Referee comments on HESS discussion paper (doi: 10.5194/hessd-8-365-2011)

Paper Title: Impact of climate change on the stream flow of lower Brahmaputra: trends in high and low flows based on discharge weighted ensemble modelling

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This manuscript evaluates impact of climate change on stream flow in lower Brahmaputra basin using outputs from a global hydrological model (PCR-GLOBWB) forced with 12 different global climate models outputs. Results presented in the study are interesting, and the method used in the study offers a quick analysis of climate change impacts at the basin scale. However, validation of two methods used in the study: (1) discharge weightage average, (2) transient time series constriction, are questionable because authors does not offer a convincing validation results. Comments presented below may help in improving the manuscript.

General Comments:

(1) Discharge weightage method: A constant weight for different climate models have been assigned in the study (Table 1) based on comparison of mean monthly discharge from observation and model outputs for the reference period (Eq. 1). The same weight have been used for studying trends in low flows (7 day minimum flow) and high flows (annual maximum) and flow duration curve (Fig. 4, 5, 6, and 8), and even averaging the trend significance. Is the same weight valid for all flow condition? An analysis of flow duration curve for the reference period (e.g. 1980 – 1999) from the observed data (using daily discharge data) and the discharge weighted model outputs may answer this question. I am not sure why in Fig. 4 and 5, flow duration curve form observations are not shown. If authors do find same weight for all flow condition, then at least authors can determine different weight for each month (i.e. one weight for January, another weight for July, and so on).

(2) Transient time series construction: This construction seems to be entirely based on selecting a random year either from reference period (1961-1990) of the future time slice (2071-2100) based on proximity of the given year to these two time slices (Eq. 4). Some problem related this method is already shown in the paper (e.g. Line 5-6, page 376, '*peak flow of larger return period – strongest increase occur in first 20 years*' this may simply be the results of few year coming from 2071-2100 time slice). If authors want to use this method, they can offer some validation using 1960-1970, and 1990-2000 as two known time slices and then constructing transient time series for 1970-1990, and comparing it with the observations or something on similar line (e.g. 1960-1974, and 1985-2000 as known time slices and transient construction of 1975-1985).

(3) A small section on observed streamflow trends i.e. for the period of 1960 to 2000 may bring more confidence in results. Are the projected trends consistent with observed trends?

(4) One main argument used in the paper is – '*this study used outputs from 12 global climate models*' (line 6-10 on page 366, line 6-8, on page 377). However, as shown in Table 1, only 4 of these climate models (MICRO, GFDL, GISS, and CCCMA) constitute 96% of total weight. This point should be mentioned appropriately in the abstract as well as in the discussion section.

(5) Figure quality presented in the paper is not good. Please consider improving figure quality.

Specific Comments:

(1) Line 16-19, Page 366 – '… *the methods presented in this study are more widely applicable* …'. Authors may want to put some examples of application in the discussion section. If sufficient examples are not there in the literature then given sentence can be accordingly modified. Something like '…method presented in this study can/ may find wide application …'

(2) Introduction section (line 20-27 on page 366, and line 1-29 on page 367) – This section presents a nice introduction about importance of climate change impacts on Ganges-Brahmaputra basin. However, it does not make a convincing argument on – ‘why this new study is needed’. I think two missing points in the introduction section are: (1) summarizing the results of climate change impact on Lower Brahmaputra River Basin (LBRB) from previous studies (what have been found previously) (2) then how this study addresses some of the loop holes or unaddressed issues in the previous studies. For example even arguments on the line ‘ Immerzeel (2008) have used only 6 global climate model outputs, this study uses 12 global climate model outputs’ or ‘ methodology proposed in the study is new and it can address some short coming of previous studies (e.g. model development)’ can be made.

(3) Line 1-4, page 369: Is there only one discharge measuring station in the (LBRB)? This study excessively relies on Bahadurabad stream flow observation. What is the effect of human intervention e.g. dam or barrage upstream of this station? Adding few lines about the quality of stream flow observation would be helpful.

(4) Line 25-29, page 370 and Fig. 2 page 384: Some models (e.g. HADGEM, and IPSL) seems to be completely off the observation does not even capture the seasonal cycle of streamflow/precipitation. Then why these models were included in the analysis? I recognize that discharge weightage coefficient penalize such models. Here question remains: does including such model bring any improvement or degradation in the final results? May be a comparison can be made by including only 4 models (MICRO, GFDL, GISS, and CCCMA) with the results presented in the manuscript (including 12 models).

(5) Line 1 to 20, page 371: see general comment # 1.

(6) Line 1 to 24, page 372: see general comment # 2.

(7) Line 6 and 7, page 373: Eq. (5) and Eq. (6) are same as Eq. (2) and Eq. (3). Please delete one pair of these equations.

(8) Line 10-12, page 373: How trend parameters (trend magnitude) are calculated. It seems that linear trend line is fitted in the excel. However, it is not immediately clear in the manuscript. There are other methods too e.g. Thiel-Sen slope (Kumar et al., 2009), that is why it should be made clear how it was calculated.

(9) Line 12-13, page 373: trend significance: authors should recognize that trend significance gets considerably affected by the method used for trend significance calculation, and auto-correlation structure in the time series (short term and long term persistence in the data, see Kumar et al., 2009). I recognize that this is not main focus of this study. Hence, author should clearly mention about the trend calculation methodology (how trend was calculated, whether serial autocorrelation was removed from the data or not).

(10) Line 13-14, page 373: ‘… explained fraction of variance R^2 …’. It is not immediately clear that which explained faction of variance is talked about. My guess is – it is the variance explained by the linear trend line in the time series, and R^2 is the goodness of fit on linear trend line with the data. It should be made clear in the manuscript.

(11) Line 24-25, page 374, and line 1-9 page 375: This paragraph does not fit into the result section. Please consider moving it to either methodology section or to the discussion section. Authors may want to add another sub-section in the methodology where stream flow statistics (importance and calculation method) can be described.

(12) Line 5-11, page 376: see general comment # 2.

(13) Line 6-8, page 377: see general comment # 4.

(14) Line 9-10, page 377: see general comment # 3.

(15) Line 22 – 23, page 377: '.. also takes account of inter-GCM uncertainty..'. Please elaborate on this – how does discharge weightage average affects variance in the data?

Technical Corrections:

(1) Page 365, in the title, change '… modelling' -> '… modeling'

(2) Line 3, page 368, change '..constructed time series of constructed transient …' -> '..constructed time series of transient …'

(3) Line 3, page 368, change '…(fore the year…)' -> '…(for the year…)'

(4) Line 12, page 368, expand 'm a.s.l.'

(5) Line 8, page 369, change '…hydrological effect models.' -> '… hydrological models'

(6) Line 9, page 369, change '…projectsions..' -> '..projections'

(7) Line 20, page 369, change '…PCRGLOB..' -> '..PCR-GLOB..'

(8) Line 3, page 377, '… extreme downstream discharge' , consider revising, use of extreme with downstream does not seem good.

Figures:

As mentioned in General Comment # 5, all figures need improvements. I also referred another paper by a coauthor of this manuscript (Immerzeel, 2008), and found figure quality is excellent. Improvement in the figure quality on the similar line would be good.

Specific suggestion for figure quality improvement:

(1) For Fig. 2 to 8: do not use gridline unless it is needed.
(2) For Fig. 4 and 5: showing y axis on log scale may improve data visibility particularly at low and high extremes (see Fig. 4 in Kumar and Merwade, 2009).
(3) For Fig. 7: Start y axis at 2000 (m^3/s) because there is no data below this point.

<u>References:</u>

Kumar, S., and V. Merwade (2009), Impact of watershed subdivision and soil data resolution on SWAT model calibration and parameter uncertainty, *Journal of American Water Resources Association*, 45(5):1179-1196.

Kumar, S., V. Merwade, J. Kam and K. Thurner (2009), Streamflow trends in Indiana: effects of long term persistence, precipitation and subsurface drains, *Journal of Hydrology*, Vol. 374 (1-2), pp. 171–183.

Immerzeel, W.: Historical trends and future predictions of climate variability in the Brahmaputra basin, *Int. J. Climatol.*, 28, 243–254, 2008.