

Interactive comment on “Impact of climate change on freshwater ecosystems: a global-scale analysis of ecologically relevant river flow alterations” by P. Döll and J. Zhang

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Final response to referee comments

We thank the two anonymous referees for their good comments which helped us to improve our manuscript. In the following, we answer to the comments of the referees.

Referee 1

General comments RC1: The effect of climate change related hydrological changes on freshwater ecosystems has been quantified by Xenopolous (2005) before by the method described in section 2.3. They used the same model (WaterGAP /WGHM)

C770

to quantify changes in discharge for the A2 and B2 scenario with data from the GCM HadCM3 as well. The new and for HESS interesting point of this paper is the use of the ecologically relevant river flow characteristics and the changes in these quantities. However the changes in these characteristics are only very briefly and qualitatively related to changes in ecosystems (page 1322, line 11-14, line 27-29). The results of the study provide mainly information on the difference between anthropogenic river flow alterations and climate change related flow alterations and is not very interesting from an ecological point of view, although the title suggest differently. AC1: Xenopolous et al. (2005) only quantified the impact of climate change on long-term average river discharge, and, based on that and an empirical relation between river discharge and number of fish species, the change in fish species numbers that may result from a decrease of long-term average river discharge. As discussed section 1, however, the importance of flow variability and thus of flow regimes for river ecosystems is well established. Therefore, the goal should be to also quantify ecological impacts of changes of other flow regime indicators. However, due to the nonexistent quantitative relations between other flow regime indicators (like high flows) and ecological variables, as pointed out in section 2, we could, in our study, only do the first of the two steps, i.e. quantify changes in ecologically-relevant river flow regime indicators due to climate change. This is the basis for the next step in which it will be necessary to quantitatively relate the changes in these other four indicators (or others) to ecological changes. We could not do this second step (except for long-term average discharges) but show the results of the necessary first step of the way towards quantifying climate change impacts on freshwater ecosystems due to flow alterations. Therefore, we think that the title of the paper is appropriate.

RC2: Especially for the three basins that have been selected for a more detailed analysis, the authors should provide information on the freshwater ecosystem and the number and kind of fish species present. Are the expected flow alterations and resulting ecosystem changes in the ecosystems relevant for the species present and what will be the impacts? Please describe in more detail. It might as well be possible to re-

C771

late the global scale flow alterations very roughly to a world map of ecosystems. AC2: Given that this paper looks at the global scale, it would not be appropriate to focus too much on three river basins. They are just exemplary basins. Answering the reviewer's request, we now offer additional information on observed numbers of endemic and total fish species for the three basins, and compare them to calculated values for 1961-1990 and the 2050s based on Xenopolous et al. (2005) (in second but last paragraph in new section 4.2). We could not identify an easy way to relate the flow alterations to the world map of freshwater ecosystems, more precisely the Freshwater Ecoregions of the World data set (www.feow.org).

RC3: Does equation 1, derived for zero order basins, also apply to upstream areas of grid cells. Discharge quantities for upstream areas may vary from maybe 1 m³/s up to 200.000 m³/s, is it in these cases still valid to apply the equation? Furthermore the equation has been derived for river basins located between 42_N and 42_S, while the authors apply the equation for the whole world (Fig. 6). Does this introduce additional bias? What is the authors opinion on the validity of the equation outside 42_N and 42_S. It is shocking to see how far of the number of species calculated with this equation is from the number of species observed (Amazone 561 iso 1800, Orinoco 279 iso 88). Is there an explanation for these deviations? Is it possible to derive one equation for the whole world, keeping in mind the variety of ecosystems and species present? AC3: We think that equation 1 also applies to upstream areas of grid cells, but we cannot prove this. We included in the revised version, as a certain caveat, a reference to McGarvey and Hughes (2008) at the end of the first paragraph of the new section 4.1: "However, for three rivers in the Pacific Northwest of the USA, McGarvey and Hughes (2008) found that it is preferable to derive species-discharge relationships not for whole river basins but for individual reaches if distinct fish assemblages exist." Regarding the applicability outside 42_N and 42_S, we have no information. As given in Eq. 1, the R² of the regression relationship is 0.57, which can be regarded a satisfactory, keeping in mind that this is a global-scale approach and many other factors except long-term average river discharge are expected to have an impact on species numbers even though

C772

long-term average river discharge has been found to be the most effective predictor of fish species richness (Oberdorff et al., 1995, as cited in McGarvey and Hughes, 2008). It would definitely be preferable if regionally differentiated regression equations or regression equations that include additional predictors would be derived. We would then use them in our computations.

RC4: Minor comments: For the historic precipitation time-series the monthly number of wet day from the CRU TS 2.1 are combined with the monthly precipitation quantities of the GPCC dataset. Why were these datasets combined? AC4: GPCC does not provide monthly number of wet days. In the CRU data set, stations for which precipitation data were available where not the same stations for which number of wet days were available, so a certain inconsistency also occurs within the CRU data set.

RC5: Section 2.1.1, line 5: a definition of consumptive water use is given. It is "the withdrawn water that does not return to the river but is evaporated". Is it correct that evaporation is the only reason why the water does not return to the river? AC5: It is the major reason. Possibly, part of the water that is not evapotranspired may recharge an aquifer that is not connected to the river. However, given the large uncertainties of computing water withdrawals and consumptive use this can be regarded as insignificant.

RC6: Section 2.1.2, line 20: What method has been used to interpolate the GCM data to the WGHM resolution? AC6: linear interpolation. The word "linearly" was added to line 20.

RC7: Section 4: Results and regime curves (Fig. 7) are given for the Danube, Volga and Missouri because large anthropogenic flow alterations have been made in the past in all three rivers. However, are these river also most interesting from a climate change point of view? AC7: They are interesting also from a climate change point of view, representing various types of climate change effects on river flows. As mentioned already in I.14-19, "we selected the stations because ... climate change will lead to

C773

either lead to either decreasing, approximately constant and increasing long-term average river discharge". The Volga represents a river that will be strongly impacted by temperature-change due to reduced snowfall and earlier snowmelt and shows strongly increased annual river discharge due to increased precipitation. For the Danube it is predicted that that annual flows may decrease and that it is affected with respect to seasonal flows, i. In the Missouri, annual values are predicted to be rather stable in all four scenarios, and there may be slight seasonal shifts, with a longer low-flow period in the summer. These different patterns of climate-change induced changes of river discharge, as provided on p. 1321-1323, will certainly have different ecological consequences.

RC8: Conclusion, page 1327, line 10-13: Is it really possible to conclude that the calculated river flow alterations will result in strong alteration of genetic levels? AC8: Pringle (1997) discussed genetic isolation, i.e reduced genetic flows, as a consequence of human fragmentation of stream continua by human actions. I have added the following sentence to line 13: "For example, genetic flow and variation of populations of aquatic biota may be reduced due to stream fragmentation that will be caused by decreasing future river discharges (Pringle, 1997)."

RC9: Table 2: In the second half of the table the median indicator values for the land areas (in %) are given. Differences between ECHAM4 and HadCM3 are large, is it possible to say something about the reliability of the two climate models or to give an explanation for the difference? AC9: No, it is generally not possible to say something about the reliability of the two climate models, or rather to say that one of the two is more reliable. The differences cannot be explained except that HadCM3 predicts a somewhat dryer future than ECHAM4 as can be seen in Fig. 1 (and Table 2), and is mentioned in l16 of p. 1317. On page 1314 we mention the large uncertainties of climate model results. For clarification, we have added the following sentence in l17 of p. 1317: "Both climate models can be regarded as equally uncertain."

RC10: In the figures results are more often given for the HadCM3 model, is there a

C774

reason for this? AC10: No.

Referee 2

RC11: This paper provides an important and interesting modeling analysis of the effects of climate change on river flow regimes, and posits a comparison of climate effects with flow regime impacts caused by dams and water withdrawals. The results in this paper summarizing the influences of climate change are certainly worthy of publication. However, there are some serious issues with the comparison between climate effects vs. dam/withdrawal effects on flow regimes. First, only a subset (6553) of the more than 50,000 large dams have been included in this analysis. This creates a spatially incomplete representation of dam impacts on flows. Second, reservoir operating rules, which strongly dictate the degree to which dam operations will alter natural flow regimes, are represented in a highly simplified fashion, on a monthly time step, that greatly obscures (attenuates) real dam effects on river flows on shorter (e.g., hourly or daily) time intervals. Third, it is known that the model used for this analysis underestimates the magnitude of alteration caused by dams and water withdrawals, based on comparisons with actual river flow data. However, the accuracy of the climate predictions is unknown. Each of these shortcomings make it unreasonable to draw conclusions such as "Climate change will have a more widespread and stronger impact on ecologically relevant river flow characteristics than dam construction and water withdrawals have had up to now" (page 1326). AC 11: We agree that we cannot draw this conclusion. We have therefore reformulated the sentence and added that our study is likely to underestimate the impact on dams on river flow regimes. The sentences in the second paragraph of the conclusions now read: "Climate change is likely to have a more widespread and stronger impact on ecologically relevant river flow characteristics than water withdrawals and dam construction have had up to now. However, the reliability of this conclusion is weakened by the fact that the impact of dams is very likely underestimated by our study because small reservoirs have not been taken into account (Döll et al. 2009), impacts of dams shorter than monthly time scales have

C775

not been considered, and also the impact of individual dams on monthly flows may be underestimated by our model.” We also added the following sentence to the abstract: “However, dam impacts are likely underestimated by our study.”

RC12: My recommendation would be to instead focus on the potential for climate change to exacerbate the dam and water withdrawals effects reported in an earlier paper by these authors. Additionally, the authors make the important point (on page 1325 and in Figure 8) that climate changes will in many regions present opportunities to improve environmental flow conditions, particularly for rivers that have been altered by dams and water withdrawals. This point should be retained and perhaps given even greater emphasis in the final paper. AC12: We followed this advice and added the following paragraph of the conclusions: ” Climate change will exacerbate flow alterations by dams and water withdrawals in some regions, but provide the potential for improvement of the ecological situation in other regions. On the one hand, for 20-30% of the land area that had suffered from a decrease of naturalized long-term average discharge of more than 10% by 2002 (16% of total land area), discharge is expected to decrease by more than an additional 10% by the 2050s. Depending on the climate model, the affected regions may include the Mediterranean, parts of North America, the Near East and Western China, the Murray-Darling Basin in Australia and Southern India. On the other hand, on 50-70%, this decrease may be reduced or even balanced by increased river discharge of more than 10%. In these river basins, climate change presents opportunities for a water management that better takes into account ecosystem water requirements. These regions may include large parts of North America and the Near East, Pakistan and India as well as Northeastern and Northwestern China.” Furthermore, we added the following sentence to the abstract: “In some of these regions, climate change will exacerbate the discharge reductions, while in others climate change provides opportunities for reducing past reductions.”

Additional Points: RC13: In the opening paragraph, the authors state that “...with one exception, transferable quantitative relations between flow alterations and ecosystem

C776

responses have not yet been derived.” This statement should be clarified and qualified. It is true that globally applicable linkages between flow alteration and ecological outcomes remain elusive. However, such linkages have been widely developed for specific rivers, and even for large regions (e.g., Tennant Method developed from hundreds of rivers in US). Additionally, there are numerous “ecosystem responses” that are of a chemical or physical nature that are well-understood and universally applicable, such as known relations between freshwater inflows and estuarine salinities, or the relationship between flow magnitudes and streambed particle sizes. The authors should clarify that they are using one particular relationship – describing the apparent influence of river discharge with freshwater species richness – to illustrate possible ecological consequences. AC13: In the statement, which is made in the abstract, the term “ecosystem responses” was replaced by ecological responses”, following the terminology of e.g. Poff and Zimmerman (2010), as we refer not to responses e.g. of the sediment but of the biotic part of the ecosystems. In addition, we have added a sentence with respect to the Tennant Method to the fifth paragraph of the introduction: “The Tennant Method (Tennant 1976), which has been applied for a reservoir outflow management in a large number of river, cannot be used for assessing the impact of altered flow regimes on freshwater ecosystems as it only refers to instantaneous flows but not to the ecologically relevant temporal sequences of flows, i.e. river flow regimes.” The second but last paragraph of section 1 now ends with the following sentence: “To illustrate possible ecological consequences of alterations of river flow alterations, we applied the empirically determined relationship between long-term average discharge at the mouth of river basins and number of endemic fish species of Xenopoulos et al. (2005), which was already used by Xenopoulos et al. for translating reduction of long-term average discharge due to climate change into reduction of the number of fish species.”

RC14: Has the relationship developed by Xenopoulos et al (2005) been tested for its predictive capability? My understanding is that it is based on a regression relationship linking discharge and species richness, but its ability to accurately predict how species

C777

richness would decline in rivers experiencing flow depletion has not been tested to my knowledge. Therefore, the authors use of this equation in a predictive manner is questionable. AC14: No, the regression equation has not been tested for its predictive capability. We have included a caveat to the last paragraph of discussion section 4.1 which reads: "Besides, Equation (1) has not been tested for its predictive capability."

RC15: In the abstract, emissions scenarios are referred to as "A2" or "B2", which may be recognized by scientists familiar with climate models, but may require more explanation upon first mention in this paper. AC15: The abstract is already very long, such that it is not possible to say more than is already said about the differences in the scenarios that was already said (that B2 emissions are much smaller than A2 emissions). More details are provided in section 2.1.2.

RC16: The authors begin a number of sentences with "Besides,...." where other words such as "Additionally," may be more grammatically correct or preferable. AC16: The occurrences of "besides" were checked, and the term was replaced by "furthermore", "additionally" or "in addition".

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