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

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We thank Anonymous Referee 1 (R1) for his/her compliment to us on the information we brought forward to question the response by Staal and Flores (2015) a.k.a. SAF. (S)he does, however, take issue in particular with the style of writing of the paper and again in particular with the polemical writing style.

While realising that our direct approach may have upset some colleagues (for which EMV duly apologises) it has also led to discussions in many departments among scientists that are interested in the same issues at hand. We have had lively debates inside and outside conference rooms and email exchanges between ourselves and colleagues that we have criticised and new grants are being/have been applied for to work on formulating and testing hypotheses that emanate from (paraphrasing R2)"ASS versus EC theory". In that sense we feel that we have reached our objective.

Responding to specific comments: We note R1's backhand criticism of our own studies that use correlation to put forward alternative explanations for tropical vegetation structure and its transitions. But that does not really hold up: For example, as regards Lloyd et al (2015), the very wording of the title of the paper reads: "Edaphic, structural and physiological contrasts across Amazon Basin forest–savanna ecotones SUGGEST a role for potassium as a key modulator". Here the "suggest" immediately alludes to the more nuanced interpretation of our dataset than has been the case for those putting forward things like bimodal distributions of remotely sensed canopy cover as unequivocal support for ASS (with that, of course, being one main focus of our critique). Also in Torello Raventos et al. (2013) we are almost entirely concerned with the use of numerical techniques to classify tropical vegetation types and so it is hard to see what the exact criticism of R1 is here. In Veenendaal et al. (2015) we use correlation and regression as descriptive statistics to show that there are trends in tropical vegetation structure which are associated with climatic and edaphic factors to, in the Discussion of these papers, simply raise the possibility that these provide alternative interpretations to ASS theory. So, this being in a public forum, for R1 to anonymously make the *non sequitur* assertion that we are guilty of similar "sins" to those we are criticising comes across as pretty much "on the nose". Likewise, our (unpublished model) was merely introduced to demonstrate that there are reasonable explanations for bi-modal distributions in vegetation cover that do not necessarily involve ASS: something the ASS community seem — up until now at least – to have not even considered as a remote possibility.

And of course, that we see a need for rigorous testing of our own hypothesis. But in responding to R1 the critical point here is that we have never presented our own ideas as anything other than that (i.e. theories to be rigorously tested), but that this to date has not been the case for the widely accepted hypotheses that it is ASS mediated by fire mediated feedbacks that dominate the observed variations in tropical vegetation structure worldwide. Hence, being prompted by SAF, our finding the need to write our critique in the first place.

Anonymous Referee 2 (R2) provides a further detailed contribution to the debate. This review almost stands alone as an additional contribution to the discussion at hand and we wholly agree with many of his/her ideas as to the importance of e.g. soil water regime, local re-distribution of runoff water, ground water resources, flooding, effective soil depth etc. These are indeed essential when understanding pattern and process in the field, and with the response of tropical vegetation cover to these edaphic drivers almost certainly also climate specific. We also note here that for the sake of clarity our commentary did not attempt a full review of all factors determining Forest–Savanna boundary patterns. But rather focussed on SAF and some associated papers. In doing so we deliberately also chose a somewhat“trenchant” approach (R1) so as to get the debate more clearly focussed and to provoke discussions on how fire soil, edaphic factors, and climate (including carbon dioxide increases) may interact. The review of R2 provides a valuable further contribution to that discussion. In terms of the specifics of what we do and do not think, we do, however, point out that the suggestion of R2 that we consider chemical effects to be more important than physical effects when considering soil influences on tropical vegetation structure and function is not correct (see for example Discussion in Lloyd et al., 2015).

R2 also questions the difference between ASS and EC proponents - and (s)he certainly has a point here if one considers that when soil conditions improve (soil fertility, soil physical conditions) – then fire impacts may be expected to be reduced under closing canopy. However, a main difference in opinion is evident. For ASS the whole concept hinges on contrasting fire tolerant trees in open pyrogenic and fire sensitive trees in closed non–pyrogenic environments, with a threshold for fire activity at a certain cover (say > 0.60). The proposed mechanism also needs a relatively short return time of fire events being the norm and all this being applicable in the climate range above 1.1 m of rainfall where most Forest–Savanna transitions exist. It also requires stable states with transitional vegetation forms being intrinsically unstable. ASS theory thus relies on no long term transitional formation type vegetation being possible: something very much at odds with our own observations. For example, there are in the humid tropical transition zones globally a wide differentiation in vegetation structure, as can indeed already be deducted from floristic literature (e.g. for cerrado/cerradão the work by Ratter and others; for West African forest and drier areas work by Swaine, Hawthorne, and others) and as implied in general for forest–savanna transitions in Torello Raventos et al (2013) with such "transitional dry forests and woodlands" in a relatively stable state under conditions determined by soil, climate, and with their own endogenous fire regimes.

More on that in later publications of course, as we fully intend in the future to develop these idea further: this most likely being in conjunction with future work on the interactions between climatic and edaphic factors in forest/savanna transitions world wide (and with the aid of a spade!).

But our commentary on SAF was never intended to be a comprehensive review and in its final version we believe it should remain sufficiently focussed as such.

Specific comments to R2.

1. Indeed we focus mostly on humid transitions (above 1 m annual rainfall) which include most forest transitions. Our model approach does indicate that (in a relative sense) fire may play a more important role on woody cover in drier regions as has also been reported by others.
2. We are hopeful that Veenendaal et al (2016) will be accepted soon (it is currently being revised having spent many months out in review).

3. We will do so although we note that unless severe human management is included (removal of woody canopy cover) not even severe droughts are likely sufficient to cause canopy damage of a magnitude sufficient to cause a permanent transition to a more open canopy type.
4. -7. We do not agree that fire exclusion studies in themselves are necessarily proof for fire as an agent of alternate stable states. This is because fire is an inherent component of open tropical vegetation with a dry season, but that in forest/savanna transitions its effect (in the absence of human activity) need not necessarily ever be large enough to cause a tipping point. We agree with other statements on effects of climate fluctuations and would also mention CO_2 increase here.

Finally we note (this being in relation to both R1 and R2) that neither referee actually raises any serious scientific concerns as to the validity of any of our arguments. Thus our questioning of some generally held ASS “truths” seems to have served a useful purpose. We also know that whilst our writing style has obviously being perceived by some as not showing a sufficient lack of humility, others have viewed our honesty of approach as more like refreshing (indeed to quote the 1930’s US journalist/author Isaac Goldberg with a few modifications: “Diplomacy mostly serves to say the nastiest things, but in the nicest way”). We hope that in the end our non–diplomatic critique will serve to not only stimulate new debates as to the nature of forest/savanna transition zones, but also to help widen appreciation as to potential pitfalls when it comes to hypothesis testing and the interpretation of available “evidence”.