

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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We are very pleased to the high quality detailed reviews by the reviewer and we believe that it has substantially contributed to the improvement of the manuscript and the quality of the paper. Our detailed response to the reviewer's comment is presented below.

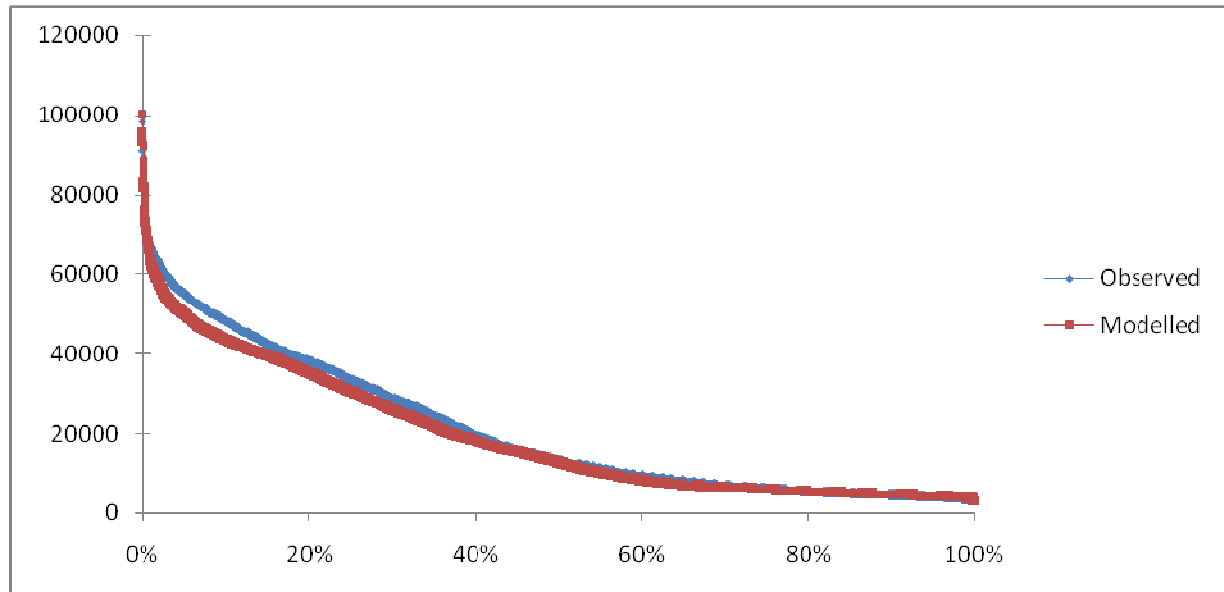
This manuscript evaluates impact of climate change on stream flow in lower Brahmaputra basin using outputs from a global hydrological model (PCR-GLOBWB) forced with 12 different global climate models outputs. Results presented in the study are interesting, and the method used in the study offers a quick 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).

Response to the General comment 1:

We acknowledge the comment and we have extended our analysis. We compare the flow duration curve of observed data and modelled data for the period between 1973 – 1995 (we choose this time slice because during this period no missing data were found in the observed data) and the analysis shows that the modeled discharge matches well with the observed data. The figure below shows these results and is added to the final version. Although it may be true that the match between modelled and observed discharge could be slightly further improved by using different weights for different months or flow conditions, however for reasons of robustness and interpretability we use a constant weight. We have also included the observed flow duration curves in figures 4 and 5.



General Comments 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).

Response to the General comment 2:

During the period 2071-2100 the peak flows are much higher than in 1961-1990 and the fact we observe the strongest increase during the first twenty years is not likely the result of a relative small number of years that are sampled from the 2071-2100 time slice. However we do appreciate the suggestion for the validation exercise though and we used 1970-1979 and 1990-1999 to construct a transient time series for 1980-1989. We compare a number of statistics for this 10 year period with the observations and the following text was added to the manuscript.

“We have validated our approach by artificially reconstructing a transient time series during the observational periods. We constructed a time series from 1980 to 1989 by sampling from the time slices 1970-1979 and 1990-1999 similar to what is described above. We then compare the daily data of the simulated transient time series with the actual observations during 1980 to 1989 and derive a number of statistics. Our analysis shows that the person correlation coefficient is 0.85, the bias is -2.3%, the root mean square error is 9323 m³/s and the Nash-Sutcliffe criterion for model efficiency equals 0.71. These numbers show that our approach is valid and that simulated discharge measure observed discharge

well. It should be noted that this may be different for river basins where seasonality in discharge is less pronounced.”

General Comments 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?

Response to the General comment 3

Now we have compared the trend of observed and modeled data for the overlapping period of 1973-1995 and the result is very consistent (as trend of observed record is $195 \text{ m}^3 \text{ s}^{-1} \text{ yr}^{-1}$ and for modelled data it was $173 \text{ m}^3 \text{ s}^{-1} \text{ yr}^{-1}$. We add following sentences in section 4.1.

“Before analysing trend of complete data series, we compare the trend between modelled discharge and observed records during the overlapping period of 1973-1995. For the observed records, the trend was $195 \text{ m}^3 \text{ s}^{-1} \text{ yr}^{-1}$ whereas, this value of modelled data was $173 \text{ m}^3 \text{ s}^{-1} \text{ yr}^{-1}$. This result shows that modelled outcomes are consistent with observed trend.”

General Comments 4

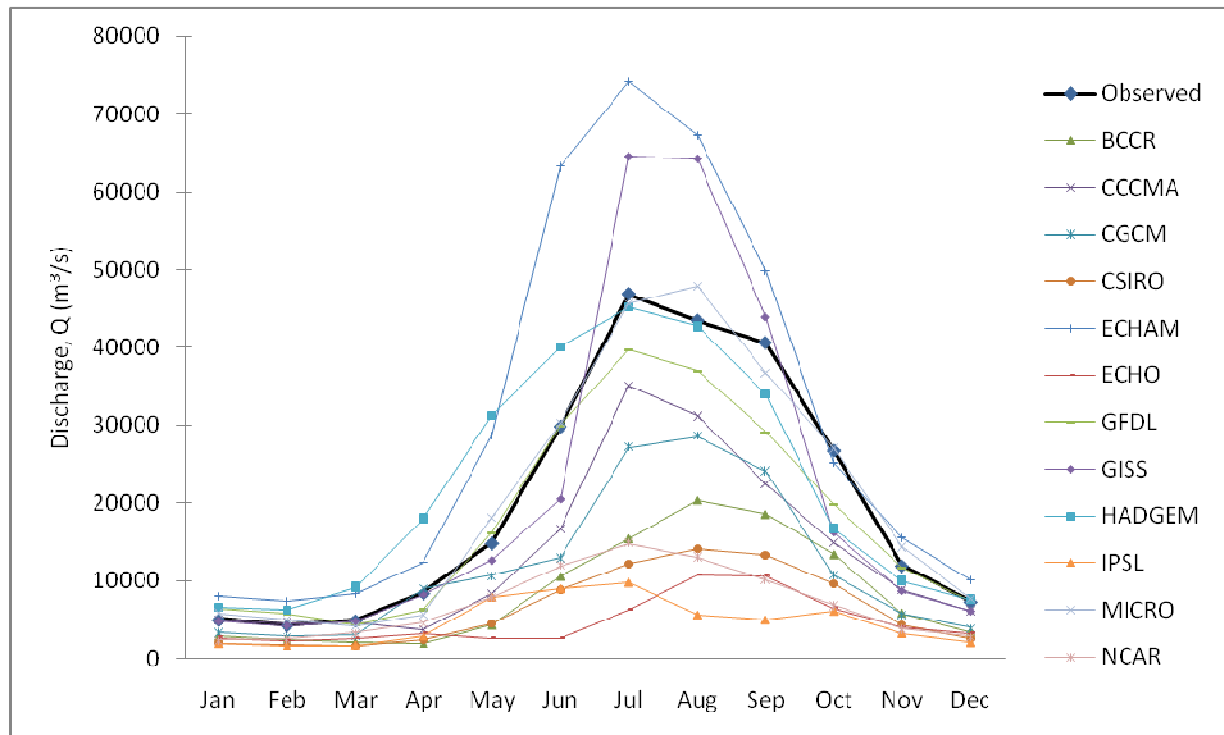
One main argument used in the paper is – ‘this study used outputs from 12 global climate models’ (line 6-10 on page 366, line 6-8, on page 377). However, as shown in Table 1, only 4 of these climate models (MICRO, GFDL, GISS, and CCCMA) constitute 96% of total weight. This point should be mentioned appropriately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

Response to the General comment 4

Some models seem to be completely different from the observation and do not even capture the monsoonal cycle of stream flow/precipitation). This was an error in our analysis and the ECHO, HadGEM and IPSL were shifted in time. We have corrected this and determined new weights and updated the figure and table with weights in the manuscript. The determined weights are however not significantly different.

	MICRO	GFDL	GISS	CCCMA	CGCM	BCCR	NCAR	ECHAM	ECHO	HADGEM
<i>previous weight</i>	0.369	0.299	0.200	0.092	0.034	0.003	0.002	0.001	0.001	0.000
<i>Updated weight</i>	0.368	0.298	0.199	0.092	0.034	0.003	0.002	0.001	0.000	0.003

This study started with an ensemble of 12 GCMs, however based on the weighting applied 96% of the weights were assigned to only 4 of the GCMs. Therefore these four GCMs, due to their good performance for the Brahmaputra basin, dominate the analysis. This finding is mentioned specifically in the abstract (line 6-10 on page 366) and discussion (line 6-8, on page 377).



General Comments 5

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

Response to the General comment 5

As suggested, in the final revised version, quality of all figures will be improved at the similar line as the quality of the article by Immerzeel, 2008.

Specific Comment 1

(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 ...’

Response to specific comment 1

We have added the following section to the Results and Discussion:

“Through this, a form of implicit downscaling is achieved that also takes account of inter-GCM uncertainty. Moreover, the method, which allows for a very quick and cheap analysis of the effects of climate change plus uncertainty, is quite generic and can be used at other locations in the world with discharge observations. The method is applicable in any case where a hydrological model is forced with an ensemble of climate models and a sufficient long time series of observed discharges is available”

Specific Comment 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.

Response to specific comment 2

We have revised the introduction and have clarified the added value of our study.

“This approach is an improvement of other methods that have previously been applied. Immerzeel (2008) for examples uses a multiple regression model to predict stream flow at Bahadurabad, but in this case the ensemble results of a physical based distributed hydrological model are matched to observed discharges and hydrological processes are likely to be captured more accurately in the results.”

Specific Comment 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.

Response to specific comment 3

We have added the following sentence to the description of the lower Brahmaputra

“This is the only station in the lower Brahmaputra for which long-term observed records are available through the Bangladesh Water Development Board. The data are of high quality and used for planning purposes and major hydrological studies and flood forecasts.”

Specific Comment 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).

Response to specific comment 4

See our response to General Comment 4

Specific Comment 5

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

Response to specific comment 5

See the response of the comment 1

Specific Comment 6

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

Response to specific comment 6

See the response of the comment 2

Specific Comment 7

(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.

Response to specific comment 7

This was an error and this has been corrected.

Specific Comment 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.

Specific Comment 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).

Specific Comment 10

Line 13-14, page 373: ‘... explained fraction of variance R2 ...’. It is not immediately clear that which explained fraction of variance is talked about. My guess is – it is the variance explained by the linear trend line in the time series, and R2 is the goodness of fit on linear trend line with the data. It should be made clear in the manuscript.

Response to the specific comment (8), (9) and (10)

We acknowledge these comments and we have changed this in the methodology section.

In our study, we consider linear trend analysis. We refer to Gain et al. (2007, 2008) for a detailed description on the method used for testing linear trends which can be summarized as follows:

Assume that $y_t, t = 1, \dots, N$ is an annual time series and N is sample size. Simple linear trend can be written as:

$$y_t = D + Mt$$

where D and M are the parameters of the regression model. Rejection of hypothesis $M = 0$ can be considered as a detection of a linear trend. The hypothesis that $M = 0$ is rejected if

$$T_c = \left| \frac{R\sqrt{(N-2)}}{\sqrt{(1-R^2)}} \right| > T_{1-\alpha/2, \nu}$$

in which R is the cross-correlation coefficient between the sequences y_1, \dots, y_N and $1, \dots, N$ and $T_{1-\alpha/2, \nu}$ is the $1-\alpha/2$ quantile of the student t distribution with $\nu = N - 2$ degrees of freedom, α is the significance level which is 5% (or 95% confidence level).

Specific Comment 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.

Response to the specific comment 11

In the revised version, one sub-section (3.4) was added to the methodology section where calculation methods of extreme value analysis are presented.

Specific Comment 12

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

See response to the general comment 2

Specific Comment 13

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

See response to the general comment 4

Specific Comment 14

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

See response to the general comment 3

Specific Comment 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?

The following sentence was added:

“Through this, a form of implicit downscaling is achieved that also takes account of inter-GCM uncertainty, because an ensemble of GCMs is used in reconstructing observed discharge.”

Technical Corrections:

(1) Page 365, in the title, change ‘... modelling’ -> ‘... modeling’

Response:

Modelling is the UK spelling and previous papers in HESS also use modelling and we have left it unchanged.

(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...)’

Response:

This was corrected.

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

Response:

It was corrected: '....5300 m above the sea level' (whereas, m represents meter).

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

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

Response:

This was corrected.

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

Response:

The term 'PCRGLOB' will be replaced by 'PCR-GLOBWB'.

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

Response:

This was corrected: "Immerzeel (2008) found a sharp increase in the occurrence of average and extreme discharge of lower Brahmaputra for A2 and B2 storyline".

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.

Response:

See response of the General Comments No. 5

Specific suggestion for figure quality improvement:

(1) For Fig. 2 to 8: do not use gridline unless it is needed.

Response:

This was corrected

(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).

Response:

For these two figures, the y axis will be shown in the log scale.

(3) For Fig. 7: Start y axis at 2000 (m3/s) because there is no data below this point.

Response:

We agree and it will be corrected accordingly.

References:

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Kumar, S., and Merwade, V.: Impact of watershed subdivision and soil data resolution on SWAT model calibration and parameter uncertainty, *J. of Am. Water Res. Assoc.*, 45(5), 1179-1196, 2009.

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

Räisänen, J., Ruokolainen, L., and Ylhäisi, J.: Weighting of model results for improving best estimates of climate change, *Clim. Dyn.*, 35, 407-422, 2010.