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# *Interactive comment on* "Climate change will cause non-analogue vegetation states in Africa and commit vegetation to long-term change" *by* Mirjam Pfeiffer et al.

**Anonymous Referee #1**

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Thank you for inviting me to review paper: "Climate change will cause non-analogue vegetation states in Africa and commit vegetation to long-term change" by Pfeiffer et al.

The central premise of the Abstract is that transients in the vegetation response imply that the land surface does not merely behave as a set of time-evolving equilibrium states as the background climate changes. Instead, inertia implies alternative vegetation features might exist and that are only possible in a transient situation. Maybe not surprisingly, these are most notable under RCP8.5 ("business-as-usual" situation).

Maybe be even more explicit why the expression "non-analogue" is used throughout.



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Discussion paper

This suggestion is because often “analogue” can refer to simply anything that is different to states that have only been observed, (either in the recent past or possibly paleo-records). Here “non-analogue” implies non-pseudo equilibrium – so states that are not equilibrium either past, contemporary or projected under climate change. Possibly an alternative term could be something like “novel transient”.

The second line in the Abstract “This implies that vegetation is committed to future changes once environmental drivers stabilise” is important, and it might be good to re-iterate that towards the end. Something in general language might be useful e.g. “conservation managers. . .. . ...should be aware that observed vegetation may continue to change substantially, even if climate drivers are held fixed”.

The Introduction is good, and it recognises that the way vegetation sees differences between equilibrium and transient responses. The Introduction makes it clear that equilibrium-transient differences can be in both the multiple elements of the climatological drivers, and in the lags of the land surface itself (affecting its structure and composition). I also like that the aims of the paper are made very clear with the bullet points 1,2,3 at the end of the Introduction.

However, like many readers, I also looked at the conclusions before reading the main bulk of the paper. Notable is that the conclusions state: “ . . .shift towards alternative stable states”. So in other words, the transient time-history of vegetation evolution may impact on different final equilibrium states, even for the same equilibrium forcings. The vegetation of Africa has always been speculated as capable of that (i.e. “multi-stable vegetation coverage”; there are many references to this). It feels as if this should be listed as an extra point 4 in the Introduction, given it is discussed in this manuscript.

It is interesting that the effects of fire can have such a substantial impact on the magnitude of lags behind any equilibrium state. Does the paper hint at targeted fire reductions i.e. by deliberate human intervention could be useful in some circumstances?

The most interesting summary diagram in my view is Figure 5. It very cleverly shows an overall lag of vegetation from equilibrium in the left-hand panel, while the right-hand panel calculates a residual term which captures the "non-analogue" distance from any past equilibrium solution. As these days, people often extract diagrams and captions from papers to put in to powerpoint talks, would it help to expand slightly the caption to this diagram.

I also have a small request concerning Figure 5. The units of the left-hand panel are intuitive, as time lags (decades). The right-hand panel is Euclidean distance, based around the nine state variables (p9) contributing to Equation (1) (p10). I cannot think of an answer to this, but it would be good if there was some sort of physical or biological units/quantities associated with the right-hand panel of Figure 5. OK, maybe readers need to then look at Figure 7, which shows which biome is most different when compared to the nearest equilibrium decade. Hence write the manuscript to encourage the reader to view figure 5 and Figure 7 simultaneously?

It would be good to see an expanded version of "Opportunities and limitations of this study". First, if I have understood the paper correctly, then only one overall forcing Earth System Model (ESM) is used - as then disaggregated by CCAM. That model is the MPI-ESM ESM. The author should state where this model sits in terms of its equilibrium climate sensitivity (ECS). Is it a fast or slow warming model – or ideally towards the middle of any distribution? The ECS numbers are available in the 5th IPCC report. I realise this is technically challenging, given the need to disaggregate via CCAM, but future work could include more ESMs and from both the CMIP5 and CMIP6 ensemble.

A second point for the "limitations" section is it feels to me as if there needs to be much more confidence in the fire model. In particular, the Methods section states "ignitions are based on a random sequence". That randomness might have to change in time, if for instance, it includes lightning strikes, the frequency of which are likely to vary under global warming. It is noted that every diagram in the paper has both with fire and without fire findings presented equally. Future analysis, with a well-established and tested fire model, should give emphasis to the simulations with fire, as they are the more process-complete simulations.

A third point for the "limitations" section is that all the analysis presented is offline. The authors might like to speculate whether they think more multiple-stable states exist if the vegetation is coupled to an atmospheric model, thus allowing for feedbacks. There is a very long literature on this, some of which might be good to cite here. See for instance, Zeng et al. "Multiple equilibrium states and the abrupt transitions in a dynamical system of soil water interacting with vegetation" and the many references in that paper.

Broadly I like this paper and I think with some minor adjustments, it is suitable for publication. I am very happy to see any revised manuscript version.

Small additional things

The Abstract feels a bit too technical in places e.g. use of word "Euclidean".

Figure 1 (and maybe similar elsewhere). The fonts of the labels and the legends appear very small. One possibility to make more space – at least in the vertical direction – could be to only mark the "x"-axis labels under panels g,h,i.

Figure 3 – the colourbar levels look slightly odd. It feels to me as if they would be neater if simply 0.0, 1.0, 2.0, 3.0, . . ..

Please check through again in general the diagrams. For instance, I realise it is obvious, but the convention in Figure 4 would be "biomes types are as annotated in panel a. The colours used are common between all four panels".

Figure 8, with the small font used in the map annotations, it took me some time to realise that the "t" and "e" mentioned in the caption to Figure 8 was added to the end of those annotations. Hence e.g. "RCP8_5e". Please improve the presentation of this diagram, along with the caption and the annotations.

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