Review ESSD-2018-90, Phase synchronizaton of temperature and precipitation …

60 MB files, one for land and one global. Big files but they download successfully, not clear the distinction between them (see below).

The basic flaw of this work relates to their complete neglect of energy (light) as the predominant factor in global GPP and NPP. In all discussion and analysis, they assume plants are water or temperature limited or controlled, but never light limited? Because they work primarily from a tropical viewpoint, perhaps, but even in moist warm tropical rainforests, light plays a predominant role in global productivity? In temperate agricultural regions, daily patterns of light and precipitation probably play a much larger role than temperature and precip. E.g. daily good energy (sunlight) coupled with nocturnal precipitation represents an ideal growing environment. These authors miss entirely these sub-daily effects and do overall a very poor job of describing the mutual roles of energy, temperature and precip. Synchronization, even if well done (different to this work), on synoptic and seasonal scales seems not relevant for many vegetation regions, as their mostly dismal results confirm. Not a quality paper, needs rejection.

Page 1 line 25, temp and precip. No discussion of energy / light? Exclusively tropical?

Page 2 line 6 “a typical climate of the phase synchronization” what does this mean?

Page 2 line 26, again, summer = energy = sunlight. If, perhaps only/if energy (sunlight) not limiting then temp precip important?

Page 3 figure 2, overlap and non-overlap functions of the log curve clearly shown, but intervals generally 10s to 100s of days? Nothing within a day, e.g. day to night?

Page 3 line 10: show us the multiyear averaged annual distributions of T and P from this data set to convince us of log behaviour. If not symmetrical, you have a skewed distribution of SI? Other studies will tend to confirm the temperature patterns albeit with different slopes and inflection points but global and regional studies generally do not confirm that NPP follows precipitation in such a smooth curvilinear relationship? We need, but do not find, an uncertainty assessment here.

GIMMS NDVI basically 40N to 40S?

Page 5, step wise linear regressions, NDVI vs T and P and then NDVI vs T, P and SI, with change in R2 due to SI? If valid, such an analysis could / should provide the basis for a rigorous uncertainty quantification?

Page 6, Figure 3: No evidence of useful signal beyond $\pm$45. No evidence that analysis excluded Mediterranean climates (which occur many areas other than around geographic Mediterranean). Greenland result for SD likely bogus.

Page 7, regional trends of SI at levels of 0.0009? Not valid, not believable, no uncertainty basis. Then, trend of -0.0005, declared not significant. Give us the uncertainty analysis and significance standards? Global mean SI trend = 0. 0 $\pm$ 5%? 0 $\pm$ 50%? None of this seems reliable without a detailed uncertainty analysis. “suggesting that there is no significant trend” Suggesting? Authors need to show and validate, not suggest.

Page 8, global trends of temperature and precipitation: “maximum increasing trend is 18 °C/year “ Nonsense, garbage, authors can’t really believe this? “4 °C/year for global lands “ Nonsense, garbage, authors can’t really believe this? For precip, no statistically valid global trend but meanwhile the authors write of increases in specific small regions, primarily at high latitudes where NDVI, SI, T and P all fail? One senses that these authors have no accurate sense of global plant biomass or productivity.

Page 9 “added contribution of phase synchronization to terrestrial vegetation productivity across global lands … the average is 0.06 for global lands … dotted sporadically across global lands”. This narrative estimate - we can’t credit it as a proper analysis - fails every reliability, validation

and uncertain requirement of a global data set. Fundamentally unacceptable, useless. Definitely not suitable for publication in ESSD.

Page 9, SI with monsoons. In Figure 6, barely 20% ("20.44%", do these authors not understand significant figures?) of lands with SI > 0.5, despite the fact that SI has - so far in this paper - no demonstrated utility or validity, correspond (how?, geographically?, seasonally?) with monsoon regions. Figure 6 demonstrates the weaknesses, not the strengths. SW American monsoon completely missing, coastal areas of the major Asian monsoons show no correlations, etc. Southern Japan and Korean peninsula show correlations? Figure 6 demonstrates failure, not success?

Page 10, Figure 7. Again, an emphatically clear demonstration of the failure of SI over most regions according to precipitation. Most of the vegetation monitoring community knows better than to rely on NDVI for qualitative analyses; cautions exist in almost any NDVI description paper. Here again: "55.42%", "42.38%"? Where do the authors get these significant figures?

Page 11 "the minimum of two coefficients of determination derived from 30 regression analyses on daily temperature and precipitation time series could be used to flag the quality of the proposed index" No, two weak correlation patterns combined do not constitute a valid quality indicator. Figure 8 conveys no useful information.

Page 12, no explanation given about differences between 'land' data set and "whole globe" data set (of exactly the same file size, as it turns out). They use a different land mask? They used different NDVI assumptions or averaging? User needs more information here to know which data set to use for which purposes.

Page 13 "robust and practical". Wrong on both counts.

Taking the guidelines from ESSD https://doi.org/10.5194/essd-10-2275-2018 (thank the editors for making that available), the present manuscript fails substantially on validation, uncertainty analysis and fundamental quality. Rejection seems necessary.