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Discussions

Interactive comment on "Influence of downscaling methods in projecting climate change impact on hydrological extremes of upper Blue Nile basin" *by* M. T. Taye and P. Willems

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I am left with the impression that the hydrology model VHM is forced with basin average quantities (in part due to the statement on p. 7868 that "The comparison is done after calculating the basin-wide rainfall from both the observations and the control runs." This is a little hard to believe, and perhaps I don't have this right – the Blue Nile is such a large basin that spatial differences in forcings and response have to be important, and most modern era hydrology models would be spatially distributed at some level (i.e., if conceptually lumped, at least applied to subcatchments with different forcings, and more commonly run over a spatial grid of some kind). This is important, as both

the GPM and weather generator discussions seem to be mapping directly to/from an observational time series, but these is only one such time series as nearly as I can tell. At one point in the paper, 11 stations are mentioned, but whether these have somehow been averaged to produce a single series isn't at all clear. In their "flow chart" (p. 7866), there is nothing to indicate looping over sub-basins, or stations. Furthermore, the stations are essentially points, whereas the GCM represents large grid cells, and the scale mismatch isn't addressed.

Response: Indeed the VHM model was used to simulate the flow from basin-wide rainfall and evapotranspiration data. The basin wide rainfall was calculated from the 11 stations using the Thiessen polygon method. This was done for the observation data as well as the perturbed observed series before running the VHM model. The flow chart was presented to indicate what was done for one station and one GCM run. Of course this was repeated for the rest of the stations and the remaining GCM runs. These will be explicitly stated in the revised version.

It is true there is a scale mismatch between the GCM grid cells and the point rainfall. This bias due to this difference is assumed to be similar for the current and the future period. Therefore, it will cancel out when obtaining the change factors (perturbation factors). These factors are applied to the observation dataset to obtain the future projected rainfall for each station. It is assumed the scale mismatch is implicitly addressed through the process (bias correction and spatial downscaling are implicitly combined in the QPM approach).

In the implementation of the weather generator, they state explicitly that it is implemented for each (of the presumably 11) stations historically. I interpret this as meaning that point data could now be synthesized for each of these 11 points for current climate, and as a next step, they adjust the weather generator parameters to reflect future climate. But, a key problem with weather generators is that to my knowledge, they don't account for coherence between the multiple points – so while the statistics should reflect spatial variations, the time step simulations do not (i.e., they don't reproduce storm characteristics, such as movement across a large river basin). This is a critical consideration for the Blue Nile, which again, is quite large, and has to be strongly affected not only by temporal, but by spatial variations in the inputs as well.

Response: It is true that the fact the weather generator does not account for coherence between the different stations was overlooked in this study. A solution is proposed, and already implemented and tested with good results, where first basin-wide rainfall is obtained using the Thiessen polygon method followed by calibrating and validating LARS-WG using this series. The coordinates of the middle of the catchment can be used as a reference point for the basin-wide rainfall series. An important question that can be raised when implementing this approach is whether the monthly change factors used to adjust the weather generator parameters are representative of the spatial differences within the catchment. To verify this, the mean of the monthly factors obtained for the individual stations were compared with what is obtained for the basin-wide approach. The results indicate that the monthly change factors from both approaches are quite similar conforming that basin-wide approach is applicable. In the revised paper we will present both methods.

The problem is much more likely to be dynamical errors in the GCMs. That being the case, the authors may want to do some screening of GCM output (e.g. from the CMIP5 archive) to select a subset of GCMs that reproduce the variables of interest to them plausibly well.

Response: Using CMIP5 GCM outputs is an option. However given that LARS was produced using the previous generation of models, the comparison between the two methods would be unreasonable.

The key result in the paper that QPM downscaling implies decreasing mean annual precipitation, whereas the weather generator implies increases, simply is not plausible. A minimal requirement of any downscaling method is that at least approximately, it needs to reflect the dominant signals from the GCMs. So if the global model says that

precipitation should be increasing, and the downscaling method says decreases, this has to be an implausible artifact of the downscaling. As I tell my students when they bring me results that don't make sense, I don't necessarily know why, but it is wrong. At this point, in my view they should have stopped, rewound the entire process, and tried again.

Response: We agree that a downscaling method should be able to reflect the dominant signal from the GCMs. In case of LARS the rainy seasons had a decreasing signal while the dry season shows very high increasing signal. The same kind of signal was observed when using the QPM method for the rainy seasons. This gives us the confidence that both methods reflected the dominant signal of the GCMs. The main difference between the two methods is the result of the magnitude of change in the different seasons. This might be related to the spatial coherence aspect raised in the earlier comment. With the basin-wide approach it is clearer that both downscaling methods reflect the dominant signals for the higher aggregation levels consistently while differences remain in the tail of extreme value distribution's shape. The basin-wide approach will be reported in the updated version to meet this comment by the reviewer.

One final comment – less important than those above: The temperature indexed PET (Hargreaves) is suspect – it's quite likely that it overestimates the sensitivity of PET to changing climate. For information on the pitfalls of using temperature indexed PET for projections outside the observed range (e.g., for climate change studies) see a 2011 J. Great Lakes Research paper by Lofgren et al; and also recent work on the subject by Chris Milly.

Response: We acknowledge that the Hargreaves method is not the best method to obtain the PET. However due to lack of data for the other variables we could not use the FAO PM method. We will clearly state the uncertainty that might be involved because of the use of Hargreaves equation.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7857, 2013.