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Interactive comment on "Seasonality of the hydrological cycle in major South and Southeast Asian River Basins as simulated by PCMDI/CMIP3 experiments" *by* S. Hasson et al.

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We wish to thank the anonymous Referee 2 for having appreciated the novelty of our basin integrated approach for the hydrological cycle analysis. We are also thankful to the referee's insightful comments and suggestions, which will be taken into account and will lead to a significant improvement of the manuscript.

The main issues raised by the Referee 2 are: i) to check the statistical significance of future changes in the hydrological variables; ii) to shorten and proofread the article; iii) to compare and discuss our results based on a basin-integrated approach with the existing literature on the Asian monsoon based on country-wide or grid-point scale

approach. We mostly agree with the referee's remarks and in the following we will say how we intend to address the referee's comments.

1) P628, line 6: perhaps "present-day" should be elaborated on, to mention the 20c3m experiment.

We will change the statement to "under present-day forcing (20c3m experiment)"

2) P628, line 7 and throughout: I think the more common classification for centuries should be used (20th, 21st etc).

We will change XX, XXI and XXII centuries as 20th, 21st and 22nd centuries where applicable throughout the revised manuscript.

3) P628, lines 16-19: the use of either/or brackets here is very confusing and I suggest this sentence is rephrased.

We will rephrase these sentences as follows in the revised version of our manuscript.

"For the future climate scenarios, winter $P - E$ decreases over all four basins while spring $P - E$ decreases over Indus and Ganges Basins only. Such decreases in $P - E$ are mainly due to the decrease in precipitation associated with the western mid-latitude disturbances. Consequently, for the Indus and Ganges basins, the runoff drops during the spring season while it rises during the winter season."

4) P628, line 11: A major omission here is the work of Sperber et al. (2012, Climate Dynamics, "The Asian Summer Monsoon: An Intercomparison of CMIP5 vs. CMIP3 Simulations of the Late 20th Century" published online http://dx.doi.org/doi:10.1007/s00382-012-1607-6), which examines Asian monsoon biases across the mean, seasonal cycle, variability on a variety of scales in the CMIP3 and CMIP5 models. This could be referred to at several points in the manuscript.

We thank the referee for pointing it out. Sperber's paper is indeed relevant for our discussion. We will cite it in our revised manuscript and discuss our findings in comparison

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with their study where it is relevant.

5) P631, lines 2-5: It may be true that MME estimates do not necessarily outperform a given model, but in Sperber et al. (2012, see above) this is the case for all chosen metrics for the Asian monsoon. This should be commented on.

We will briefly add the comment on this and would refer to the literature (Lucarini et al., 2008, Liepert and Previdi, 2012, Hasson et al., 2013), which explain it in more detail.

6) P634, lines 14-22: A useful reference here would be Immerzeel et al. (2010, already cited), which shows little contribution of snow/glacier melt to Ganges flow.

We agree and we shall refer to Immerzeel's findings, which are in line with the statements given on line 14-22.

7) P636, line 20: is this calculated on the basin mean or at each gridpoint first?

These are calculated on the basin integrated quantities. We will explicitly mention this in our revised manuscript.

8) P637, lines 1-6: I found the logical statements here quite confusing and I suspect other readers would too. I suggest an alternative is found.

We agree with the referee. In the revised version the logical statement will be removed and an alternative will be found.

9) P638, line 25 onwards: Sperber et al. (2012, listed above) and Annamalai et al. (2007, already cited) should be mentioned here in the context of their findings of onset timing for the CMIP3 models. Later in the paragraph, commonalities may be find with the Sperber et al. (2012) study and their comparison of onset timings and duration.

We will discuss our results regarding the monsoon onset, its retreat and duration in the context of the findings of Sperber et al. (2012) and Annamalai et al. (2007) and the commonalities and differences between the findings will be reported here.

10) P640, Evaporation: please remind the reader that no observations are used.

We will remind the reader that no observed evaporation has been used.

11) P642, lines 3-7: where do the assumptions for discharge time of the river basins originate?

For the Ganges and Brahmaputra basins, our assumptions about the discharge travel time are based on Jian et al., (2009), who show that the average travel time to the outlet for these basins is less than one month. This justifies our assumptions about the travel time of the basin-integrated runoff quantities for such two basins.

Concerning Indus and Mekong basins, our assumptions are based on the equation (T = L/V) as well as the domain knowledge about the different parts of these basins, where most of the runoff is generated. The T is the travel time and L is the length of the river (the distance of the farthest grid cell to the outlet of the basin), which is taken from Hasson et al, 2013. The V is the simulated flow velocities for these basins, which is taken from Arora and Boer (1999). Considering the total length of the river, the equation provides the possible maximum routing time of the fast runoff from the farthest grid cell to the outlet of the Indus basin as 46 days and of the Mekong basin as 53 days. In case of Indus, we know that the considerable amount of runoff is generated in the northwestern part (Hasson et al., 2013; Immerzeel et al., 2010) and in the Indian Summer Monsoon dominated region (roughly between 30-35o N) at the southwest of the Himalayan terrain. Therefore, we assume that the average routing time for the basin integrated runoff is around one month. On the other hand, most of the runoff in the Mekong Basin, as explained in response to the comment 17, is generated over the lower part of the basin, we expect it has shortest possible routing time delays to the outlet. Therefore, our assumption of the routing time of less than one month for the basin integrated runoff quantity is quite valid for this basin.

We will very briefly mention this in our revised manuscript.

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12) P643, lines 14 18: how can HadGEM1 have good qualitative agreement yet an onset delayed by one month?

We thank the referee for pointing it out. We will remove the HADGEM1 from line 14 of page 643.

13) P643, line 15 onwards: how do the good models chosen here compare with the whole monsoon in Annamalai et al. (2007) or Sperber et al. (2012)?

We will compare the performance of these models with the outcomes of the Annamalai et al. (2007) or Sperber et al. (2012) in the revised manuscript.

14) P644, lines 1-2: wind speed, having a twin-peaked annual cycle for monsoon areas, is also important for evaporation.

The referee is right. We will mention the wind speed as a factor influencing evaporation in addition to soil moisture and insolation.

15) P645, lines 3-4: where is the evidence for models' negligible spring snowmelt?

We will remove these lines from the revised version of our manuscript. It was based on the fact that the snowmelt season mainly spans from April to June in the study area, and the models produce a negligible runoff during such period.

16) P646. Lines 1-3: as earlier, it would be useful to compare with the onset metrics used in Sperber et al. (2012).

We will compare our findings with Sperber et al. (2012) wherever possible throughout the manuscript in the revised version.

17) P649, lines 1-3: why does one expect a shorter delay (I'm guessing due to the length of the river/size of the basin: it would be useful to include this information for the non-hydrologist)? Can the difference be quantified?

The lower Mekong Basin generates most of the basin's runoff as the maximum of the

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basin precipitation is incident over it (below 23oN). This case is true for the observation and the studied models (Hasson et al., 2013). So the maximum runoff generated at the lower Mekong Basin experiences a shorter travel time due to the shorter distance to the basin-outlet/Sea as compared to the upper part of the basin, which produces only a smaller amount of the runoff. Kindly see response to the comment 11.

18) P649: it might be worth splitting sections 4.1 and 4.2 into new sections 4 and 5 respectively. In addition, it would be worth referring to more of the published literature on climate change/monsoon throughout section 4.2. This will help relate this manuscript, and its river basin/hydrological focus, with existing work that often approaches the topic from a country-wide or grid point scale.

We agree with the referee to split the section 4.1 and 4.2 into sections 4 5. We will discuss and compare our basin-wide findings with the relevant literature on climate change and monsoon, which is based on country-wide or grid-point scale analysis.

19) P654, line 8: can the suggested increase in snowmelt not be measured from the model outputs?

Yes it can but this would require analyzing additional variables (snowfall, snowmelts and temperature) from CMIP3 models. Actually this could be the subject of a new study. Furthermore, the increase in winter snowmelt in future climate projections has been reported in several studies (e.g. Kundzewicz et al., 2008), which justifies our statement.

20) P654, lines 13-18 and throughout: no mention is made of how the statistical significance of the results (or lack thereof) is assessed. Some attempt should be made to understand the significance of the differences found (e.g., from present-day to sres climate), perhaps using Student's t-test or otherwise.

We agree with the referee and we will apply a t-test to check the statistical significance of the changes in the relevant parameters between the present-day experiment and

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the future projections.

21) P655, conclusions: it would be sensible to reference other works looking at Asian monsoon/climate change here, as well as those which examined model fidelity, in order to draw comparisons with the results here.

Here, we will compare our findings with other studies focusing on model fidelity in terms of the representation of the hydrological cycle over the study region.

22) P656, line 26 onwards: It is certainly a useful goal to be assessing the runoff generated from the various land-surface component models, but to do this accurately the observed discharge at various points in the river basin would be required. Can the authors comment on the need for these data to be made more easily available to the (hydro) climate community?

We will add the following comment on P657, line 2 "Moreover, in order to properly validate the model generated runoff and its various components, the observed river discharges at the various places in the basin should be more easily available to the hydro-climatic community."

23) P659: Most of the discussion here focuses on the additional information that could be provided by dynamical downscaling. However I feel this misses the point somewhat. Without improving the models, these are only going to give more detail on the nature of the error (useful in itself). But what is really needed is more reliable and available discharge observations from various points in each of the river basins.

We agree with the referee that more reliable discharge observations from various places within the river basins are of high importance. However, we would like to draw attention of the referee to the point that the regional climate models being used for downscaling purposes at present are complete climate models, even with higher complexity, offering an increased flexibility and range of selection regarding the structural and physics-based options. Such models are presently being applied not only to down-

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scale the GCMs' outputs at a finer resolution but also to bring an improvement to the forcing datasets. Therefore, we recommend the downscaling of the GCM outputs, provided the GCMs do not breach basic conservation laws (Hasson et al., 2013) and they provide adequate forcing fields at the boundaries of the considered domain.

24) P659, final paragraph: it may be useful to refer to specific examples, such as the Pakistan floods of 2010, where human/land use factors have undoubtedly played a role in the severity of the damage.

We will briefly mention it in the revised manuscript.

25) Figures 3-5: perhaps a narrower range could be used for the evaporation scale.

The scale for the evaporation is set according to the scale of precipitation to clearly show the differences between the two fields. Similarly, scale for the P-E and runoff are set accordingly.

26) Figure 6-7: sub-titles on each panel would help these figures.

Sub-titles will be added to all the panel figures.

27) Spelling/grammar: P628, line 21 and throughout: replace "precipitations" with "precipitation". P629, line 14: replace "GCMs' " with "GCM". P630, line 11: the phrase "have been performed in this regard" does not fit well in this sentence. I suggest replacing with "have all demonstrated model biases ". P635, line 22: change "high" to "higher"; lines 26-27: I suggest putting "more likely" in brackets "( )". P637, line 18: replace "during" with "from". P639, line 1 and throughout: it seems more common to give the CMIP3 models lower-case names. This is unimportant however. P642, line 13 and throughout: "the" is missing before monsoon; line 21: replace "such model" with "PCM" for clarity. P644, line 23: replace "what" with "that". P645, line 14: replace opening of sentence with, "The Brahmaputra basin's precipitation is "; line 18: replace "at qualitative level" with "at a qualitative level". Again on line 20; line 24: replace "CIMP3" with "CMIP3". P646, line 9: replace "models' "with "model". Again on p648 line 11, and

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throughout. What you have written is not incorrect, but the alternative is less clumsy. P649, lines 16-18: the first sentence here does not make much sense and it would be useful to rephrase; line 19: perhaps "seasonal cycle" would be more suitable than "intra-annual". P651, line 1: please define "HKH"; line 28: Insert "The" before "only". P652, line 1: remove "a"; line 3: insert "the" before "same"; line 5: replace "here" with "the reader"; line 27: insert "a" before "scenario". P653, line 12: change "suggest" to "suggests". P656, lines 3-11: this whole sentence is far too long and convoluted. I suggest it is re-written; line 28 onwards: this sentence is too long and should be re-written. P659, line 18: correct "proceduers". P661, line 17: correct "Wesbster".

We will correct the mistakes and proofread our revised manuscript before its re-submission.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 627, 2013.