Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-189-RC1, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Spatial Extent of Future Changes in the Hydrologic Cycle Components in Ganga Basin using Ranked CORDEX RCMs" *by* Jatin Anand et al.

Anonymous Referee #1

Received and published: 27 June 2017

The paper presents downscaled outputs for hydrological variables in the Ganga river basin. Although the title conveys otherwise, in my opinion, the paper presents nothing more than standard studies using GCM/RCM simulations in conjunction with hydrological models to provide future hydrological projections, except that this study also ranks the climate models. In fact, instead of 'Spatial Extent of Future Changes in the Hydrologic Cycle Components in Ganga Basin using Ranked CORDEX RCMs', the paper could have simply been titled 'Downscaled hydrologic projections in Ganga Basin using Ranked CORDEX RCMs'. As a case study the paper may be of value, however, the current version of the manuscript falls seriously short on scientific contributions or providing new insights for the region. For example, I referred to three other papers

recently published in HESS on related topics as follows:

Maurer, E. P., Ficklin, D. L., and Wang, W.: Technical Note: The impact of spatial scale in bias correction of climate model output for hydrologic impact studies, Hydrol. Earth Syst. Sci., 20, 685-696, doi:10.5194/hess-20-685-2016, 2016.

Grouillet, B., Ruelland, D., Vaittinada Ayar, P., and Vrac, M.: Sensitivity analysis of runoff modeling to statistical downscaling models in the western Mediterranean, Hydrol. Earth Syst. Sci., 20, 1031-1047, doi:10.5194/hess-20-1031-2016, 2016.

Guimberteau, M., Ciais, P., Ducharne, et al.: Impacts of future deforestation and climate change on the hydrology of the Amazon Basin: a multi-model analysis with a new set of land-cover change scenarios, Hydrol. Earth Syst. Sci., 21, 1455-1475, doi:10.5194/hess-21-1455-2017, 2017.

Each of these studies present at least some new scientific insights that can guide further studies in these areas. The present study, on the other hand, simply reports statistical findings for the region under consideration instead of providing actual physical reasons that can help in process understanding or discussing justifications for the choices they make, nor does it ask or answer any new questions. Firstly, the authors should justify why only climate change may play a major role in hydrology of the basin instead of changes in landuse/land cover, particularly in this largely irrigated areas with changing cropping patterns and rapid urbanization. What role does human water withdrawal play, and how are they accounting for that in their analysis?

The authors also present scenarios till 2100. Is it sensible at all to assume that the LU/LC patterns, the vegetation patterns and the irrigation technologies will remain unaltered till 2100? If these processes have changed in the last 30 years, it is likely that they will change in the future as well. Many studies are now looking at LU/LC projections (e.g. the third paper listed above).

Further, bias correction is something that one should be cautious about. The climate

model simulations are obtained by conserving laws of physics. However, if some statistical exercise is carried out on their output to deliberately match observations, one may violate basic physics of the problem. Moreover, bias correction can map unrelated variables and distribution-based methods (such as the QQ mapping) may not detect lack of a link (refer to papers authored by D Maraun). In fact, some experts liken the process of bias correction to 'transforming elephants to mouse' (Will hydrologists learn from the world around them?: Empiricism, models, uncertainty and stationarity, U Lall, AGU Fall Meeting Abstracts 1, 01, 2010)! Therefore, the ability of the current analysis to inform practitioners and water managers at the end of the next century is questionable.

Next, why SWAT? Why not others? And why RCP4.5? Though umpteen number of studies used SWAT earlier for similar analyses (many from the authors' institute), here, the authors are attributing some of the future changes to alterations in snowmelt (paragraph 20 for example). Have the authors used any snow module in SWAT? Simply considering the low flows as snowmelt may not be accurate. What about groundwater? What is its relative contribution to low flows? If more precipitation falls as rain and less as snow (as expected from global warming), this translates into an increase of winter discharge and an increase in recharge and base flows, but the paper reports otherwise (para 15). The locations considered in this analysis (for example, Rishikesh) are not regions of permanent snowpack. Also, RCP8.5 is the most realistic scenario based on evolution of the current climate since 2005. Not clear why the authors chose RCP4.5.

The authors might want to include a plot of historical and future monthly climatology. Since the region is highly monsoon dominated, why should one expect changes in winter precipitation (say, more precipitation falling as rain instead of snow) to cause major alterations in monsoon rains and resultant runoff?

Further, I don't agree with the authors that RCMs have 'been a boon to study changing climate conditions'. Studies have shown that CORDEX RCMs show little improvement over their parent GCMs (Singh et al., Clim. Dyn., 2017) for the Indian monsoon region. Firstly, the authors' evaluation of the RCMs on simulated runoff might be contaminated

by performance of the SWAT model in differing precipitation ranges; therefore, they should be evaluated on a variable that they simulate, namely, precipitation.

More importantly, why do these three models rank better than the others? Are they permitting more physical processes (such as convection?) or are their parameterizations any different than the rest? How sensitive are they to the variables and indices considered for ranking? In general, the authors state that flows are likely to 'increase' or 'decrease' in future. It may be worthwhile to examine whether such changes are actually statistically significant as compared to the observational record. Again, examining the causes of such change would rather be a more meaningful contribution.

Finally, the performance of the CORDEX RCMs in simulating the Indian monsoon is not known to be satisfactory. If large-scale monsoon features are not well represented by these models, any 'good performance' of these models on simulated runoff may be an artefact of exercises on the data or of using calibrated SWAT. For conclusions on seasonal analysis as attempted in this paper, this needs to be re-examined.

Other comments: How are the performances of the model for different gauging locations combined? Not clear from the description provided. What drainage data has been used for SWAT? Is the calibration and validation of SWAT done with monthly data? How do the authors make inferences on extremes with monthly data? Fig. 7 and similar plots: the box plots should be explained within the figure caption. Fig. 9: since these are discrete locations, it is not correct to join them. Fig. 11: 'obs' also presented, while figure caption says 'projected'. Para 10: What are 'sudden changes'? Is 'climate change' a 'sudden' change?

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