Response to review of “Why increased extreme precipitation under climate change negatively affects water security” submitted to *Hydrology and Earth System Sciences* for consideration for publication.

We warmly thank the reviewer for the positive and constructive review of our manuscript. Below we provide a response to the concerns and explain which revisions were implemented and why a certain approach was taken. All changes are indicated in the document with indication of track changes.

Referee #2

I have read with interest the manuscript by Eekhout et al. and I believe its subject fits with the content of HESS-D and that its findings are relevant for regional to global scale hydrological impact studies. Eekhout et al. present a comprehensive model study for a sub-humid to semi-arid basin in SE Spain focusing on the effect of increased rainfall intensity on water security for four scenarios of climate change. Strong points of the study are the breadth and coherence of the modelled effects, the use of state-of-the-art climate scenarios, the inclusion of climate change uncertainty and a formal treatment of their outcome in terms of robustness and significance. On these grounds, I’d recommend this manuscript for publication provided several corrections and improvements are made.

We thank the reviewer for his nice and constructive comments on the manuscript. Below we have responded to the specific comments raised by the reviewer.

At its core, I have two problems with the manuscript. First, there is no formal definition of water security and this is expressed in different manners but the relevance of the metrics and their relation to water management are not expressed. I’ll address this in more detail below.

In the Introduction we have defined water security “as a condition in which the population has access to adequate quantities of clean water to sustain livelihoods and is protected against water related disasters (UN-Water, 2013).” Water security is not a metric in itself and, indeed, it may be interpreted in different ways. In the manuscript, we use four indicators to quantify water security in the Segura River catchment. In other regions water security may be quantified using different indicators. Therefore, we have added the reasoning why we choose to use these specific indicators to section 2.4.

Second, the study is thorough in its modelling setup and analysis proper but lacks a clear quantification of the effect of increased extreme precipitation. I concur with the authors that this is important in this environment for runoff generation and that changes will have an effect but the exact nature of these changes are not investigated whereas these are important and the effects not necessarily straightforward. Also, I believe that low flows are essential to ensure water security and this is not mentioned or analyzed at all. At the moment the hypothesis is formulated but not fully underpinned and insufficient quantitative analyses are done to isolate the effect of extreme precipitation on water security convincingly.

In the present study we do not aim to isolate the effect of extreme precipitation. However, we aim to apply state-of-the-art future climate projections and take into account all projected climate changes, such as an increase of dry spells and temperature. While the increase of extreme precipitation may be one of the most *relevant climate signals, we think that the other climate signals are equally important. By combining all these changes in one comprehensive study, we show that climate change may have significant impact on water security. While an additional sensitivity analysis to isolate the effect of extreme precipitation may be useful, such analysis have already been performed in similar environments and show that runoff and soil erosion are indeed sensitive to extreme precipitation (Pruski & Nearing, 2002; Nunes et al., 2009). In the revised manuscript we refer to these studies in a new paragraph, where we discuss the multiple impacts of extreme precipitation.*

Many studies include low flows as an indicator for water security, however, we argue that plant water stress is a more direct indicator for arable and natural land uses. Plant water stress takes both evapotranspiration and soil moisture into account and is crop/plant specific. Low flows, however, only capture the amount of water that is eventually routed to the reservoirs and thus fails to link low water supply to the areas where it has the most impact, i.e. arable and natural land uses. Low flows may be important for river catchments where navigation is an important economic activity, but here that is not the case.

In addition, the study has a number of weaknesses that need at least clarification and probably improvement and that I group per category:

Climate and climate change: As we know the rain in Spain does not fall mostly in the plain. Looking at the elevation within the catchment, I am curious to what degree orographic effects are captured by the downscaling of the climate models. This is important as downscaling reproduces the climate but not necessarily the extremes of precipitation in terms of depth, frequency and persistence when compared to the historical period. For this reason, hydrological impact studies in the ISI-MIP project also consider the historical period of the climate models to provide an unbiased reference period (Hempel et al., https://doi.org/10.5194/esd-4-219-2013). It is unclear at the moment how the climate model output and the historical datasets are used consistently (section 2.5) and if the changes in Figure 2 (particularly the lower panel) are indeed truly representing the forecasted change and do not include any bias. In the best possible case clarification is in order and the results of the climate downscaling can be evaluated in the supplementary information (SI).

We use quantile mapping as bias-correction method. Quantile mapping uses historical observations, historical climate model output and future climate model output in its bias-correction routine. First the probability of occurrence of the future precipitation is determined from the empirical cumulative density distribution function (ecdf) of the historical climate model output. Then a correction factor is determined by feeding this probability into the inverse ecdfs of the historical observed and historical climate model output. Finally, the correction factor is added to the future precipitation. The spatial differences in observed annual precipitation sum clearly reflect the orographic effects (compare Figure 1c and Figure 2, upper left). Quantile mapping uses the historical observations. Therefore, the orographic effects are transferred to the bias-corrected future climate projections. Themeßl et al. (2011) argues that quantile mapping performs particularly well for the highest quantiles. Here we focus on the impact of changes in extreme precipitation, therefore, quantile mapping was selected for the current study. We have included a detailed description of quantile mapping in the revised manuscript.

The study overlooks the effect of evaporation completely but this is a non-negligible part of the water balance, affecting both soil moisture and water storage in reservoirs. This driver needs explanation as it becomes more important with higher temperatures and may be decisive in the S4 scenario (RCP 8.5, 2081-2100), more so than precipitation. To analyze this effect and to quantify the effect of precipitation extremes, two control runs with changing only the precipitation and keeping all other factors equal and vice versa, with changing temperature and evaporation but present-day precipitation, is in order. Vegetation change is here a complicating factor, see below on the model setup. In addition, the computation of the potential evaporation and its form in Equation 3 (reference, crop specific?) remain unexplained in the manuscript or the SI.

Indeed, under the most extreme climate conditions of scenario S4, evapotranspiration may become an important contributor to the water balance. However, we argue that the plant water stress indicator captures changes in (potential) evapotranspiration and, therefore, its effect is included in the model results. In addition to the plant water stress, we have added a table to the Supporting Information, which shows the changes for a number of hydrological indicators (i.e. precipitation, actual evapotranspiration, surface runoff, infiltration and soil moisture content). These results are discussed in a new paragraph in the Discussion and Conclusions section.

Potential evapotranspiration is determined by multiplying the crop coefficient with the reference evapotranspiration. Crop coefficients are obtained from NDVI, as explained in the Supporting Information. We used a log-linear model to determine NDVI for future climate conditions, hence, potential evapotranspiration is affected by changes in future vegetation conditions. We refer to Terink et al. (2015) and Eekhout et al. (2018), for a comprehensive description of how potential and actual evapotranspiration are determined in the SPHY model.

A remark on the uncertainty analysis (section 2.5): This is well executed but it may be good to indicate that this looks at climate uncertainty only. The other types of uncertainty are also large and relevant but harder to capture, hence my suggestions for additional simulations to capture their effects).

Indeed, our uncertainty analysis only accounts for climate model uncertainty. However, we would like to stress that the current study is a model application, rather than a study on model development. Expressing model uncertainty is indeed important, however, here we focus on the impact of state-of-the-art climate model output and the comprehensive impact on water security. Furthermore, sensitivity analysis on the impact of extreme precipitation has been performed by previous studies (see Introduction of the revised manuscript). Therefore, we restrict our uncertainty analysis to climate model uncertainty only. We have clarified this in section 2.5 of the revised manuscript.

Model setup: The model setup is ambitious and comprehensive. However, some facts are poorly explained and explored. To start with, the interaction between hydrology, erosion, vegetation and soils is a complex one and using an empirical vegetation growth model may complicate the analysis and is sensitive to the underlying assumptions and may insufficiently capture spatio-temporal variations in cover. Thus, a control run with the current vegetation may be necessary to quantify this adequately. Or the differences in vegetation cover should be presented for the four scenarios in the SI and the effect on the crop specific potential evapotranspiration and the actual evapotranspiration analyzed there. Without excluding this effect, rivalling explanations for the simulated changes cannot be excluded a priori and will the conclusion be tentative at best (see below).

We argue that the vegetation model accounts for the spatio-temporal variability of the vegetation cover. The spatial and intra-annual variability was obtained from the long-term average 16-day period NDVI for the period 2000-2012. The inter-annual variability was determined based on a log-linear relationship between the annual precipitation sum, annual average temperature, annual maximum temperature and annual average NDVI for each of the 57 landuse classes for the period 2000-2012, see the Supporting Information. Figure S5 shows the differences in NDVI (vegetation cover) between the reference scenario and the four future scenarios. To account for the plant-specific impact of climate change we have included an additional figure to the Supporting Information that shows the changes of plant water stress for the future scenarios with respect to the reference scenario. Plant water stress is not only affected by changes in potential evapotranspiration, but also by changes in soil moisture, which is equally important for crop/plant development. The figure shows that rainfed agriculture is most affected by climate change, followed by natural land cover and irrigated agriculture.

Although there are several weaknesses to the modelling of such a varied landscape, the authors have tried and cover this as well as possible. Still, it would be good to mention the resolution of the model in the text. This remains obscure now.

We have mentioned the resolution in the revised manuscruipt.

Also, the model is calibrated and validated and this has implications for its applicability for scenario modelling when conditions will change from the present-day conditions. Looking at the calibration-validation results, both the hydrological and erosion parts show a decrease in performance in terms of model efficiency and bias when moving from the calibration to the validation period. This suggests over-parameterization and its effect may worsen further in the future. Hence, the calibration and validation should be included in the main text and the implications covered in the discussion.

Accurate model calibration and validation is indeed very important and often one of the main challenges and time-consuming aspects of climate change impact assessments, with a continuous risk of over-parametrization. We have applied a calibration strategy minimizing risk of over-parameterization. To prevent overfitting and achieve most realistic model calibration we set most of the potential calibration parameters at literature values and maintained the other parameters within reasonable physical limits of the parameter domain. We have included this explanation to the revised manuscript (SI).

With regards to the erosion model, I am wondering to what extent the sediment delivery is adequately included when moving from the hillslopes (with fairly coarse resolution I presume) to the channel. The same applies to the transport capacity and whether this can be applied directly for the slopes and channels as sediment transport involves different mechanisms in these domains (bedload v. washload). Clarification of these details would be appreciated.

This is indeed a very important aspect of erosion and sediment yield modelling. The model uses the same equation to determine hillslope and channel erosion while a separate sediment deposition equation is used for channels (see Eekhout et al., 2018). Indeed, hillslope erosion and channel erosion are not captured by the same processes and should be handled with different formulae. However, currently, separated hillslope and channel erosion processes are only captured by detailed soil erosion models (such as WEPP), which cannot be applied at regional scales. This is acknowledged and discussed in more detail in Eekhout et al. (2018). In fact, we are currently working on an additional river module to improve this aspect of the model.

In terms of the scenarios, four scenarios result from a combination of two RCPs and two time periods. But what does this mean in terms of simulations? Are they ran consecutively or are they different simulations, representing a sort of dynamic equilibrium? This aspect is very important as it affects those components that have a memory ranging from short-term effects on the soil to longer term ones in relation to vegetation, groundwater and reservoir storage.

The model simulations were performed consecutively and included one start-up year to reach a dynamic equilibrium state for storage components (e.g. reservoirs and soil layers). We have clarified this in the revised manuscript (section 2.3).

Furthermore, aspects pertaining to water management are not explained. Irrigation is widespread in the basin and water supply the purpose of most of the reservoirs. Yet, there is no information on the extent of irrigated areas, how this is covered by the models and how this interferes with reservoir storage and reservoir operation. Without this vital information, the reader cannot evaluate the merit of the simulations on his/her own.

Irrigation is not handled by the model. While large areas of the catchment are irrigated, our results focus on assessment of overall water availability in surface water that could potentially be used by irrigation or other demands. While interesting to assess the role of irrigation water demand, we argue that a separate irrigation module would introduce large uncertainties into the model outcome. There is also little information on additional irrigation water sources, i.e. from deep aquifers and from the Tagus-Segura water transfer. Such additional water supplies are hard to capture in a climate change study, especially, since we have no information on how this supply will change under future climate conditions. We have included more detailed information on other irrigation water sources to section 2.1. Furthermore, we have included discussion on the impact the increase in reservoir inflow, given future projections related to other sources to the Discussion and Conclusions section of the revised manuscript.

Water security: As mentioned at the start, water security is not defined and only indirect measures of water stress and reservoir inflow are defined.

Water security is defined in the first paragraph of the Introduction. Depending on climate and water use, this definition can have different interpretations. Therefore, we have included an explanation why these particular water security indicators are the most relevant to quantify water security in the Segura catchment to section 2.4.

Yet, one could argue that vegetation in the area is adapted to the adverse climate conditions.

> *Indeed, vegetation in this area is adapted to the current climate conditions. Desertification is a very urgent problem in semi-arid regions. While natural vegetation may be adapted to the current climate, it is doubtful if natural vegetation can resist future extreme climate conditions. A local plot-scale study showed that natural vegetation does not adapt to an increase of climate extremes (Léon-Sánchez et al., 2018).*

On cultivated lands irrigation is widely used to avoid stress conditions. Similar for reservoirs, the inflow may vary (as shown by shift in inflow in the manuscript; Figure 4 and S7) but the overall inflow increases and therefore more water can be stored and used for irrigation.

> *Indeed, as a result of an increase of reservoir inflow more water would become available for irrigation. However, currently the irrigation infrastructure is already under pressure, due to the increasing demand for irrigation water. Besides, the reservoirs are not the only source of irrigation water, which is also obtained from deep aquifers and from the Tagus-Segura water transfer. We have clarified this in section 2.1 and discussed this in the Discussion and Conclusions section of the revised manuscript.*

In terms of water security, the main question is if long periods of drought can be survived (by the vegetation or by the dwindling levels in reservoirs). This facet, however, is not covered at all. This means that more direction should be given to the analysis and intensity, frequency and persistence should be covered as well. In the particular case of the plant water stress (PWS), it is doubtful that it can be averaged in space (natural vs agricultural vegetation) and in time as it is dependent on the growing season that is different for the different species and cultivars. Also, PWS will have different effects depending on the time of the year; water shortage over summer for natural, drought-tolerant species will have little effect and it will be more damaging during the wet season. The same holds for winter wheat. PWS is intuitively a useful metric but it should be handled with care and covered independently for different vegetation types.

> *We agree that plant water stress should be handled with care. An analysis of crop/plant specific impact of increased plant water stress would indeed be very interesting. Some crop/plant specific information is required to perform such analysis, such as the time a crop/plant can resist a certain (high) level of plant water stress. Unfortunately, this information is very difficult to obtain at regional scale. However, we have included an additional figure to the Supporting Information where we show the intra-annual variation of plant water stress for 9 aggregated landuse classes. This shows that most extreme increases are projected for rainfed agriculture, followed by natural land cover and irrigated agriculture. The figure is discussed in the Discussion and Conclusions section of the revised manuscript.*

For the analysis of the sediment yield, I would like to see some further clarification on the yields (1.29 to 6 tonnes per hectare per year) seems quite large and I am wondering how much actually is fed to and trapped by the reservoirs.

> *The soil erosion values are in the range of the literature data we used to calibrate the soil erosion model (i.e. Cerdan et al., 2010; Maetens et al, 2012). The amount of*

sediment that is trapped in the reservoirs is shown in Figures 5 and S9. Reservoir sediment yield was calibrated with local reservoir sedimentation data, as described in the Supplementary Information.

Discussion and conclusion: As mentioned at the beginning, the manuscript does not succeed yet in quantifying the effect of extreme precipitation on water security. Overall, the findings agree with earlier studies undertaken at coarser spatial scales but these generally looked at water availability or hydrological extremes without investigating in detail spatial differences or changes in precipitation patterns as the manuscript by Eekhout et al. intends to do at the regional scale. Additional evidence here is needed and this may concentrate on the contribution of direct runoff compared to slow flow, runoff fractions and frequency of different rainfall intensities etc. Without this, the conclusion has too narrow a base and the relevance of the global picture of Figure 6 is not so great, the more so as it does not take the changes in precipitation in the future in account. In terms of the validity of the study, some additional discussion (and analysis) is required on the effect of vegetation, quality of the downscaling, calibration and validation and the coverage of irrigation etc. by the model. At the moment, some information appears quite magically near the end, such as the details on the land cover and the relevance of the findings for water management. While the introduction is succinct and relevant, some reworking of it in light of the discussion and conclusion will be in order.

We would like to stress that not all our findings agree with previous studies. We argue that previous studies may have underestimated the impact of extreme precipitation by not accounting for infiltration excess surface runoff. Many previous studies show a decrease of runoff, as a result of a decrease of annual precipitation. However, for many locations an increase of extreme precipitation is expected, which could lead to an increase of runoff and an important redistribution of water between green and blue water as shown in the current study.

As suggested in the previous comments by the reviewer, we have revised the manuscript with respect to the impact of extreme precipitation on different environments, the impact on the irrigation infrastructure and water demand, the description of the bias-correction method, the calibration strategy and details about the landuse. See previous comments for the specifics regarding these issues.

Overall, it shows care was taken to produce the text, figures and tables, also in the SI. This is much appreciated. Just some minor points:

Page 3, line 21: is the capacity of all 33 reservoirs 866 Hm3 or just the 14 for irrigation? And how does this compare to their inflow (Table S1)?

The total capacity of the 14 reservoirs used to store irrigation water is 866 Hm^3. Under the reference scenario, the total annual reservoir inflow amounts to 400 Hm^3, which is 46% of the total capacity of the 14 considered reservoirs. We have included this information in section 3.2 of the revised manuscript.

Equation 1: add the condition that this holds if theta_t < theta_pws else PWS= 0.

In the text below Equation 1 the following sentence was included: "PWS equals zero when $\theta(t) > \theta_{PWP}$".

Equation 3: what are the values for dtab and what was done for natural vegetation, they are not covered by Allen to my knowledge. Also, clarify ETp here.

The values for the depletion fraction range from 0.2-0.7. Indeed, Allen et al. (1998) mainly covers agricultural crops. For natural vegetation, we adopted values for vegetation types that are most closely related to natural vegetation, i.e. conifer trees for forest and grazing pasture for shrubland. We have clarified this in the revised manuscript. Potential evapotranspiration was obtained from the hydrological model, which is described in Terink et al. (2015).

Section 3.2: Redistribution of water. This is not a logical structure and the term does not connect to the previous part. Divide this into the part on PWS and the reservoir storage.

We have renamed this section: "Impact on Water Security".

Figure 3: what are the dots, next to the daggers and asteriskes? And please explain the design of the box plots. (what do the lines, boxes and bars mean?)

The dots are outliers, we have explained this in the caption, in addition to an explanation of the box plots.

As I said, an interesting read and I hope my comments and suggestions help to improve and publish the manuscript.

We would like to thank the reviewer for his nice comments on the manuscript and appreciate his suggestions that have certainly helped to improve the manuscript.

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