Reviewer comment: 9. p9489, l19W20: ". . . at high precipitation, the fuel does not dry out sufficiently to promote fire spread. This may be caused partly be the average fire probability used in this version of SPITFIRE. . .". I do not see the connection of the former sentence (related to drying out to promote fire spread) with the average fire probability, which is related to the representation of fire triggering. Better explanation of what you meant. Besides, has LPJWGUESS been benchmarked for Africa? The authors mentioned DGVMs are not parameterized for tropical ecosystems, but could not find any commentary on a specific usage of this model for Africa.

YOUR REPLY: To connect the two parts better, we rephrased the sentence in the following way:

"This may be partly due to the fact that this version of SPITFIRE uses a probability of fire spread which depends on average fuel conditions, and thus it is not fully coupled to the drought periods.

It is necessary to better explain the text here. This sentence is not understandable and the reader cannot follow the argumentation. Please refer to the specific SPITFIRE equation as published in Thonicke et al. 2010 and if applicable how this has been modified for your version. Then you need to explain how this relates to the drying of dead fuel.

However, this assumption is necessary because the temporal extent of remotely sensed data for burned area (now ca. 10 years) is well below the temporal extent of the available climate data (50 years)."

I do not understand the logic of this argumentation. Ten years of burnt area detection are already a considerable data set for model evaluation which can be used to evaluate the fire module. Please clarify and improve the manuscript text accordingly.

About benchmarking, LPJCGUESS simulations of vegetation distribution have been compared to potential natural vegetation at global scale (including Africa) in Sitch et al. (2003). So, far only aDGVM has been applied specifically to Africa.

I disagree with this argumentation. The Sitch et al. 2003 is not a suitable reference as this is the LPJ original paper, while you are using LPJ-GUESS (Smith et al. 2001 and follow-up publications). Why don t you cite, e.g. Lehsten et al. 2009 or Hickler et al. 2005, 2009? This needs to be improved in the text!

Reviewer comment: 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.

YOUR REPLY:

We agree with the reviewer that nutrient cycles are fundamental for many tropical savannas and forests and it is very important for CO2-enriched experiment. However, within the ecological

literature, nutrient limitations are currently not considered as one of the main reason behind savanna existence. Moreover, for the conceptual CO2-experiment, including a nutrient cycling would not have dramatic impacts (as nutrient limitations would mainly reduce, or eventually stop, the CO2-fertilization effect). Because we only discuss the direction of change within our conceptual experiment, this would not change our interpretation. About the description of the CO2 experiment, please see also above the reply to point #2 of this reviewer. The JSBACH version used was indeed the CMIP5 version of the model without N and P cycles.

This is an important point which must be added to the manuscript text. So please add the role of nutrient limitation to your thought-experiment in the text and check recent literature on FACE experiments, e.g. Zaehle et al. 2014. Also clarify in the method description that the JSBACH version that you used was not the one with N and P cycles. Please add how this model version could have helped in your analysis.