

Interactive comment on “Coherence among the Northern Hemisphere land, cryosphere, and ocean responses to natural variability and anthropogenic forcing during the satellite era” by A. Gonsamo et al.

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We thank the reviewer for the time she/he spent and for providing very helpful and extensive comments, which will help us to improve the manuscript. We reply below after each numbered comments.

1. In this paper, the authors selected a lot of indicators of land, cryosphere and ocean from different observational datasets to assess their changes over time, in particular focusing on interannual variability. They also analyzed the relationships with each other to show how different components of earth's climate system are responding to forc-

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ings and attributed these observed changes to natural variability and anthropogenic forcings. Major comments: The authors attempted to provide a comprehensive assessment of current changes in climate system, which is kind of a mini version IPCC report. Overall this is really a big topic because almost each part can be an individual research field. In this case, it is understandable that the analysis for each part cannot go very deep. I just have a feeling that the authors put much stuff in the paper but I am so sure what is actually “new” out of the results. Because for all changes detected, there are numerous related papers from those specialized fields that not only detected such change but also studied the mechanisms. So the authors really have to make it clear and emphasize what is new in this paper compared with other similar research, which is something I didn't get so far after reading it.

We have given extensive background on the scope of the study in the introduction. Although, the attribution of climatic conditions and detection of the anthropogenic signal based on models is now a mature discipline, two analyses still remain rare: (1) data-based detection and attribution is rare because there are not many high quality long-term observations of various climate system indicators, and (2) the impact attribution, i.e., quantitative attribution of the observed impacts to relative contributions of anthropogenic forcing and natural variability is still rare. The first is about detection and attribution of climate variables, understandably mostly temperature and the second is the detection and attribution of the observed impact on physical, biological, and human domains.

For the second aspect, the key literature (e.g., Parmesan et al. *Ecol. Lett.*, 16, 58-71, 2013; Rosenzweig et al. *Nature*, 453, 353-357, 2008; Stone et al. *Clim. Change*, 121, 381-395, 2013; Poloczanska et al. *Nature Clim. Change*, 3, 919-925, 2013) suggest that for impacts, not only are the requirements of observed data more complex and the number of influencing drivers potentially more numerous, but the attribution problem presents additional challenges, including the need to synthesise information from a much broader range of disciplines. The IPCC's Fifth Assessment Report (AR5)

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concludes that impacts of recent regional changes in climate on natural and human systems are documented across the globe, yet studies explicitly linking these observations to anthropogenic forcing and natural variability are scarce. To this regard, our study is a novel and significant contribution to literature. We will review our objective to make the focus more clear in the new version of the manuscript.

2. I don't quite understand the rational for these particular indicators chosen in this study. Why it has to be these indicators instead of others to represent land, crosphere and ocean, as well as the forcings? Any strong reasons for these indicators? And how well are they in representing land, crosphere and ocean? The authors need to explain their considerations when choosing these indicators. To me some variables are not very relevant. For example, it seems cosmic ray doesn't really matter to climate change, and I didn't see any benefits of including Stratospheric Aerosol Optical Thickness in the analysis. For land, I think there are many more important indicators like extreme events, precipitation, vegetation productivity, and hydrology variables that need to be evaluated but are missing here. For phenology, the authors only use few variables to reflect the spring phenology, while summer and autumn phenology are not included. These questions again are related to the authors' motivation and purpose.

We provided extensive rationale in the last three paragraphs of the introduction for why we selected the variables included in the current study. For example, detection and attribution study on extreme events and precipitation is rather mathematically impossible to assess to first order in a deterministic sense. Another example, spring phenology is much easier to estimate accurately and responds to first order to temperature change than the slow browning in autumn therefore we have excluded variables which may carry over large uncertainty in the assessment. We further average many of the variables to diminish random errors. Missing drivers, due to lack of high quality long-term time series, are attributed in our study to unexplained variances. We have explained all these in detail in the last three paragraphs of the introduction and we believe that any further explanation will make the manuscript unreadable. We here quote the last

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two sentences of the introduction section which give brief explanation for the use of the selected variables: "We have selected several indicators for which high-quality, long time series satellite observations, covering most or all of the Northern Hemisphere are available, and relate to temperature. This is because temperature fulfils the key assumption of detection and attribution studies where the response to external forcing is a deterministic change and to first order, and signals and noise superimpose linearly (Meehl et al., 2003)."

3. There are many terms or categories used in the paper relating to the selected variables but have never been defined explicitly. Since there are 15+ variables in the paper, so without clearly defined, it may cause some confusions especially when the authors refer to something like "forcing and response variables" "natural variability and anthropogenic forcing variables" and "teleconnection variables". In many cases, I don't know what exactly these terms indicate to.

We will revise the introductory paragraphs of section 2 to make the categories more clear, and provide the missing definitions under each variable in the same section in the new version of the manuscript.

4. The overall presentation needs to be improved before publication. One problem is that figures and tables are not well integrated into the text. The table and figures are very informative but it seems a lot of information has not been conveyed effectively into text. For example, there isn't much discussion about Figure 3 in the text. Tables and figures are complex with lots of numbers and curves, so the meanings are not easy to interpret directly. And because of this, in many cases, I got lost when the authors refer a sentence to a figure. When I look back at the figure/table, I don't know from where the authors' statement gets its support. One solution is to explicitly describe the key features or patterns in table/figure and directly referring them in the text.

We will directly cite the panels and figures of each table and figure in the text of the new version of the manuscript.

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5. Minor comments: P2 L2-4: Hasn't the IPCC report provided enough quantitative evidence to attribute observed change to human and natural forcings? For future climate projection, the differences among models are substantial, but models do pretty well in simulating historical changes. I have seen a lot of attribution studies so from my perspective they are not rare. Maybe the authors should be more specific on this point.

Please see the answers to question 1 for this suggestion.

6. L16 and L17: What aspects of human and natural systems have these studies looked at in regard to the climate change impacts? It is better to provide direct information or a summary from these papers, because listing only the author names has very little practical meanings to readers.

We will provide a summary of each cited paper in the new version of the manuscript.

7. L30-33: Please explain how the variations in solar radiation and cosmic rays can influence global climate trends.

We will provide further explanation in the new version of the manuscript.

8. P3 L12-17: What are the considerations for choosing these particular indicators rather than many alternative indicators? For example, for land indicators, any particular reasons for not including vegetation greenness or productivity? And for phenology, why only spring is included? Since the title is about coherence, does that mean any indicators that exhibit inconsistent response among land, cryosphere, and ocean responses are naturally excluded in the analysis? What about these inconsistencies?

Please see the answers to question 2 for this suggestion. For example vegetation greenness or productivity as estimated from the growing season integrated NDVI may be affected not only by temperature but also precipitation and drought regimes in summer time unlike the spring phenology estimate in the northern hemisphere which mostly responds to changes in temperature. All the indicator variables we selected respond to temperature at least to the first order. Temperature fulfills the key assumption of de-

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tection and attribution studies where the response to external forcing is a deterministic change and to first order. We explained this in the last paragraphs of the introduction section

9. For section 2, it is better to have a summary table including all these variables, their categories, with additional information (e.g., gridded or station data, sources, time span, etc.). When introducing each variable in the text, group them into proper category that is consistent in the following content, such as land, ice, ocean, response, forcing, natural or anthropogenic factors.

We agree with the referee comment. We will make a summary Table of in the new version of the manuscript as suggested.

10. L20: Please point out the location of Point Barrow since not everyone is familiar with this place. Also for Kiel station. Giving their latitude and longitude would be enough.

We will provide the geo-location information for both stations in the new version of the manuscript.

11. L27: Please define satellite era.

We will define the satellite era in perspective of the starting year for continuous full global coverage, i.e., 1979, in the new version of the manuscript.

12. L31: Missing reference for SSM/I data? Every dataset needs to come with a source.

We will provide references for each dataset in the new version of the manuscript.

13. P4 L10: Why only use the flower bloom day of Canada? Phenology has quite large regional difference. I am not sure if the flower bloom day of Canada is a suitable indicator for the entire northern hemisphere. How many stations are there and what is their spatial coverage?

Canada-only integrated spring indicator helps to study the impact of natural variability;

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particularly the North Atlantic Oscillation (NAO) and the Scandinavia Pattern (SCA) which have a contrasting impacts on vegetation activity on the North American and Eurasian parts of the circumpolar region (see Fig. 4). We will provide further details on the flowering phenology data in the new version of the manuscript. The cited reference (Gonsamo et al. Sci. Rep., 3, 2239, 2013) provides details of the datasets.

14. L29: Undefined acronym TOPEX, VIRGO, SOHO, ACRIM.

We will provide the full names in the new version of the manuscript.

15. L23-29: What is the spatial coverage of RAD? The total solar irradiance we used is a point measurement usually acquired using instruments like sun photometer, luminosity oscillation imager.

16. P6 L15: Why anomalies are calculated only for winter and why trends are removed here?

Teleconnection indices are already anomalies from long-term normals, the normal period being always updated to reflect the background (for example externally forced changes in sea temperature or atmospheric pressure). We use the common approach, where winter, defined here as December of the preceding year and January, February and March of the current year, because (i) most of the leading teleconnection indices are only active during the northern hemisphere winter, and (ii) they are indicator of the climatic regime to come during the ensuing growing seasons (e.g., Gonsamo et al. Glob Change Biol. doi:10.1111/gcb.13258; Gonsamo, A., and Chen, J. M. Proc. Natl. Acad. Sci. USA, 112, E2265–E2266, 2015). The long-term sum effect of the natural climatic oscillation on climate variables should be zero. Therefore, we remove trends to enforce this zero sum impact for the data period we studied for each variable. We will provide this explanation in the new version of the manuscript.

17. Table 1: I felt Table 1 is difficult to understand. Perhaps Table 1 can be reorganized in a way that variables are grouped into response and forcing variables, or other

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meaningful categories. I don't understand what different shades actually mean here. Adding a new row and a column to specify the name of each category is helpful. Also, I don't understand how the number in italic bold font represents both long-term and interannual co-variability. Some of correlations make very little physical sense. For example, it shows 63% interannual variability of temperature can be explained by spring thaw, while 29% interannual variability of spring thaw can be explained by temperature. Even 63% is higher, but it has little physical meaning because we know it is temperature variability that drives spring thaw but not the opposite.

The shades indicate different categories of variables such as temperature, biosphere indicators, greenhouse gases, and natural variability. We have defined the category of each variable in the first two introductory paragraphs of section 2. We will add category column and row in Tables 1 and 2 in the new version of the manuscript as follows: temperature, land indicators, cryosphere indicators, ocean indicators, natural variability, and anthropogenic forcing variables. We will also provide a separate Table to explain each variable (see answer to question 9).

We will also modify the table caption as follows: "Table 1. Percent interannual (lower left) and long-term (upper right) variances in indicator A explained by indicator B or vise versa."

The table is a simple square of the Pearson correlation coefficient matrix from correlation analyses based on detrended data (lower left) and raw data (upper right) as such A does not necessarily drive B or vise versa. The italic bold font represents a pair of correlation for which both long-term and interannual covariability shows statistically significant relationship. This way we partially avoid spurious correlation from further discussion and interpretation if there is no correlation both for raw and detrended datasets.

From the examples you given above for relationships between spring thaw and temperature, temperature and spring thaw have R² of 0.29 (29%) from detrended correlation

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analysis and 0.65 (65%) from raw data analysis. We add the signs (- or +) to show the direction of relationships.

18. For analysis section in P7: How did the datasets with different temporal periods treated in the correlation analysis of interannual variability and trends, by using the overlapping period?

Each interannual analysis is done based on detrended datasets using the common period of pair of variables. We will add the following text in the Analysis section in the new version of the manuscript: "All interannual covariability assessments were done based on detrended time series at annual time scale using the common base period of each analysis.

19. P7 L5: Have the response and forcing variables here clearly defined earlier in the manuscript?

We have already defined the category of each variable in the first two introductory paragraphs of section 2. We will move the peak-to-trough amplitude (AMP) into land indicator category and temperature as a free variable in section 2 introductory paragraphs in the new version of the manuscript. Please also see answers to question 9 and 17.

20. L15-17: How many PCAs are selected? Maybe Table 2 should be referred here.

As presented in Table 2, 3, and Fig. 5, different numbers of PCAs and different outputs of PCA were used in each analysis. We think citing results section in the method section may add confusion. We provided detailed captions for each table and figure to explain how the PCAs are used throughout the manuscript.

21. L24-25 I don't understand the meaning of "temperature mediation"? This has been frequently mentioned in the paper but I didn't see any explanations prior its appearance.

We will add explanation in the new version of the manuscript. The term "temperature mediation" refers to the impacts of forcing and natural variability on the indicator

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variables through changes in temperature (as opposed to changes in precipitation, radiation and associated variables such as cloudiness, humidity, ...). For example, in table 3, the explained variance derived from a PCA and stepwise regression analyses in column 4 include the temperature mediation by that the explanatory variables include radiation, teleconnection and temperature. On the other hand, column 3 gives the results of explained variances by radiation and teleconnection without temperature mediation.

22. P8 L3: Such four categories should be defined or mentioned earlier in dataset sections.

Please see answers to questions 9, 17 and 19.

23. L12: It obvious that both RF of WMGHG (steady rise) and temperature (with fluctuation) increased through time (1980-2010), I don't understand where the "highly correlated" come from.

We will modify the sentence as follow in the new version of the manuscript: "During the study period, the radiative effects from the increased WMGHG concentrations follow the rise in global surface temperatures (all $p < 0.1 \times 10^{-7}$), whereas the solar irradiance is not and has an overall declining trend (Fig. 1(b)).

24. L21-25: Which exact number in Table 1 is referred to support "significantly correlated" in this sentence "ST and SOS are also significantly correlated with temperature after data detrending (Table 1) indicating both long-term and interannual covariability ($p < 0.01$)". Because there are two sets of numbers: A explained by B and B explained by A, I don't know which one of them is the case here. According to Table 1, there is no significant correlation between FEB and T, but in figure 2(b) they show some kind of co-variability. Why do these two places show inconsistency with each other?

Please see answers to questions 17 for this suggestion. The interannual covariability is given in the lower-left and the long-term covariability in upper-right sections of Table 1.

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The exact number would be 39% and 44% for interannual and long-term covariability, respectively. We just figured out different software such as R, EXCEL, EXCEL with VB gave slightly different p-value results for the slope significance for linear relationship with two tails particularly related to decreasing the decimal of p-value into two digits ($p=0.05$) for deeming significant due to cut-offs without proper rounding. This affected FFB because the actual p-value was close to threshold ($p=0.05$). The effect on others is minimal because the p-values are either much smaller or larger than the threshold. We have recalculated all p-values and there is no long-term relationship between FFB and temperature as correctly shown in Table 1. We will correct this in Fig. 2 in the new version of the manuscript.

25. Table 2: Please add full name of each variable. It is very hard to remember these acronyms since too many of them are contained in the paper.

We will add the full name of each variable in caption footnote in the new version of the manuscript.

26. L30-31: I don't know how this statement comes out of Table 2. And again, what is the temperature mediation? Without explaining this term in the beginning, I cannot quite follow the rest of this paragraph.

Please see answers for question 21 regarding definition of temperature mediation. The statement on L30-31 is from Table 3 not Table 2. Table 3 provide the explained variance of land, cryosphere and ocean indicators by natural variability and anthropogenic forcing with and without temperature mediation calculated from the detrended data.

27. Figure 4. It seems the growing season annually integrated normalized difference vegetation index (NDVI) appears here but is not chosen as an indicator. Why is that?

We use the integrated NDVI only to discuss some of the peculiar results. For the reason why we have not included the integrated NDVI in the main analysis please see answers to question 8.

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28. Table 2: Please clearly define land, cryosphere and ocean "indicators", and natural variability and anthropogenic forcing "variables" throughout the paper. It is unclear from the table that which variable belongs to what category, especially when they are referred in the text.

Thank you for this comment. We will define the variables in the new version of the manuscript. Please also see answers to several of your suggestions above related to this.

29. P11 L13: I suggest listing the "several explanations" one by one (first, second. . .) for clarity.

We will provide the explanations one by one as suggested in the new version of the manuscript.

L26: By what criteria these variables grouped into three sets are considered to be coherent for their interannual pattern?

We will expand the explanation in the new version of the manuscript. Basically, Table 2 gives us the first results of coherence assessment in light of total variances by the variables considered in this study (well unexplained variances from missing drivers remain). Variables which load on the same axis are coherent. Table 1 gives further support whether or not the loadings in Table 2 are statistical artefact of projection into a low dimensional subspace, by that the variables should at least show either or both long-term and interannual covariability which is statistically significant. We will add this explanation in new version of the manuscript.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2015-92, 2016.

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