Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-485-RC3, 2017

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Climate change alters low flows in Europe under a 1.5, 2, and 3 degree global warming" *by* Andreas Marx et al.

Anonymous Referee #3

Received and published: 29 September 2017

This manuscript deals with a multi-GCM and multi-hydrological models assessment of changes in low flows across Europe between a present-day period (1971-200) and 3 different global warming levels: 1.5K, 2K, and 3K (and between them as well). It therefore contributes to document the effects of climate change on low-flow hydrology in Europe in the context of the Paris Agreement. This manuscript thus deals with a topical and important topic, and fits well into the scope of HESS. It is generally well structured and written, and conclusions are generally well supported by results shown. I have however two main comments (as well as specific comments) detailed below that should be addressed before the manuscript is published in HESS.

1 Main comments

1.1 Hydrological calibration and simulation over influenced catchments

The calibration details (specific comment #9 and #10) as well as the validation results (specific comments #12, #13, #14) do not give enough confidence on the quality of hydrological modeling, and highlights the issue of calibrating and/or validating – seemingly natural-catchment-only – models against highly influenced catchments like the Ebro or the Rhône, especially for low flows. First there is not enough information on the calibration process, and even the catchments used for that are not identified. Second, validation is done for a large part over influenced catchments, and also over ensembles of highly nested catchments. Both points should be reconsidered in a future revision of the manuscript.

1.2 Scale of catchments selected for presenting and averaging results

There are numerous inconsistencies throughout the manuscript in terms of the minimum catchment size used for presenting results (and giving averaged figures), see specific comments #20, #21, #22, #24, #27. Addressing this comment may imply reformatting all results, but this is also intrinsically linked to main comment #1. Indeed, the manuscript state that the runoff routing scheme prevent using results for catchments smaller than 10000 km2, and near-natural catchments are usually only smaller than that in Europe. In parallel, maps of results are given over a river network encompassing drained areas much small than the indicated threshold. This thus shades doubts (maybe unjustified, but is has to be demonstrated) on the validity of models and results, together with issues highlighted in main comment #1.

Printer-friendly version

Discussion paper

2 Specific comments

1. P1L2, “1.5, 2 and 3 K”: please specify that this is with respect to the preindustrial period
2. P1L10, “-12%”: What is the baseline period here? This is all the more important that there could easily be confusion with the baseline used for the global warming level (see above).
3. L11-12: this sentence is ambiguous. Less snowmelt may imply less streamflow in some conditions (e.g. constant liquid precipitation or declining total precipitation). Please rephrase and make it clearer.
4. P1L13-14: This sentence is also quite ambiguous. What is exactly preventing distinguishing between 1.5 and 2K warming effects? Is it the interannual variability which prevents distinguishing statistically estimates of period-averaged changes-for a given GCM-HM combination? Or is it the uncertainty due to the multimodel ensemble that prevents distinguishing ensembles of multimodel period-average estimates between present and future? Or both? Please make it clear here.
5. P2L11: The low-flow component of the 2015 drought event has been specifically studied by Laaha et al. (2017). I believe this reference is worth adding to the manuscript.
6. P2L21-27: What is the time slice that corresponds to the quantitative and qualitative results recalled here? Please make it clearer.
7. P4L7-10: First, this interpolation step should not be called downscaling as the latter refers to methods that actually add information for each day (either through regional climate models or empirical-statistical downscaling models) to the larger scale GCM fields. I would therefore strongly recommend using “disaggregating” or “disaggregated” instead of “downscaling” or “downscaled” in the manuscript.

8. P4L7-10: Second, this interpolation step should be better documented here, in order for the reader to understand the advantages and shortcomings of this approach, which are essential for assessing the quality of subsequent hydrological simulations. This interpolation step should ideally be assessed using a global reanalysis against high-resolution gridded datasets, like RCMs are actually assessed (see e.g. Kotlarski et al., 2017, among many others, for a recent example). This would critically allow distinguishing errors coming from (1) the spatial interpolation technique (and their large-scale forcings), and (2) the hydrological models. Please at least add some comments on that in the manuscript. Plus, the reference used for this interpolation technique is incomplete in the list of references.

9. P4L24: What are these 9 catchments? Please provide some more information (location, surface, etc.). Are they near-natural or influenced catchments?

10. P4L24-26: What is the period used for calibration? And what are the calibration criteria (for both automatic and manual calibration)? Are they specific for low flows? Please carefully specify all this in the manuscript.

11. P4L27-29, “The assessment. . . (Gosling et al., 2017)”: This is a very strong statement, which I tend to disagree with at least as a general conclusion. This is moreover hardly supported by the reference given in the manuscript, which compares global hydrological models and catchment hydrological models for the Rhine and Tagus (and other catchments, but not located in Europe). Results for a low flow indicator (Q95) show a large divergence of the two types of models with increasing global warming level (Gosling et al., 2017, their Fig. 2). As a conclusion, I would therefore strongly recommend removing this statement from the manuscript.

12. P4L33-P5L4 and Figure 1: The assessment of HMs is very light and not strongly supported by Fig. 1. Indeed, this figure is potentially misleading, as it basically only checks that catchments have equally small/large indicators (Q90 or Q50)

for both observations and simulations, which is mainly driven by the size of the catchment. I would therefore recommend using a different and more informative representation of differences, preferably in terms of relative errors (in percents), and also preferably as maps in order to show the potential spatial pattern in errors. This representation would also greatly help in comparing present-day errors to relative changes presented later in the manuscript. I personally would not give too much credit for a model showing for a given location present-day errors as large as 3K future changes...

13. Figure2, right: This figure shows the location of validation gauges used in Fig. 1. First, it shows that many points in Fig. 1 comes from the same rivers and are necessarily highly correlated, which inherently bring some bias to the results that should be representative of the whole Europe. I would strongly recommend removing redundant points scatter plots like presented in Fig. 1. This would not be a problem however with suggested spatial representations (cf. above).

14. Figure 2, right: The second point is that several validation gauges are located on highly influenced rivers. For example, the Ebro river (Spain) is heavily influenced by water abstractions for irrigation, and the seasonal regime of the Rhône river (France) is heavily influenced by all the hydropower reservoirs located in the Alps (and other surrounding mountain ranges). There are many other cases that can be spotted on the map. As a consequence, observed streamflow indicators for low flows simply cannot be compared to natural (i.e. without human influence) hydrological simulations for these catchments. A good fit to observations may indeed reveal that physical parameters in HMs are tweaked to compensate for no representation of human influence. This may not be a problem in itself (at least for practical modeling purposes if not scientifically satisfactory) if human influences would not have changed and would not change in the future. Which has happened and definitely will. As a conclusion, I would strongly recommend using only near-natural catchments as validation (and also calibration) gauges for

natural hydrological modeling (as I suppose it is the case in the manuscript, even if some HMs considered may represent human influences). A number of reference hydrometric networks have recently been developed at the country scale (Hannaford and Marsh, 2008; Giuntoli et al., 2013; Murphy et al., 2013), and one should take advantage of these. Note that these networks overlap for some countries (but not for some other) with stations tagged "climate sensitive" in the Global Runoff data Centre.

15. P6L3: The 0.46K figure has uncertainties attached to it, according to the reference cited (Vautard et al., 2014). Please do mention these uncertainties in the manuscript, with possibly additional references that provides 1971-2000 estimates of global warming level.

16. P6L20: The use of calendar year is not entirely satisfactory for computing Q90 in snow-influenced catchments where the low-flow period (or one of the low-flow periods, which is a more difficult situation) may span two calendar years. Please consider changing the calculation procedure or at least justify this approximation.

17. P7L8-9: Please mention here (rather than in the results section) that the robustness is compute as the percentage of projections showing a significant change.

18. Table 2: Please make clear that "1980s" refers to the 1971-2000 period.

19. P8L3-9: I don't really understand this peculiar choice of method for computing the relative contributions of uncertainty from GCMs and HMs. Many studies demonstrated that simple Analysis of Variance (ANOVA) approaches are perfectly suited to this case, and it has been recently widely applied to compute contribution from GCMs and HMs (see e.g. Giuntoli et al., 2015; Vetter et al., 2017, among many others), even by some of the authors of the present manuscript (Mishra et al., 2015). ANOVA approaches can critically take account of GCM/HMs interactions, which is presumably not the case of the method used, and of the different sizes

of fixed effects. The set-up is here rather simple compared to more complex ones that consider unbalanced number of runs from each GCMs and/or multiple sources of uncertainty (see e.g. Addor et al., 2014; Vidal et al., 2016). I therefore strongly recommend using a simple two-way ANOVA approaches for the present study, or at least check current results against a simple two-way ANOVA approach. Indeed, I am unsure of how this sequential sampling approach relates to the more traditional ANOVA approach, and what their respective underlying hypotheses are. I would welcome some online discussion on this.

20. P8L13-15: First, this should come much earlier in the manuscript. Second, this is not consistent with maps of streamflow changes that seemingly include results for catchments with a surface lower than 10000 km2. This should be clarified. This is closely linked to specific comment #14.

21. Figure 3 (and Fig. 5 and Fig. 6). See comment above. Plus, the figure indicated above each map is seemingly a continental average of the plotted value along the river network. First, this should be clarified. Second, this value is closely related to the choice of the minimal catchment surface area considered. Values would be very different if, as stated P8L13-15, only catchments with an area larger than 10000 km2 would be considered. Please make all these statement and results consistent across the manuscript.

22. P10L3-4: This statement is somewhat inconsistent with the choice of the calendar year use for the calculation of Q90. Please clarify this in the manuscript.

23. P10L7, “models”: I presume this should be “simulations”.

24. P11L1, “new spatially explicit information”. This is again contradictory with the 10000 km2 statement. Cf. comments above.

25. P11L16-17.This sentence is ambiguous. The increased spread along the 1:1 line (i.e. when smaller and larger values are considered) does indeed contribute to

a higher coefficient of determination, which is not the case for the spread across (i.e. with higher residuals from) the 1:1 line. Please rephrase.

26. Figure 4: Several presumably regression lines are given on the graph. Please either define and comment them, or remove them. Also, please add lines delimiting the quadrants.

27. Figure 4: The legend states that only catchments with a surface area higher than 10000 km2 are considered. This is again not consistent with values provided by other figures.

28. P13L3-5: This is already written P10L35-P11L2. And this is commented in specific comment #24.

29. Title of Section 3.2: The difference between section 3.1 and section 3.2 are not understandable based on this title, and the reader may be unsettled at this point as I was. There should be something of a "between the levels of warming" somewhere. Please rephrase.

30. Figure 5. Cf. comment #21.

31. P15L15-16: The increase in winter low flows would not necessarily lead to a higher hydropower potential. It actually depends on the evolution of total precipitation. And the possible evolution of hydropower production would depend on the type of reservoir management, as well as management rules constrained by possible other water usages (sustaining summer low flows downstream, irrigation, recreation, etc.). Moreover, a decrease in low flows does not necessarily imply a decrease in overall water availability average over the year, and the water stress is conditional on the respective weight of water availability and water demand for a given time. So I would recommend adapting the statements according to the above comments.

32. P15L17-18: I however completely agree with the need of regional adaptation options. Except that adaptation strategies should be put in place now, without waiting for the 3K level to be reached or not.

33. P16L6, “the result is independent of the sign of change”: Well, this is a potentially serious issue. Indeed, how to interpret a situation where e.g. out of15 projections, 5 give a significant upward change, 5 other no significant change, and the last 5 a significant downward change? I would recommend interpreting this situation with particularly no robust signal! So please make clearer in the manuscript all the different possible cases and the way to interpret them. An alternative for presenting robustness would be the one used in the IPCC AR5 WGI report, i.e. the percentage of projections agreeing on the sign of the change.

34. P16L11-13: I totally agree with this sentence, but it comes here out of the blue. Please consider moving it to the introduction, discussion, or conclusion.

35. Figure 6. The choice of colour breaks is here particularly unfortunate here. For the SNR, I would appreciate having a break in value 1, in order to see where the median change is higher than the uncertainty in projections. For the ratio of GCM to HM uncertainty contribution, this is all the more important to see where this crosses the 1 value. An alternative would be to use bivariate colour scales (Teuling et al., 2011) to jointly plot the evolution of both sources of uncertainty.

36. P17L4-5: This exact sentence has already been written P8L3-4, and commented above (comment #19)

37. P17L8-P18L3: I am more or less OK with what is written here, but I do not understand why this would imply that the ratio of HM contribution to GCM contribution is higher at the 3K level. Please provide some explanations in the manuscript. Couldn’t this be related to timing of threshold crossing in HM behavior that would

differ from one HM to another, e.g. going from energy-limited to water-limited evaporation process?

38. P18L4-20: This whole paragraph tends to support the above hypothesis. This should be related in the manuscript to recent uncertainty decompositionresults obtained for a catchment located in the Southern Alps. It showed that the increasing spread of changes in future low flows by different HMs is linked to increasing spread in simulated evaporation and snow water equivalent (Vidal et al., 2016).

39. P19L28-30, “We conclude. . . support the adaptation process.” Well, this is actually only a wish. Nothing in the paper allows asserting that, even I personally hope this is the case. So please rephrase.

3 Technical corrections

1. P1L5, “unprecedented”: it is a bit far-fetched, given that (1) GCM forcings are only disaggregated to this resolution without adding any downscaling information, and (2) results are seemingly partly given only for catchments >10000 km2 (P8L13-15).

2. P1L6: “combination”

3. P2L2: “independently”?

4. P2L22-24: I believe that the sentence is not grammatically correct.

5. P2L30: “2”? in reference (UNFCC, 2015)

6. P2L34: “because of”

7. P3L3: please check missing or incorrect “the”

Printer-friendly version

Discussion paper

8. P3L8: “southern Europe”
9. P11L11: “extent”
10. P13L5: “political” -> “policy”. Also P15L23.
11. P15L22, “distinguished”: please rephrase.
12. P15L24, “ensemble members”: Please clarify what they are.
13. P20L1, “pronounced”: What is? Please rephrase.
14. P21L5-8: Wrong formatting, cf. IPCC report citation rules.
15. P23L34: line feed
16. P24L25-26: extra information to be removed

4 References

Addor, N., Rössler, O., Köplin, N., Huss, M., Weingartner, R. Seibert, J. (2014) Robust changes and sources of uncertainty in the projected hydrological regimes of Swiss catchments. Water Resources Research, 50(10), 7541-7562. doi: 10.1002/2014WR015549

Giuntoli, I., Renard, B., Vidal, J.-P. Bard, A. (2013) Low flows in France and their relationship to large-scale climate indices. Journal of Hydrology, 482, 105-118. doi: 10.1016/j.jhydrol.2012.12.038

Giuntoli, I., Vidal, J.-P., Prudhomme, C. Hannah, D. M. (2015) Future hydrological extremes: the uncertainty from multiple global climate and global hydrological models. Earth System Dynamics, 6(1), 267-285. doi: 10.5194/esd-6-267-2015

Gosling, S. N., Zaherpour, J., Mount, N. J., Hattermann, F. F., Dankers, R., Arheimer, B., Breuer, L., Ding, J., Haddeland, I., Kumar, R., Kundu, D., Liu, J., van Griensven, A., Veldkamp, T. I. E., Vetter, T., Wang, X. Zhang, X. A comparison of changes in river runoff from multiple global and catchment-scale hydrological models under global warming scenarios of 1.5°C, 2°C and 3°C (2017) Climatic Change, 141(3), 577-595. doi: 10.1007/s10584-016-1773-3

Hannaford, J. Marsh, T. J. (2008) High-flow and flood trends in a network of undisturbed catchments in the UK. International Journal of Climatology, 28(10), 1325-1338. doi: 10.1002/joc.1643

Kotlarski, S., Szabó, P., Herrera, S., Räty, O., Keuler, K., Soares, P. M., Cardoso, R. M., Bosshard, T., Pagé, C., Boberg, F., Gutiérrez, J. M., Isotta, F. A., Jaczewski, A., Kreienkamp, F., Liniger, M. A., Lussana, C. and Pianko-Kluczyńska, K. (2017) Observational uncertainty and regional climate model evaluation: a pan-European perspective. International Journal of Climatology, in press, doi:10.1002/joc.5249

Laaha, G., Gauster, T., Tallaksen, L. M., Vidal, J.-P., Stahl, K., Prudhomme, C., Heudorfer, B., Vlnas, R., Ionita, M., Van Lanen, H. A. J., Adler, M.-J., Caillouet, L., Delus, C., Fendekova, M., Gailliez, S., Hannaford, J., Kingston, D., Van Loon, A. F., Mediero, L., Osuch, M., Romanowicz, R., Sauquet, E., Stagge, J. H. Wong, W. K. (2017) The European 2015 drought from a hydrological perspective. Hydrology and Earth System Sciences, 21(6), 3001-3024. doi: 10.5194/hess-21-3001-2017

Mishra, V., Kumar, R., Shah, H. L., Samaniego, L., Eisner, S. Yang, T. (2017) Multimodel assessment of sensitivity and uncertainty of evapotranspiration and a proxy for available water resources under climate change. Climatic Change, 141(3), 451-465. doi: 10.1007/s10584-016-1886-8

Murphy, C., Harrigan, S., Hall, J. Wilby, R. L. (2013) Climate-driven trends in mean and high flows from a network of reference stations in Ireland. Hydrological Sciences Journal, 58(4), 755-772. doi: 10.1080/02626667.2013.782407

Teuling, A. J., Stöckli, R. Seneviratne, S. I. (2011) Bivariate colour maps for visualizing climate data International Journal of Climatology, 31(9), 1408-1412. doi: 10.1002/joc.2153 Vautard, R., Gobiet, A., Sobolowski, S., Kjellstrom, E., Stegehuis, A., Watkiss, P., Mendlik, T., Landgren, O., Nikulin, G., Teichmann, C. Jacob, D. (2014) The European climate under a 2 degrees C global warming. Environmental Research Letters, 9(3). doi: 10.1088/1748-9326/9/3/034006

Vetter, T., Reinhardt, J., Flörke, M., van Griensven, A., Hattermann, F., Huang, S., Koch, H., Pechlivanidis, I. G., Plötner, S., Seidou, O., Su, B., Vervoort, R. W. Krysanova, V. (2017) Evaluation of sources of uncertainty in projected hydrological changes under climate change in 12 large-scale river basins. Climatic Change, 141(3), 419-433. doi: 10.1007/s10584-016-1794-y

Vidal, J. P., Hingray, B., Magand, C., Sauquet, E. Ducharne, A. (2016) Hierarchy of climate and hydrological uncertainties in transient low flow projections. Hydrology and Earth System Sciences, 20(9), 3651-3672. doi: 10.5194/hess-20-3651-2016

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-485, 2017.