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Interactive comment

Interactive comment on "Are fire mediated feedbacks burning out of control?" *by* J. Lloyd and E. M. Veenendaal

Anonymous Referee #2

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General Comments

In this paper Lloyd and Veenendaal address several issues raised by Staal and Flores (2015) in response to a recent publication of Veenendaal et al. (2015), where Veenendaal and co-workers have argued that their field data were inconsistent with the hypothesis that tropical forest and savanna can be considered alternative stable states through a feedback between fire and low tree cover. In addition to refuting the arguments raised by Stall and Flores, Lloyd and Veeenendaal stated more clearly their argument that under uniform climate conditions, savanna-forest transitions would be more the result of differences in soil physical and chemical properties than the result of fire-vegetation feedbacks. Although they do not deny the importance of fire, they claim that fire-effected feedbacks simply serve to reinforce and sharpen the boundary between those two contrasting vegetation types, but they deny its role as determinant

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factor governing the balance between humid tropical savanna and forest.

Interestingly, although they lay their argument considering both soil physical and chemical properties, they have a clear "bias" towards soil chemical properties. In this aspect both the alternative steady state (ASS) supporters and the edaphic–climate (EC) supporters agree that soil nutrient availability is a key-factor. In fact, there is such a tenuous difference in the arguments of both groups, that sometimes one thinks that they are both saying the same things, as I show in the following example. Hoffmann et al 2012a (Ecology Letters, (2012) 15: 759–768) claimed that soil characteristics per se cannot explain the persistence of savanna on well-drained clay soils that are widespread throughout the seasonal tropics. And quoting Hoffmann et al. (2012b; Austral Ecology (2012) 37, 634–643): "As fire can maintain open savanna conditions where climate and soils (here meaning enough soil nutrients) are otherwise able to support forest, fire-vegetation feedbacks permit the existence of alternate stable states". Lloyd and Veeenendaal disregard Hoffmann et al (2012a) statement as supportive for ASS by saying it was a review that considered this question only in passing. However, (and I quote Lloyd and Veeenendaal themselves) they restate that "under uniform climatic conditions all that is required for there to be a mosaic is for there to be some differences in soil physical and/or chemical characteristics which, especially in transition zones, means that the overstorey canopy does not have sufficient resources (meaning soil nutrients) to close, then allowing grasses to establish and fires to occur and with subsequent reductions in canopy cover and biomass ensuing (and the vegetation having fire-adapted traits). But where edaphic conditions are sufficient for the relatively high canopy cover to effectively prevent the establishment of grasses, then (with more shrubs in the understorey) a very different vegetation type ensues".

Are soil nutrients such a definitive determinant of vegetation structure in the Tropics? A clear example of the limitation of the soil nutrients-vegetation association can be seen in Figure 1 of Hoffmann et al. 2009 (Ecology, 90(5), 2009, pp. 1326–1337). As pointed by the authors, "forest soils had significantly greater pH, C, N, P, Ca, Mg, Mn, K, and

Zn and less available Fe and Cu than soils in adjacent savanna. However, there was considerable spatial variability in this overall trend, with two transects exhibiting little tendency for increased nutrient availability in the forest". I have no doubt that both EC and ASS supporters probably agree that local edaphic factors, such as shallow, sandy or seasonally flooded soils, might prevent some sites from ever becoming forest during fire suppression. However, these are not exceptions but the rule. Soil fertility and effective soil depth (as determined by the presence of concretions in the soil profile or nearness of seasonal or permanent water table to the soil surface) are both key determinants to govern changes in vegetation in tropical landscapes.

Local variations in vegetation physiognomy and floristic composition are more determined by soil properties and soil water regimes, particularly for savannas such as the ones of Central Brazil, where the presence of small watersheds throughout the biome provides the framework for an infinite variety of soil and vegetation mosaics. There is no single physiognomic type which covers the whole of a watershed in the cerrado landscape. Associated with variations in relief, ground water table level, drainage patterns and soils, the vegetation also changes. This is true not only for the tropical savannas but for the wet tropics as well. The mosaic of vegetation types in the Amazon region is another clear example of these complex interaction. Although in this paper the authors clearly stress the importance of soil properties in general, this was not true in their original paper (Veenendaal et al. 2015), where they were much more dogmatic and relied mostly on plant available soil water and soil cation status to explain observed changes in vegetation structure.

We should deal with the complexity of savanna and forest landscapes in the sampling designs and perhaps a watershed or landscape approach would be a more effective framework to understand past and predict future scenarios for tropical savanna and forests resulting from pressures imposed by changes in land use, fire regime and climate. Variations in relief, ground water table level, drainage patterns and soils have to be taken together in order to better understand patterns of distribution of savanna and forests in tropical regions. In addition to vegetation structure, we should also incorporate species composition or at least functional groups to develop more realistic scenarios of changes in vegetation in response to anthropogenic global warming. Global and regional modeling efforts would also be more effective by taking into consideration the current fragmentation of tropical forest and savanna biomes.

In short, instead of just spending all this effort in deconstructing Stall and Flores arguments, Lloyd and Veeenendaal should move beyond and provide new insights to make this manuscript a novel contribution to our understanding of savanna-forest dynamics. Rebuttal, irony, deconstruction, self-confidence are not enough. I think readers would expect more from their "senescing" minds.

Specific comments

1. Perhaps I missed, but I could not find anywhere the range in rainfall. Are we discussing humid (wet; mean annual precipitation > 800 mm) savannas only?

2. The modeling approach adopted here by Lloyd and Veenendaal is still under review, and as such, not accessible (makes it hard to ascertain model assumptions and restrictions). In this way, it is still questionable at this stage and may change pending reviewer comments. However, as it was used more to illustrate the point that models cannot be used as "evidence" and that simulation results are strongly dependent on the model assumptions, it is perhaps acceptable and worthwhile to have it here.

3. It seems to me that their modeling approach requires uniform and steady state climatic conditions. Perhaps authors should state this more clearly.

4. Lloyd and Veenendaal argue that all references cited as providing "evidence" for alternative stable states by Staal and Flores lack foundation. This is perhaps true for the ones cited by by Staal and Flores, but there are several fine examples in the literature that this is happening. One of the most interesting that I know of it is the work of Pinheiro and Durigan (2009; Revista Brasil. Bot32, pp. 441-454) where they used

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aerial photographs to show a change of open savanna physiognomies to a forest physiognomy with a continuous tree stratum in protected areas after a few decades. I raised this example to show the limitations of developing the whole argument based only on vegetation structure (plant cover). Whether this particular example would support one view or the other will depend on definition of what is a “forest” and what is a ”savanna”. Some would claim that in this particular case, the vegetation structure is changing towards a savanna woodland and others, to a dry or xeric forest. Or stating in other words, species composition does matter, not only cover or structure. Sadly, both the ASS and EC supporters have based their whole argument (at least in this exchange in Biogeosciences) on vegetation structure per se. Species composition or perhaps better differences in functional diversification between the two types of vegetation is not much considered, as well as the many other vegetation types that are all lumped together as either “savanna” or “forest”.

5. The potential expansion of gallery forests into the savanna has also been demonstrated (for instance Silva et al. 2008; Global Change Biology 14, 2108–2118), using 14C analysis and Carbon isotope ratios of of soil organic matter and later confirmed by vegetation surveys of seedlings, juveniles and adult trees across the studied savanna-forest boundaries (Geiger et al. 2011; Journal of Vegetation Science 22 (2011) 312–321), highlighting the importance of forest-savanna interfaces. It is therefore reasonable to expect that forest tree establishment along borders allows the long-term persistence of forest patches, and promotes fast forest expansion under favorable climatic conditions.

6. On the other hand, it is true that this is not happening everywhere or for all savanna-forest interfaces. Reality is much more complex, as shown by Silva et al. (2010; Plant Soil 333:431–442) and using the same techniques, past vegetation changes or stability in Amazonian Savannas (Sanaiotti et al. 2002; Biotropica 34: 2-16. Similar patterns were also reported for Cameroon by Desjardins et al. (2013; Comptes Rendus Geoscience 345: 266–271. Pollen and charcoal records also point towards a complex

picture of savanna and forest temporal dynamics. In this regards, see for instance Ledru (2002; Late Quaternary history and evolution of the Cerrados as revealed by palynological records. In Oliveira and Marquis (eds), The Cerrados of Brazil: ecology and natural history of a neotropical savanna).

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