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Interactive comment on "Trend and climatic sensitivity of vegetation phenology in semiarid and arid ecosystems in the US Great Basin during 1982–2011" *by* G. Tang et al.

Anonymous Referee #1

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General comments

The Tang et al. paper presents a study investigating the trends and interannual variability (IAV) in vegetation greenness and associated drivers the semi-arid/arid ecosystems in the US Great Basin over the 1982-2011 period. The two main findings of the paper are that the warming trend is the main driver of the increased Growing Season Length (GSL) due to a later autumn senescence but that precipitation drives the IAV in greenness.

The study is a valuable contribution to the literature as there is a relative lack of publications investigating trends for semiarid ecosystems compared to temperate/high latitute

regions, as the authors point out. The aims of this study are nicely written and there is a detailed analysis of the possible drivers of the changes. There is a lot of detail in there that could potentially benefit further clarification in the text – I will attempt to summarise these points below.

One issue that needs to be addressed is the difference between drivers of change in IAV and trends. There is a mix of the impact of overall trend and IAV in the linear regression that needs to be explained more clearly. The linear regression/correlation analysis (in Section 3.4 and Figures 6-8) is based on the anomalies (the delta in these figures is the inter-annual anomaly from my understanding, as the figure caption does not give enough detail). This delta (the anomalies) will include the changes due to both the IAV the trend. The results however are only discussed in terms of the drivers on IAV.

Then in Section 4.1 all the results (both the description of long-term trends and the drivers of the anomalies) are brought together to explain the long-term trends in a slightly confusing way, which is not helped by the fact that new (and crucial) results are introduced (Figure 8 and Table 3) – so as a side point I think these results should be described in detail in the results section. For example Figure 8a shows a positive relationship between the temperature and GSL anomalies, but this could be the same even without any trend in either variable (i.e. in years with warmer temperatures, you would get a longer GSL). Then the authors refer to Figure 2, which shows the long term trend, to suggest (together with Figure 8a) that the increase in long term temperature causes the increase in GSL (and NDVI). Although the logic mostly follows I am not sure that all the pieces are there to make this picture.

I think it would be clearer if the analysis in 3.4 wasn't just presented as a change in interannual variability but as anomalies that will include the underlying IAV and trend. I think a trend analysis could include a regression the trend (slope) in NDVI against the trend in temperature for each grid point (and see if there's a correlation). It would be interesting to assess just the drivers of IAV by de-trending the curve before peforming

the linear regression analysis. I am also unclear whether the multivariate regression compares the anomalies or the long-term trend.

I suggest that a bit more analysis to bring everything together, as well as a slightly clearer description of what's being analysed, would strengthen this paper. In addition I have a few remarks below about data processing that if addressed would further reinforce the conclusions drawn.

Methods Some technical data processing issues should be discussed further in the methods section (again, details below) in order to strengthen the analysis. It is unclear in Section 2.4 whether the regression will be performed on the trend (per grid cell for example) or the anomalies (per year). This should be clarified. I am unclear why a univariate and then a multivariate regression are performed, I would have thought that only a multivariate regression would be needed. It would be good to have the equations here, as well as for the AIC metric.

I was slightly surprised by the fact that GIMMS NDVI is used and not the latest version (3g). This is freely available as far as I am aware and uses an updated algorithm that accounts for some of the issues of the earlier version. It would be good to compare your analysis for both versions.

I also think that all trend analyses should ideally be verified by performing the same analysis with an independent dataset (e.g. MODIS). I know of one study that has says that trends derived for GIMMS in arid regions should be interpreted with caution (Fensholt and Proud, 2012, RSE, 119, 131-147), though I note they have used this dataset in another analysis that you cite. It would signicantly strengthen your analysis if you repeated the analysis with an independent dataset.

Finally, the methods used to interpolate between data points and to derive the SOS and EOS dates will be subject to some uncertainty. This is also true if there are multiple cycles, or many little "bumps" in the time series – how do you deal with this scenario? The IAV in particular of those dates might be strongly affected by this. An exhaustive

uncertainty analysis and quantification is probably too much to ask, but it would be good to do a few tests to try and see how much different parameters or methods of interpolation affect your results, at least to mention this qualitatively in a few lines.

Results and discussion

More detailed figure captions are needed for those who might look at the plots first. For example for Figure 6 given the description in the results I assume each point corresponds to a year here (therefore the anomalies from figure 2 etc) but if I just look at the plot I am not sure whether in fact we are looking at the long term trend (change in temperature/NDVI) for each grid box.

As stated above, the fact that the regression analysis will include the effects of both IAV and trends should be discussed.

The discussion does nicely try to bring all the analysis together to form a clear picture, which is difficult given all the metrics, time periods and difference between trends and IAV detailed in the results. The main message is there but at times it's a bit confusing and needs to be described more clearly, and the physical reasons could be discussed or emphasised.

One main conclusion is that increased temperature SSA is responsible for the increased GSL, but from what you show earlier that it appears to be the advancing EOS that is increasing the GSL, but the summertime temperature that appears to be dominating the increase in SSA temperature. At the same time the summer temperature has a negative relationship with summer NDVI (this makes sense as if it's hotter the vegetation suffers from water stress). This means that you are implying that increased summer temperature then has a positive effect on the length of the growing season in the autumn, even if the plant has suffered water stress. This might be shown further by considering pre-season temperature as well as precipitation in your correlation analysis? You then show that summer and autumn NDVI is correlated with wintertime (and autumn) precipitation and this explains why increased temperatures can explain

the longer growing season. I.e. the temperatures overall are increasing in the summer, and despite any water stress that might decrease magnitude of the NDVI a positive precipitation anomaly helps the overall trend in temperature. This is despite the lack of trend in precipitation, so this should be clearly explained. This summary of what's happening also perhaps explains the lack of relationship between SSA temp and mean NDVI (i.e. you have a longer GSL but a decreased summer magnitude contributes to no trend in SSA mean NDVI overall). I feel this kind of discussion is nearly there but could be more complete. It might be good to examine the amplitude in your analyses as well to complete the picture. Note also here that it might be worth stating that by considering summer NDVI you are effectively (mostly I guess) looking at the NDVI magnitude, whereas the SSA NDVI will include both magnitude and length.

I would like to see a discussion of whether the vegetation type influences the spatial patterns of the trends seen, and not just latitude (if you think there is a pattern – but I would be surpised if there was no effect). Although there was a strong negative trend in all seasons in the SW, this was not really discussed.

Finally, when you describe changes in trends within the study period, have you used a trend change point detection method to infer that or is that just by eye? I would suggest that it should be based on an established method, and if so this needs to be detailed in the methods. Also it might be good to try and explain why this occurs in terms of any changes in driving variables (as this is already your aim).

Minor points: P11389 Line 1: rather than saying e.g. forests and water for biotic and abiotic I would suggest that it's more accurate to say (biological versus physical).

Line 9: Might be good to suggest what the implications are for the terrestrial C, W, E, e.g. it defines the period of C uptake, or the partitioning of sensible and latent heat flux etc.

Line 23: See also Poulter et al. 2014 Nature doi: 10.1038/nature13376 and Ahlstrom et al. 2015 Science 348, 895-899 Line 26: Could you give an example of the conse-

quences for ecosystem services to provide some context. P11391 Line 24: Not sure Botta is a good reference for the fact that evergreen forests have little to no seasonal cycle, even though in her paper there's no evergreen model because of that reson. P11395 Line 6: What is the mix of vegetation in the pixels? It might be nice to know how much the signal is affected by trends in a certain vegetation type to try to understand the processes at play. P11397 Line 21: Sentence restructure: Something like In spring 12% of the points exhibited a significant negative trend from 1982-2011, and most... P11400 Line 17: Fensholt (2012, not 2011) at least also suggests this might be due to precipitation and not just warming (actually they state "widely different explanations"), unless you're suggesting it's an indirect of warming, but I'm not sure we know that? I think it would be useful to add that in. P11401 Line 1: The discrepancies may also be due to different data processing and time periods considered? P11405 Line 4: I would be surprised if deep roots are the cause for grasses. Are there any studies that have looked at this for these regions – any observations of soil moisture or groundwater? Also deep roots would alleviate any effect of higher temperatures on summer NDVI that you appear to see. Line 19: I believe there are quite a number of studies looking at the invasions of non-native species in grasslands of the US? Could these help your discussion here? Table 1: is STP/PSP one variable as a ratio?

Interactive comment on Biogeosciences Discuss., 12, 11387, 2015.