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Interactive comment on "Towards a global understanding of vegetation–climate dynamics at multiple time scales" *by* Nora Linscheid et al.

Anonymous Referee #1

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This paper analyses temporal variability in vegetation greenness (NDVI), air temperature (T), and precipitation (P) over broad range of time scales and over the entire terrestrial landmass at 0.5 degree resolution. The overall purpose of the paper is to identify vegetation-climate co-variations. I am impressed by the scope of the work presented in this paper and by the very detailed description of the data and methods.

The use of the NDVI as a metric of vegetation greenness could be controversial but the authors gave a good argument in favor of their choice. The choice of 0.5 degree as the spatial resolution has not been explained, I suspect that it was needed for averaging purposes due to lapses in data coverage at the higher resolution. However, it is important to remember that such resolution amounts to about 55 km on the equator. This is a significant area over which to average NDVI, T, and P, especially in the moun-

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Discussion paper

tainous and coastal areas where significant spatial variability of these quantities are expected at that spatial scale. A decision to work on the lat-lon grid is also left without comments. Using the lat-lon grid for global studies is questionable even if it is common in the literature. This problem is multiplied by the use of large grid cells, the shape and areas of cells at different latitudes change significantly. Projection to other grids, more appropriate to global analysis, is possible. Authors should discuss their choice of using the lat-lon grid with large grid cells. The Fourier transform method of dividing signal into short-term, seasonal, and long-term is standard and effective.

The global maps of NDVI, T, and P variance decomposed into three regimes of temporal variability (short, seasonal, and long) shows (Fig.1) mostly what I would expect with the exception of short-term variability of NDVI. I think it would be useful if the paper would mention that seasonal variability of NDVI and T is a "default" variability – something expected. Long term variability of these two variables is something that we can understand, but short-term variability is surprising, especially for the NDVI which should have more "inertia" than T. On the other hand, short-term variability of P is expected as we all know from experience, seasonal variability of P is also expected and long-term variability could be understood. More generally, the paper describes results in detail but lacks commentary in a spirit of what I outlined above. This is not that we don't have any expectations of how the results should look like, so it would be effective to stress the unexpected parts of the results.

Authors have designed a clever scheme to show "co-oscillation regimes." It classifies grid cells' into 11 classes based on co-occurrence of dominant temporal variability regime (S, A, or L) for each variable (NDVI, T, P). This yields a compact and easy to understand map (Fig.2). Again, I argue that it would be useful to stress that AAS and AAA classes are expected by default – something that we would predict on the basis of prior knowledge without analysis presented in this paper. Thus focus should be on the remaining classes. The LAS class can be easily explained, these are semi-desert or shrubland areas with only slightly seasonal-dependent vegetation and sporadic rainfall.

On the other hand SSS, SAA, and SAS classes are difficult to understand. Authors do not try to understand them; instead they just say that this points to a complex climate-vegetation dynamics. I think that this is a way of saying “we tried to understand them but failed” or “we did not try to understand them in this paper”. Both statements are acceptable but should be stated clearly. To me these classes are difficult to accept, for example, what does that means that some regions in Indonesia are characterized by predominantly short-term variability of vegetation and temperature? It seems counter-intuitive and contrary to prior knowledge. Could it be a data artifact, or method artifact? One possible explanation is that the short-term temporal variability include all periods < 0.9 year, so maybe in these regions the actual variability is $\sim$0.9 year which would be more intuitive.

Authors compare their map of “co-oscillation regimes” with the map of land cover (GLC) and, separately, with the map of climate (Koppen-Geiger or KG). The reason for such comparison is not clearly explained. On one hand authors describe their classification as new and different (from climate and land cover classifications), but, on the other hand, they look for similarities with those maps. I am not saying that such comparison is uninteresting, just that the rationale is not explicitly given. Also, maps could be compared using methods especially designed for such purpose (see International J. of GIS, 2018, v32(12), pp. 2386-2401).

Authors also constructed maps (Fig.3) on which a color (organized into a bivariate table) indicates correlation between NDVI and T signals, and, at the same time, a correlation between NDVI and P. As in the previous two figures, I found that the text should stress that seasonal cycle is a default and short and long term cycles are the “new” results. In this case, however, because of the uniqueness of the bivariate map, even the seasonal map is new and could use more explanation going beyond just describing what we already see in the map.

Overall, I find this paper to be solid, very interesting and illuminating; however, it is, to my taste, overly skewed toward technical description at the cost of not providing plausible explanations to some of the more unexpected results.

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