Disclaimer: This review was written by MSc student Le Lu as part of his course work on "scientific reviewing", under supervision of dr Arnold Moene from Wageningen University. The comments were submitted because they can contribute to the scientific process, and because they contain helpful questions and suggestions for the authors. Although the structure of this review follows the formal conventions, it is thus not a solicited peer-review from the editor of ACPD.

Review of *Towards a global understanding of vegetation–climate dynamics at multiple time scales* by *Linscheid et al.* by reviewer Le Lu

Linscheid et al. provide a global assessment of vegetation-climate variability at multiple time scales. Motivated by the existing knowledge gap regarding how the global terrestrial biosphere responds to multi-timescale variability of climate, the study aims to explicitly examine the variability of biosphere and climate time series, explore spatial variability of vegetation-climate dynamics at each time scale, assess the potential link between the resolved temporal patterns and traditional land cover classification and compare correlations of climate and vegetation across multiple time scales. By decomposing over 30 years of remote sensing records of NDVI and climate time series with Fast Fourier Transformation (FFT), variances in each variable contributed by different time scales from short-term oscillations to seasonal and long-term trend were compared and co-oscillations between NDVI and climate variables (i.e. temperature and precipitation) were discussed. Moreover, the oscillations regimes were compared with traditional classifications of land covers and climate zones. For every time scale, the researchers also examined the correlations of NDVI with climate variables. Finally, the potential effects of land cover change, limitations of FFT and NDVI on time series decomposition were also considered.

The results show heterogenous response by vegetation to climate variables both temporally and spatially. Section 3.1 presents that NDVI and temperature variability is dominated by seasonal cycle whereas 52% of the variance in precipitation is contributed by short-term oscillations on a global scale. However, differences exist as the long-term oscillation in NDVI is strongly associated with shrub and herbaceous land cover types. In section 3.4, a comparison of correlations of NDVI with climate variables suggests that correlations strongly vary with time scales and space. In South Africa, correlation with temperature is negative on short-term scale and long-term scale but positive on seasonal scale. Different correlations patterns are observed among tropical forest regions. Section 4.4 concludes that the results are proven to be robust by repeating the analysis with Enhanced Vegetation Index (EVI) and NDVI from the Moderate Resolution Imaging Spectroradiometer (MODIS) as indicators of vegetation dynamics or Empirical Mode Decomposition as the time series decomposition technique.

The study is, as its authors claim, the first of its kind. It is the first attempt to examining the global vegetation response to climate across multiple time scales. What is particularly strong about this study is that its methodology takes into account various sources of possible confounding factors that may compromise the results. As mentioned above, time series decomposition is repeated with a more data-adaptive approach. Considering the noise introduced by cloud cover, which NDVI is subject to, vegetation indices from MODIS are included as part of the repeated analysis so the results can be proven robust. The effect of land use and land use change (LULUC) is evaluated as well given the large time span that the study incorporates. Last but not least, the presentations of results are clear and easy to follow, particularly figure 2a in which the dominant oscillations of NDVI, temperature, and precipitation are indicated by a three-letter scheme.

Overall, the study is well-thought-out and well written. This is a study of biosphere-atmosphere interactions, so it falls well within the scope of the journal. It serves as an important contribution to our understanding of past and current climate-vegetation interactions on separate times scales, and its findings may even help us project the future interactions. In the meantime, however, there are a few issues that I believe require further attention and revisions before publication. These issues are discussed below in detail.

Major arguments

1) The methodology of this study relies on the use of long-term remote sensing records of NDVI. It is good to see some limitations of remote sensing NDVI are addressed by comparing the results with alternative data source and vegetation index such as EVI and NDVI from MODIS. Yet other known issues of NDVI are not discussed in the manuscript.

   It is well documented that NDVI can be biased in regions where biomass is dense (Pettorelli et al., 2005). NDVI saturates as plants grow and thus is insensitive to dense biomass (Huete et al., 2002). the saturation of NDVI may cause underestimated seasonal cycles in tropics, where vegetation is typically dense. This potential outcome could be an alternative explanation to the contradiction between the results of this study and Huang et al. (2019) regarding the negative correlations with temperature in the tropics. Furthermore, a recent study, which is cited for other parts of the study, attributes the low response of vegetation to precipitation in African tropics as partly a result of saturation (Hawinkel et al., 2015), contradicting the claim that the correlation between NDVI and precipitation is almost always positive. In another previous study on global NDVI time series, tropical evergreen forest is even excluded because of the low signal/noise ratio associated with saturation (de Jong, Verbesselt, Schaepman, & de Bruin, 2011).

   Besides, NDVI is also influenced by reflectance from soil when the Leaf Area Index (LAI) is under 3 (Pettorelli et al., 2005; Huete, 1988). Specifically, darker soil substrates would result in higher values of NDVI for a given amount of vegetation. Huete (1988) notes that the NDVI suffers from soil reflectance issues across different soil types especially in global data sets. This is critical to this study and likely to have implications on its results since the global scale is exactly what it deals with and involves naturally various soil types. For example, the results of the study show strong associations between the long-term NDVI regimes and arid regions where vegetation is sparse, but this could be partly the result of varying brightness of soil controlled by soil moisture. Meanwhile, the short and long-term correlations with precipitation presented by the study may be overestimated due to the higher NDVI values associated with darker soil substrates led by precipitation. These unaccounted impacts on the current results, without further looking at the pattern of variations in soil brightness, is unpredictable. Nonetheless, it would either overestimate or underestimate the contribution of oscillations at certain time scales to the overall variability considering the influence of noise introduced by soil background on NDVI.

   To address the above limitations of NDVI, I recommend validating the results by repeating the analysis with another two vegetation indices just as what was done with MODIS EVI, at least for regions where vegetation is dense or sparse. The two indices are the Soil-Adjusted Vegetation Index developed by Huete (1988) and the Wide Dynamic Range

Vegetation Index by Gitelson (2004), which are designed to overcome the noise from soil background and saturation of NDVI, respectively. Both indices are modified versions of NDVI and require only the reflectance inputs in the same waveband as what NDVI does, so there is no need to introduce any new data. Although the values of different indices are not directly comparable, which may compromise the global scale that the study aims at, in this way it nevertheless would be clear whether the two limitations of NDVI have any major implications on the results, providing a more accurate characterization of vegetation-climate dynamics.

2) The study attempts to assess the influence of land use and land use change (LULUC) on its reported NDVI classifications and concludes no widespread effect in section 3.3. The cutoff of land use change is set at pixels with 25% change in fraction of vegetations. There is no reason given regarding why 25% is chosen, which makes me curious to learn what the results would be if a threshold lower than 25% is used, for example, 20% or 15%. More pixels would be inspected with a lower threshold, and maybe more pixels would reflect change in NDVI due to LULUC. In addition, the study does admit the absence of marked signs of LULUC is perhaps because of the coarse spatial resolution of the data. Despite knowing the limitation, the study still concludes there is LULUC has no widespread effect on the results, which I find very puzzling. To me, given the limitation in spatial resolution the best that could be concluded would be "no definitive conclusion can be given" instead of lines 224 and 225.

   As a side note, some questions regarding the validity of the study may be asked because the coarse resolution of the data is proposed as the explanation for the absence of signs of LULUC in NDVI. If this explanation holds, would it be suggesting that the spatial heterogeneity of vegetation dynamics is underestimated by the coarse resolution?

   Regarding the assessment of the effect of LULUC, I recommend to either give reasons for the chosen 25% cutoff or compare the results with a lower threshold value. For the validity of the conclusion on LULUC effect and even the entire study, it is perhaps wise to recognize the coarse resolution as a limitation to the study design and be clear about possible implications. This addition could be put into section 4.5 Limitations and Outlook to give a more balanced reflection on the methods and results.

3) Section 3.5 compares results from CEEDMAN with the FFT in order to prove the results are robust. However, hardly any justifications are given for carrying out this analysis other than lines 265 – 266 "the data-adaptive empirical mode decomposition (EMD) could be expected to be better suited for exploring instationary ecological processes over time". It inevitably leaves readers to wonder about the relevance of this repetition, especially given that the results are largely the same. Moreover, the manuscript does not provide enough information about how the repetition was implemented. It recognizes the constraints posed by a higher spatial heterogeneity in implementing a global analysis with CEEDMAN, and it appears to allude the implementation includes a test case over Europe. However, it is not clear if any other regions were included as a part of the repeated implementation and if the results were based on any regions in addition to Europe.

   I recommend the authors to be explicit in this section. Multiple papers are already cited regarding CEEDMAN, and I do not see why not convincing readers about the use of it by enumerate the benefits/advantages of this alternative method and the expected differences that the method may bring. It could also help readers better understand the actual yielded differences shown in lines 268 to 270. It may also be useful to include more details as what the spatial scope of this repeated analysis instead of simply presenting the results of a test case over Europe.

Minor issues

1. In line 39 the authors state that only reflecting greenness is a limitation of NDVI, followed by multiple citations. After skimming the cited manuscripts, I found no relevant claims. One of the studies says the ability of NDVI to quantify greenness is what allows it to correlate with vegetation biomass and dynamics. In a rather thorough literature review I read regarding the use of NDVI for assessing vegetation response (Pettorelli et al., 2005), no relevant information is given about whether reflecting only greenness as a limitation of NDVI. It could be a factual error or the experience of the authors, but unfortunately no elaborations on this point are given. Please offer some evidence to support this claim.
2. In line 297, the authors suggest radiation as the main driver of NDVI in tropical regions. However, the interpretation of the source cited after the claim does not seem entirely accurate. Nemani (2003) says the tropical area "may have either a sustained dry season or nearly perpetual cloud cover that limits solar radiation", implying water availability and radiation may both be limiting factors in the tropics. However, the study only mentions radiation but not water availability to support an explanation for its results, which constitutes an inaccurate interpretation of the reference. I recommend rephrasing the sentence to accurately reflect the reference.
3. Page 9, figure 2a: No legends are given for land cover classes represented by the two types of red colors that are used in the figure.
4. Page 10, line 235: In other parts of the manuscript, it is being referred to as evergreen broadleaf forest (EBF), but in line 235 it is broadleaf evergreen forest. Replacing it with the acronym would do as this is not its first appearance.
5. Page 10, line 244: This is essentially a repetition of what was discussed in line 235 without adding any new information.

Other than the above issues, the work is well done. I am confident that it will be accepted after these issues are properly addressed.

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