vegetation were doing very poorly (e.g., zero correlation between original SEBS and SCOPE heat fluxes at this specific site). It would be of interest to see that SEBS performs better now at some relevant sites (e.g., from the FLUXNET network) where observed fluxes are available.

Some specific suggestions and comments are given below.

Title. The title may be a bit misleading. The reader may expect a full (global) evaluation of the SEBS models, when the paper concentrates on a case study for tall vegetation.

P2864.L5. This sentence seems a bit premature if WACMOS objective is to produces global fluxes. To the reviewer knowledge those fluxes are still not produced and WACMOS-SEBS still seems to be in a refinement phase. The few reported SEBS-based global fluxes (old/original SEBS versions), for instance at Vinukollu, 2011, do not suggest that for the moment SEBS has an edge over other methods.

P2864.L6. By saying "most remote sensing algorithms" the author seems to neglect another "school" of remote-sensing based ET algorithms based on modified Penman-Monteith/Prisley-Taylor approaches. Although it is certainly true that for the moment there are no grounds to establish that one methodology is superior to the other, most of the published global estimates currently come from this alternative approach (e.g., same journal, Miralles et al., 2011, doi:10.5194/hess-15-453-2011).

P2864.L20. Eddy covariance measurements are also broadly used, it may be good to mention them.

P2870.L1. Energy balance fluxes? Does it mean the radiative downward and upward shortwave and longwave fluxes, so the net radiation is available?

P2870.L10. As mentioned above, Section 3 would benefit from some re-writing to clearly identify what exact in situ measurements are used by SCOPE, which outputs from SCOPE are used as inputs to SEBS, and what outputs from SEBS are compared with SCOPE outputs and/or in situ measurements.

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P2871.L2. The reference seems to be missing.

P2871.L13. The first line says that LAI is not part of the original formulation, but that LAI is used in SEBS-based investigations. What LAI parameterization are we trying to improve here?

P2871.L17. Where is the NDVI coming from? SCOPE simulation of vis and near-IR bands?

P2872.L6. How are A and B optimized? Using the SCOPE/field NDVI-LAI relation shown in Figure 3 (open circles)? Would not that automatically imply that the new parameterization fits the data better?

P2872.L10. Su 96 seems far from the NDVI/LAI plotted, but the original Song, 08 seems to be doing a very good job, considering that it has been driven independently from this dataset (if my previous comments about the optimized NDVI/LAI relation are true). Saying that Song 08 produced too high results does not seem right based in the figure.

P2872.L28. The text says that the values seem to depend on the vegetation type, but then only one set of values is given. Is it valid for all vegetation types?

P2873.L2. I may be missing something, but how can the incoming optical radiation be coming from SCOPE? Is not the incoming radiation always an input in these models? Do you mean the net radiation, taking into account incoming radiation from somewhere (field measurements/modeling) and some SCOPE parameterizations to estimate the outgoing?

P2873.L8. Based on Figure 6 it seems that measurements of Rn exist. Could have we also used measured Rn as we also use the measured Go? What is the rationale of using Rn only from SCOPE to investigate the Rn/Go relationship (specially once we use observed Go and not SCOPE Go)?

P2874.L10. What is the RMSE if the fitted (to the G0/Rn/LAI relation) parameterization

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is used in SEBS, instead of Kustas 93? That may be of interest to know, even if for the given reasons this parameterization is not used.

P2876.L22. How is I derived? All maize canopy has $I = 0.03$? Or is I fitted to this specific observations?

P2876.L2. Observed values? Better to say SCOPE estimations as previously?

P2876.L5. Looking at the values for LAI and hc in Figure 2, one gets the impression that for days $> \sim 230$ the Bosveld 99 parameterization will not be used, as $LAI < 1.5$. However, looking at Figure 5 one sees that even for days $> \sim 230$ Bosveld 99 is much closer to the SCOPE simulated KB-1 than the Su 01 (that would be used here, if this sentence is correct). I think this requires some further explanations.

P2876.L14. What is the time of the day for the instantaneous fluxes? Corresponding to SCOPE simulated AATSR measurements? It is indicated later on in Figure 7, it will be useful to have it here.

P2877.L26. SCOPE assumes surface/canopy energy balance, right? At some days we can see that SCOPE Go can be 100 W/m² larger than the measured values, while SCOPE and observed Rn agree relatively well. Would not that imply that $H+LE[SCOPE] \sim= Rn-Go[SCOPE]$ is an underestimation of the true $H+LE[OBSERVED]=Rn-Go[OBSERVED]$?

P2877.L10. It may be of interest to add the original SEBS fluxes in Figure 6 (only one extra line) to have a feeling about the original fluxes. As the original SEBS formulations have been applied by other colleagues, that extra line may be quite informative.

P2877.L17. I may be missing something here. Based on the energy balance that SEBS assumes, it is clear that the observed underestimation of instantaneous H produces an overestimation of instantaneous LE. But if the instantaneous fluxes are discussed here, I do not fully understand how the EF may be playing a role here.

P2878.L15. What happens for 220-230 at $\sim 16h$? EF > 1 means H < 0 ?

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P2878. L22. It may also be of interest to see the daily fluxes produced by SCOPE (integration of hourly values) and SEBS (EF constant assumption), to complement the EF figure.

P2881.L2. If the EF is estimated by SEBS by using a sensor with a different overpass time, would the EF change? I guess so, as it seems that EF is not constant with time for this type of vegetation. If that was the case, would that EF still be representative of the daily average? Perhaps this sentence need to be better discussed, here or in the previous section, to signal that the assumption of wrong instantaneous EF resulting in right daily averaged EF may only apply to this specific sensor/biome combination.

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