Interactive comment on “Challenges and opportunities in modelling savanna ecosystems” by Rhys Whitley et al.

Anonymous Referee #3

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This paper presents a helpful and informative review of some of the major challenges in quantifying and predicting structural and functional dynamics of savanna ecosystems with numerical models. The paper focuses on terrestrial biosphere models that mainly aim to predict water, energy, and carbon fluxes and balances as they interact with the atmosphere. This is a worthwhile focus but the title should probably be modified to reflect this specific focus. The manuscript has a lot of valuable content, and offers constructive advice regarding ways to improve TBM performance for savanna ecosystems. However the paper is less comprehensive than, I’d argue, it should be given its aims. Also the paper omits some important theory and themes in savanna ecology, and misrepresents some of the broad geographic context of global savannas. This review focuses on those elements and recommends revisions in those directions.

0) Regarding Root Water Uptake: This is a worthwhile focus for improvements of TBMs

1) A host of other processes of importance and interest in savannas are missed. For example, pulse response processes, stand-scale vegetation composition, plant-scale competitive interactions, stand-scale vegetation structure, landscape patterning of vegetation, nutrient cycling and interactions with herbivores, and more are all given lit-
tle if any attention. Arguably many of those processes are important for representing savanna-atmosphere interactions, and for assessing savanna responses to global change factors. Given that this paper is intended to be a review of key processes that need to be considered to accurately model savanna ecological responses to global change factors, I would encourage additional discussion of these missed processes and their implications and importance for the stated aims.

2) Savanna ecologists would be underwhelmed by the three dynamic processes that are highlighted: phenology, root water uptake, and fire, given that these have long been the focus of their work going back many decades (e.g. Walter 1973). For example, the seminal work by Brian Walker (1981) is surprisingly absent from the present review even though this was foundational work identifying the importance of root zone separation and differential uptake zones for grass/herbaceous and woody PFTs in the savanna matrix. This was nicely tested in the Scholes and Walker (1993) book which is also missed. It is surprising that competitive interactions and differential resource access are not noted here, nor differential response of PFTs and species to single and multifactor drivers of CO2, drought, warming, and increasing VPD. While I agree that the three features highlighted in the present paper are essential and yet poorly represented (if at all) in TBMs, such models will still not be up to the task of predicting responses to global changes without representation of a host of other factors. Walter H (1973) Vegetation of the earth in relation to climate and the eco-physiological conditions. New York: Springer. Scholes, R. J., and B. H. Walker (1993), An African Savanna: Synthesis of the Nylsvley Study, Cambridge Univ. Press, New York. Walker, B. H., D. Ludwig, C. S. Holling, and R. M. Peterman (1981), Stability of semi-arid savanna grazing systems, J. Ecol., 69, 473–498.

3) Grazing and browsing are of central importance in many of the world’s savannas, strongly influencing vegetation cover, loss of productivity and biomass, species composition, and affecting site fertility but this driver is hardly mentioned, receiving just one or two sentences. A bit more on this subject would seem warranted for such a review.

5) Discussion of the global context and diversity of savanna attributes and strategies is lacking and in some ways misleading. Section 2.1, particularly Line 169+: The language here misrepresents the growth and longevity strategies of woody plants in Africa. Many of the woody species in at least southern Africa do indeed have deep roots but groundwater is deep (probably deeper than in much of Australia) so there is less potential to rely on near surface (<10 m) water sources. The Archibald and Scholes '97 paper does not mention roots once, and says nothing about strategies of water access. The Higgins '11 paper also offers little on root water uptake. Both of those papers do indeed discuss and quantitatively document phenological dynamics, but neither indicates that the full woody component of southern African savannas is deciduous (indeed Acacia sp. often retain leaves consistently through the dry season). However both represent only southern African ecosystems at best (really, Kruger Park). Yet this statement is as grandiose as generalizing from these studies to all African and South American savannas! That’s stretching it a bit, no? A much broader literature must be invoked if the authors truly want to discuss geographic patterns of root water uptake, and diversity in savanna traits and properties. Furthermore, this must consider not just phenology
but also water availability in the unsaturated and saturated zones, and not confuse mesic and arid savanna types. The present interpretation seems to conflate shallow groundwater availability or its absence with a difference in plant strategy. However, woody species of savannas around the world “favour a long-term strategy of conservative growth that is insured against an unpredictable climate”, not just those in Australia. To include more on the global biogeography of savannas relevant to a modeling context I’d recommend some additional reading (and citation of) works in: Hill, Michael J. and Hanan, Niall P. eds (2011). Ecosystem Function in Savannas: Measurement and Modeling at Landscape to Global Scales. (CRC Press, Boca Raton, Florida) 559 pp.

vanna. Oecologia,

7) Section 3.1: Possibly also mention potential for additional measurements to inform root water uptake dynamics (maybe around L590): - experimental use of isotopes to trace root water uptake dynamics (see work of Todd Dawson’s lab for example). - standard field-measured sapflow and leaf gas exchange are surprisingly not mentioned but can be particularly useful when coupled with detailed soil moisture profile measurements, where changes over time directly indicate the effects of water uptake. - weighing lysimeter studies, while very intensive, have also been used to detect whole plant uptake. - groundwater wells would also be enormously helpful and are so often missed in ecological and even hydrological studies in savannas (and other ecosystems), yet are critical for characterizing the availability and dynamics of deep water sources. - groundwater maps, where available, are low hanging fruit for incorporation into spatial applications of TBM. - another key thing that is missing is detailed mapping of C3 and C4 vegetation types (grasses/herbaceous), and their separate phenologies. - remotely sensed surface temperature is another valuable constraint on ecosystem water status (I think Damian Bonal was working on this and published on it).

8) Conclusions go uncomfortably beyond what is supported in this paper and stray from the paper’s clear focus on how to improve TBM performance for savannas. For example: “Projected higher temperatures and rainfall variability, potentially promoting more frequent fires, could favour C4 grasses in mesic savanna, while drier conditions are expected to increase tree mortality in semi-arid savanna. Conversely, increases to atmospheric CO2 are expected to favour C3 trees, reflecting woody encroachment that is already observed in many savannas globally (Donohue et al., 2009). Climate change therefore has the potential to alter the carbon balance, which may have major feedbacks on global climate and biogeochemical cycling.”

9) Again, it is recommended that the authors expand the scope of highlights to also emphasize ecosystem structural and compositional dynamics that are of central importance to TBM processes: particularly differential resource acquisition (primarily water)
and competitive interactions. E.g. around L694... model and data efforts should also
target those attributes of savannas. Perhaps the authors roll all of that into “phenology”
but I’d argue that this is a mistake, where phenology is only one component of vegeta-
tion dynamics. The underlying competitive interactions, mortality and growth dynamics,
and how these shift in response to a suite of climate, atmospheric compositional, soil
fertility, land use and other global change factors could receive more attention in this
review.

Some Details:

Why is root-water hyphenated? Do you mean ground-water or soil-water? Probably
just drop the hyphen throughout.

Line 69+: not just “environmental conditions” but also biophysical and ecological con-
ditions... that is, the ecosystem properties are themselves changing and this must be
represented.

Line 96: “confronting task” reword, unclear

L100: “underperformed for savanna ecosystems” is too vague... what, specifically,
lacks accuracy? “under” relative to what, other PFT or biome types, compared to data?

L105: “physical [and biological]”... most of these are not physical parameters.