

Responses to comments

Anonymous Referee #1

1. My main bone of contention with the manuscript is that the analysis focuses entirely on crop yields, but the results are continuously referred to as terrestrial productivity, which is a very different thing. As the tree ring increments do not show any significant relationships, and the rest of the analysis only relates to locations where crops are present, the authors should replace all reference to 'terrestrial productivity' with 'European crop yields'. I would also suggest removing the tree ring increment data from the manuscript as it does not contribute to the results and leads to a less focused manuscript.

Response: Thank you very much for your comments.

In this study we tried to perform a synthetic analysis regarding the relationship between the climate variations and terrestrial productivity using EUROSTAT crop yield statistics, remote sensing NDVI and FAPAR and empirical up-scaled GPP data. All these data streams are regarded as proxies of terrestrial productivity and we believe that the information obtained from all of these data is of interest to the potential readers.

In the results and discussion parts, we showed and compared the results derived from all these data matrices. We observed a consistent pattern in both the IAV and its climate response from all of the productivity proxies. But, we restricted ourselves mainly to the crop results in the main text to improve the clarity of our manuscript. The results from all of the other proxies are shown in the supplementary materials.

We centered our analysis on Europe cropland area (majority of central and southern Europe) because we can compare the results derived from all of these productivity proxies in a consistent spatial domain.

Following your suggestion, we removed the tree ring data and results in our revision. Finally, we really hope to keep the term of terrestrial productivity.

2. Line 15: “[we observe an] increasing sensitivity of productivity to water availability in dry regions of Europe, which is likely attributable to the recently increased IAV of water availability in these regions” This sentence is confusing. Either the sensitivity of productivity to water availability is changing (so for a fixed IAV in water availability you get an increased IAV of productivity), or the IAV of productivity is changing because the IAV of water availability is changing. Of course both could be true, but I suspect the authors have shown the latter, not the former. Please clarify.

Response: Thank you very much for your comments. In our study, we found that both the IAV of water availability and terrestrial productivity (from crop yield, NDVI, FAPAR and up-scaled GPP) are increasing in dry regions of Europe. As mentioned by you the increasing IAV in terrestrial productivity is probably attributable to the increasing IAV of soil water availability. Interestingly, there is a nonlinear response of terrestrial productivity to climate variability as revealed by an increasing sensitivity of

productivity to water availability (evaluated using moving least-squares regressions). These findings suggest that the IAV of terrestrial productivity has become more sensitive and more vulnerable to changes in water availability in the dry regions in Europe.

3. Page 17514: “Cox et al. 2000”. Cox et al., 2013 is a much better reference here.

Response: We have changed the reference.

4. Page 17514, Line 22: Richardson et al., 2007 might be a better citation here. Richardson, A. D., D. Y. Hollinger, J. D. Aber, S. V. Ollinger, and B. H. Braswell. 2007. *Global Change Biology* 13:788–803.

Response: We have changed the reference.

5. Page 17515, Line 9: “exert poorly understood, yet great effects on productivity,” consider rephrasing.

Response: We revised the sentence (please see line 35-36 in our revision).

6. Page 17516, Line 14-22: This gap-filling could be quite problematic, and it is not clear why it is necessary. Surely linearly interpolating for large gaps (<30% could mean 10 years for some time series!) biases IAV low (if the interpolation is temporal, not spatial), and produces many years whose IAV does not correlate to IAV in climate. The authors’ claim that “Our evaluation showed that this gap filling exerts minor effects on crop yield IAV (data not shown)” is not convincing. I am sure the authors had a good reason to gap-fill but this needs to be better explained here, along with how the interpolation was done (temporal vs. spatial).

Response: Thank you for your insightful comments. We agree with you that the linear interpolation for large data gaps could bias a low IAV. In the revision, we re-select the crop yield data meeting the following criteria, which are now also added to the revised version of the manuscript:

- 1. The available crop yield record should be longer than 10 years (consistent to our former criteria).**
- 2. The total length of data gap should be < 15% of the available crop yield series (at most 35×15% (~5) years).**
- 3. The length of single data gap should be ≤ 3 years (termed short gaps in our manuscript).**

The linear interpolation is a compromise to get as many sliding data segments as possible with enough data samples (i.e. 10 years in our study) in each data segment for estimating the changes in IAV of crop yield and IAV sensitivity of crop yield to climate variations.

7. Page 17517, Line 23-24: Please explain the evaluation criteria more clearly. What is sample depth, and what is an expressed population signal.

Response: Thank you very much for your comments. The two terms are commonly used

in dendroclimatology. The sample depth is the number of samples for a given year. The expressed population signal (EPS) is a statistics for deciding the agreement of sample chronology with the population chronology. Wigley et al. (1984) suggest that the part of the chronology with $EPS > 0.85$ can be accepted as reliable for dendroclimatic reconstructions.

But in our revision, we removed the tree ring data and analysis.

8. Page 17517, Line 20: *Were other alternatives to a cubic spline examined? Given the co-authors, I would have expected SSA to be a better option?*

Response: SSA may be a good option to decompose a times series and hence get the high frequency time series. Here, we applied state-of-the-art data treatment similar to the tree-ring data, which usually involves smoothing with cubic splines with specifications given in the methods section. This approach is proven to be a reliable way to remove the low-frequency signals (Cook & Peters, 1981), and its successful usage is backed by an immense body of dendroecological and dendroclimatological literature. To achieve a consistent data treatment for all carbon proxies, we used this method also to remove the low-frequency signals in crop yield and other series.

9. Page 17517, Line 25: *Is the gridded TRI dataset species specific, and therefore limited in extent to the species range? Or were southern species TRI's used to generate a gridded dataset predicting TRI's in scandenavia? That would seem to not make a lot of sense. More details on the methodology used are needed in order to fully understand what was done here.*

Response: Thank you for your valuable comments. In our revision we followed your suggestion and removed the TRI data, as you pointed that “it does not contribute to the results and leads to a less focused manuscript”.

10. Page 17518, Line 3: : : :to match the other: : :

Response: Thank you! Done.

11. Page 17520, Line 26: *“The definition of growing season for NDVIGs, GPPgs and FAPARgs : : :”. Unnecessary to state this here?*

Response: Thank you for your comments. We have removed this sentence.

12. Page 17521, Line 10: *“with 1 yr lag during”. Why is a 1 year lag assumed?*

Response: Thank you for your comments. This is really a compromise between getting as many as sliding windows and long enough time series (number of samples) in each sliding window (10 years).

13. Page 17521, Line 18-19: *“warm temperate arid zone” is stated twice. Typo?*

Response: Thank you for your reminding. We have revised the first warm temperate arid zone to warm temperate humid zone.

14. Page 17522, Line 6: *“the interannual sensitivity of the [interannual variability]”. Please*

rephrase.

Response: Thank you for your reminding. We have revised this sentence to “we estimate the interannual sensitivity of each productivity proxy to climate factor,...”.

15. *Page 17522 equation 1. Is this estimated for each proxy individually, or all proxies together? This is quite important, as this γ_{IAV} is later used throughout the text, but it is not clear how it is defined.*

Response: Thank you for your comments. In our study we estimated the γ_{IAV} for each kind of proxy. We have changed the sentence to “we estimated the interannual sensitivity of each productivity proxy to climate factor, ...”.

16. *Page 17526, Line 15: The reason for a lack of correlation between TRI and IAV of any other proxy or driver could certainly be examined closer. Babst et al., 2013 show that TRI is consistent with IAV of GPP at eddy covariance sites, so why would it not be consistent with the Jung et al., GPP, which is based on eddy covariance sites. Is it that there is no correlation, or the analysis is not refined enough to detect the correlation (i.e. correct specification of temporal intervals)?*

Response: Thank you for your valuable comments. In our revision, we followed your suggestion and removed the tree ring data and results to make our manuscript clarity.

17. *Page 17526, Line 17-23: This is not very suitable discussion, as by detrending the TRI data you eliminate this issue. Why was ring increment used instead of basal area increment? It is standard practice to use this conversion to correct for the effect of changes in tree size.*

Response: Thank you for your comments. In our revision, we removed the tree ring analysis to make our manuscript clearer.

18. *Page 17526, Line 29: Please rephrase “multi-perspectives”*

Response: Thank you very much for your comments. We revised our representation to “This conclusion is confirmed by increasing evidences from ground-truth observations and remote sensing”.

19. *Page 17528, Line 8: Again, Cox et al. 2013 is probably a better reference here.*

Response: We changed the literature.

20. *Page 17528, Line 9: Specify exactly what metrics of productivity. The statement extrapolates to TRI, for example, which is not valid.*

Response: Thank you very much for your comments. We have revised our manuscript according to your suggestion: “We observed a consistently significant negative γ_{IAV} of spring crop yield, NDVI, FAPAR and GPP to ...”.

21. Page 17528, Line 12: “the terrestrial productivity in these regions is sensitive: : :” Crop productivity. Please correct all instances of the use of ‘terrestrial productivity’ to refer specifically to what is being studied.

Response: Thank you very much for your comments. After careful consideration, we keep the term of terrestrial productivity (in detail please see our explanations for your comment #1).

22. Figure 1. The first sentence in the footer is repeated in the second. Please revise. This comment applies to many of the figure footers.

Response: Thank you for your reminding. We have revised our figure captions and make them clearer.

Anonymous Referee #2

1. *Wu et al. present an interesting analysis of Interannual variability, expressed as the coefficient of variation of climate on the productivity of crops, expressed as yields (the interchangeable use of both terms is slightly confusing, as their meaning is rather different, particularly is one realizes that yield is FAO harvested yield only). They conclude that IAV of climate, and in particular water availability explains 20-40% of the spatial variability in IAV in yield (Table 1). This leaves more than 50% for other factors to contribute, but these are hardly mentioned, in fact there is a story told that climate sensitivity is increasing and the IAV of yields shows this. While I agree, that this is what one would expect, I would also assume more emphasis on the areas where no correlations were found, and what the cause of that could be, other than uncertainty in one of the fields. I often had the feeling that the authors set out to tell a dramatic story (17515, 20) of a changing climate and its impact on productivity, but have not quite adapted to the fact that their data and subsequent analysis only partially supports that story line.*

Response: Thank you very much for your insightful comments.

1. I agree with you that it is different between productivity of crop and crop harvested yield. In this study we tried to perform a synthetic analysis regarding the relationship between the climate variations and terrestrial productivity from different perspectives, including historical EUROSTAT crop yield statistics, remote sensing NDVI and FAPAR and empirical up-scaled GPP data based on FLUXNET observations (all these data matrices are used as proxies of terrestrial productivity).

2. You are right that the climate variations can only explain 20-40% of the spatial variability in IAV in productivity proxies (Table 1). In our manuscript, we discussed about this point but we cannot give a detailed discussion because the magnitude and individual contribution (such as land use, climate regimes, etc) of these processes to the IAV of productivity remain largely unknown. But your insightful comments give us valuable insights for future studies.

3. We think a lot about the spatial heterogeneity in the IAV of multi-proxies and their climate-relations. As you pointed that many other factors besides climate variation

could contribute this spatial heterogeneity of productivity IAV. However, as mentioned above it is hard for us to separate the contributions of many different factors.

2. One particular issue that worries me was the lack of filtering for irrigation. Particularly in Southern Europe, and in drier areas in general, such filtering would be essential to take out the management component of the IAV. Aquastat/FAO also provides information on this variable I remember. This should be used to filter out the irrigated areas.

Response: Thank you for your insightful comments. Yes, you are right. The irrigation could have important effects on the crop yield changes.

Following your suggestion, we checked the irrigation area data from FAO (<http://www.fao.org/nr/water/aquastat/irrigationmap/index10.stm>). But if we constrained using this irrigated area data (even using a higher threshold, e.g. 40%), we would have only few regions (only some sparse regions in eastern and central Europe) left in our study region (in detail see figure below).

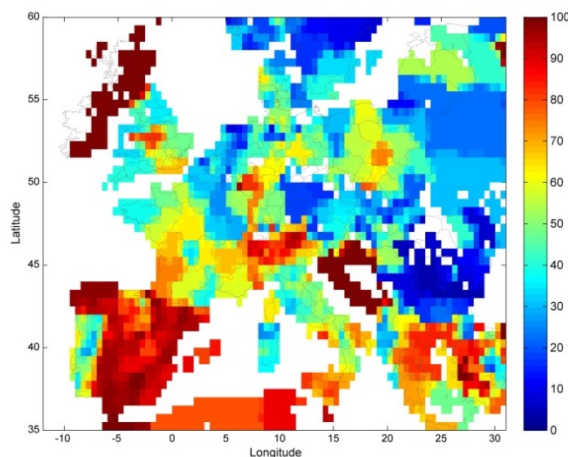


Figure note: Distribution of the irrigated area fraction of cropland in Europe.

In this study, we fitted a rigid cubic smoothing spline (3rd order) to each series of crop yield, which can, at least partly, eliminate long-term effects stemming from changes in management, including irrigation.

3. I also wonder why the tree ring exercise is presented here. I understand that forests are constrained by climate and management practices similar as those in crops, but there is a large difference between looking at annual crops and perennial trees in their response. Conversely, if the authors wanted to show that difference, they should have written another paper, now it hardly contributes.

Response: Thank you for your comment. In our revision, we followed your suggestion and removed tree ring data and results to make our manuscript clearer.

4. page 17524. paragraph 3.2 what is the difference between consistent and coherent in this paragraph. Please be more clear what you mean.

Response: Thank you very much for your comments. We have harmonized our representation.

5. What about covariance between IAV of WAI and T? One would expect from energy balance consideration that this is quite substantial.

Response: Thank you for your insightful comment. We investigated the spatial pattern regarding the covariance between IAV of WAI and temperature in our study region (see figure below).

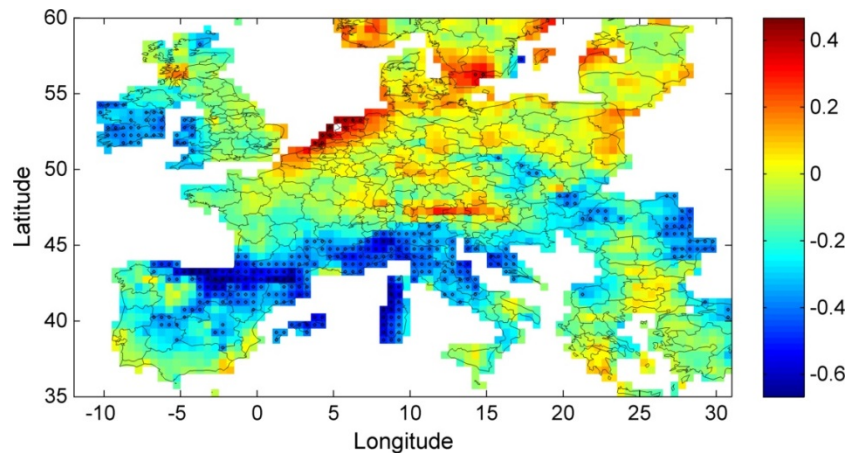


Figure note: Spearman correlation coefficients between IAV of WAI and mean annual temperature during 1975-2009. Blue points indicate a significant ($p < 0.05$) correlation.

Results reveal a significant negative correlation between mean annual temperature and total annual WAI in most of Mediterranean regions and some parts of Eastern Europe (mainly dry regions). However, in Western Europe (oceanic climate) there is not an obvious relationship between them. It implies that some other factors could modify the relationships between temperature and water availability index in such climate.

6. Page 17528. I do understand the reference to an oceanic climate regime. Is this not expressed in WAI and T?

Response: Thank you for your comments! We followed your suggestion (your comment #4) and performed an analysis regarding the relationship between temperature and WAI. Results from this analysis show that there is not an obvious relationship between them, which implied some other factors could modify the relationships between temperature and water availability index in such climate.

References

Cook ER, Peters K (1981) The smoothing spline: a new approach to standardizing forest interior tree-ring width series for dendroclimatic studies. *Tree-Ring Bulletin*, **41**, 45-53.