Editor Decision: Publish subject to revisions (further review by editor and referees) (01 Nov 2017) by Ian Holman
Comments to the Author:
All reviewers recognise that the scope of this manuscript represents an interesting and relevant topic, relating to the uncertainty in precipitation data products in a region of relatively poor instrumental data. However, all reviewers identify important inadequacies in the current version, which can be summarised as a :
• Lack of in-depth spatio-temporal comparison and analysis of the absolute precipitation values within the datasets;

Reply: We first thank the editor and the reviewers for your positive evaluation of our work in general, and we appreciate very much the professional and constructive comments raised by reviewers and editor.

In response to Editor's first comment of providing in-depth spatio-temporal comparison and analysis of absolute precipitation values within the datasets, we further did trend analysis by the Theil-Sen median method and significance test by the non-parametric Mann-Kendall method for areal mean of each month and every individual grid for summer and winter. The results are summarized in bar-plots, density plots as well as spatial maps. We also analyzed precipitation across latitude and elevations. We further added precipitation-evaporation-runoff analysis in the Beas catchment by three nested sub-catchments. Based on the in-depth comparison and analysis, finally, we recommended the APHRODITE data and the WRF data for the study regions due to matched scales with hydrological processes and considerable spatial variation.

• Lack of rationale for the selection and length (5 years) of the inter-comparison period, and whether any differences found are meaningful in light of inter-annual variability and large-scale meteorological forcings;

Reply: We extended trend analysis by the robust Theil-Sen median method and significance test by the non-parametric Mann-kendall test, to study the inter-annual variability of the whole period. The five-years' comparison was used for illustrative purpose to show temperature changes with respect to elevation for glacier survival analysis. Because temperature products are more reliable and less uncertain than precipitation products. It is also well known that temperature in the Great Himalayas Region increases fast since 1980s. There is no need to go through many temperature products and to do trend analysis and significance test. Three trends of the months were significant at the 5% confidence level and and overall 10% of grids were significant at the 10% confidence level.

It is difficult to conclude why the Northern India of the Western Himalayas showed an increase in summer precipitation. However, Bollasina et al. (2011) found the same increasing monsoon precipitation in the Northern India, whereas decreasing in the Central Asia. They used a series of climate model experiments, concluded that such pattern was a robust outcome of a slowdown of the tropical meridional overturning circulation, which could be attributed mainly to human-influenced aerosol emissions. Therefore, we believe that the trends will continue and become more significant with time if greenhouse gas emission continues as usual.

• Lack of consideration of the implications of the different spatial scales (grid sizes) of the data products

Reply: Thanks for the good suggestion and challenging comment. Here we used two spatial scales, i.e. aggregated areal mean and individual grid. The results were shown in scatter-plot, bar-plot, density plot as well as spatial maps. Here we selected three nested catchments in the Beas catchment with respect of different elevation and climate and analysed the precipitation-evaporation-runoff relationship, which was at the scale of hydrological process. Based on the in-depth comparison and analysis, for hydrological studies, we recommend the APHRODITE data and the WRF data due to matched scales with hydrological processes and considerable spatial variation.

• Lack of any benchmarking to determine which is the 'best' dataset for use in hydrological modelling and other types of studies, particularly given the uncertainty in deriving actual precipitation amounts from instruments that do not adequately capture spatial (and vertical) heterogeneity in these data sparse regions

Reply: Thanks for the comment, which points out the challenging nature of study in spatial (and vertical) heterogeneity and data sparse regions. We selected three nested catchments in the Beas catchment with respect to different elevation and climate. We indeed tried The Global Runoff Data Centre for more catchments, but the overlap period of runoff and precipitation was too short. The comparison showed precipitation was underestimated. Though the relationships were different, all datasets were consistent in terms of temporal changes and errors were systematic within each dataset. We also analyzed the MODIS Global Evapotranspiration Project annual mean actual evaporation, but the values were far from truth. For hydrological studies, we recommend the APHRODITE data and the WRF data due matched scales with hydrological processes and considerable spatial variation.

The authors' response to the reviewers lacks specificity but suggests that they will address the concerns raised - for the paper to be published, the identified weaknesses will need to be comprehensively addressed. They identify that they do not have access to the IMD 0.25o dataset for use as a benchmark, but propose to utilise observed river flow and MODIS ET data. This is an interesting approach, although its utility will depend upon the spatial scale of the river basins with available data (i.e. whether their extent leads to excessive spatial averaging) and the importance of agricultural irrigation in the river basins (and therefore how different gauged flows are from naturalised flows)

Non-public comments to the Author:
This paper has the potential to be a very useful and widely cited paper given the uncertainties in precipitation data in this region, if the authors can adequately address the weaknesses identified by the papers.

Reply: We are very much grateful to the Editor's affirmation and encouragement, we believe we have done our best in revising the paper following reviewers' and editor's comments and advice, given the limited availability of data in the region. Limitation of data, on other hand, strengthened the necessity and usefulness of such a study, as the Editor pointed out.

Reply to reviewers' comments

Dear Editor and Reviewers:

We thank the handling Editor and the reviewers for your comments and suggestions concerning our manuscript entitled "Precipitation Pattern in the Western Himalayas revealed by Four Datasets" (Manuscript Number: hess-2017-296). These comments are all valuable and very helpful not only for improving this paper but also beneficial for our research in general. We have carefully studied these comments and will address them in making revisions. The point-by-point responses to each of the comments are presented as follows.

Anonymous Referee #1

Received and published: 4 July 2017

Major comment 1: The methodology of attempting to distinguish precipitation trends from the four types of dataset is not scientifically valid. Firstly, there is no attempt to calculate the inter-annual variability of precipitation for each of the datasets. Therefore, it is impossible to tell whether the changes between the 2003-2007 and 1981-1985 periods are meaningful. There was also no justification for why these periods were even chosen. It is also impossible to tell whether choosing five years for each period is large enough to capture the precipitation representative of the 1980's and 2000's, i.e. these periods could easily have been anomalous. There is also no physical justification given as to whether the e.g. increase in summer precipitation during these two periods is physically consistent with either dynamic or thermodynamic large-scale changes, such as in response to the observed weakening of summer monsoonal precipitation (e.g. Bollasina et al. 2011). The differences also aren't quantified, and are referred to in the Abstract as 'an increase in summer and a decrease in winter with large variations'. The fact that there are large differences between the datasets is also a cause for concern, with the differences between datasets being possibly greater than the magnitude of the differences between the two periods.

Reply: Thanks for the reviewer's comments. There are three questions and we answer each individually.

For first question, we calculated and compared the inter-annual variability of precipitation for each of the datasets by the Theil-Sen median method and the non-parametric Mann-Kendall method for every individual grid as shown in Section 3.2 Temporal variations and changes. The results showed an overall increase in summer precipitation and decrease in winter precipitation.

For second question, we agree that it is not clear why the 2003-2007 and 1981-1985 periods were chosen, and whether they were representative. Therefore, we analyzed the whole period. This was done for areal mean of all months and every individual grid for summer and winter. The two periods, first and last five-years were only used for illustrative purpose to show temperature changes with respect to elevation for glacier survival, because temperature measurements were of higher quality and less uncertain than precipitation. There was no need to go though many datasets. It is also well know that temperature is increasing fast in the Great Himalayas Region, so there was no need for the trend analysis and the significant test.

It is difficult to conclude why the Northern India of the Western Himalayas showed an increase in summer precipitation. However, Bollasina et al. (2011) found the same increasing monsoon precipitation in the Northern India, whereas decreasing in the Central Asia. They used a series of climate model experiments, concluded that such pattern was a robust outcome of a slowdown of the tropical meridional overturning circulation, which could be attributed mainly to human-influenced aerosol emissions. This was also in the manuscript.

Secondly, the authors showed that the linear precipitation trend was insignificant, which surely contradicts their claim that precipitation patterns have changed with time.

Reply: Sorry that we failed to state that clear enough in the original version. The trend analysis used the Theil-Sen median method and the significance test used the non-parametric Mann-Kendall test. Both test were done for areal mean for each months and every individual grid for summer and winter. Most of the trends, were not significant at the 5% confidence level by the Mann-Kendall test, but some are indeed significant, i.e. three trends for monthly areal mean at the 5% confidence level and overall 10% trends for grids at the 10% confidence level. As we know, statistic test results are affected by sample size, and we believe the trends will become more significant with time if greenhouse gas emission continues as usual. We have added some more discussion on this aspect.

Thirdly, Figure 6 shows little agreement in the trends during 1981-2007 for the four datasets, with large differences in magnitude as well as even differences in sign.

Reply: This is true. As we mentioned in the reply of previous comment, there are large differences among the datasets with respect to absolute magnitude. However, their spatial pattern, intra-annual variability as well as relationships with runoff are similar. All datasets show similar trend of winter precipitation in the northern part as shown in Figure 10. To find difference and similarity among the datasets and discuss their implication in hydrology are the objective of this manuscript.

Bollasina et al., Anthropogenic aerosols and the weakening of the South Asian Summer Monsoon, Science, 2011.

Major comment 2: The description of the datasets is poor and overly generalised, and does not focus enough on the study regions. For example, the description of the IMD dataset does not say explicitly how many stations are used in the study region, and what altitudes. Instead, vague language such as 'less stations near the borders of India and the in the northern part' are used. This is insufficient information to make any robust judgement of the veracity of the data. This type of vague description is continued for APHRODITE. For example, in the description of APHRODITE, evidence of its representativeness of precipitation distribution is given by the claim that it is better than the MRI/JMA AGMC model – when in fact it is data that should be used to ground-truth models, and not the other way around. My understanding of both these datasets is that due to the sparcity of gauge measurements in the Himalayas, and particularly the lack of measurements at high altitudes, that these datasets are highly biased. In the description of ERA-Interim, it is stated that 'the spatial resolution of the ERA-interim dataset is limited in representing the spatial variability'. If this is not representative of precipitation, then why is it being used? Moreover, a vague statement that 'precipitation is adjusted based on GPCP v2.1. before release' is included. What is GPCP data? How does this affect the representation of precipitation over the Himalayas in ERA-interim? None of these questions are answered.

Reply: Thanks for the professional and constructive comments. We apologize for the vague description and have added more details in the revised version on the description of all the datasets used in the study as well as about GPCP. This part of the manuscript has been largely improved.

Finally, it was odd that the WRF model run used the configuration of EURO-CORDEX, rather than that recommended by e.g. papers by Maussion et al. (2011) or Collier and Immerzeel (2015) which focused on the Himalayas. This unfortunately gives the impression that the authors were using the model has a 'black box', and had little understanding of regional atmospheric modelling. This is reinforced by statements such as 'The ERA-Interim and WRF datasets are products with different dynamical models' and referring to both of these as 'the products from dynamic models do not suffer from an undercatch', which suggests that the authors aren't properly aware of the considerable differences between reanalysis products and numerical weather prediction modelling. The claim that the model run was not optimised was 'due to the complex orography' is unfounded, as studies such as Maussion et al., Collier et al. have shown that the choice of model grids and physics parameterisations is critical. There are also no details as to the spatial resolution of the WRF model, and claims such as 'the climate model has been proved to produce the regional precipitation at a fine scale' are for models running at kilometre scale for small regions around 100 km in size (See Collier and Immerzeel).

Collier and Immerzeel, High resolution modelling of atmospheric dynamics in the Nepalese Himalayas, JGR, 2015.

Reply: Thanks for the comment, and sorry that we did not state it clear enough. In the revised version, we provided more discussion and information about the WRF model configuration in manuscript and uploaded the completely model-setting file as supplementary file. Maussion et al. (2011) indeed did a very good comparison of model configuration at the Tibet Planet (TiP) area, which is around 10 degree eastern of our study area. However, they conclude, "Our study reveals that there is nothing like an optimal model strategy applicable for the high-altitude TiP, its fringing high-mountain areas of extremely complex topography and the low-altitude land and sea regions from which much of the precipitation on the TiP is originating. The choice of the physical parameterization scheme will thus be always a compromise depending on the specific purpose of a model simulation. Our study demonstrates the high importance of orographic precipitation, but the problem of the orographic bias remains unsolved since reliable observational data are still missing". Our WRF configuration has also been used in other projects, which focus on the western Asia and our results show that this configuration is reliable at the study area. We used the same microphysics and land surface as their reference experiments and Li et al. (2017) has tested the microphysics, cumulus and land surface scheme.

Li, L., Gochis, D. J., Sobolowski, S., & Mesquita, M. D. S. (2017). Evaluating the present annual water budget of a Himalayan headwater river basin using a high-resolution atmosphere-hydrology model. *Journal of Geophysical Research: Atmospheres*, *122*(9), 4786–4807. article. https://doi.org/10.1002/2016JD026279

Major comment 3: The Abstract begins by saying that 'data scarcity is the biggest problem . . . in the Himalayas' and that 'high quality precipitation data are difficult to obtain'. Yet the paper never properly addresses which of the four datasets is, despite their deficiencies, best able to represent Himalayan precipitation patterns. This might have been a worthwhile objective. Indeed, the abstract states that 'all the datasets can give a good overview of the precipitation'. How can this be possible when one of your datasets is ERA-Interim and another is WRF-based downscaling of ERA-Interim? It is unclear how this conclusion is reached, other than the broad generalisation that all the datasets show a wetter summer compared to winter. Moreover, does this mean that all datasets would give broadly the same answers if they were used as input to hydrology models? Additionally, many of the findings are well known, such as 'the highest precipitation locates at the foothill of the mountains and stretches from southeast to northwest'. Some of the results seemed distinctly unoriginal. The authors cite the Bookhagen and Burbank (2006) study, which did a very thorough job of describing precipitation characteristics in the Himalayas. I was unsure whether one of your aims was to show which datasets could recreate their findings? Also, any results which claimed to show something original, such as changes in precipitation, were highly flawed (see comment above).

Reply: We agree with reviewer's concern, and we apologize that we failed to address our important objectives clear enough, i.e. to look at differences among datasets and their implications on hydrology and glaciers. To achieve this, we added a new section "Comparison with runoff data" under "Discussions". Here we selected three nested catchments in the Beas catchment with respect of different elevation and climate. We indeed tried The Global Runoff Data Centre, but the overlap period of runoff and precipitation is too short. The comparison shows precipitation was underestimated. Though the relationships were different, all datasets were consistent in terms of temporal changes and errors were systematic within each dataset. We also analyzed the MODIS Global Evapotranspiration Project annual mean actual evaporation, but the values were far from truth.

Major comment 4: The manuscript is poorly organised, and lacking depth and understanding of the topic. For example, much of the results section is filled with material which should have been in either sections 1 or 2. The authors cite the study of Li et al. (2016) as indirectly proving that the WRF model can realistically simulate precipitation as it was able to force a hydrological model which was able to simulate discharge values. However, possibly the hydrology model was tuned to get this result? Moreover, the WRF model is highly sensitive to choice of physics and model setup (as the study by Maussion et al. (2011) shows), so much more details should have provided of how your model setup agrees with that of Maussion et al. This again illustrates that the authors are not suitably experienced in modelling to have included the WRF output.

Reply: We reorganized the structure of the paper, into Introduction, Study area and data, Methodology, Results and Discussion, Conclusions. “Results” were divided into two subsections, i.e. “Spatial variations” and “Temporal variations and changes”. “Discussions” were divided into two sections, i.e. “Comparison with runoff data” and “Implications for glaciers”.

The WRF-hydro is physically based land-surface process model. The tuning is used to find correct values for parameters, which is due to lack of measurements of vegetation, soil and river channel characteristics. In the revised version, we had more discussions about model setting and gave more details of the WRF model setting in Table 2. The complete settings are in a supplementary file.

Anonymous Referee #2

Received and published: 5 July 2017
Precipitation data are a key input in hydrologic modeling and the present paper compares four precipitation datasets which include data obtained by ground based measurements, interpolation, and reanalysis data. The paper addresses an important issue faced by hydrologists. I have the following comments on the paper.

Reply: Thanks for reviewer’s positive evaluation in general, and the specific comments that are detailed below.

1. The strengths and weaknesses of the four type of gridded precipitation datasets explained in lines 15 to 30 on page 2 can be better explained by means of a table. Each row of this table may correspond to a particular dataset and the columns could the how the data is obtained, its strengths, and weaknesses.

Reply: We thank the reviewer for this good suggestion and we added a table, Table 1 in the revised version.

2. In this paper, IMD dataset at 1 degree grid has been used. Currently, data at 0.25 degree resolution are also available.

Reply: This research is based on 1-degree grid data and we obtained this dataset from our Indian research partner. This resolution is comparable with other datasets used in the study. Thanks for introducing the 0.25° IMD dataset, tough it is not available for the authors at moment and will be considered in the future study.

3. There is a view that the precipitation data obtained from instrumented stations does not reveal the actual values over a catchment in Western Himalaya because the network of stations does not have the desired density and most stations are located in valleys. Thus, the actual precipitation in the hill tops is not known.

Reply: The actual precipitation is largely unknown at the hilltops. Some methods are available to partly overcome this problem. One is to interpolate the ground observations with elevation and undercatch correction. Numerical models are also valuable to estimate precipitation at high mountains, especially where observations are not reliable or not available. This manuscript is an attempt to compare four datasets produced by different methods. We added a new table, Table 1 in the revised version to summarize their weaknesses.

4. Authors mention that the four datasets are similar in terms of spatial and temporal variation but there is very large variation in absolute values from 497 to 819 mm/year. Given this, a reader would expect clear view from the authors: a) what is their assessment of mean annual rainfall, and b) which dataset(s) can be used in applications such as water yield assessment, flood forecasting, climate change impact assessment, and so on.

Reply: We agree these questions are very important and we must give a clear view. To achieve this, we added a new section “Comparison with runoff data” under “Discussions”. Here we selected three nested catchments in the Beas catchment with respect of different elevation and climate. We indeed tried The Global Runoff Data Centre, but the overlap period of runoff and precipitation is too short. The comparison shows precipitation was underestimated. Though the relationships were different, all datasets were consistent in terms of temporal changes and errors were systematic within each dataset. We also analyzed the MODIS Global Evapotranspiration Project annual mean actual evaporation, but the values were far from truth. Based on the in-depth comparison and analysis, for hydrological studies, we recommend the APHRODITE data and the WRF data due to matched scales with hydrological processes and considerable spatial variation.

Anonymous Referee #3

Received and published: 6 July 2017

This paper is dealing with an interesting data challenge in Himalayan region through comparison of four globally available gridded precipitation data sets. Though this work is interesting and publishable in terms of regional importance of Himalaya, illustrations in present format are not very strong.

Reply: Thanks for reviewer’s positive evaluation in general, and we appreciate very much the constructive comments. A point-to-point reply is below.

Major demerits are 1. This paper looks like a quick dissemination with limited analysis from authors (some figures just direct illustration of latitude wise and seasonal raw data).

Reply: We added more details in response to specific comments, new content and reorganized the structure.

2. Lack of appropriate inter-comparison technique which ensures comparability of spatial patterns with different grid size.

Reply: The precipitation datasets are at different grid size and it is quite challenging. The results show in forms of spatial maps, individual grids and regional mean as well as density plot.

3. Lack of detailed discussions on spatial patterns and issues of scale.

Reply: Agree. To achieve this, we added a new section “Comparison with runoff data” under “Discussions”. Here we selected three nested catchments in the Beas catchment with respect of different elevation and climate. We indeed tried The Global Runoff Data Centre, but the overlap period of runoff and precipitation is too short. The comparison shows precipitation was underestimated. Though the relationships were different, all datasets were consistent in terms of temporal changes and errors were systematic within each dataset. We also analyzed the MODIS Global Evapotranspiration Project annual mean actual evaporation, but the values were far from truth.

4. Lack of verification indicators (example: POD, TS, FAR, FBI etc.) in comparison with available rain gauge data or IMD data as reference.

Reply: Thanks for the comment and suggestion. Some of the verification indicators as used in Sikder & *Hossain (2016*), i.e., POD: the probability of detection. TS: threat Score. FAR: the false alarm ratio. FBI: the frequency bias index, will be used in the revised version. These indexes were used to assess forecasting results with observations. Here we do not have "true observations"- rain gauge data. The IMD dataset will be used as reference although it is interpolated by the India Meteorological Department from rain gauge data. During the period of 1981-2007, the average number of stations per grid point (1×1) varies from 0.2 to 4.4. Fewer stations near the borders of India and in the northern part of the study area and observations are available near the latitude of 35.5N and its north.

Sikder, S., & Hossain, F. (2016). Assessment of the weather research and forecasting model generalized parameterization schemes for advancement of precipitation forecasting in monsoon-driven river basins. *Journal of Advances in Modeling Earth Systems*, *8*(3), 1210–1228. article. http://doi.org/10.1002/2016MS000678

Some specific comments are given below:

As this work is dealing with comparison gridded data sets with varying spatial resolution and other features, the discussion needs to be strengthened incorporating these uncertainty aspects and how reliable/meaningful these absolute precipitation values are, which are used for comparison.

Reply: Thanks for the comment. To achieve this, we added a new section "Comparison with runoff data" under "Discussions". Here we selected three nested catchments in the Beas catchment with respect of different elevation and climate. We indeed tried The Global Runoff Data Centre, but the overlap period of runoff and precipitation is too short. The comparison shows precipitation was underestimated. Though the relationships were different, all datasets were consistent in terms of temporal changes and errors were systematic within each dataset. We also analyzed the MODIS Global Evapotranspiration Project annual mean actual evaporation, but the values were far from truth. For hydrological studies, we recommend the APHRODITE data and the WRF data due matched scales with hydrological processes and considerable spatial variation. Careful local corrections are required.

Need some consistency in acronyms used in this text (e.g.: authors have used both AGMC and AGCM)

Reply: This is typos. We revised it in the revised version.

Bit more clarity is needed in the description of seasons in the study region (e.g.: in Page 3 authors have considered November-April period as winter, and May to October as summer).

Reply: We apologize for this ambiguity. Here it refers to summer monsoon season and winter monsoon season. The analysis was based on northern meteorological seasons (spring: March to May; summer: June to August; Autumn: September to November; Winter: December to February).

More clarity and justifications through references are required to strengthen "do not suffer from the undercatch problem" explained in the page number 3.

Reply: Thanks. Undercatch means amount of precipitation is not measured by gauges mostly caused by wind turbulence especially for snow. The undercatch leads to a dry biase for unshielded and single gauges at high latitude and high altitude. The undercatch applies to most types of precipitation observations and their products. Here we meant that Regional climate models simulate precipitation based on mathematical equations and the results are considered not affected by undercatch problem. This part was revised.

Better clarity is needed on the selection of 5 year time slices (1981-1985, 2003-2007) for comparison.

Reply: We will clarify that trend is calculated by the Theil-Sen median method and significance is by the non-parametric Mann-Kendall test, for areal mean of each month and every individual grid of summer and winter. Results showed winter was becoming drier and summer was becoming wetter. However, few (May by the WRF dataset; June by the IMD and ERA-interim datasets, and 10% of grids) of them were statistically significant at the 95% confidence level. As we know, statistic test results are affected by sample size, and we believe the trends will become more significant with time if greenhouse gas emission continues as usual.

Why seasonal comparison is limited to selected two months (Page 6) only?

Reply: The seasonal comparison was shown in Figure 3 whereas these months was purposefully selected to have a better visual impression of spatial distribution and interaction with topography. JA (July and August) and ND (November and December) had respectively highest and lowest precipitation and these months clearly showed the changes.

Page 2 Paragraph 2. This part of text emphases more on to a specific project and associated difficulties- it appears to limit the scope of this work. It would be more appropriate if you define these site-specific difficulties and rationale of this paper through proper references from Himalayan region than describing it as a 'INDICE project' related issue.

Reply: We agree with the reviewer and revised the manuscript accordingly.

I am doubtful about the usefulness of west-to-east latitude based precipitation comparisons for hydrologists in the region. It would be more useful for the hydrologic research community if you could include precipitation PDFs comparisons of larger river basins (Indus River and the upper Ganges River) in this study domain

Reply: This figure aimed to show interactions between atmosphere and topography in JA (July and August, high precipitation months) and ND (November and December, low precipitation months). As shown in the figure, the precipitation changed at different locations and the changes varied among the four datasets. We selected three nested catchments in the Beas catchment with respect of different elevation and climate. We indeed tried The Global Runoff Data Centre, but the overlap period of runoff and precipitation was too short. The comparison showed precipitation was underestimated. Though the relationships were different, all datasets were consistent in terms of temporal changes and errors were systematic within each dataset. We also analyzed the MODIS Global Evapotranspiration Project annual mean actual evaporation, but the values were far from truth. For hydrological studies, we recommend the APHRODITE data and the WRF data due matched scales with hydrological processes and considerable spatial variation.

Titles of Figure 7 and 8: you need to clearly write the details of data points with different colors.

Reply: In Figure 7, the blue color showed the period 1981-1985 and the orange showed the period 2003-2007, as shown in the legend. Figure 8 was removed, because we extended the trend analysis and the significance test to the whole period for every individual grid. Results were shown in Figures 8-12 in the revised version.