

## ***Interactive comment on “A three-pillar approach to assessing climate impacts on low flows” by G. Laaha et al.***

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This manuscript is based on the presumption that the combination of statistical analysis, process-based modeling using climate and stochastic projections as well as expert judgement is the best way to assess climate impacts on low flows. Without any further analysis, one could dare say that this premise should be true considering that this approach has much more information than any single analysis and thus should have less chance of not finding an answer that is closer to the true one. The authors strive to demonstrate the advantages of the proposed approach and the validity of this premise with a regional study conducted in four Austrian river basins. The manuscript is well written although it is a bit too long in my opinion. The topic of the study is relevant for HESS but the manuscript requires a substantial revision before publication. Below, I

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provide a number of issues to be clarified before publication.

- My first remark refers to the terminology chosen for this manuscript. My impression after reading the abstract and the introduction is that the names given to the various methods and the proposed “three-pillar” approach can be considerably simplified without diminishing the message that the authors try to convey. On the contrary, it will help the reader. I wonder, for example, what a data-based method has to do with a downward approach (downward refers to “toward a lower place, point, or level” )... and conversely a mechanistic one with an upward approach ... I know that these terms have been used in current literature, but in my opinion, these buzzwords can be replaced by method A and B without changing the meaning of the sentences. I suggest either to justify the meaning of “downward” and “upward” in the present context or even better, to simplify the text. In my opinion, the so-called “downward approach” is a classical statistic method, so I wonder why not calling it simply like that.
- In this study, old IPCC nomenclature for emission scenarios (A1B, B1, A2 etc) are still used instead of the newer RCPs proposed by the IPCC. Newer climate projections (e.g., CMIP5) are readily available for quite some time. Please explain why.
- Authors do not formulate in the introduction a research hypothesis to be tested. I guess, the authors intend to test that the “Three-pillar approach” is superior than any of the single ones, but failed both to explicitly mention this hypothesis and to present statistic evidence that corroborates this assertion.
- L19, P9. If a hydrologic model is used in this study, I do not understand why a runoff index is not used instead of a meteorological drought index like SPEI. Streamflow, and thus low flow characteristics, are the outcome of the whole hydrologic system that is represented by a hydrological model. Moreover, it is

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well documented in the literature that atmospheric drought indices are quite transient whereas those related to soil moisture, groundwater, and runoff are not (Samaniego et al JHM 2013 and sources therein). Thus, the stochastic dependence of SPI or SPEI with any low-flow index is, in general, not significant (Kumar et al. 2016 HESSD). It should also be explained why a Gaussian transformation (perhaps due to a long tradition... ) should be applied to a variable that is definitely non-Gaussian (i.e.,  $P - E_P$ ). L14 P9. A more reliable approach to “check the realism” of the ensemble climate simulations would be to estimate a runoff index over a historical period in which reanalysis (or hindcasts) and historical meteorological forcings are available. This is probably the best way to know whether a RCM or a Numeric Weather Prediction Model output can explain observed low-flow spells or other kinds of drought events as proposed by Thober et al. 2015.

- L18 P.5 It is not clear to me why the “first and second pillars” do not use local information used in the third pillar. After all, trends are based on local meteorological observations and any rainfall-runoff model, to my knowledge, uses local observations of rainfall, temperature, and discharge. Please elaborate why they have to be different (L22)?
- L17 ff, P5. I guess authors demand too much from downscaled GCM-RCM forcings. GCM and RCM are climate models describing the evolution of physical processes in the atmosphere, ocean, cryosphere and land surface at large temporal and spatial scales (about  $2.5^\circ$ ). They are not intended to describe transient states, consequently one can not say that they are reliable or not. They do not have all the process necessary to describe rainfall generation at smaller scales like high resolution numerical weather models have if they are run at 1 km to 2 km spatial resolution. RCMs at  $1/4^\circ$  resolution and larger would be hardly able to estimate convective precipitation over mountainous areas like Austria. For GCMs, this is almost an impossible job. If this is known, I wonder why the hydrology community insist on getting “reliable” daily precipitation (say from RCMs in

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reanalysis mode) from these models so that low-flow statistics can be estimated ... Dynamic and stochastic downscaling may help a bit but many studies have shown, for example, that very few RCMs from the ENSEMBLES project are even able to get extreme statistics of the observed rainfall fields at monthly time scales (see e.g., Soares et al. 2012 JGR in Portugal, and Thober & Samaniego JGR, 2014 in Germany). As a consequence, low-flow statistics and its variability (e.g.,  $Q_{95}$ ) obtained from reanalysis (e.g., WATCH) should be evaluated as expectations over reasonable periods (e.g., over decades). Likely yearly statistics are too short a period. See for example Schewe, J. et al. as an alternative.

- L13 p8. The area of the river basins and the sampling size used in this study are probably too small to derive conclusive results. Authors should consider that the area of a GCM grid cell like ECHAM5 is at least  $9 \times 10^4 \text{ km}^2$  and that of a RCMs used in Reclip:century is approximately  $1 \times 10^2 \text{ km}^2$  (based on the project report). As a rule of thumb, due to the Courant–Friedrichs–Lewy condition, it is not recommended to use prognostic values of state variables or fluxes obtained by numeric integration for areas less than four times the area of a typical grid cell. This implies that the minimum area to be considered in this case is a basin with at least  $4 \times 10^2 \text{ km}^2$ . Three of the study areas do not fulfill this condition. As a result, the uncertainty of the numerical model plus that of the downscaling techniques would increase dramatically which, in turn, would negatively affect the impact analysis. I recommend to test this approach in large basins that fulfill this condition and to enlarge the sample size considerably.
- L15 P11, I suggest to use a non-parametric test to estimate confidence bounds considering that the underlying variable is certainly non-Gaussian. In this case, parametric t-Student estimations for confidence bounds do not apply.
- The structure of the manuscript is cumbersome in some sections. I suggest that methods and results from every approach is presented separately to ease

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reading. The number of sections is quite large for a research paper in my opinion. This manuscript is a bit long too.

- L31, p19. Authors do not attempt to estimate “how strongly the pillars agree”. It will be very enlightening to see a statistical analysis in this respect.
- L2 ff p 26 As I said earlier, I have no doubt of this statement. In general, more information should lead to more reliable results. I do not see novelty on this statement. This can be inferred, for example, from simple parametric statistical tests by gradually changing the sampling size and estimating the effect on the confidence bounds for a given statistic. L29 ff is a consequence of this. Authors should present results and make statistical tests that demonstrate with large degree of certainty that adding information gradually leads to better results in this case. I have, however, reservations, on how soft data (e.g. historical reports), or subjective impressions can be used in a formal statistical analysis to “correct” confidence bound.
- Fig 11 is quite dense. It is supposed to be a synthesis, but I hardly can understand it. Sorry.

In my opinion, this manuscript could become a nice contribution to the field if these issues are addressed before publication.

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