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Interactive comment on "Forests, savannas and grasslands: bridging the knowledge gap between ecology and Dynamic Global Vegetation Models" *by* M. Baudena et al.

Anonymous Referee #2

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This is an interesting study that tackles the ecological reasons behind forest-savanna transitions from the point of view of modeling. The authors opportunely use observational data to evaluate model outputs and enrich the paper discussion. The general aim of the paper of identifying gaps in the assumptions or process representation related to the forest-savanna transition issue is laudable. Nevertheless I have serious doubts the authors use the right reasoning and technics to reach their conclusions. Four major points called out my attention and I suggest the authors put a bit of thinking over them for a revised version of the paper:

1: On page 9495 authors conclude that water limitation upon tree growth and grass-fire feedbacks are the two most important processes to be considered in model that intend

to capture well the forest-savanna transition dynamics. But notice that on page 9474, in the introduction section, authors state the same. It gives the impression authors already had an opinion on what processes were important for models, regardless of the study they conducted.

2: In page 9483 the simulation protocol is described. The fact that the 3 employed DGVMs are run with different input data compromises the intercomparison of their results, since it increases the degrees of freedom in the modeling exercise. We cannot assure for example that model results are a consequence of elevated CO2 or different precipitation patterns. Authors should have a strong argument – rather than that these were the model runs available at hand – to justify such a experimental design in light of the proposed paper objectives.

3: All models seem to be overestimating tree cover in low to moderate precipitation levels (using the regression lines as a basis for comparison). Let's get the 50% tree cover as a benchmark: observed data places 50% tree cover in roughly 700-800mm of annual rainfall. The 3 considered models reach that benchmark by 200-400 mm. Please comment.

4: It is interesting to see the evaluation on how elevated CO2 can affect the dynamics of forest-savanna transition zones. But remember nutrient dynamics have shown to play a key role in elevated CO2 responses of forests (e.g. Norby et al. 2010). Many tropical forests and savannas are nutrient limited (especially P-limited). However it seems like the role of nutrient dynamics is poorly explored here. I understand that none of the employed models have nutrient cycling (even though I was curious because JSBACH was one of the first DGVMs to implement N and P cycle, but the authors probably used an earlier model version), but the topic could be further explored. Otherwise the scientific utility of the elevated-CO2 exercise (which in fact is not properly explained in the method section) is reduced.

Minor points:

p. 9475, l 3: “enhances open savanna formation presence”

p. 9475, l 26: “leading to woody savanna expansions”

p. 9476, line 14: even though DGVM have a considerably limited ability to deal with the enormous plant trait diversity found in tropical regions. Generally tropical forest is represented by only onw or two PFTs.

p. 9479, l 12: “Max Planck Institute”

p. 9486, l1-8: That is in fact a good argument for investigating these other factors with the use of models. It could have been better explored in the article. . .

p. 9486, l25: This sentence is not in accordance with the model-data comparison the authors do subsequently.

p. 9489, l25-27: But isn’t this rainfall range selection a little artificial? The study is not convincing in that aDGVM really shows a bimodal tree cover distribution such as evidenced by observational data. Maybe a statistical tool would help here to prove whether the aDGVM modeled distribution – or part of it – is bimodal or not.

p. 9492, l10: And so do many tropical forests (to be associated with nutrient poor soils)

p. 9493: “Indeed, the three models predict reasonably well the current tree cover along the mean annual rainfall gradient in Africa, as derived from ground and satellite observations.” Are the authors sure of this statement? A numerical/qualitative comparison would do very good here.

p. 9494: I have the impression the text discussed here would fit better in the discussion section rather than in this “concluding remarks” section.

Interactive comment on Biogeosciences Discuss., 11, 9471, 2014.