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 latitude 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 summarize 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 and the trend. The results however are only discussed in terms of the drivers on IAV.

Response: Thanks for your good comments. According to your suggestion, we conducted additional correlation analysis based on trends of a climatic variable and trends of a vegetation phonological index to examine the climatic drivers that may be responsible for the long-term trends of vegetation phenology during 1982-2011. We added new results to the section 3.4 for clarity.

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.

Response: Again, based on additional analyses on the driver responsible for the trends of vegetation phenology, we revised the section 4.1 for clarity.

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

Response: Thanks very much for your constructive comments. We conducted additional trend analysis by regressing NDVI against temperature or precipitation for the study region. We have added new results including new figures in the revised manuscript. The multivariate regression analyses were based on anomalies. The purpose of developing multivariate regression models was to examine what combination and interaction of climate variables may better explain the interannual variation of vegetation phenology. As you mentioned, because anomalies also included trends, we revised related text for clarity.

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.

Response: We conducted additional analyses in the revised manuscript as you suggested. For example, we revised the last paragraph of the introduction section for clarity. We clearly stated that "the objectives of this study were to utilize the dryland ecosystems at lower elevation zones of the U.S. Great Basin (Fig. 1) to (i) quantify long-term trends in mean vegetation greenness (represented by Normalized Difference Vegetation Index (NDVI)), SOS, EOS, and GSL in the dryland ecosystems that may have occurred during the most recent 30 years of climate warming; (ii) explore the spatial variation of long-term trends in vegetation greenness; (iii) and examine the climatic sensitivity of trends and variability of vegetation phenology in the study region." New results based on additional trend analyses were added to section 3.4 and discussed in the discussion section in the revised manuscript.

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.

Response: We revised the whole sub-section of 2.4 for clarity (see the new section 2.4 in the revised manuscript). The purpose of conducting univariate linear regression analysis is to examine if temperature or precipitation by itself can explain the interannual variability of vegetation phenology during 1982–2011. The multivariate regression models based on temperature, precipitation and their interaction were developed to analyze the contribution of variation in temperature, precipitation and their interaction to variations in vegetation phenology during 1982–2011.We mentioned these in the revised section. The reason that we did *not list those multivariate regression models was because they were determined through step-wise regression analysis (i.e., they were not determined in advance. The model with smallest overall p-value was selected as the best multivariate regression model in the process of step-wise regression).*

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.

Response: Sorry for our carelessness. The NDVI data we used are in fact GIMMS NDVI3g, which can be deduced from the study period and also acknowledged in the acknowledgement section in the original manuscript. We revised related text to make this explicit.

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 significantly strengthen your analysis if you repeated the analysis with an independent dataset.

Response: Thanks very much for your good comments. Actually, we used MODIS NDVI data at the earlier stage of this study. For example, the solid redline in the following figure shows the variation of MODIS NDVI in shrub- and grass-dominated lands in the study region during the period of 2000 to 2010. In terms of NDVI trends (based on NDVI anomalies after subtracting the long-term mean NDVI), we found that there is a relatively good agreement between MODIS and GIMMIS NDVI. However, big difference in terms of the magnitudes of NDVI values may exist between MODIS and GIMMIS NDVI data for the study region. Unfortunately, the external drive that we used to store both MODIS and GIMMIS NDVI data at the early stage of this study was dead and none of these original data were retrievable. When we repeated this study, we did not include MODIS NDVI data for two reasons: First, the mismatch of time period between MODIS and GIMMIS NDVI data (MODIS NDVI started only from February of 2000); Second, although there are some discrepancies in the magnitudes of NDVI values between MODIS and GIMMIS NDVI data, the two datasets are positively correlated with each other, suggesting that the overall trends (2000 to 2010) of NDVI between the two datasets were similar. Therefore, when we started to repeat this study, we only collected and processed the GIMMIS NDVI3g data because this dataset has a longer time period allowing us to use a longer climate data. In addition, while the addition of MODIS NDVI data can help test the robustness of our research findings it may raise new questions (as you mentioned) that require further study. In order to address these questions, we would need to lengthen our manuscript and we feel that these additional analyses would be beyond the scope this paper. Nevertheless, a comparison of MODIS and GIMMS NDVI3g for the study region can indeed improve our understanding of vegetation phenological dynamics in the US Great Basin.

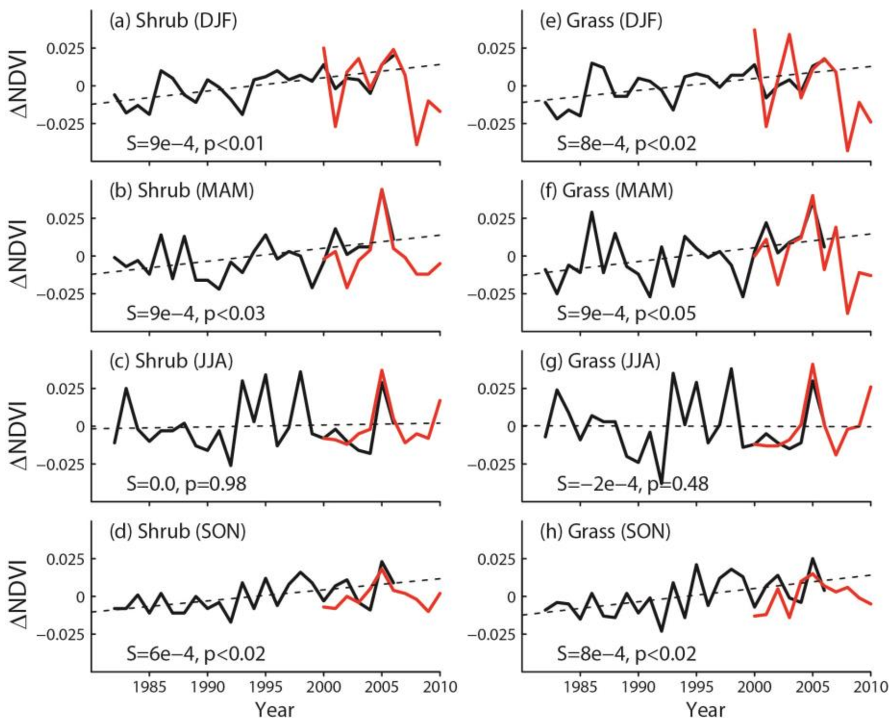

Figure 1. Example of comparison of MODIS (solid red line) and GIMMS seasonal NDVI (solid black line) data for the US Great Basin

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?

Response: We agree with your comments. Generally, before interpolating bi-weekly NDVI data into daily values, we excluded those extremely abnormal NDVI values (e.g., negative NDVI value). Because there were 4154 NDVI points considered in this study. It was hard for us to check the little bumps in each of NDVI time-series for each of 4154 points. Although a few little bumps may affect the determination of SOS and EOS at a given NDVI point, their effects on the calculation of basin-wide SOS and EOS was minimized because of the numbers of points (4154) used in this study. In addition, when using the cubic spline program to interpolate time-series NDVI values at each grid point for a given day (e.g., march 7 of 2001) we used at least more than ten continuous bi-weekly NDVI values. This approach ensured interpolated data fell within the range (between the lowest and highest NDVI values) of all original NDVI values.

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 parameter or methods of interpolation affect your results, at least to mention this qualitatively in a few lines.

Response: Thanks for your good points! We added sentences such as "although we are confident in our calculation of SOS and EOS, a validation of interpolation of time-series bi-weekly NDVI data to daily values may further enhance the accuracy of SOS and EOS estimates" in the discussion section (see section 4.5 in the revised manuscript). As we mentioned before, because *the analysis of IAV was based on basin-wide average anomalies, we believe the impacts of NDVI abnormal values were greatly minimized.*

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.

Response: We revised the captions of most figures to improve their readability.

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

Response: According to your comments, we conducted additional trend analyses and added new results to the revised manuscript.

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.

Response: Thanks. We have tried to revise the discussion section to make it easier to follow.

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.

Response: Based on results from additional trend analyses, we found that the 30-year significant positive trends in both EOS and GSL were mainly attributed to the positive trend (although statistically non-significant) of mean temperature in autumn. Although statistical analysis suggested that mean temperature in summer was significantly correlated with the variation of both GSL and NDVI in SSA, we suspect that these relationships were pseudo-relationships and have no physical meaning given that temperature in summer was strongly negatively correlated with summertime NDVI. We agree with your comments. Because NDVI in summer was significantly and negatively correlated with summertime temperature while summertime temperature had a significant increasing trend during the study period, this may partly have caused SSA temperature to be uncorrelated with mean NDVI in SSA. We mentioned such a mechanism in the discussion section of the revised manuscript.

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.

Response: Thanks for your good comments. At the first stage of this study, we have tried to summarize vegetation phenological dynamics by vegetation types, i.e., Grass-dominated lands, shrub-dominated lands and cheatgrass dominated lands (see Figure example below). We have used to NLCD 2001 data and GLCF land cover data to refine the distribution of these vegetation types across the Great Basin. However, the summary of vegetation phenology by vegetation types encountered two challenging questions: First, some scientists argued that there are no realistic grass-dominated lands in the Great Basin. Second, grasses also exist in the interspace areas of patchy shrublands. Thus, based on current NDVI data, it is challenging to separate vegetation signal of grasses from those of shrublands. Later, when we repeat this study, we ignored vegetation types and summarized NDVI only by the points considered in this study. Based on our earlier study, it appeared that in some part of the northern Great Basin where grasses might be dominant (e.g., especially invasive species cheatgrass, blue points in panel (b) in the following figure), the magnitude of changing rates in vegetation greenness during the study period were greater than those in the southern part of the study region, where shrubs are dominant vegetation types.

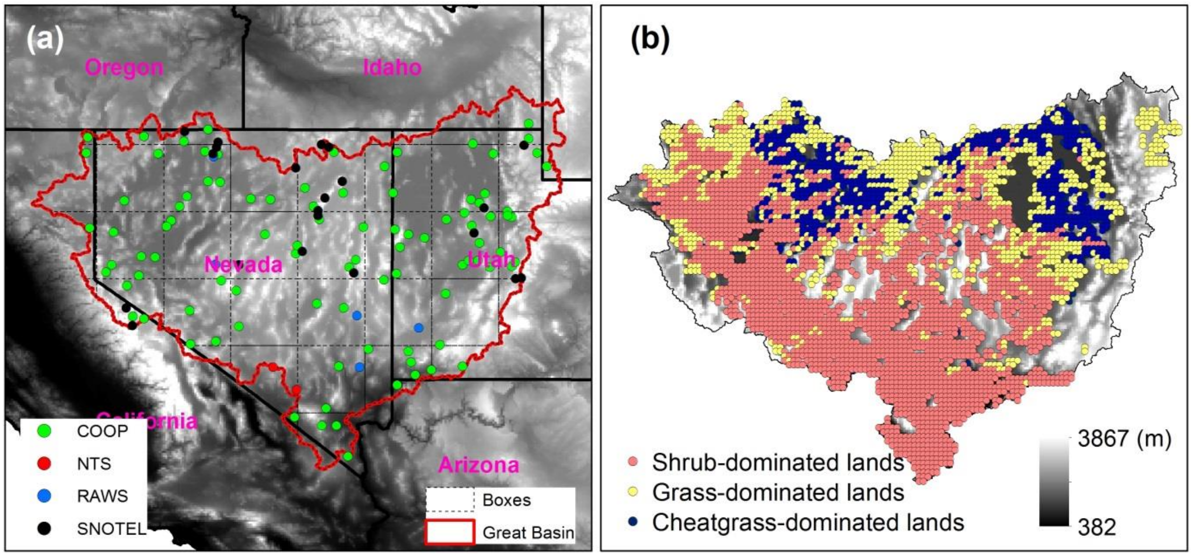

Figure. The potential distribution of shrub-dominated, grass-dominated and cheatgrass-dominated lands in the US Great Basin.

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).

Response: Similar to determining the long-term (30-year) trend, we also used the Kendall-Tau approach to analyze the trend in different time intervals during the study period. The change point was first determined by eye followed by a Kendall Tau trend analysis. We mentioned in the methods section that the Kendall-tau approach for trend analysis was also applied to different time-intervals during the study period 1982-2011.

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).

Response: We changed "biotic (e.g., forests)" to "biological" and "abiotic (e.g., water) to "physical" in the revised manuscript.

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.

Response: We added some examples in the related text to demonstrate the implications.

Line 23: See also Poulter et al. 2014 Nature doi: 10.1038/nature13376 and Ahlstrom et al. 2015 Science 348, 895-899

Response: We cited these two papers in the revised version.

Line 26: Could you give an example of the consequences for ecosystem services to provide some context.

Response: We provided two examples of ecosystem services. They are maintaining livestock and freshwater.

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 reason.

Response: Thanks for your good comments. We have tried to check but failed to find other references that might be better to cite here. Botta et al. (2000) clearly stated that "We excluded the evergreen broadleaf forest biome from our analysis as it has little or no leaf seasonal cycle". It is why we cited this paper here.

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.

Response: This is a very good but very tough question. Without additional and detailed field study, it is so hard to answer this question. The NDVI data used in this study depict the overall greenness at each grid cell. The data do not provide information about how different vegetation types affect vegetation greenness in a grid cell.

P11397 Line 21: Sentence restructure: Something like In spring 12% of the points exhibited a significant negative trend from 1982-2011, and most. . .

Response: We revised this sentence following your suggestion.

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.

Response: We added Fensholt et al, 2012 in the related citations.

P11401 Line 1: The discrepancies may also be due to different data processing and time period considered?

Response: Following your suggestion, we revised this sentence.

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.

Response: For grasses, we are not quite sure either. For shrubs, theoretically, the deeper the roots are, the more likely the plants are able to take up soil water from the saturated zone, or deeper in the vadose zone, and thus moderate the effects of droughts on foliage and vegetation greenness (e.g., Smith, S. D., Monson, R. K. & Anderson, J. E. Physiological Ecology of North American Desert Plants [Springer, Berlin, 1997; Whitford WG 2002 Ecology of Desert Systems, Elsevier Science, London, 345 pp]).

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?

Response: Yes, there were several studies focusing on effects of invasive species especially cheatgrass on vegetation phenology in the US Great Basin. We have cited some of these studies in our manuscript (e.g., Bradley, B. A. and Mustard, J. F.: 2008, global change biology). We also contacted some authors for potential vegetation data like the distribution of cheatgrass in the US great basin. However, it is challenging to separate signals of shrubs from that of grasses without field observations or more detailed image studies. In addition, there is no precise information about the distribution of invasive species in the Great Basin. These challenges require additional studies. We revised the heading in Table 1 for clarification. Originally, STP/PSP refers to the corresponding variable could be either STP or PSP.