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Biogeosciences Discussions

Interactive comment on "Time series of vegetation indices and the modifiable temporal unit problem" *by* R. de Jong and S. de Bruin

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Reviewer #1 Thank you for your constructive comments, which will be very helpful for revising the manuscript. We address your questions and concerns individually below.

1. [. . .] The authors fail to show that these difficulties plague previous results. Furthermore, the issues raised by the authors probably are not the greatest problem associated with efforts to fit time trends to time series of vegetation indices.

> We realize that many issues arise when fitting temporal trends to time series with multiple cyclical, trend and noise components and that the problem we described is only one of the issues. At the same time, we believe it is an issue that is easily disregarded. Many studies deal with the MTUP by aggregation over yearly or seasonal

periods, which is the optimal bin size for the described problem. In this sense, it is probably correct to state that the MTUP is not the greatest problem for trend detection, but, as your summarized correctly in your review, we argued that the problem involves more than solely the bin size, for instance shifts in the start point of the time window with respect to phenology. Such issues might be of importance in grassland and shrubland regions, especially in the Southern Hemisphere. We address this in one of our answers below. The purpose of our study is to provide an insight into possible MTUP effects for satellite time series. As such, we do not question the validity of previous studies, we only highlight one of the issues which analysts might run into and we provide a framework for dealing with this. However, we appreciate your comment and we included some examples in the revised manuscript (see answers below).

2. One issue concerns the statistical significance of results. I assume that the authors use t-tests that are implied by the text on lines 17-20 on page 8549. But the results of the t-tests are not reported in any of the tables. For example, do all of the results reported in Table 1 reject the null hypothesis that the coefficient associated with time is statistically different from zero? And if so, at what significance level?

> We apologize for not having been clear on this matter. Where possible, we performed significance tests and, indeed, disregarded all trends which could not be confirmed using a significance level $P < 0.05$. We failed to mention this threshold in the original submission and we will correct this in the revised version. In Table 1, the t-test results from the OLS were not reported because they are incorrect due to serial autocorrelation of the data used (we mentioned this on P8548 L19ff).

3. Another issue concerns the degree to which the results are robust. The authors analyse a function that has a peak-to-peak amplitude that is consistent with a temperate non-forest environment. If the amplitude is increased or decreased, what happens to the results? My guess is that the importance of the distortions reported by the authors declines as the amplitude is decreased. Ad absurdum, a seasonal cycle with no amplitude would not generate the types of distortions described by the authors. So,

the authors may want to identify the amplitude at which these distortions appear and the type of ecosystem that is consistent with this 'threshold amplitude.'

> The reviewer correctly points at the fact that the magnitude of detected change depends on the amplitude of the seasonal cycle. In fact, the relationship between the two is linear as we mentioned in the manuscript (P8550 L24): "The slope coefficient is linearly related to the amplitude used for the seasonal model and is inversely related to the extent of the time series (or segments)." We used amplitude 1 in Table 1, so that the listed value can be multiplied by the actual amplitude to obtain the amplitude-specific result. In Figures 1 and 2 we used an amplitude of 0.6 which is closer to actual VI time series, e.g. to a temperate non-forest environment. We agree that the MTUP as we described it cannot affect analysis of time series without seasonal amplitude, e.g. in dense tropical forests. But the use of vegetation indices is disputed in these regions anyway, because of signal saturation issues. We will include a more thorough discussion on this issue.

4. Furthermore, I am not sure about the relevance of some of the authors' cautions. To date, most analyses of vegetation indices focus on mid and high latitudes (in part because cloud cover limits observations at low latitudes). At these mid and high latitudes, there is only one growing season per year, and so concerns about time series with different start and end dates are largely moot. As such, this largely alleviates the concern expressed by the authors on page 8551, lines 11-12 "but in reality calendar years may not fit the periodicity of VI time series because of shifts in vegetation phenology and variations in growing season length". In the Northern Hemisphere, the growing season at mid and high latitudes fits within a single growing season.

> In the high northern latitudes the growing seasons indeed fit within a calendar year. Therefore, aggregation by calendar year is an effective method for dealing with seasonal variation. Seasonal aggregation might be affected by phenological shifts, if the aggregation window is not chosen with sufficient margins. Other methods might be affected by the typically steep onset of greening, described by Beck et al.

(2006, DOI 10.1016/j.rse.2005.10.021), but that falls without the scope of this paper. There are, however, many other regions were temporal aggregation is not straightforward, e.g. in the Southern Hemisphere (e.g. Wessels 2009, DOI 10.1111/j.1475-2743.2009.00195.x) or in regions which, unlike snow-covered regions in the high latitudes, lack a distinct window to separate subsequent growing seasons, e.g. regions with multiple cropping regimes. Another region which is in the spotlight of VI studies is the semi-arid Sahel. In such regions, growing seasons have less predictable behaviour than in the high northern latitudes and temporal compositing is more likely to affect the analysis (e.g. Hüttich et al. 2011, DOI 10.1016/j.rse.2011.05.005). The issue of aggregation by multiple cycles is valid for all cases.

5. Indeed, I think the paper would gain added relevance if the authors could identify some analyses that they think suffer from the distortions, which they describe. For example, the Zhou et al (2001) paper referenced by the authors (the earliest paper and hence most likely to suffer from such problems) analyzes trends over time in which NDVI for single growing seasons or a specific month are dependent variables. Although I am not an expert in this area, I don't know of any published analyses in which the authors simply fit a time trend to a continuous series of monthly or biweekly values.

> We are not aware of studies directly fitting linear models to un-aggregated seasonal data, but that part of our manuscript (Figure 1 and Table 1) has an introductory and illustrative function of the ad absurdum case. The studies referred to above will be included in the revised manuscript to support our point.

6. Finally, I think that the authors miss the most important issue associated with fitting time trends to time series of vegetation indices. These indices partially reflect biomass. As such, an increase in biomass persists from one period to the next. Similarly, a loss of biomass, such as a tree fall, persists from one period to the next. As such, it may be reasonable to represent the vegetation index using the following data generating process: VIt = VIt-1+ mt In which VI is the vegetation index and m is a disturbance to vegetation. As such, the vegetation index contains a stochastic trend. This implies

that the time series contain a unit root or near unit root. Even if m is iid, the VI time series will meander around its initial value. Under these conditions, fitting a time trend to the VI time series (e.g. VIt = a + bTime + et) will generate very misleading results. Specifically, Monte Carlo simulations indicate that the coefficient associated with Time (b) will be statistically different from zero about 80 percent of the time if the data generating process that creates VI looks like equation 1. Such results are termed 'spurious regressions. As such, spurious regression results may be a more important pitfall for efforts to fit time trends to vegetation indices.

> The reviewer rightfully questions correctness of the linear trend model for the type of data at hand. Alternatively, a random walk model is proposed. In the discussion section of the reviewed paper we will include this suggestion and argue that MTUP may also play a role in random walk models if the data are aggregated over time intervals. In the main text we will however stick to the linear trend model because of its current popularity for NDVI time series analysis (literature is replete with this methodology, e.g. Paruelo et al. 2004, DOI 10.1080/01431160310001619526; Herrmann et al. 2005, DOI 10.1016/j.gloenvcha.2005.08.004; Olsson et al. 2005, DOI 10.1016/j.jaridenv.2005.03.008; Heumann et al. 2007, DOI 10.1016/j.rse.2006.11.025; Bai et al. 2008, DOI 10.1111/j.1475-2743.2008.00169.x).

Reviewer #2 Thank you for your kind words and your suggestions.

1. Similarly to a recently published short paper (http://geoanalytics.net/ica/icc2011/coltekin.pdf), authors make an analogy to MAUP in temporal dimension, call it MTUP, and present why the same is important for temporal considerations.

> We are aware of the short paper that is mentioned by the reviewer, but we are not sure where it was published. For that reason we could not refer to it. For the revised manuscript we will make an effort to find out if the work was published and in that case we will refer to it.

2. [. . .] Based on the open commentary from another reviewer, I see that perhaps there is some skepticism to this (case examples matching the message) and I recommend authors to ensure the validity of their case examples and encourage them to publish on MTUP further.

> We will pay due attention to the issues which were brought up as we highlighted in our answers above.

Interactive comment on Biogeosciences Discuss., 8, 8545, 2011.