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 #1

The paper "Why increased extreme precipitation under climate change negatively affects water security" explores the redistribution of surface water (blue water) and soil water (green water) under future climate scenarios. The primary result is that increasing precipitation intensity will reduce green water and thus increase plant water stress.

Overall I think that the central questions and technical work in this paper seem to be fine. The primary area in need of improvement is the presentation and discussion of the results. In particular, I feel that: a) some of the conclusions are overstated (or rather not properly qualified), b) the implications of the key results are not discussed in a precise way, and c) there is unnecessary repetition in some sections. Having said this, the paper is generally well written from a grammatical perspective.

I have two broad comments (listed below), and several minor comments (in the attached .pdf) for the authors to address.

We would like to thank the reviewer for his nice comments on the manuscript. We have revised the conclusions and implications of results to be more cautious and revised the manuscript for unnecessary repetitions. Below we have responded to the general and minor comments raised by the reviewer.

1. I find the title/abstract and the results of the paper to be somewhat incongruous. I think that the authors should include more discussion of how the magnitude of the trends that they find impact water security issues in a more precise way. The basic causal narrative that comes across in the abstract is very clear, but the supporting evidence for this narrative is not clear. For example, it is hard to assess whether the impacts on reservoir storage via changing soil erosion are of substantial enough magnitude to significantly change the prospects of irrigation in the study region.

The concept of water security is defined 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; we used four indicators to quantify water security in the Segura River catchment that we considered most relevant in the local context. We acknowledge the need to quantify impacts as much as possible. Therefore, for a more comprehensive discussion of the magnitude of the trends and the impact on water security, we have now included an analysis of the impact of changes in reservoir inflow to irrigation water demand. Unfortunately, we cannot provide the full details of how irrigation water supply will change under future climate conditions. Irrigation water is also supplied from deep aquifers and from the Tagus-Segura water transfer, from which it is very uncertain how *supply will change under future climate conditions. We have included this information in section 2.1 and discussed the future prospects of irrigation in the Discussion and Conclusions section of the revised manuscript.*

Loss of reservoir storage capacity is another aspect affecting water security in Spain and many other areas worldwide (de Vente et al, 2005; Wisser et al., 2013). Therefore, we estimated the capacity loss due to reservoir sedimentation (Figure S9). Under the reference conditions, the annual capacity loss for 14 reservoirs used for irrigation equals 0.11%. This is indeed lower than the global and Spanish national average and not substantial enough to claim that storage capacity is threatened by increased soil erosion under climate change. While this may partly be an artefact of insufficiently accounting for channel erosion processes as explained in the discussion, we have adjusted the claims regarding the impact of sediment yield on the storage capacity of the reservoirs in the abstract, the Discussion and Conclusions sections.

The other two water security indicators (plant water stress and hillslope erosion) can only be interpreted in a qualitative way. It is likely that an increase of plant water stress and hillslope erosion has a detrimental effect on the agricultural productivity due to lack of water availability, of fertile soil and reduced water retention capacity. However, detailed crop-specific information is needed to be able to quantify the impact of these two indicators. While highly relevant, quantitative assessment of impacts on crop yield is beyond the scope of this study.

2. I think that the paper would benefit from a more thorough literature review. There is previous literature that discusses the implications of decreasing precipitation frequency and increasing precipitation intensity on runoff and water stress (e.g. Fay et al. 2003, and Knapp et al. 2008).

We have added a new paragraph to the introduction that discusses the impact of decreasing precipitation frequency and increasing extreme precipitation on natural, arable and urban landuse classes.

Page 2, lines 5-6: There is a moderate amount of repetition here from the last two sentences of the previous paragraph. I think that the presentation could be streamlined, although this point is not critical.

We agree, therefore, we have removed this sentence from the manuscript.

Page 2, lines 22: I believe that this statement should be qualified since it is not the case that there is evidence of increasing extreme precipitation into the future in ALL regions. Perhaps something like: "Considering the estimated [or anticipated] future increase of extreme precipitation in many regions..."

Thanks for the suggestion. We have changed this sentence in the revised manuscript.

Page 2, lines 22-23: Since these are already cited in a previous paragraph for the same reason, I do not think that they are necessary here. This is a very minor concern, however.

We agree. We have removed these references from the manuscript.

Page 3, lines 5-6: It would be good to define SPHY and MMF

We have added a definition of the two models to the revised manuscript.

Page 3, line 15: Likely it would be helpful to include a short definition of these two classifications since some readers may be unfamiliar with the details of the K-G classification system.

The manuscript includes both the description of the climate (Mediterranean and semi-arid) and the Köppen classification (CSa and BSk). Additionally, the manuscript includes information on the annual precipitation and temperature (Figures 2 and S3). We argue that this is sufficient to get an idea of the climate in the study area.

Page 5, line 2: I think that this is more accurately described as two climate scenarios assessed over two time periods, rather than four climate scenarios. (This is a minor point)

Indeed, this would be a more accurate description. However, in the manuscript we refer to these scenarios with the indicators S1-S4. We prefer to keep this logic to increase readability.

Page 5, lines 6-7: The validity of this statement depends on the variable that is being downscaled and bias-corrected as well as the measure of performance that is used to compare methods. Please be more specific.

Indeed. 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 added this reasoning to the revised manuscript.

Page 5, lines 7-9: Please elaborate on this and describe the method more precisely. This is particularly important since I presume that your results are somewhat sensitive to this correction process.

We have included a detailed description of quantile mapping to the revised manuscript.

Page 5, lines 13-14: Please provide a brief argument for why this set of parameters provides a good indicator for water security. It seems to me that you are informing some specific areas of water security, but your statement here seems to be a bit more general.

We have added the following sentences to the revised manuscript: "These indicators are specifically important for this study area, which is dominated by rainfed and irrigated agriculture. Changes in plant water stress and hillslope erosion may affect agricultural productivity, while changes in reservoir inflow and reservoir sediment yield affect water availability for irrigated agriculture and drinking water."

Page 5, line 21: I assume that "PWP" should be "PWS"

Indeed. We have changed this in the revised manuscript.

Page 5, line 21: Missing a "."

We have changed this in the revised manuscript.

Page 6, lines 7-13: Both of these measures of uncertainty deal with how certain we are of the sign of the response. This is fine, but it should noted that many readers may also care about whether the magnitude of the response is substantial enough to care about.

We have discussed this issue in the first general comment.

Page 6, line 22: It would be helpful if you were specific about how you define extreme precipitation somewhere in the main text, rather than only in the figure caption.

We have added the following sentences to the revised manuscript to define extreme precipitation and dry spells: "Extreme precipitation is defined as the 95th percentile of daily precipitation, considering only rainy days (>1 mm day^{-1}; Jacob et al., 2014). Dry spells are defined as the 95th percentile of the duration of periods of at least 5 consecutive days with daily precipitation below 1 mm (Jacob et al., 2014)."

Page 7, line 2: It may be interesting to also present and discuss the min and max daily plant water stress under the different scenarios, rather only the seasonal averages.

Plant water stress is determined from soil moisture and potential evapotranspiration, which both do not show much daily variability. Therefore, we argue that a medium-term analysis of this indicator is sufficient to quantify its impact. To increase the understanding of the impact on different landuses, we have added an additional figure to the Supporting Information that shows the temporal variation (monthly) of plant water stress for 9 landuse classes. The figure shows that rainfed agriculture is most affected by climate change, followed by natural land cover and irrigated agriculture. The figure is discussed in the Discussion and Conclusions section of the revised manuscript.

Page 9, line 2: I think that you need to close the loop here and clearly state what the change to storage capacity is for the estimated changes to erosion and sediment yield. This is particularly important because the absrtact states that: "This affects plant water stress and the potential of rainfed versus irrigated agriculture, and increases dependency on reservoir storage, that is increasingly threatened by an increase of soil erosion."

We have responded to this issue in the first general comment.

Page 10, line 5: “a”.

We have changed this in the revised manuscript.

Page 10, line 6: based on Figure 5, I think that this statement needs to be qualified. For example, there are some very large decreases in SY in several of the reservoirs of S4.

We have responded to this issue in the first general comment.

Page 10, lines 7-9: Please provide clear explanations for these assertions of causality. Most importantly, what is the rationale for saying that the decrease in annual volume impacts the distribution of blue/green water to favor blue water?

We argue that changes in annual precipitation volume have a smaller impact on the redistribution of water than changes in extreme precipitation and precipitation frequency. In lines 9-12 (page 10 of the original manuscript) we explain that an increase of extreme precipitation leads to an increase of surface runoff, which is the main cause of the increase of reservoir inflow (blue water). Furthermore, an increase of surface runoff leads to a reduction of infiltration, negatively affecting soil moisture content (green water). To support these claims, 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).

Page 11, lines 1-4: There is a fair bit of repetition in this (already short) discussion. In particular, the authors bring up the lack of consideration of infiltration excess surface runoff and the impact of changing extreme precipitation on surface runoff in consecutive paragraphs. I think that the discussion could be slightly restructured to avoid this repetition.

Thanks for the suggestion. We have critically edited the Discussion and Conclusions section.

Page 11, lines 12-13: I find this portion of the discussion to be inconsistent with the portion of the abstract that states "... increases dependency on reservoir storage, that is increasingly threatened by an increase of soil erosion." Please clarify and/or change the abstract where necessary.

We have responded to this issue in the first general comment.

Page 11, lines 20-21: It is not clear to me how your results illustrate that suitable bias-correction methods are crucial for accurate climate change impact assessments. Please elaborate.

Many studies use the delta change method, which potentially could lead to an opposite direction of change for runoff and soil erosion. We argue that bias-correction methods that explicitly account for projected changes in precipitation distribution, like quantile mapping, are essential in climate change assessments. We have included the following *sentences to the Discussion to clarify this: "Furthermore, we applied a bias-correction method (quantile mapping) that explicitly accounts for changes in the projected precipitation distribution. Many previous studies applied the change factor (or delta change) method, which does not fully account for the changes in rainfall intensity. Studies that apply this method often show that a change of annual rainfall leads to a similar direction of change of runoff and soil erosion (e.g., Shrestha et al., 2013; Correa et al., 2016). Therefore, future studies should consider bias-correction methods that account for changes in frequency and intensity of extreme events that affect both hydrology and soil erosion (Mullan et al., 2012; Li and Fang, 2016)."*

SI, page 5, line 3: Please defend your choice for calibration and validation years. Fitting on 10 years of data and validating on the 14 years prior seems arbitrary and tempts the reader to wonder whether many candidate periods were computed until a satisfactory fit emerged. This would largely defeat the purpose of a validation.

Only a limited amount of data was available for model calibration and validation. Discharge data were available from 1987-2015. NDVI, precipitation and temperature data were only available until 2012. We choose to use individual NDVI images for the calibration period, which were only available from 2000 onwards. Given these limitations, we decided to calibrate the model from 2001-2010 and validate from 1987-2000. We have clarified this in the revised manuscript (SI).

SI, page 5, line 9: I assume that this means minimize. Please clarify what you are optimizing.

Indeed. However, we choose to use "optimized", which is common jargon in hydrological studies.

SI, page 5, line 10: Same as previous comment

In the case of Nash-Sutcliffe, which has a value of 1 in the case of a perfect fit, the model efficiency is maximized. However, we choose to use "optimized", which is common jargon in hydrological studies.

SI, page 5, lines 29-31: Please briefly discuss the impacts of this assumption on your results. Specifically how the results may change for locations with a) higher and b) lower ratios of maximum hourly rainfall to daily rainfall.

We have changed the sentence in the revised manuscript (SI) as follows: "This fraction may vary globally and global extrapolation introduces uncertainty in regions where this fraction differs from our estimate. A higher (lower) fraction may lead to an increase (decrease) of the area prone for infiltration excess surface runoff. Nevertheless, in the absence of better estimates we extrapolated the fraction to illustrate the potential extent of global sensitive areas to infiltration excess runoff."