EUROPEAN COMMISSION
JOINT RESEARCH CENTRE
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Climate Risk Management Unit

To the Editor of the *Hydrology and Earth System Sciences*
Bart van den Hurk
KNMI, Atmospheric Research
P.O. Box 201
3730 AE De Bilt
Netherlands

Ispra, 29 October 2013

Dear Dr. van den Hurk,
Please find enclosed an electronic copy of the revised manuscript (hess-2013-378) entitled:

Ensemble projections of future streamflow droughts in Europe by G. Forzieri, L. Feyen, R. Rojas, M. Flörke, F. Wimmer and A. Bianchi

We have revised our manuscript in accordance with the comments and suggestions received from two reviewers. A 'response-to-reviewer' document is provided along with this revised submission. We believe we have properly addressed all concerns and added the necessary material to the text and figures to strengthen our manuscript. The material contained in this manuscript is not under consideration in any other publication.

Please let me know if I should provide any additional information. I look forward to your response.

Kind regards,

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Responses to comments on "Ensemble projections of future streamflow droughts in Europe" (hess-2013-378)

G. Forzieri, L. Feyen, R. Rojas, M. Flörke, F. Wimmer and A. Bianchi

Revised for *Hydrology and Earth System Sciences*

Please consider that corrections are marked in red fonts in the revised document and sentences in "Response and Actions Taken" (see right field in the following tables) marked in italic are part of the revised manuscript.

Responses to the Anne Van Loon's report (report received and published on HESSD 14 September 2013)

Referee Comment	**Response and Actions Taken**
In this paper the authors simulated future changes in streamflow drought deficit and low flow indices across Europe by using forcing data from a variety of models (GCMs and RCMs) with a climate change scenario as input for a hydrological model. They also included the effect of a scenario of changes in water use by coupling the hydrological model to a water abstraction model. They conclude that rivers in large parts of Europe will be negatively affected by climate change and that water use will aggravate drought conditions especially in Southern, Western and Central Europe. This research is interesting and relevant and the topic deserves publication in HESS. Although the paper is quite long and could at some points be more concise, I think that it is very well written and the results are presented in a clear manner. As reviewer I really appreciate the effort that the authors took on writing a good manuscript.	We thank Anne Van Loon for her positive comments. According to her suggestion we have shortened several paragraphs of the revised manuscript, especially in the Methodology section. However, to properly take into account all the reviewer's suggestions, the revised manuscript is slightly longer than the original version.
My main concern with the research presented is related to the fact that the authors neglect the effect of using multiple climate change scenarios, multiple hydrological models, and multiple water use scenarios. For example, Hagemann et al. (2013) found that the "spread resulting from the choice of the hydrology model is larger than the spread originating from the climate models over many areas." Here, the authors mention in the conclusions that "hydrological uncertainty – here not accounted for – may further increase the variability in the low flow projections as suggested by the considerable discrepancy between large-scale hydrological models in the evaluation of drought propagation", but they do not mention the uncertainty related to using different climate change scenarios and water use scenarios. The whole	According to the reviewer's suggestion, we have updated the Methodology section to emphasize the many potential sources uncertainties that may affect the projections of streamflow droughts, including climate and water use scenarios as well as hydrological modeling (see 2.1. and 2.5.2.). Furthermore, we have better explained our modeling choices in the Methodology section, here briefly reported. <u>Choice of the climate scenario</u> *We focus here on the ENSEMBLES SRES-A1B dataset as to date it is the only large ensemble of high-resolution climate simulations for Europe that allows for a finer assessment of the climate model uncertainty (van*

idea of scenarios is that all of them should be used to get a clear picture of the range of possible futures as scenarios are all possible realisations of the future. I understand that doing the analysis for a multitude of climate scenarios, a multitude of climate models, a multitude of hydrological models, and a multitude of water use scenarios is very demanding and might not be feasible for one research group in a limited time span. However, I think it is very important to clearly discuss this issue in the current paper. Now the reader cannot compare results of this study with other studies, e.g. that of Feyen and Dankers (2009), because both a different climate change scenario and a different hydrological model were used. These issues cannot be neglected. The least the authors should do is to underpin the choice of climate scenario and hydrological model in the introduction or methods section and discuss the effect of these choices in the discussion or conclusion section.	*der Linden and Mitchell, 2009).* See Section 2.1. Choice of the water use scenario *The EcF scenario was selected as it is the most coherent with the IPCC SRES A1B.* See Section 2.1. Choice of the hydrological model *LISFLOOD has been specifically set-up for European catchments by optimally exploiting several databases that contain pan-European information on soils (King et al., 1994; Wösten et al., 1999), land cover (European Environment Agency, 2002), topography (Hiederer and de Roo, 2003) and meteorology (Rijks et al., 1998).* The full access to the source code and the focus on the European scale make LISFLOOD preferable for our purposes to other hydrological models. See Section 2.2. In the Conclusions Section we have recalled the potential sources of uncertainties. *We point out that our analysis focuses on the A1B pathway of climate change and a consistent water use consumption scenario, and that possible uncertainty in streamflow droughts arising from the hydrological modelling is neglected. Future developments should focus on an ensemble-based approach that considers multiple combinations of emission/water use scenarios, GCMs-RCMs and hydrological models to depict a picture that is comprehensive of all possible realisations of future low flow conditions and that accounts for all the involved sources of uncertainty.*
Furthermore, the authors could test the effect of the four different water use scenarios, because I estimate that that is most easy to implement in their modelling scheme.	*The EcF scenario was selected as it is the most coherent with the IPCC SRES A1B.* See Section 2.1. Combining climate information forced by A1B climate scenario and water use scenarios, such as Fortress Europe (FoE), Policy Rules (PoR), and Sustainability Eventually (SuE) results in inconsistencies given the large differences in the storylines between the drivers of GHG emissions and water use consumptions. We have better described the A1B emission scenario to better link it to the EcF water use scenario and to understand the coherence between them in the revised manuscript.
Another general point is that the authors state in the introduction that drought is a natural phenomenon, but in the results and discussion section use the word drought also for the situation influenced by water use. The authors should refer to the discussion of the definitions of drought and water scarcity, as it is summarised by the European Expert Group on Water Scarcity and Drought at the following website:	We agree with the reviewer that is fundamental to properly distinguish drought and water scarcity phenomena especially for water managers, which have to plan different adaptation/management strategies to face with these problems. Therefore, according to the reviewer's suggestion, we have better clarified the distinction between drought and water scarcity in the Introduction as follows.

http://www.globalwaterforum.org/2013/08/26/how-to-distinguish-water-scarcity-and-drought-in-eu-water-policy/.	*Drought is a natural feature of the water cycle that can occur in all climatic zones. It originates from a temporary aberration of the normal precipitation regime over a large area, but other climatic factors, such as high temperatures and winds, increased evapotranspiration demand or low relative humidity, can significantly aggravate the severity of the event. Anthropogenic drivers, such as intensive water use and poor water management, can further exacerbate low-flow conditions in watersheds with a consequent increase in vulnerability to drought (e.g., Vörösmarty et al., 2000; Tallaksen and van Lanen 2004; Döll et al., 2009; Wada et al., 2013a). Water scarcity reflects the imbalance that arises from an overexploitation of water resources, caused by consumption being significantly higher than the natural renewable availability (Schmidt and Benítez‐Sanz, 2013; Van Loon and Van Lanen, 2013). Albeit water scarcity may relate to any hydrological condition, it is more likely to occur under drought conditions due to reduced water availability.* However, we believe to have properly referred to the term drought throughout the manuscript. In our analysis, we did not analyze the balance of water resources (availability vs. consumption). Instead, the increasing demand for water, as analyzed in our work, serves to quantify the potential increase in severity of the drought conditions. Excessive water use and human activity can intensify and even cause a drought to develop (e.g., Tallaksen and van Lanen 2004, pag. 6, Döll et al., 2009; Wada et al., 2013).
Specific comments: p.10721, l.1-2: Please provide references.	According to the reviewer's suggestion, we have provided four references about the intensification of droughts due to human influence. Döll, P., Fiedler, K., and Zhang, J.: Global-scale analysis of river flow alterations due to water withdrawals and reservoirs, Hydrol. Earth Syst. Sci., 13, 2413–2432, 2009. Tallaksen, L.M., and van Lanen, H.A.J.: (Eds.) Hydrological Drought: Processes and Estimation Methods for Streamflow and Groundwater, Dev. Water Sci., vol. 48, Elsevier, Amsterdam, 2004. Wada, Y., van Beek, L.P.H., Wanders, N. and Bierkens, M.F.P.: Human water consumption intensifies hydrological drought worldwide, Environ. Res. Lett., DOI:10.1088/1748-9326/8/3/034036, 2013. Vörösmarty, C.J., Green, P., Salisbury, J., and Lammers, R.B.: Global water resources: vulnerability from climate change and population growth, Science 289, 284-288, 2000.

p.10722, l.4-5: “Undisturbed catchments”: as this does not relate to human influence anymore, use “however” to show the contract with the previous sentence.	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
p.10722, l.6-8: “dryness”: vague term > leave it out.	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
p.10722 “agricultural”: better to use the term “soil moisture drought” as other sectors might be impacted by low soil moisture levels.	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
p.10722 “water supply”: related to water distribution issues > change to “water availability”	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
p.10723, l.27-28: “multiple driving climate scenarios”: this is not done in this study. Please clarify.	We agree with the reviewer, we have wrongly mentioned “multiple driving climate scenarios”. Our approach is based on the exploitation of an ensemble of multiple combinations of GCMs-RCMs. According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
p.10725, l.5-6 & p.10726, l.26-27: By bias-correcting precipitation and temperature, but not bias-correcting the other meteorological variables that are needed for the calculation of potential evapotranspiration, like vapor pressure, wind speed etc., inconsistencies will arise between precipitation, temperature and PET data. Please discuss the implications.	We agree with the reviewer. *We point out that variables such as dewpoint temperature, solar and thermal radiation that are employed together with the bias-corrected temperature fields to calculate the evapotranspiration components driving LISFLOOD are not corrected for potential bias. This could violate the energy balance and potentially introduce bias in the simulated hydrological patterns (e.g., Rojas et al., 2011; Hagemann et al., 2013). However, experiments performed using the same bias correction method with a different impact model showed that the relative values of projected hydrological change are very similar if other climate variables are also bias corrected (Haddeland et al., 2012). Thus, we can reasonably presume that the impact of these inconsistencies is generally rather small.* We have clarified this in the revised manuscript (see Section 2.2. and 3.1.).
p.10727, l.11: Please provide some basic information on the calibration of the LISFLOOD model. This is needed because the calibration of the model is used as an argument in the discussion on p.10747, l.16.	According to the reviewer’s suggestion, we have added some information about the calibration tasks in the revised manuscript. *LISFLOOD was calibrated using at least 4 years of historical river flow data in the period 1995–2002 in 258 catchments and sub-catchments distributed throughout Europe.* For additional details we refer the reader to van der Knijff et al. (2010) and Feyen et al. (2007, 2008).
p.10728, l.12: “Maximum Likelihood method” > introduce the abbreviation (ML), because it is used later on in the paper.	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
p.10729, l.1: “Q80”: I guess you used a fixed threshold,	*Time series of daily discharges are split up in nonfrost*

so equal threshold throughout the year (see l.28 "annual analysis")? Please add this.	*and frost seasons based on the monthly average temperature in the upstream area (see criterion described in section 2.4.). Streamflow drought indices are estimated separately for each season. Note that for deficit volumes we use different* Q_{80} *threshold values calculated from the FDCs corresponding to the respective season.* We have clarified this in section 2.4.
I also guess you recalculated the threshold for observed and simulated time series (see p.10737, l.12)? This is important because it implies that you are only considering relative differences between droughts in observations and simulations in Fig. 3.	We agree with the reviewer that this is the case for the streamflow deficits. According to her suggestion, we have clarified this in the revised manuscript.
p.10731, l.15-17: "the multi-model average or median can be expected to outperform individual ensemble members": this is also shown for low flow and drought, see Gudmundsson et al. 2012 and Stahl et al. 2011. You might want to include these (or comparable) references.	According to the reviewer's suggestion, we have included the mentioned references in the revised manuscript.
p.10732, l.6-9: You might want to consider including a formula to express this statement more clearly.	We have rephrased the description of the statistical method in the revised manuscript making the sentence clearer. *The statistical significance of the changes for the projections of streamflow drought indices is evaluated by the use of the Welch's t test, assuming that the variances of the control period and the different time slices are not necessarily the same (Welch, 1947; see also Von Storch and Zwiers, 1999, p. 113).* We think that after this correction there is no need to include the formula.
p.10733, l.15-17: Where do the data come from? From the EWA database?	*Daily discharge values have been collected within the ECA&D project (http: //eca.knmi.nl) and by the Global Runoff Data Centre (GRDC), CEDEX-IEH Banque HYDRO and ARPA Emilia Romagna.* We have specified this in the text of the revised manuscript.
p.10733, l.21: Is the minimum contributing upstream area of 1000 km2 the result of your selection on p.10730?	Stations have been selected to match catchment sizes that guarantee perennial river conditions in the LISFLOOD analysis (Section 2.4.).
p.10734: Be very careful with the use of the r2 as statistical measure in validation as a high offset or a negative correlation also gives a high r2, so either disregard r2 in validation or give it much less attention than EF, which is a much better measure for this purpose.	According to the reviewer's suggestion, we have avoided the use of r2 for validation assessment and we have focused on EF and PBIAS. Text and figures have been accordingly corrected in the revised manuscript.
p.10735, l.6-7: "7 day average minimum flows": is that all annual 7-day minimum flows averaged per station?	Yes, we refer to "average annual 7-day minimum flows" as the average (over the respective time window) of the annual 7-day minimum flows (hence for a time window

	of 30 years the average is over 30 values of 7-day minimum flow). We have properly updated the text and figure caption in the revised manuscript.
p.10735, l.24-28: Could this underestimation also be related to the omission of reservoirs in the simulation?	The omission of reservoirs in the hydrological simulations can additionally explain the underestimation of the modeled streamflow drought conditions. We have clarified this in the revised manuscript (see Section 3.1.). However, given the systematic underestimation occurring over most of the stations in Europe, we retain that the main reason of such behavior is mainly due to the combined effect of underestimation of the low-end percentiles of the bias corrected precipitation, and a potential overestimation of the number of dry days obtained from the fitting of the transfer functions (e.g., Dosio and Paruolo, 2011; Rojas et al., 2011).
p.10736, l.18-20: Or the simulations are more peaky than the observations because the model response to precipitation is too fast. This is shown for many large-scale hydrological models in many studies. Please consider showing some example time series of simulations and observations and their thresholds and deficit volumes.	We have compared our results with the work of Feyen and Dankers (2009), which used the same streamflow drought indices derived from LISFLOOD simulations. Feyen and Dankers (2009) employed climate data from the HIRHAM model driven by the SRES A2 emission scenario to force LISFLOOD. No bias correction was applied to the climate data. In their validation exercise, a systematic negative PBIAS cannot be observed on both the indices. Even if climate scenario and climate models are different from our experiments, differences in PBIAS seem to be related to the effects of the bias correction (applied in our study but not in the Feyen and Danker's study). This corroborates our interpretation that the negative PBIAS found here for minimum flows and deficit volumes is mainly connected to the different effects of the bias correction on the low flow spectrum. We have clarified this in the revised manuscript (see Section 3.1.). We believe that adding some figures on the time series of discharge and corresponding drought indices would not add relevant information to our analysis. Furthermore, *control climate simulations do not reproduce the historical weather of the 1961–1990 period, but only the average climate conditions. This does not allow a day-to-day or event-to-event comparison.* We have clarified this in the revised manuscript (see Section 3.1.).
p.10736, l.28: "capturing extreme streamflow droughts" > "capturing the statistics of extreme streamflow droughts"	According to the reviewer's suggestion, we have corrected the text in the revised manuscript.
p.10737, l.6-7: "(2) an incorrect parameterization of the groundwater storage due to bias in the observed winter precipitation": how is the groundwater storage in the frost season influenced by the winter precipitation which	*Uncertainties in the observed winter precipitation (Goodison et al., 1998; Yang et al., 2001) that was used in the calibration of the LISFLOOD hydrological model may have affected the parameterization of the*

falls as snow and does not result in recharge to the groundwater system?	*groundwater reservoir.* We have clarified this in the revised manuscript (see Section 3.1.).
p.10737, l.16-22: Maybe you should mention here already that you recalculate the frost and nonfrost season for the future.	According to the reviewer's suggestion, we have clarified this in the revised manuscript to better harmonize Section 3.1. with the following sections.
p.10738, l.1-2: "at the location itself": what do you mean with location? Grid cell? Or gauging station (fig.1)? If grid cell, then what is the contributing area? If gauging station. then only include the station locations in Fig.4 (like in Fig.7).	We refer to grid cell. *Because streamflow at a given location depends on the hydroclimatological conditions over the upstream river basin, these maps show average changes over the upstream area that contributes flow to that location rather than the change at the grid cell itself.* We have clarified this in the revised manuscript.
section 3.3: the headings of the subchapters are unequal. You could add "in the nonfrost season" to the heading of subsection 3.3.2, 3.3.3 and 3.3.4.	According to the reviewer's suggestion, we have corrected the heading of subsection 3.3.2, 3.3.3 and 3.3.4 in the revised manuscript.
p.10740, l.5: "Langness, Isohaara and Dau Gavpil, Neuhausen" > "Langness, Isohaara, Dau Gavpil, and Neuhausen".	According to the reviewer's suggestion, we have corrected the text in the revised manuscript.
p.10740, l.13-15: Vague sentence, please rephrase.	According to the reviewer's suggestion, we have rephrased the sentence in the revised manuscript (see Section 3.1.). *However, streamflow droughts also depend on hydroclimatologic conditions prior to the onset of the frost season, especially for extreme events as they reflect imbalances in water availability over longer time spans.*
p.10741, l.17-19: The changes in actual ET can be quantified from the model output.	Yes, they can be quantified from the model output. We have slightly rephrased the sentence in the revised manuscript to explicit that they are available information.
p.10745, l.14-17: Vague sentence, please rephrase.	*Water use abstraction will exacerbate minimum low flow conditions by ca. 10-30% over the Mediterranean regions, especially where maximum rates of seasonally water demand of irrigated crops overlaps with drier periods (see e.g. stations Seros, Lugo, Ponte Lago and Beaucaire in Figure 6). This suggests that even in front of a relative reduction in total annual water abstractions (compare with Figure 7), the combined effects of alterations in climate and human water consumption will strongly aggravate streamflow drought conditions.* According to the reviewer's suggestion, we have clarified this in the revised manuscript (see Section 3.3.).
p.10746: The uncertainty in climate change scenario and water use scenario does increase significantly over time.	According to the reviewer's suggestion, we have clarified this in Introduction of the revised manuscript.

Please mention this.	
p.10750, l.2: “increased competition for water”: this is not a result of this research. Be very careful with these broad conclusions.	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript. […] *The first conclusion is that due to global warming many river basins in Europe are likely to be more prone to severe water stress.* See Conclusions.
p.10752, l.1: “aus der Beek” > “Aus der Beek”	According to the reviewer’s suggestion, we have corrected the reference in the revised manuscript.
Table 1: Move footnote “b” to next line.	Such visualization issue is likely due to the automatic editing process of HESSD. However, according to the reviewer’s suggestion, we have corrected the footnote in the revised manuscript.
Fig. 5 & 6: The text in the figures is not legible. Please increase figure, increase font size or find another way to provide the information.	According to the reviewer’s suggestion, we have changed the text information in Figure 5 and Figure 6 of the revised manuscript. In the revised figures we have only reported the most relevant information including upstream area and minimum and maximum discharge values used for the normalization. Text is in uppercase and font size has been increased making the text clearer.
Fig. 9: “20yr minimum flow” > “20yr return level minimum flow”	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
Fig. 11: “Welch’s test” > “Welch’s t test”	According to the reviewer’s suggestion, we have corrected the text in the revised manuscript.
	We believe we have properly addressed all concerns and added the necessary material to the text and figures in order to strengthen our manuscript. We thank the reviewer for her constructive comments.