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Discussions

Interactive comment on "Why increased extreme precipitation under climate change negatively affects water security" *by* J. P. C. Eekhout et al.

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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.

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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. 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 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). The study overlooks the effect of evaporation completely but this is a non-negligible part of the water balance, affect-

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ing 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. 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).

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). 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. 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

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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. 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. 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. 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.

Water security: As mentioned at the start, water security is not defined and only indirect measures of water stress and reservoir inflow are defined. Yet, one could argue that vegetation in the area is adapted to the adverse climate conditions and that 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. 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

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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. 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.

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.

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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)?

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

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.

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.

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?)

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

Rens van Beek

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