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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.

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Author responses to comments of anonymous referee 1
Responses are highlighted in bold font.

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.

Thank you for taking the time and making the effort to read and evaluate our

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manuscript.

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. 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".

Thank you for pointing out the difficulties of the term "non-analogue". We are aware that "non-analogue" is often used in the context of comparison between palaeo-vegetation states and present or future vegetation states that have not been found in this form in the past. However, what we refer to is the comparison between (hypothetical) pseudo-equilibrium states and the composite transient vegetation states that cannot be represented by any of the pseudo-equilibrium states. We found it difficult to find a term that would describe this discrepancy in an appropriate way and therefore decided to use the term "non-analogue". As you suggest, we will add a more concrete definition of how we define "non-analogue" in the context of the study (i.e., in the sense of not having an equivalent equilibrium state) in the introduction section to make it as clear as possible what we mean.

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".

We will pick up your suggestion and add a corresponding sentence in that sense towards the end of the abstract.

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.

Thank you, we are glad that the introduction convincingly transferred the message to you that we wanted to convey.

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.

We will include a mention of the possibility that shifts towards alternative stable states may be affecting African ecosystems as a consequence of climate

change in the introduction, as you suggest, and include references to studies that focused on this topic. We know that multi-stability of ecosystem states, in particular in connection with Africa's savanna ecosystems, has been studied and proposed previously by a variety of authors (e.g., Staal et al., 2016, Li et al., 2019; Pausas & Bond, 2020). Therefore, we decided to not focus on this topic again in our study. Ass we are not specifically investigating muli-stable states in this study, it is not a direct aim/working hypothesis and we therefore will not put it with the bullet points at the end of the introduction, but keep it within the more general part of the introduction.

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?

We are not considering fire management effects in this study, but have done so in previous studies with aDGVM. In Scheiter et al. (2015), we showed how different fire return intervals and early vs. late dry season management fires influence biomass and other state variables. In Scheiter & Savadogo (2016) we showed that management can slow down or accelerate tipping point behavior and hence the magnitude of lags. The effect of fire on vegetation state is ecosystem-specific and strongly depends on the management goals. Without fire, the majority of open and semi-open ecosystems in Africa are simulated to display higher woody cover and biomass. Targeted fire reduction therefore could help to increase the size of the African carbon sink. This would, however, come at the cost of losing unique ecosystem types and their associated biodiversity and ecosystem functions. In particular grasslands and savanna ecosystems are threatened by targeted fire reductions as fire plays a pivotal role in the dynamics of these ecosystem types. We will add a brief statement highlighting

these conflicting fire management targets (trade-off between carbon storage vs. ecosystem conservation) in section 4.1 of the discussion.

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.

Thank you for the comment. We will provide a more detailed figure caption in the revised version of the manuscript in order to make the figure self-explanatory without having to rely on the manuscript's main text. We suggest the following expanded figure caption: *Continental-scale spatial average of lag time (panel a) and residual distance (panel b) between transient decade and most-similar equilibrium decade (closest-decade partners (CDPs) based on Euclidean distance), for the four scenario pairings between CDPs. Error bars represent standard deviation of spatial averages in a given decade. Lag time increases over time for all scenarios, and scenarios with fire start to diverge from scenarios without fire after 2030. Residual distances between CDPs are different from zero and indicate that transient vegetation states are not time-shifted trajectories of equilibrium vegetation states.*

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?

We are well aware that Euclidean distance is a general measure that integrates over a variety of possible causes. Based on the distance alone, it is indeed not possible to discern the major cause that underlies the difference. In addition, due to the normalization of variables used to derive the Euclidean distance, the distance itself becomes unit-less, i.e., abstract.Therefore, your idea to more explicitly point out the connection between Euclidean distance in Fig. 5b and Fig. 7 that shows the fractions of variables that dominate the Euclidean distance at a given time is quite helpful to make the integrated distance shown in Fig. 5b more tangible for the reader. We will add a sentence to results section 3.7 and to the caption of Fig. 5 to encourage readers to view both figures conjointly in order to compare the average size of the distance at a given time with the respective fractional variable contributions.

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.

Thank you for pointing this limitation out, we will add it to the limitations discus-

sion section (4.3). We are currently in the process of publishing a parallel study where we used aDGVM in combination with an ensemble of CCAM-downscaled projections of climate change under RCP4.5 and RCP8.5 from 6 different ESMs to test the influence of increasing CO_2 concentration on carbon storage and to evaluate uncertainties regarding future biome change.In this study, we quantified the effect size of the explanatory variables (CO_2 scenario, i.e., fixed vs. elevated CO_2, RCP scenario and GCM) and their two-way interactions on the variability of the dependent variables (aboveground vegetation carbon and water use efficiency) between 2000-2019 and 2080-2099 using ω^2 metric. The ω^2 metric indicated that variation in total carbon between all 24 ensemble members was mainly explained by the CO_2 scenario, followed by interaction effects of CO_2 and RCP scenarios. The choice of GCM had the smallest effect on the simulation outcome. The biomass values simulated with MPI-ESM were slightly below the ensemble mean of the 6 ensemble members, which could indicate a tendency towards slightly more-than-average temperature increase and MAP decrease. This result agrees with the slightly-above average ECS value of 3.6 for MPI-ESM-LR (ensemble mean: 3.2 $\pm$ 1.3, Tab. 9.5, IPCC2014). With this, the MPI-ESM is none of the models that lie on the edge of the range and therefore should provide reasonable input climatology.Due to the necessity to conduct 13 equilibrium simulation runs per RCP and fire scenario in this study, in addition to the 4 transient runs (i.e., 56 simulation runs total for one ESM), we refrained from using more than one ESM's output in order to keep computational time within feasible limits.

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.

We agree that fire is a complex disturbance regime that depends on many influencing factors and is associated with various uncertainties. For Africa, it is estimated that the majority of ecosystems are currently not ignition-limited, i.e., ignition rates are more than sufficient to burn the available fuel, so climate and landscape connectivity combined with human fire management strategies are the main limiting factors on fire occurrence (Archibald et al., 2012, and references therein). Although the current implementation of fire in aDGVM does not account for explicit ignitions, it has heuristically been calibrated such that the ignition rates and resulting fires agree well with observed fire patterns and fire frequency (Scheiter & Higgins 2009). Therefore, the calibrated ignitions in aDGVM at least for Africa should not be limiting, even if currently not modeled explicitly. This implies that the simulated amount of fire is driven by the other two components of the fire triangle, i.e., fuel load and quality and fire weather conditions (i.e., fuel moisture). As fire intensity and spread in aDGVM are linked to fuel moisture, fuel biomass and tree cover (increasing tree cover reduces fire spread), fire regimes thereby change in response to climate change and vegetation change. Based on past personal experience from developing a more complex process-based fire model (Pfeiffer et al., 2013), I can say that such a detailed implementation of fire-related processes not necessarily improves the accuracy of fire representation due the increasing number of parameters that need to be estimated and defined, which increases uncertainty. We will add a section in the discussion where we elaborate on the points mentioned here.

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.

It is hard to speculate how an online coupling between aDGVM and an ESM would influence simulated vegetation dynamics due to the non-linearity of feedback mechanisms the spatially differentiated nature of such effects that will vary between different types of ecosystems. We can therefore only provide rather speculative answers to that question. The work of Zeng et al. (2004) suggests that multiple equilibrium states are possible in semi-arid areas, with grasslands vs. desert being alternative stable states. They also suggest that the range of parameter space over which these equilibria can coexist may be increased by positive feedbacks of evapotranspiration on precipitation (e.g., Wang & Eltahir, 2000). Zhu & Zeng (2014) evaluated the difference between offline and online simulations, but vegetation in their simulations was prescribed and therefore could not respond to climate change. In line with Zhu & Zeng (2014), we would expect that in particular albedo effects, canopy transpiration and evaporation, and temperature effects mitigated by vegetation could alter local to regional climate, in turn feeding back on vegetation dynamics. Where such two-way feedback mechanisms between vegetation and climate exist, we would expect that lag times, bi-stability and non-linear tipping behavior between different vegetation states could be even more pronounced, because stability is likely enhanced by feedback mechanisms that foster such stability. For example, tropical forests that transfer large quantities of water vapor to the atmosphere via transpiration locally create clouds and precipitation that sustain the existence of such forests even if regional-scale precipitation patterns without

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such feedbacks showed decreasing trends (see, e.g., Staal et al., 2018). In that sense, such forests foster climatic conditions that sustain their existence. However, even fully coupled ESMs may be unable to consistently predict how future feedbacks between vegetation and climate will shape terrestrial vegetation state, as shown by Bathiany et al. (2014) in the context of future Sahel greening trends simulated by three different ESMs with dynamic vegetation. We will add a section on this topic to the "limitations" discussion section.

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.

Thank you.

Small additional things

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

We will rephrase the sentence with the first occurrence of the term "Euclidean distance" to make it more clear that this is used as a measure of dissimilarity between vegetation states. i.e., the sentence "Euclidean distance between simulated transient and equilibrium vegetation states based on selected variables was used to determine lag times and similarity of vegetation states" will be rephrased as follows: "We determined lag times and dissimilarity between simulated and transient vegetation states based on the combined difference of 9 selected state variables using Euclidean distance as a measure for that difference." In line 15/16, the term "Euclidean distance" will be replaced by "dissimilarity".

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.

Thank you for pointing this out. Given the possibility to zoom in on figures in digital form, we apparently did not pay enough attention to this. We will re-plot this figure and increase font sizes as much as possible to ensure that labels and legends are more easily legible, in particular on multi-panel figures.

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, ...

We will re-plot the figures in the main text and the supplementary material with a colorbar that has more even breaks.

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”.

Thank you for pointing this out, we will change the figure captions according to your suggestion.

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.

We will increase the font size of the legend as far as possible and highlight the caption of the legend in bold to make it easier to read, and change the figure caption to point out which of the panels are representing transient and which ones are representing equilibrium scenarios.

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