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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Linking drought indices to impacts to support drought risk assessment in Liaoning province, China" *by* Yaxu Wang et al.

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Manuscript nhess-2019-310 "Linking drought indices to impacts to support drought risk assessment in Liaoning province, China" – Point by point response to referee 3 comments

We thanks referee #3 for the feedback to our manuscript. The comments and suggestions are particularly useful for us to revise the manuscript. Based on the suggestions, we have added the definition of drought and compared with other related studies. We emphasized the link between historical droughts studies and this manuscript, and added more explanations on how we did the quantitative vulnerability studies and how this method can be used in other regions. We have responded to each comment in

turn below in bold.

General suggestions

The first suggestion is regarding the absence of a clear drought definition. The sentences in the Introduction [R36-37] are insufficient in describing what kind of 'numerous droughts' China has experienced, and how this study is related to drought studies in China or globally. The specific naming of the 2000-01 event and the frequent occurrence of drought [R118] calls for a rigid definition of a drought. Later in the Introduction, it only becomes clear that this study focuses on meteorological and (soil moisture) agricultural droughts. In my opinion, this should have been stated earlier and clearer.

Thank you for your suggestion. We will extend the drought definition in the Introduction to clarify these points in the revised manuscript. We will also add some explanation of the kind of drought China has experienced, and also add relevant literature to explain the relationship between this study and other related drought studies.

In addition to this, the manuscript gives little explanation of previous meteorological or agricultural drought events, even though multiple authors have described droughts in China on both national and regional level (Wu, et al. 2001; Zou, et al. 2005; Leng, et al. 2015, Xiao-jun, et al. 2012; Wang, et al. 2016). It would be beneficial to the manuscript to explore the link with previous studies and build on other national-scale drought studies to claim further implications of this study. For example, the presented dataset seems unique and unpublished, although the term 'China water resources bulletins' in Xiao-jun, et al. (2012) suggests that there are multiple sources of drought impact data. I would suggest that acknowledging of these relevant studies, as it helps to rightly place this new study in context of previous research and thereby support the claim of further implications of this study [R97-98 and R364-366]. In lines R364-366, it is stated that the method could be applied to other areas, although it remains unexplained how to do so. Results in Figure 3 and 4 suggests that the linking between drought impact data and climate indices is fruitful despite the large climate variability.

The results show the strong relation between SPEI6 and Drought suffering area (DSA), SPEI6 and drought impacted area (DIA), and yield reduction and NDVI. These relations could be explored further in the Discussion section (R364-366), if a rigid drought definition is applied and the findings are related to relevant studies. That would increase the outreach of the developed method and would therefore benefit the manuscript significantly. In other words, I would strongly recommend to 1) provide a definition of the studied drought events, 2) relate them to past events –strengthen objective 1- and 3) link the findings to other drought studies in China to show the relevance of this study. Given the current structure of the introduction, I would expect that these suggestions would strengthen both the first, second and sixth paragraph [R87-91].

We thank the reviewer for these comments and we fully agree with him on these points. As the reviewers said, readers will have a lot of confusion without a clear definition of drought. Therefore we have added the definition of drought in the introduction. We explained what kind of drought China has experienced, and also added relevant literature to explain the relationship between this study and other related drought studies.

We have added the relationship analysis with the previous related studies in nation scale, such as the historic drought events, drought indices, and how this method can be applied in other regions. The details are as follows.

‘ In China, many indices were used for types of drought monitoring, such as Palmer drought index, SPEI, SPI, China-Z index, relative soil moisture, vegetation indices and remote sensing indices (Hong et al., 2001;Wang and Chen, 2014;Wu et al., 2012;Yanping et al., 2018), which found that serious drought events occurred in 1972, 1978, 1991 , 1999, 2000 and 2006. Based on previous drought studies, SPI, SPEI, soil moisture and NDVI were selected in this research. ’

‘The methods used here can be applied in other areas to better understand drought impacts and drought vulnerability, since similar data (e.g. drought impacts, meteorological data) can be collected in other regions.’

In addition to suggestion 1, I would suggest to include relevant drought studies in China that have explored a meteorological index (Wu, et al. 2001), agricultural droughts (as referenced) and water resource management strategies (Xiao-jun, et al. 2012). The current overview given in paragraph 6 does not reflect the full spectrum of relevant studies, hence I would strongly suggest for a thorough review of relevant studies in China to emphasise the link between previous studies and these findings. These studies have also performed analysis using multiple sources of information and could therefore strengthen the second paragraph in the discussion R321-335

We thank the reviewer for this important comment. We made some comparison with other related studies in data, method and results. According to your suggestion, we have added the comparison of relevant literature in the introduction and discussion section.

‘In drought monitoring, the index selected in this study is similar to the method in Leng et al. (2015), where the SPI, standardized runoff index (SRI) and standardized soil moisture index (SSWI) were selected to assess droughts from meteorological, agricultural, and hydrologic perspectives. In terms of drought impact data, Xiao-jun et al. (2012) collected drought affected and damaged area, losses in food yield from China water resources bulletins, which is the secondary data.’

‘The above results are also in general agreement with Hao et al. (2011), their study used a higher temporal and spatial resolution for drought impacts. It collected 10-day affected crop area data to assess drought risk in China at county unit. Their result shows that West Liaohe Plain has a high risk, northwestern part of Liaoning province are located at West Liaohe Plain.’

This is consistent with existing research by (Yan et al., 2012;Zhang et al., 2012), which established a drought risk assessment index system to assess drought risk in northwestern Liaoning. In Zhang et al. (2012), indices such as precipitation, water resources, crop area, irrigation capacity and drought resistance cost are used to mea-

sure drought risk, result shows that high drought risk was identified in Fuxin, Chaoyang and Shenyang.

The second suggestion concerns another definition; the use of the term vulnerability and the vulnerability assessment. In the Introduction, the relationship between drought indices, impact, and vulnerability is mentioned [R73-74], although in that same paragraph there is very little background given on the term 'drought vulnerability' or the chosen approach of this study. Later in the manuscript, R147-150, it becomes evident that vulnerability factors are related to agricultural productivity. It would strengthen the claim of 'developing a drought vulnerability evaluation' [R97], if the choice of vulnerability factors was justified earlier in the manuscript, perhaps supported using relevant literature to drought vulnerability.

We will add more background information on drought vulnerability as a term, and improve the definition in the introduction. As for the vulnerability factors, as most of the impacts available for Liaoning Province relate to agriculture and the rural economy, we selected the vulnerability factors to reflect this, also taking guidance from the studies of Junling et al., 2015 and Kang et al., 2014. We will add this information to the revised manuscript.

Also we added more explanation in how do we quantitatively assess drought vulnerability.

The vulnerability factors themselves (Table 2) require some additional adjustment in my opinion. Currently, these factors do not relate to normal conditions, or below-normal conditions, i.e. drought conditions. The standardisation in R215-220 shows that vulnerability factors are a ratio that is relative to the maximum amount measured for an unknown time scale. It remains unknown how these factors are measured or would change over time and since these vulnerability factors are not given as a reduction from normal conditions, it remains unclear to the reader how they represent vulnerability. Without the full understanding of the vulnerability factors, the impact of Figure

8 is limited, as these vulnerability levels do not indicate vulnerability as such, solely a reduction from the maximum number. For example, it remains unclear what 'most vulnerable to' implies in Figure 8, and more explanation is required to understand which factors are in or excluded for which cities. If so, it would require some more explanation regarding the rationale behind these 'most vulnerable to' factors. Once the vulnerability factors are converted into a deviation from the long-term mean (or however a drought is defined), the combined effect of these factors would become clearer. I do not expect the results to change, although the factors will and potentially show the deviation from the mean (or normal) conditions and therefore emphasise the change during droughts. The results

might show an amplified effects, which will help to strengthen the claim in R288-289. Along the same lines, I would also change the PHD, NLH and DELA into a percentage or ratio that relates to normal conditions. In the conclusion, relatively strong statements in R288-289 suggest that there is increasing drought vulnerability. However, from Figure 8 or Figure 7, it remains unclear how the vulnerability changes in Liaoning province, and these suggestions might aid the general analysis of the vulnerability factors.

Thank you for your comments. The manuscript may not be clear here before. The vulnerability factor is relative static to a specific city, which is the characteristics of the city. The maximum value refers to the maximum value among 14 cities in Liaoning Province, not the maximum value of a city for a period. In this paper, we ignore the changes of vulnerability for a period time, mainly emphasizing the difference of the vulnerability factors between cities.

We assumed that these factors was static for a period of time and that are collected by local government.

For each city, we analyze the relationship between vulnerability (measured by types of drought impacts at the same drought severity) and vulnerability factors to explore the contribution of vulnerability factors to each type of drought impacts.

For example when SPEI6 is equal to -1.5, the regression results show that yield loss due to drought is 5 thousand ton in Chaoyang whilst it is 1 thousand ton in Huludao. It means that in the term of the yield loss due to drought, Chaoyang is more vulnerable than Huludao.

Thank you for your suggestion using the percentage of the drought impacts. It would be better if drought impacts are display with percentage. However some drought impacts are difficult to convert to percentage, such as economic losses [0.1b], it's difficult for us to get a value to be divided to obtain the percentage. Similarly, due to the total number of livestock is not available in each city, we can't get the percentage too. Above all, it is difficult to show the impacts in the form of percentage.

The third suggestion is regarding the varying time scale of the multiple datasets. The presented data and analysis combine multiple datasets of varying quality and sources into one product. That in itself is a fine bit of work, although I would suggest to show the applied time scale in the correlation analysis and in the random forest modelling. It is not a major concern, but it would strengthen the manuscript to frame a defined study period that matches all data analysed in the correlation analysis, i.e. 1990-2013. In R191-192 and in R211-212, a short statement is written regarding the limitations of the soil moisture data and the NDVI data. Perhaps, an additional note regarding the applied study period is best written here.

Thank you very much for your suggestion. We agree with the reviewer that we need to add the period. In the method section, we have added the period of time series for each analysis.

For consistency, I would also emphasise the applied time period for the random forest algorithm (as introduced in the third section of the Methods). In the current manuscript, the applied time period remains unknown for the Random Forest algorithm. In fact, to enhance clarity, a brief summary of the work of Bachmair, et al. (2016) would be beneficial for readers that are less familiar with this algorithm. Again, minor adjustments in

the text would enhance the understanding of applied methods and therefore improve the manuscript.

We will add some more background to the approach in the revised manuscript and as mentioned above clarify the time periods over which the random forest analysis was conducted. We have added some explanation of MSE%, also we've added an example to explain the MSE% to make it clear.

Last correction I would suggest is the text along with Figure 5. In the figure, the coloured matrix gives the mean squared error in percentage. Firstly, I would strongly suggest to adjust the colour scheme to allow a non-experienced reader to see the difference between positive and negative percentage changes. Secondly, the change in MSE % suggests given a certain impact factor changes the error. If I read it correctly in R209, the change shows how much the accuracy decreases given the effect of the variable. This can be explained better than just one line of text, as a positive change in MSE% would imply not more MSE, but a more accurate model. Given the colour scale and the limited information available, the findings are somewhat hidden in this Figure despite the quality of the work. Hence, I would argue to change the colour scale accordingly and elaborate more in the text, i.e. give some examples.

We thank the reviewer for this comment and we fully agree with him on this point. According to reviewer's suggestion, we tried other color schemes, including blue, gray, brown, etc. to highlight the difference between positive and negative values. Finally, according to the visual effect and other references, we changed the color scheme.

Specific comments

Regarding the aggregation of impact data to an annual time scale, I would suggest to dedicate a short paragraph in the Discussion [R340-349] to show if results change for a multi-year drought (2000-01) or for a one year drought (2009). You might be better placed to identify example drought events, but it would strengthen statements in R334-346.

Thank you for your suggestion. We have added some explanations about the difference between the results of multi-year drought and single year drought.

The NDVI results show both positive and negative correlations. In lines R334-335, it is stated that this could be due to diversity of land cover, but given the detailed vulnerability factors, I would assume that there could be a more elaborate answer to these correlations. It would strengthen the discussion section to highlight some of correlations to plausible explanation regarding, e.g. land cover, change of cropping, use of perennial crops, etc.

We thank the reviewer for this important comment. According to reviewer's suggestion, we will add some detailed explanation. In other studies NDVI is mainly used to identify vegetation (agriculture) impacts. In this research, affected human and livestock are also collected to measure drought impacts.

Given the large spatial and temporal variability in precipitation [R108-110], it would be relevant to indicate the difference in water resources in addition to the variability in precipitation. The current annual average volume [R114-115] might not be relevant to drought conditions or vulnerability to droughts. The deviation from normal (annual average conditions) is relevant for drought research, how these droughts relate to the already water stressed areas might be detected by the climate indices.

We agree with the reviewer that the spatial and temporal variability in water resources need to be detected, we added the distribution characteristics of water resources in Liaoning Province.

The skewed distribution of water resources might play a part in the results of the DSA and DIA. It would be useful to indicate the deviation from mean, or the difference in source of water, rather than the amount that is available [R336-339]. In 358-360, the source and diversity of water sources is again linked to the vulnerability. This statement could benefit from an example case, where the source or variability in water resources indeed increased the vulnerability, as your results show.

We thank the reviewer for this important comment. We have added some examples the difference of water sources between NLH and PHD. To illustrate the importance of different water sources as you suggest.

Change the layout of Table 1 so that the vulnerability factors are easier readable. This would shift the focus from being on the spatial variability (which would be better shown in a map than a table) to the different vulnerability factors

We thank the reviewer for this comment and we fully agree with him on this point. It would be better shown in a map than a table. We have tried to plot a map to display the vulnerability factors. There will be ten maps (one for each type of drought impact), more space needed for these maps. Also the threshold of each type of drought impacts need be identified. Therefor we used table to show the vulnerability factors.

Depending on the applied drought definition (see general comment 1), mark this in Figure 2 to show the identified droughts. That will make it easier for the reader to deduct how the authors come to their findings in R128.

We agree with the reviewer and we added the definition of drought as suggest. In Figure 2, we use the SPEI as an example to illustrate the historical drought situation in Liaoning Province.

Change the current volumes and amount in [0.1b] yuan of drought impact in percentages. For a reader that is not familiar with current production levels in Liaoning province, it is hard to grasp the loss of 1.89 million tons, or the impact of an economic loss 1.87 billion yuan when the normal conditions are not provided [R120-121]

Thank you for your suggestion. We agree with the reviewer that it is more readable using percentages. Some drought impacts are difficult to express as a percentage, such as economic losses [0.1b], it's difficult for us to get a value to be divided to obtain the percentage. Also, due to the total number of livestock is not available in each city, we can't get the percentage value. As similar with Yield loss due to drought. Above all,

it is difficult to show the impacts in percentage since “normal conditions” means there is no drought occurred with no drought impacts.

Repeat the abbreviations in Table 1 in the text and perhaps in Figure 3,4, and 5. The abbreviations are used throughout the result sections, but are only fully explained in Table 1. I would suggest to repeat the abbreviations in the text to enhance the readability. For example, include (DI) in R124 and (SDI) R218. Same for the vulnerability factors NLH and PHD [R223]. It would be better to first write them full, before abbreviating even though these are given in table 1

We thank the reviewer for this comment and we fully agree with him on this point. For Figure 3, figure 4 and figure 5, we have added the full name of the drought impact rather than abbreviations to enhance the readability.

Also we have used the full terms in Discussion and Conclusion when it is first appear.

Need to support claims in drought mitigation strategies (e.g. sinking(?) more wells to enhance resilience to drought) R362-363.

We agree with the reviewer and we have added more drought mitigation strategies.

Could the authors clarify that the drought vulnerability map [R361] is indeed Figure 8?

Based on the results of the vulnerability analysis, figure 8 shows which cities have a higher vulnerability to which drought impacts. It displays that which city is vulnerable to what kinds of drought impacts.

Other than in the abstract (R29-31), no findings are related to future applications for other regions in China. Please revise the abstract, as these statements cannot be supported given the current manuscript.

We thank the reviewer for this comment and we revised the abstract as suggest.

In R124 the meteorological data is introduced, I assume that this data is obtained from all stations in Figure 1, please indicate which stations were use, or refer to the figure in

R124. The same holds for the soil moisture data in [R129]

Thank you for your suggestion. We added explanatory text to explain all the sites in Figure 1 were used.

Explain the difference between the applied SPEI using the log-logistic probability distribution (Yu, et al. 2014) [R165-166] and the often used method of Vicente-Serrano, et al. 2010).

Thank you very much for your suggestion, we changed the references.

Timeframe in R231 is 1990-2013 not 2016. Or, perhaps there is a mistake in the Figure 3 legend

Yes, thank you for your suggestion. We have corrected it.

Rephrase line 158-159

Yes we rephrased the sentence to make it clearer.

Rephrase line 286-288

Yes we have rephrased the sentence to make it clearer.

Rephrase line 314-316

Yes, the sentence has been corrected and made clearer.

Add ‘of RF’ in R356

Corrected.

Rephrase line 358-360

Rephrased.

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