Review Wang, et al. 2019 –NHESS

The work of Wang, et al. presents not only an extensive and unique drought impact dataset, but it also shows how these impacts are linked to climate indices and how that relates to vulnerability to agricultural droughts in North East China. The presented case study shows that drought indices and impacts are linked throughout the region despite the large climate variability. Considering the novel application of linking these impacts and indices to a Chinese case study, I consider this work to be novel and its findings should be published in order to further ongoing drought research. However, I would suggest the following comments to improve the manuscript. My main suggestion is regarding the definition and use of drought terminology (see general suggestion 1 in attached PDF). Given the vague drought definition in the present manuscript, it is unnecessarily difficult to compare this research with other drought studies in North East China. For example, the droughts identified in the first result section are not compared to meteorological or agricultural droughts of national scale. Relatively minor changes to the manuscript could emphasize the link between historical droughts and therefore increase the outreach of the research.

The second section of the Results, the drought vulnerability results, shows that some cities are more vulnerable to agricultural drought than other cities. This result is extremely valuable and underlines the potential for furthering the developed methodology. Furthermore, the evaluation of drought vulnerability in Liaoning province also shows that the developed method can be applied despite large differences in climate. This also strengthens the general thought that the research is highly valuable and applicable to other regions in China. It is, however, not entirely clear how the vulnerability assessment would be applicable to other regions (given the available data). Improving this would strengthen the use of the developed methodology. Hence, a few minor changes in the phrasing of the term 'vulnerability' would aid to the general understanding of the study approach and applicability. Furthermore, adjustments regarding the vulnerability factors would strengthen current findings and align these with previous drought research.

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. 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].

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.

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.

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.

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.
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.

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.

Specific comments

The following section contains some specific comments to the text or figures:

There are four statements that specifically require an example to illustrate the statement:

- 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.
- 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.
- 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.
- 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.

The following specific comments are meant to increase the readability of the manuscript and might require a quick confirmation of the authors or adding of reference in order to avoid misunderstanding of terms.

- 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.
- 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.
- 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].
- 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.
- Need to support claims in drought mitigation strategies (e.g. sinking(?) more wells to enhance resilience to drought) R362-363.
- Could the authors clarify that the drought vulnerability map [R361] is indeed Figure 8?
- 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 can not be supported given the current manuscript.
- 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].
- 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).
- Timeframe in R231 is 1990-2013 not 2016. Or, perhaps there is a mistake in the Figure 3 legend?
- Rephrase line 158-159
- Rephrase line 286-288
- Rephrase line 314-216
- Add ‘of RF’ in R356
- Rephrase line 358-360

Additional references:

Leng, G.; Tang, Q. & Rayburg, S. Climate change impacts on meteorological, agricultural and hydrological droughts in China Global and Planetary Change, 2015 , 126 , 23 - 34

Wang, L.; Yuan, X.; Xie, Z.; Wu, P. & Li, Y. Increasing flash droughts over China during the recent global warming hiatus Scientific reports, Nature Publishing Group, 2016 , 6 , 30571

Wu, H.; Hayes, M. J.; Weiss, A. & Hu, Q. An evaluation of the Standardized Precipitation Index, the China-Z Index and the statistical Z-Score International Journal of Climatology, 2001 , 21 , 745-758

Xiao-jun, W.; Jian-yun, Z.; Shahid, S.; ElMahdi, A.; Rui-min, H.; Zhen-xin, B. & Ali, M. Water resources management strategy for adaptation to droughts in China Mitigation and Adaptation Strategies for Global Change, 2012 , 17 , 923-937

Zou, X.; Zhai, P. & Zhang, Q. Variations in droughts over China: 1951--2003 Geophysical research letters, Wiley Online Library, 2005 , 32