Hydrol. Earth Syst. Sci. Discuss., 11, C1936–C1939, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C1936/2014/

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Model study of the impacts of future climate change on the hydrology of Ganges–Brahmaputra–Meghna (GBM) basin" *by* M. Masood et al.

Anonymous Referee #1

Received and published: 20 June 2014

Review report for manuscript 'Model study of the impacts of future climate change on the hydrology of Ganges-Brahmaputra-Meghna (GBM) basin' by M. Masood et al

The authors have used HO8 model and future climate projections from a GCM (MRI-AGCM3.2S) to investigate the influence of future climate one the water resources in the GBM basin. The HO8 model is well accepted as is the GCM that they have used for the study. But I have some major issues with the way the analysis has been done and reported (which can be quite misleading). Although some of my comments below are critical, I should acknowledge that the authors have put enormous effort in undertaking this study and I congratulate them for the work. 1. The paper is well

organised but the writing need to be improved substantially (English editing) for publication in HESS. 2. The authors have used WFD forcing data when there are a number of publications which show that the APHRODITE reanalysis data is the best available climate data for this region. 3. I have a major issue with the way the authors have used bias correction for the GCM rainfall. The authors state that the GCM does okay for pre and post monsoon as well as for the drier winter months but it underestimates the monsoon high rainfall. The bias (underestimation) when compared to WFD rainfall is due to this underestimation by MRI for monsoon high rainfall events. But the authors apply an annual scaling factor (multiplier) which will push up all the rainfall throughout the year by a small amount instead of only the monsoon high events. This will lead to underestimation of monsoon high rainfall and eventually high runoff events as evident from Figure 6 a and b. The authors should be adjusting (bias correcting) different rainfall amounts based on seasons differently to overcome this issue. 4. Page 5756 top paragraph: Having worked in this region for a long time, I do not agree that the authors should be ignoring crop growth (as most of the area is under agriculture) and reservoir operations components of the HO8 model. This is a major shortcoming of this analysis. And later on in the paper when the model simulations are poor, the authors speculate that this is due to ignoring these components. They should be switching on the components and show whether they can explain the processes. 5. 'Soil moisture is expressed as a single-layer bucket which is 15 cm deep for all soils and vegetation types'. This is surely not valid for this region. 6. Section 3.1 Parameter sensitivity: The analysis the authors have undertaken is not really Monte Carlo as they are just sampling 5 random seeds for the entire parameter distribution. The five points picked can be all away from the optimum. 7. Discussion on page 5759 'Figure 4 shows that……unchanged'. I do not agree that we need to do any model simulations to find out what the authors are reporting here. Having used the model before, the model equations/formulation already tells you this and you don't need to do any model simulations. 8. Page 5761 - 3.2 Calibration and validation (bottom of this page ' This is likely ……..present model simulation'. This statement is factually incorrect as it is a well accepted fact that backwater effect is larger under low flow conditions than high flow conditions'. 9. Page 5762 – 4.1 Seasonal cycle: 'Lower ET of Brahmaputra .....compared to other two basins'. Brahmaputra NDVI is 0.38, Ganges is 0.41 and Meghna is 0.65. The physical/hydrological explanation for the results provided by the authors is incorrect as Brahmaputra and Ganges have very close NDVI (0.38 and 0.41). 10. Page 5768 – 4.5 Uncertanity in projection due to model parameter (towards the bottom of this page 'Therefore, uncertainty of future.........). the authors are missing some key references here which sheds light on parameter usability under climate change or variable climate conditions. Coron, L., Andréassian, V., Perrin, C., Lerat, J., Vaze, J., Bourqui, M., Hendrickx, F. 2012. Crash testing hydrological models in contrasted climate conditions: an experiment on 216 Australian catchments, Water Resour. Res., 48, 5, doi:10.1029/2011WR011721. Vaze, J., Post, D. A., Chiew, F. H. S., Perraud, J.-M., Viney, N., Teng, J., 2010. Climate nonstationarity - Validity of calibrated rainfall-runoff models for use in climate change studies. Journal of Hydrology, Volume 394, pp. 447–457, doi:10.1016/j.jhydrol.2010.09.018. 11. Page 5769 i) toward the top – 'uncertanity band for runoff is low' this is partly because you are showing total runoff and not the components (surface and subsurface); ii) just below the above statement ' from Fig 5 it is observed that.....' This statement is misleading as you are looking at the 10 simulations and all of them being similar to each other does not imply that uncertainty is low. Iii) The discussion on this whole page is speculative and misleading. You are comparing results from multiple realisations of the model and all of them giving similar answer only tell that the predicted/simulated variable is insensitive to the parameter value. The real value is when you compare the simulations to observations, either on-ground or secondary such as satellite ET and soil moisture data. Page 5786 Figure 6 – a) The figure shows that the model cannot reproduce peak flows well (this is due to the fact that the bias correction method you have used underestimates peak rainfall – see comment 3 above). The model cannot reproduce the peaks in the validation period as well. Page 5769 – 7th line from top 'Lower uncertainty...........could be ignored'. Your calibrated model is not able to reproduce the peak flows in calibration

and validation. What confidence do you have in the model simulations for the future climate conditions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 5747, 2014.