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Discussions

Interactive comment on "A model inter-comparison study to examine limiting factors in modelling Australian tropical savannas" *by* R. Whitley et al.

Anonymous Referee #2

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This paper is timely and pertinent, as a number of groups are recognizing the important role that savannas play in the global carbon cycle and surface-atmosphere energy exchange, and models have developed to the stage where we can now tackle some of the issues that have made model characterization of these ecosystems elusive.

The paper spends a lot of capital on benchmarking and the 3 empirical models. Figure 5, especially, is a sort of 'beauty contest' where the models are graded against each other and the empirical models. Figures 2-4 also focus on 'big picture' evaluation of how the models behave when confronted with eddy covariance (EC) data. Are our models of savannas mature enough that we can treat them as black boxes and evaluate them

based solely on the final (surface-to-atmosphere) fluxes? As a modeler who has been thinking about savannas and our ability to model them for a number of years now, I believe the honest assessment is that our ability to realistically simulate biogeophysical processes in savannas is in its infancy. In that case, I would rather see a detailed evaluation of within- and between-model behavior, with comment on those processes that the authors think are realistic, and those they think are faulty. Comparing model output to observed fluxes of ET and carbon (and sensible heat flux! Why no sensible heat flux in the analysis?) is informative, sure, but I don't think informed readers will be surprised by the result that none of the models demonstrates a definitive superior ability to simulate ecosystem behavior across the NATT. What those of us who are working on this (my model was not a participant in this study) are really interested in are the details.

Such as:

1. Functional composition: How is tree/grass fraction determined from MODIS (for those models that use it), or from the DGVM? What are those fractions? MAESPA and SPA have time-varying tree/grass fraction (following Donohue); does that mean the other models do not? Do those other models follow the work of Hansen, Bucini, etc? What is their partitioning? Are they similar to ground-based observations? Section 4.3 says "(p)rescribing LAI can be problematic depending on the time-scale and how it is partitioned between tree and grass layers." This is critical to our ability to simulate savannas. How are the models treating it, and what is our evaluation of this treatment?

2. Grass Phenology: We know that C4 grasses follow a seasonal cycle of wet-season greenup followed by senescence as the soil dries. Are the models consistent in their representation of mean seasonal cycles, in terms of timing/maximum LAI? Why or why not? Which model has the most realistic grass phenology based on observational data?

3. Tree phenology: Many (most?) of the readers will not be immediately familiar with

the trees from the latin. Are the trees at all sites evergreen? Deciduous? Semi-deciduous? Mixed? How are they mixed? How is this heterogeneity represented, if at all, in the models?

4. I appreciate seeing the partition between vegetation and soil ET, but I would really like to see a further partition of vegetation ET (and carbon flux) into tree and grass components. Are these components consistent with the fractional composition and phenology cycles? Why or why not? Are trees 'activated' quickly (like the grasses) following seasonal rains, or is there a lag time as water infiltrates into the soil? In section 4.2 the authors say "(d)uring the wet season as much as 75% of total LE arises from understory herbaceous transpiration and soil evaporation." What do the models say?

5. Two of the main take-home messages are that 'models must treat grass as co-dominant' and 'models need a dynamic representation of LAI…', yet current model treatment of these two processes are not explored in detail. How are the models, in their current form, failing?

The paper is competently written, and I have no problems with the analysis. However, I find myself dissatisfied after reading it; the analyses I really wanted to read about were not performed.

The authors are under no obligation to write the paper that I want them to. Barring grammatical or analytical errors/problems (of which I don't find many), I can't reasonably demand rejection or major re-writes. If the authors can defend their reasons for declining to address the issues I outline above, I will concede that the paper is suitable for publication with minor revisions. But I will maintain that in that case an opportunity has been missed, for a detailed evaluation of contemporary process-level model behavior would be a much-cited paper as model groups move forward.

Specific comments: âĂć Something the authors don't mention, which is critical to studies of savanna, are the limitations of the 'traditional' eddy covariance (EC) observa-

tional suite in these environments. There are a lot of moving parts in these ecosystems, and a single observation of net flux does not provide the detailed partitioning that we really need to understand with respect to relative contribution of trees, grass, and soil. I'd like to see the authors comment on the ability of above-canopy EC measurements to constrain simulations of spatiotemporally heterogeneous system such as savannas? Yes, there are a number of papers that describe site-level analyses; has a consensus or climatological pattern emerged? What does this mean for our ability to simulate these systems and evaluate our models?

âĂć Are the u-star/respiration models used to partition observed net carbon flux into respiration and GPP robust in savannas?

âĂć Was 5 years enough time to spinup the soil? In my experience it can take 10 years or more. Was spinup tested?

âĂć As of January 2016, Moore et al (Contribution of trees and grasses to an Australian tropical savanna) does not appear to have been published. Can it still be cited?

âĂć Is one study (Hutley 2000) enough to define tree/grass ET contributions across all savannas? Or is that description valid only at Howard Springs?

âĂć Page 19017, line 12; Insert 'of' between 'adjustments stomatal'

âĂć Page 19018, line 8: '(lacking observed grass LE)' is redundant

âĂć Page 19021, line 16: 'are therefore do not mechanistic' needs a rewrite

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Interactive comment on Biogeosciences Discuss., 12, 18999, 2015.