Response to comment of Referee #1

Please find in Black the reviewer's comments and in Blue our responses.

Comment: "Dear authors,
many thanks for your highly interesting submission to HESS/NHESS. I'm always glad to see some added value/application off the EDII.
Overall the manuscripts reads very well and is embedded in the current state of literature. Nevertheless, I do have some major points of critique I would like to discuss:"

Reply: Dear Veit Blauhut, we thank you for reading our manuscript and for your suggestions on how to improve it. Below, we provide answers to your comments and hopefully we have updated the manuscript to address your points of critique.

Comment: "Data:
One of your first take home messages is "We encourage drought impact information to continue to be collected and given importance in future drought impact studies". I'm sorry to say, but this is nothing novel. All publications with or on EDII data claim this...which brings me to my major point of critique."

Reply: We have now replaced the message "We encourage drought impact information to continue to be collected and given importance in future drought impact studies" with "Drought impact information from this database has already been successfully linked to drought hazards and shown to have potential for impact forecasting (Blauhut et al., 2015, 2016; Stagge et al., 2015; Sutanto et al., 2019; Bachmair et al., 2015, 2016b); it also proves to be useful here when assessing links between different drought indicators and impacts" (see lines 482-485).

Comment: "the version of EDII data you applied is quite outdated (I guess you downloaded the online version at EDC). Besides well known reporting biases in space and time, you missed recent major drought event(e.g. 2017/18). Therefore your data is likely lacking representativity (?).You should invest some time to update the Spanish case (you might contact ruth.stephan@hydrology.uni-freiburg.de or me for assistance and an actual offline version of the EDII). Furthermore, I'm afraid you did not do an EDII-data quality check of the Spanish case? Since 2015 we did learn quite a lot of how to use the data (more below)."

Reply: We carefully consider your point that the version of the EDII data we used is somewhat outdated. However, we would like to note that although the more up-to-date version includes the recent major drought event in 2017-2018, it is not publicly available. In addition, we find that conducting a quality control of a new data set is out of the scope of a revision. While impact entries have been added to some regions of the data base, however, Spain has not been amongst them and correct classification, duplications and typos have not been checked for Spain (communication with Ruth Stephan). For these reasons, we believe the use of the original data base is more appropriate here. We did a comparison of the common period and both data sets are very similar.

In the revised manuscript we now mention that the version of the EDII data used is somewhat outdated and that it misses a recent drought event. See lines 561-564 in the discussion section.

Comment: "From the methodological site, I appreciate your work using RF. Nevertheless I do have remarks to consider. Furthermore, some ideas to upgrade (?) your paper and to move from "showing a method which has been done already for the case of Spain" towards a little bit more."

Reply: We agree that the aim and what our study did was to similarly reproduce a method that showed success in some regions to another region. We believe that such a study is useful in a region where droughts cause severe impacts. By following your suggestions, we have upgraded our paper. Our study also examines the impact of teleconnection patterns, which to our knowledge has not been carried out before.

Comment: “The EDII data is a collection of impact reports which were attributed to 1 of >100 impact types. These impact types are categories to 1 of 15 impact categories. Accordingly, these impact categories pool sometimes very different drought types together! E.g. agriculture (1.2 – Reduced productivity of permanent crop cultivation; 1.7. Regional shortage of feed/water for livestock, 1.8 others). These impacts occur at very different stages and/or types of drought. Furthermore, “others” can be anything related to drought. Forestry for example might be “impacts on mushroom harvest” but also reduced tree growth/ dieback of trees. Again, very different effects needed to cause these impacts.

The method to “simply use the impacts categories (as Stagge et. all and Blauhut et al. did) is not as easy and requires “a dive into the data” to maybe re-categorise impact types or not to use all impact types within a single category. Anyway, I cannot at all recommend to use all impact categories together. Yes, Bachmair et al. did it, BUT you will get a way better signal if you do it category separated. You will probably not be able to use all, only the ones with good data, and monthly data, but this will make way more sense this way. Also, you should consider the “logic” of impact occurrence with regard to their nature (time of occurrence and duration). E.g. agricultural impacts can only seldom have a beginning and end by month. Normally, harvest is “weighted” ones a year. There is not “drought impacts on grain occurred from May to July). Its only quantified ones! Of cause, impacts on meadows might occur three times a year (at least in Germany). In contrast, impacts on hydropower production can of cause have a clear timestamp on it (low flow from may – September). Hence – this is another reason why you cannot merge the impact categories.”

Reply: We are aware that aggregating all impact types to a single category was not an optimal choice, however, the impact data we use is of a small size, in comparison to these other studies mentioned. We have plotted sector-specific drought impact occurrences in Fig. 1 below. Each sub-region shows three or less types of impact categories, except for the S region that shows eight (this can also be seen in Fig. 1 of the manuscript). For this reason we think that a sector-specific analysis is not appropriate here, since there are not enough data points to investigate categories separately. We now explain why a sector-specific analysis is not conducted in lines 265-268 of the revised manuscript.

To consider the logic of impact occurrences, we visually examined impact reports and their descriptions. We observed that most agricultural and livestock farming impacts lasted more than one month, as you mention (see Fig. 1 in this document). Most of the impact reports that lasted several months reported estimated economic losses for the period considered due to reduced productivity during that period. Example of a report: “livestock farming economic losses are estimated in 393,72 million euro for the period November 1, 2004 - April 30, 2005.”

Our method of quantifying reports therefore assumes that the period during which economic losses are reported approximately represents the period during which the sector is affected by drought impacts. If harvests are weighed once per year, and we created an impact occurrence only for that month, we would not be considering that the sector is actually impacted for a longer time. Hence, we believe creating impact occurrences during the reported periods, as we do, is a better way to quantify impacts on these sectors.

It also seemed ‘logical’ to quantify impacts as impact occurrences for most of the remaining impact types. Other impact types that were reported to last several months included: freshwater ecosystems, terrestrial ecosystem and public water supply. We also believe that the method used to quantify these impacts was ‘logical’, since impact reports describe impacts that are mostly long-lasting for these sectors. Here are some examples of impacts on these sectors that lasted several months:

- Drying up of lakes
- Satellite data showed increase of the hydric stress of the vegetation and/or a period where a degree of water stress in vegetation was registered to have increased
- Death of fish due to toxic waste discharge and minimum river flows
- Villages depended on water import via trucks
- Water restrictions/bans that took place.

Furthermore, we checked that impacts that should usually do not last more than one month by nature, were reported like so. These were: wildfires, air quality, human health and public safety. The reported duration of these impacts was one month for all of them.

In the revised manuscript we now discuss this in lines 251-259, we add: “We visually examined impact reports, specifically their durations and descriptions, to make sure that the quantification of impacts (impact reports to DIOs) was logical. Most of the quantifications seemed sensible, since the duration of impact reports agreed with their description and was logical with regard to the nature of the impact. However, quantifying impacts on agricultural and livestock was the most complex, since most of the impact reports reported total economic losses over a period of time. Translating total economic losses over a period of time into impact occurrences is challenging, however, we assumed this period of time represented the period during which the sector was most affected by drought impacts. We suggest that to better study drought impacts on these sectors, future studies should use alternative measures of impacts, for example, time series of crop productivity or economic impact data from insurance companies (e.g. Sainz de la Maza and Del Jesús, 2020). However, we believe using impact occurrences to quantify these impacts still captured the duration of drought impacts well”.

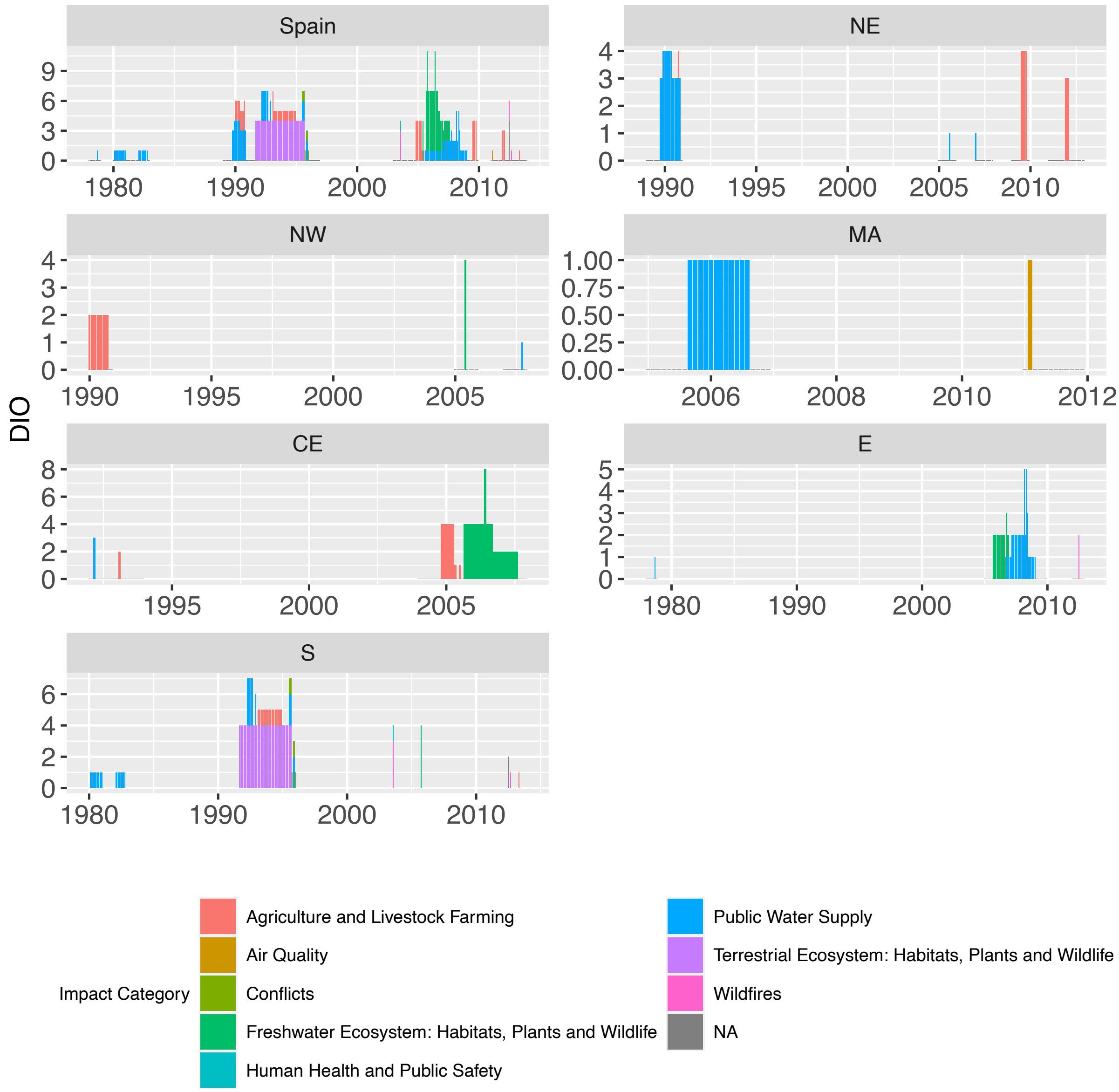

Figure 1. Sector-specific drought impact occurrences (DIOs) total for Spain and for each NUTS-1 region.

Comment: “Using total numbers of impact reports is ambiguous- but possible. Nevertheless, the strong bias over time (1976- 2020) has to be considered somehow. Reporting culture and media has changed dramatically since. à you might consider also testing on binary signals, rather than totals.”

Reply: We acknowledge this and we now point this out. We already tested binary signals in the RF analysis when we included a classification model (see lines 282-284 old version of manuscript). The classification model classified impact occurrences into the two categories: “impact” or “no impact”. In the revised manuscript we now mention that including a binary signal time series in the RF models (classification type) also considers the fact that the reporting of drought impacts might have changed over the years due to improvements in reporting and data collection because it excluded a potential bias in DIOs (see lines 302-306).

In the results section, we now also mention that both the regression and classification (binary signal) RF models showed similar performance when compared to each other (Sect. 3.2.2). Also, as discussed in Sect. 3.3, the predictor importance results were also very similar (see Fig. 2 below to confirm this). Therefore, we conclude that a potential bias in reporting culture over time does not seem to affect our RF results (lines 438-440).

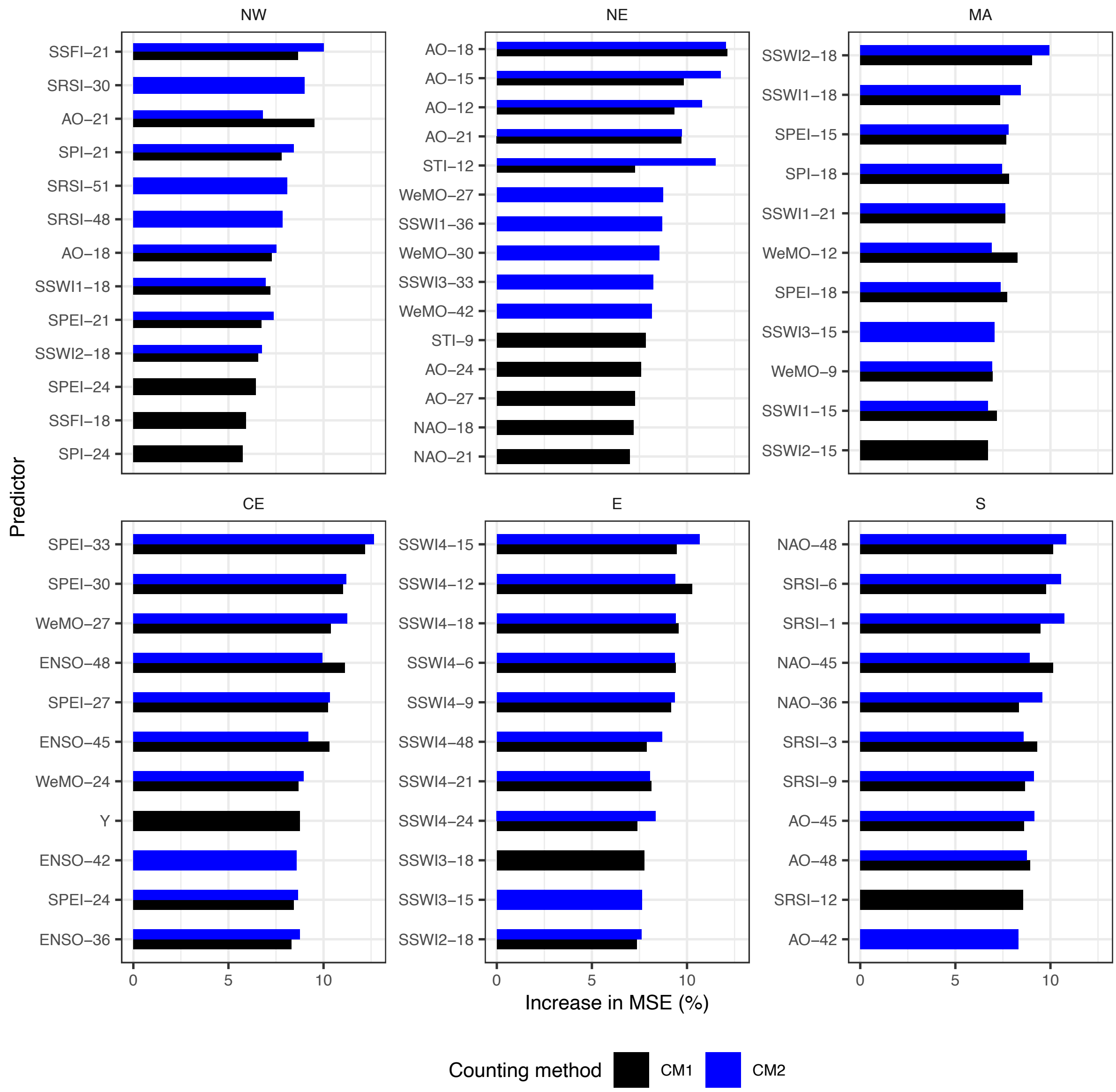

Figure 2. Predictor importance when using the **classification** RF models and the two most censoring counting methods. The top 10 predictors for each counting method and sub-region are shown.

Comment: "Since you are using RF, I would like to consider some more factors which could increase your model performance. Blauhut et al (2016) included vulnerability factors as determinants (vulnerability factors) to explain LIO beyond the hazard. Including such increased model performance for most cases!"

Reply: We have now added the following vulnerability factors as impact predictors in the RF models:

- Public water supply
- Unemployment rate
- Gross value added by industry (except construction)
- Gross value added by agriculture, forestry and fishing
- Gross value added by all NACE activities
- Population density
- GDP per capita

And landcover data from the Corine land cover data set:

- Agricultural areas
- Artificial surfaces
- Forest and seminatural areas
- Waterbodies
- Wetlands
- Arable land
- Artificial, non-agricultural vegetated areas
- Forests
- Heterogeneous agricultural areas
- Industrial, commercial and transport units
- Inland waters
- Inland wetlands
- Marine waters
- Maritime wetlands
- Mine, dump and construction sites
- Open spaces with little or no vegetation
- Pastures
- Permanent crops
- Scrub and/or herbaceous vegetation associations
- Urban fabric

The data has been obtained from the Instituto Nacional de Estadística, Eurostat and the Corine land cover data set. Most of these factors have been reviewed and tested as drought vulnerability factors by Blauhut et al (2016). The data was available for yearly timesteps, except the Corine land cover data, which had data at six-year intervals. The data was linearly interpolated to create monthly time series.

Because data for several of these factors was only available starting from the year 2000, we had to reduce the time period for this analysis. We run the RF models for each sub-region over the years 2000-2012. We first built the RF with the vulnerability factors as the only predictors. We then built models with both the vulnerability factors and all the drought indices. We then compared the model performance of these model runs. We could not compare the results from our original analysis since that analysis was done over a longer time period (starting from the year 1975).

Regions that had very few DIOs (NW and S regions) were not considered in this analysis. The RF regression results, showed similar (although slightly higher) RMSE values than the original RF models. The R^2 values were also very similar to the original results. The overall effect of including the original drought indices in the models was hard to determine since there seemed to be no clear effect. For the classification models, the models with drought indices (in addition to vulnerability factors) show a slightly better model performance for most regions.

We have added a new figure (Fig. 12) in the revised manuscript which shows the predictor importance plots for RF models that included all the original predictors and the new vulnerability factors. The NE region, has the AO as a top predictor and MA shows SSWI as a top predictor. These predictors are also top predictors in our original results. However, the top drought index predictors for the rest of the regions were different to our original results. This questions the robustness of the results obtained here; our models shows different predictor importance results than the models built using the full time period.

For this reason, we have not included the details of this analysis in the revised manuscript. Instead, we have added a paragraph discussing that "including drought vulnerability factors when modelling drought impacts has shown to increase model performance (Blauhut et al., 2016), however, because most of the vulnerability factors studied here (e.g. GDP per capita, GVA by industry except construction, by agriculture, forestry and fishing, and by all NACE activities, public water supply and unemployment) were only available starting from the year 1999 or 2000, models built with these factors were not as robust as the rest of our models. Therefore, we cannot assume that the results found would reproduce themselves for the rest of the study period." (see new Sect. 3.4 'Drought vulnerability analysis').

We also add: "However, from an exploratory analysis, we find that vulnerability factors, in particular, the landcover types: 'forest and seminatural areas' and 'agricultural areas', and factors, such as, unemployment rate and GVA by industry (except construction), do increase the accuracy of the models when they are included in especially two regions (CE and E). Also, the exclusion of the drought indices does not substantially decrease the model performance, in both the regression and classification Random Forest models. Therefore, we conclude that including drought vulnerability factors, in some cases, does seem to improve the accuracy of some models. This is shown in the the variable importance results (Fig. 12). Regions that had very few DIOs (NW and S) were not considered in this analysis" (see lines 467-473 of the revised manuscript).

Comment: "With regard to model quality I please you to also present the Area under ROC curve characteristics, as Stagge, Sutanto and Blauhut did. A cross-correlation is not expedient."

Reply: We now also present the area under the ROC curve (see new Fig. 8). We also explain why we preferred to use precision and recall measures in lines 321-323. The dataset was imbalanced, i.e., there were few impact occurrences in each timeseries. This caused sensitivity values to be very high (since the models tended to predict less DIOs than those that occurred) and hence produce very high AUC values. We discuss this in lines 399-402 of the revised manuscript.

Comment: "Furthermore, you could use your novel, self-investigated data to test your model?"

Reply: We believe that the impact data used in this study is sufficient to test our models here. However, we discuss (in the discussion) the limitations of the impact data set used and suggest that future studies consider using different drought impact data sets (lines 561-566).

Comment: "By now your work is only a copy of... if you would separate the impact types as Blauhut et al 2015/6 and apply your indices, and forecasting... this would become a more beneficial contribution. Furthermore, you could consider to include catchment specific characteristics (vulnerability factors) such as is the catchment managed (yes/no), how many reservoirs ....a lot of opportunities to consider and I would be happy to discuss about."

Reply: After making all the suggested changes we think we provide enough new results to warrant publication and that we are not just reproducing similar studies for a different region. We also believe our study provides new and useful results for drought predictions for Spain.

Comment: "Some minor comments on text and figures can be found in the PDF."

Reply: We have made the minor modifications suggested in the revised manuscript.

- The descriptions of how the counting methods were created is now in a table format (see new Table 1).
- All graphs in Fig. 2 now have the same scaling.

Comment: "To wrap up, I highly appreciate your work! But you should take the chance to give it a bigger meaning for the community. Thus, the first step would be to investigate more impacts and then 2nd you should consider to re-categorise the impact reports, only use specific impact types (e.g. with high counts of reports) or maybe use different prediction models in comparison (e.g. zero inflated models?). Anyway, a big added value to the community would be a comparison of such.

Please feel free to contact me for an open discussion or assistance."

Reply: We very much thank you for your review and also highly appreciate it. We hope that with the modifications done, our work will now have a bigger meaning for the community. We have considered all of your comments very carefully.

Response to comment of Referee #2

Please find in Black the reviewer's comments and in Blue our responses.

Comment: "**1 General Comments**
The manuscript investigates the link between meteorological drought indicators and drought impacts and, based on that, further attempts to predict drought impacts in a modelling study. While I find the basic idea behind the study intriguing, I think the manuscript is not particularly well executed in terms of structure, logic and intelligibility. It was at times not easy to follow the read thread and to grasp what had been done methodological. But my two main concern relate to the underlying research questions and justifications for this paper as I further explain hereafter in the specific comments. Technical comments and corrections are included further below."

Reply: Dear Claudia Teutschbein, we thank you for your review. We appreciate your time spent and all of your suggestions and critiques. We hope we have addressed all of your comments and suggestions.

Comment: "**2 Specific Comments**
2(a) Link between drought hazard and impacts
"Trying to make a link between meteorological drought and climate indicators (i.e., the hazard) to the actual impacts, which – as the authors themselves state – are "highly dependent on a region's vulnerability to drought" (line 27), without properly discussing the conceptual frameworks for vulnerability and the importance of exposure (the latter term is not once mentioned in the manuscript) is a major flaw in this study. Exposure is related to the tangible entities exposed to the hazard and can be made up of buildings, people, livestock, crops etc. Vulnerability on the other hand, is the susceptibility of a system to be negatively impacted by the hazard. Consequently, impacts will not be reported to EDII just because there has been a drought hazard, but only if a certain region/economic sector or ecosystem has actually been exposed to the hazard and is actually vulnerable.

This issue becomes apparent in the results of the study where two of the regions (NE and CE) show lowest correlations with the drought indicators (line 311). When comparing Figure 1 of the manuscript to a population density map of Spain, these two regions clearly are least populated (i.e., less exposure), which might have affected the number of reported impacts."

Reply: We realise that we did not discuss the importance of vulnerability and exposure. In the revised manuscript we have added a new section 'Drought vulnerability and data sets', that discusses drought risk as a function of hazard, exposure and vulnerability and defines these terms (lines 199-203). We also explain that we have introduced vulnerability factors as additional drought predictors in our analysis (suggestion of Referee #1). In the correlation results section (lines 342-344), we now mention that indicator-impact correlations may also be "affected by a region's exposure to drought. This could explain why we find the weakest hazard-impact correlations in two of the least populated regions (NE and CE), since impacts are reported by a region only when it has been exposed to the hazard and has been vulnerable to it."

Comment: "To round up, similar attempts have been made by other authors and e.g. Sutanto et al. (2019) particularly suggest to "consider the vulnerabilities and exposure of the impacts in each [...] region, which can provide a good measure for drought impact forecasting". In addition, Blauhut (2020) states that "the single use of impact information has to be seen critical. The information on past impacts merely proxies past vulnerability to drought. It does not inform on potential drivers of vulnerability nor provide an actual state of the present situation. **Accordingly, the impact forcing driver besides the hazard, namely vulnerability to drought, has to be integrated to drought risk analysis**".

Reply: We agree with this statement, we cannot imply that what happened in the past will reproduce itself in the future, especially because factors such as exposure and vulnerability are not modelled in our study.

As suggested by reviewer #1, we have added vulnerability factors as impact predictors in the RF models (see new Sect. 2.3 and 3.4) and investigated differences in model performance when these factors are included. We also investigate the predictor importance of these factors in the models. This integrates drivers of impacts that are not only the hazard in our models, as you mentioned. However, as discussed in the new manuscript, the time coverage of the data used was not long enough to include it in our general analysis.

We now also incorporate in the discussion that future studies should "model exposure and vulnerability (in addition to drought hazard) to understand how future drought risk will change (Blauhut, 2020)" (see lines 566-567).

Comment: "**2b The potential of indicators as predictors for drought impacts**
The authors tested the suitability of different indices to be used as predictors for drought impacts and argued that it takes about 15 to 33 months for droughts to cause impacts (though this number depended on the index under consideration). So, if I understand correctly, in order to calculate drought indices that can be useful to reliably predict impacts, one would need sufficiently long records (i.e., 15-33 months of data). Thus, in practice, I wonder how useful it will be to "predict" potential impacts with help of these indices? I would argue that a region will already suffer from severe impacts after more than 1 year of drought conditions and that – after having lost some harvests or after reaching certain thresholds of low groundwater or reservoir levels – there is no added value of starting to look at the data of the past 15-33 months to try to predict the already ongoing impacts... To me that is in fact the nature of droughts, i.e., that they are considered "creeping disasters" with slow onsets and difficult to predict their magnitude and impacts. Therefore, the real question here still remains: **How can we use drought indices over short(er) periods of time to predict impacts of ongoing and potentially much longer droughts, if the study results suggest that only long-term data can actually be used to predict them?** I guess this is somewhat of a chicken or egg dilemma."

Reply: This is correctly understood, our study shows that in order to calculate drought indices that can be useful to reliably predict impacts, one would need data of the past 15-33 months (for meteorological indices) in this region. The study results show that long-term data can potentially be used to predict impacts. To predict impacts of an ongoing drought, one would use forecasts and combine these with data of the past months to make impact predictions.

Performing a sector-specific analysis (if a data set allows) would probably yield different results. Impacts on some sectors might be related to drought indices at shorter accumulation periods. For instance, Sutanto et al. (2019) found that shorter accumulation periods (1-3 months) were best for agricultural impacts and longer accumulation periods for water-borne transportation and water supply. This could mean that if future studies (that have a greater availability of impact data) investigate relationships between indicators and impacts, they might find linkages at shorter timescales for specific sectors. Also, because drought impacts respond to hydrological droughts and deep soil moisture droughts, using hydrological and agricultural drought indices to predict impacts would require data for a shorter period of time. We now state that "our results indicate that if we want to predict drought impacts at short time scales, we should use hydrological and deep soil moisture drought indices" (lines 528-529).

Comment: "3 Technical Comments/Corrections

Page 6, line: 164: where did the evaporation data come from, how was it calculated/measured?"

Reply: To calculate the SPEI, a calculation of a simple climatic water balance (Thornthwaite, 1948) is required. This is calculated using the monthly difference between precipitation and potential evapotranspiration (PET) at different time scales. We the approach of Vicente-Serrano et al. (2014) to calculate the PET; this is a simple approach that only requires data for monthly-mean temperature. We now make this clear in the lines 153-156 of the revised manuscript.

Comment: "Page 6, line 184: The climate indicators receive only very little attention in the methods, while they are discussed in much more detail in the results. It is actually easy to overlook their short description in the methods part. They should be explained in more details, especially what their abbreviations mean and why they might potentially be relevant as drought predictors"

Reply: The climate indicators and their abbreviations are introduced in lines 81 to 92 (old manuscript), in the introduction. We have now moved the explanations on why they were chosen to the 'Methods and data' section of the revised manuscript to avoid them being overlooked (lines 167-173). Their abbreviations are now in lines 82-84.

Comment: "Page 8, censoring methods: CM1 and CM2 seem too similar to me. To me they are not separate censoring methods, because they actually do not "censor" the given problematic cases in different ways, they simply imply a different way of counting the DIOs."

Reply: We realise this and in the revised manuscript we now do not describe CM1 and CM2 as different censoring methods. We only use this term to compare them against CM3 and CM4.

Comment: "Page 9, Figure 1: colour choices are not optimal, there are too many similar colours that are difficult to distinguish."

Reply: We now use more distinguishable colours in the revised manuscript.

Comment: "Page 13, line 309: perhaps emphasize that drought indicators are actually negative in case of droughts, which would explain the negative correlations"

Reply: We have now added this explanation to this line (line 327).

Comment: "Page 13, line 311: NE and CE (the two regions I highlighted above to have the lowest population density) clearly stick out in terms of correlations. The authors should discuss potential reasons for that. Perhaps a closer look at different sectors could help (e.g., less agriculture, less industry in these regions? Or simply less people to notice any impacts?)."

Reply: As mentioned above, we now discuss this in lines 337-338.

Comment: "Table 2a/Table 2b and Figure 10: as mentioned above, I think CM1 and CM2 are too similar and, thus, demonstrating the results only for those two cases is not showing the full picture. Especially in Figure 10, it would be very easy to add additional bars (with different colors) for CM3 and CM4"

Reply: The correlation analysis showed that generally, the less censoring counting methods showed lower correlation strengths. Also, most significant correlations (with drought indices) disappeared in the two regions (NW and NE) (see Fig. 5 of manuscript). For this reason, we focus our results on the two most censoring counting methods. A fuller picture of the predictor importance is shown in Fig. 1 of this document. The figure shows disagreements in the results for regions E, NE and MA. We believe that the addition of CM3 makes the figure more complicated to read. We therefore keep the original figure in the new manuscript but have moved the explanation as to why we chose to exclude the results of CM3 and CM4 to the results section ‘Predictor importance’. We now also briefly discuss the results of CM3 and CM4 to show a fuller picture (lines 454-456).

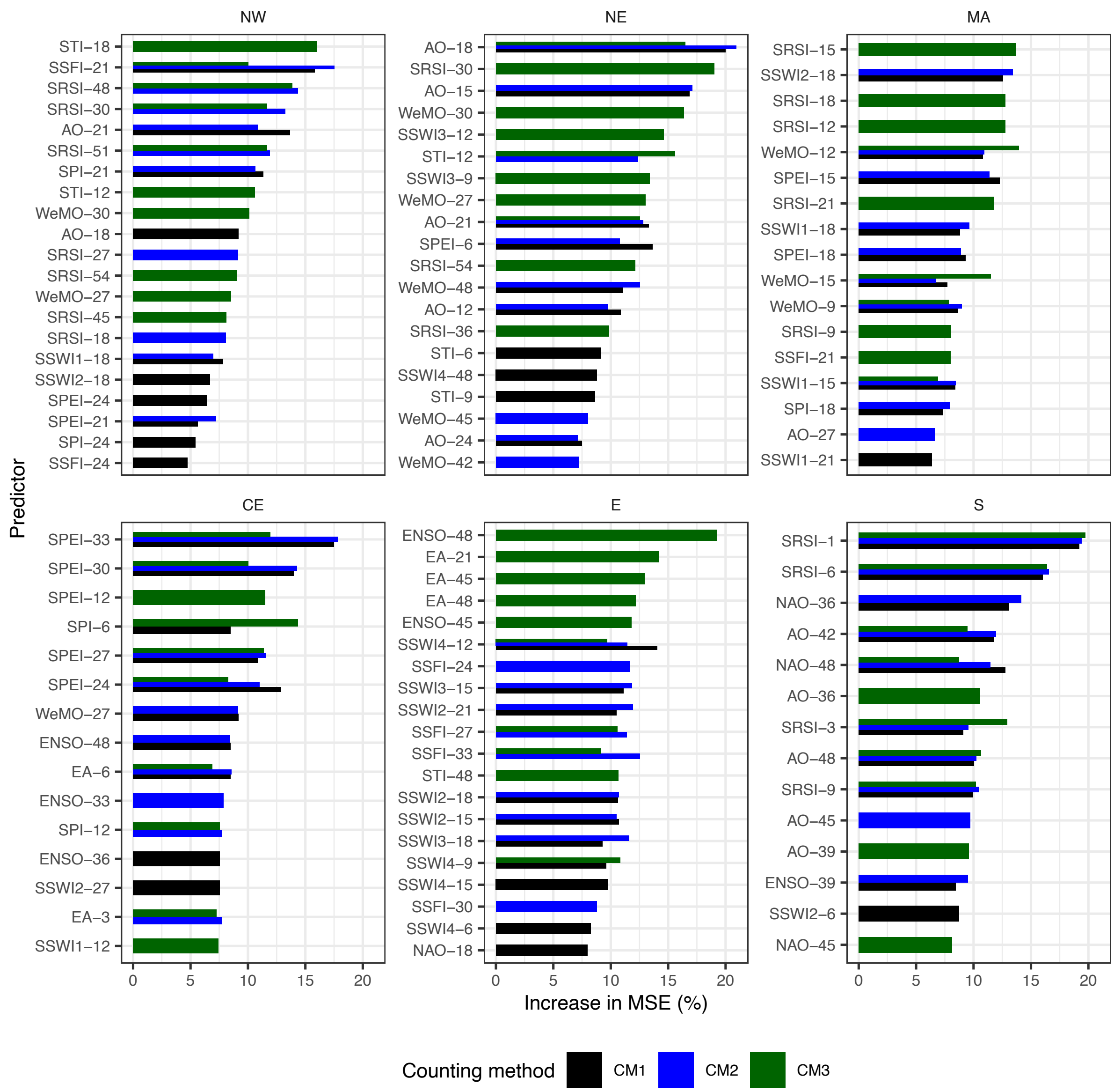

Figure 1. Predictor importance when using the regression RF models for different counting methods. The top 10 predictors for each counting method and sub-region are shown.

Comment: “Page 25, lines 479-481: The statement of drought impacts responding fast to hydrological or soil moisture drought is directly implied by the drought propagation chain suggested by Van Loon and Laaha (2015).”

Reply: We now mention the drought propagation chain suggested and cite these authors in the statement (see lines 527-528).

Comment: "Page 25, line 491: you cannot be sure that agriculture and livestock farming were the sectors suffering from most impacts, but you can rephrase to that these two sectors were "most frequently reported to be affected". So, my point is that there is a difference between actually experiencing impacts and looking at a list of reported impacts, which could be highly biased."

Reply: We agree and have now rephrased this (see line 539).

Comment: "Page 26, line 514: authors state that due to the reduced sample size of impact reports, the analysis did not include an investigation of sector-specific impacts. However, the authors include Figure 1, which explicitly lists the impacts by sector and different regions, which becomes somewhat obsolete if there is no subsequent analysis of the different sectors."

Reply: Although we were not able to conduct a sector-specific analysis, the figure helps visualise which and how many sectors were reported to suffer most impacts in each region. It also helps visualise why a sector-specific analysis was not conducted (all except one region show three or less types of impact categories, therefore, there are not enough data points to investigate categories separately). Also, we reference this figure when stating that the sectors with most frequent impacts reports are sectors that depend on reservoir systems for storing water, and that this could explain why it takes a long time for precipitation anomalies to propagate to impacts. We believe that because of these references to the figure, it is worth keeping in the new version of the manuscript.

We thank you very much for your comments again. We believe that your suggestions have improved the quality of the manuscript.

References:

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