

Interactive comment on “Consistency of satellite precipitation estimates in space and over time compared with gauge observations and snow-hydrological modelling in the Lake Titicaca region” by Frédéric Satgé et al.

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We are sincerely grateful to the two anonymous reviewers for the time and effort spent in reading this manuscript and making numerous suggestions for improvement. The paper has been substantially revised based on their comments. We acknowledge all the points raised and we have spent time on carefully addressing them all. See our answers below. We also wish to inform you about an additional change concerning one of the Satellite-Based Precipitation Estimates (SPPs). We originally considered SM2Rain-CCI v.1, but, after receiving a personal communication from data developers,

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we now use the updated version SM2Rain-CCI v.2.

Responses to comments from Referee 2

Referee's comment:

The manuscript presents an evaluation of several satellite-based precipitation products over a watershed in the Central Andes, using three different evaluation methods: (1) direct comparison with rain gauge data; (2) evaluation of the Nash-Sutcliffe efficiency of the calibration of a hydrological model forced with each precipitation data set on observed stream flow; and (3) evaluation of a hydrological model's capacity to predict snow cover when forced with each of the products. The study finds a quite consistent performance of the products over the three methods, with in general more recent and higher-resolution products performing best. I agree that there is a need for better insights in the performance of satellite-based precipitation products, especially over complicated and data scarce terrains such as the central Andes. This makes the study relevant from both a scientific and operational perspective.

Authors' response:

Thank you for the time and effort you spent reading the manuscript.

Referee's comment:

However, despite the manuscript's length, I do not think that the presented findings are significant enough to merit publication. The main issue is that the evaluation methods applied in the study are not designed to gain any insights in the underlying processes that differentiate the different products. Instead, the evaluations seem to be selected based on two specific applications: predicting streamflow in medium-sized watersheds, and predicting high-elevation snow cover. I can see how those applications may be very relevant in a local operational context, but they add very little to the scientific understanding of satellite precipitation and how satellite-gauge merging algorithms can be improved.

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Authors' response:

As indicated in the title and at the end of the introduction, the aim of this paper is to report on the performance of SPPs (Satellite-based Precipitation Products to replace Satellite Precipitation Estimates used in the previous version) in space and over time compared with gauge observations and snow-hydrological modelling (i.e. from the point of view of the end-user for the management of water resources) rather than to provide insights into the respective techniques and algorithms behind their estimates. As we mention in the introduction, previous studies reporting on the potential of SPPs usually based their methodology on a single indicator (compared with precipitation gauges or the sensitivity of hydrological modelling to SPPs) and on a single time window. However, as underlined in the introduction and in our findings, SPP performance can vary with the indicators, the time window, and the specific region or sub-region analyzed. Substantial adjustments have been made to the abstract, introduction, method and conclusion (see following comment) to clarify the study objectives and main findings. We acknowledge that the added value of our analyses could be limited for readers who are deeply involved in improving SPE algorithms but we believe that these readers will search for that kind of information in journals specialized in remote sensing techniques, which incidentally, have already reported on these topics.

Referee's comment:

Indeed, despite the author's claim to present a new "protocol" for evaluation, the applied methods are very common (apart from perhaps the snow prediction) and some of the implementation decisions further reduce their capacity to gain process insights, . . .

Authors' response:

As mentioned above, the originality of our work is combining different indicators, spatial scales and time windows to test the performance of SPPs. This is clearly a new way to assess SPP consistency (see introduction, section 1.3). The present study investigates the influence of the selected indicators and time windows on the assessment

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of the space-time consistency SPPs. The method relies on a comprehensive protocol based on different indicators (gauge observations, streamflow observations via sensitivity analysis of a lumped hydrological model in different catchments, and snow cover observed from satellite imagery via sensitivity analysis of a distributed snow model in an unmonitored mountainous area) applied to four different time windows between 2000 and 2012. Although the individual methods (gauge and hydrological assessment) we use for evaluation are common (apart from the evaluation of the snow cover as you acknowledge), the originality of the protocol is the combination of different indicators, spatial scales and time windows to assess SPPs. As a result of these combinations, we clearly show that the use of a single indicator is not sufficient to assess SPP potential (SPPs rank differently depending on the indicator) and may hide part of SPP potential and/or limitations. What is more, as we demonstrate in this paper, the use of a single time window to assess SPPs may also hide part of the potential and/or limitations depending on the time window considered.

Modifications to manuscript:

Your comment made us aware that the study objectives were not sufficiently clear and we have consequently modified the introduction, part 1.4:

"From the previously established state of the art, this paper investigates the influence of selected indicators and time windows on assessments of the space-time consistency of SPPs. The comprehensive protocol relies on different indicators: (i) gauge observations; (ii) observations of streamflow using sensitivity analysis of a lumped hydrological model in different catchments; and (iii) snow cover observed from satellite imagery via sensitivity analysis of a distributed snow model in an unmonitored mountainous area, applied to four time windows. The aim of using different indicators was to evaluate whether the efficiency of the SPPs varies with the assessment method, whereas different time windows are used to evaluate a potential variation in SPP performance over time. The Lake Titicaca region was selected as study area because it includes all the specific features considered as potential limiting factors for SPPs (high mountain mas-

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sifs, large water bodies and snow covered areas) to evaluate the potential of SPPs in an extreme context in terms of the sensors' limitations with respect to the orographic effect (i.e. mountains) and high temperature/ emissivity contrast (i.e. Lake Titicaca and a snow-covered region). It also offers the opportunity to provide feedback on the use of SPPs over poorly monitored regions."

We have also modified part of the original abstract from lines page 1 lines 25-32:

"... whereas the SPP's ability to reproduce the duration of MODIS-based snow cover resulted in poorer simulations than simulation using available precipitation gauges. Interestingly, the potential of the SPPs varied significantly when they are used to reproduce gauge precipitation estimates, streamflow observations or snow cover duration and depending on the time window considered. SPPs thus produce space-time errors that cannot be assessed when a single indicator and/or time windows is used, underlining the importance of carefully considering their space-time consistency before using them for hydro-climatic studies. Among all the SPPs assessed, MSWEP v.2.1 showed the highest space-time accuracy and consistency in reproducing gauge precipitation estimates, streamflow and snow cover duration."

We also made modifications in section 3 page 10 line 21-23:

"Each assessment step was analyzed according to three 4-year time windows (2000-2004, 2004-2008, 2008-2012) and one 12-year time window (2000-2012) in which a hydrological year corresponds to a period starting on 1st of October to the following 30th of September. The aim of the proposed protocol was to investigate the influence of the selected indicator (gauges, streamflow modelling, snow modelling) and time window to assess the SPPs space-time consistency. More details of the proposed protocol are presented in the following sections."

We also made some modifications to the conclusion page 32 line 4-8:

“SPPs present space-time errors that cannot be assessed when only one indicator

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and/or time window is used. Indeed, the use of a single indicator is not representative of SPP performance (SPPs may be ranked differently depending on the indicator used) and may conceal part of SPP potential and/or limitation. Similarly, the use of a single time window for SPP assessment may also conceal part of SPP potential and/or limitations (SPPs may rank differently depending on the time window used). Referee's comment

... in particular:

1 - The temporal aggregation to a 10-day period essentially reduces the test to an evaluation of the bias and seasonality of the satellite products, eliminating any insights in their capacity to capture individual events and higher intensities, and their propensity for false alarms.

Authors' response :

Referee 1 made a similar comment. We agree that using a 10-day time scale may conceal part of the difference among the datasets, notably by eliminating any insights into their ability to capture individual events and higher intensities. However, we chose the 10-day time scale for several reasons. First, some of the datasets are only available at a daily time step with a daily accumulation period, which does not match the gauge datasets. Inconsistencies between the reference and the assessed datasets are thus to be expected because of this temporal discrepancy. Secondly, as precipitation is characterized by high spatial variability, many precipitation events detected at the grid-cell scale may not be detected at the point-gauge scale (Satzé et al., 2016). This is even more obvious when a single gauge is used for comparison with the corresponding average grid-cell measurement (which is mostly the case for the pixels considered due to the scarcity of local gauges) (see the comprehensive study by Tang et al, 2018). This will inevitably increase the likelihood of false alarms (as suggested) not because of SPP deficiencies but because of the different spatial scale between point-gauges and grid-cell average measurements (see section 5.2 page 28 lines 19-25). Finally,

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another reason is the snow modelling analysis. In this part of the protocol, snow cover distribution (SCD) derived from gap-filled MODIS snow products is used as reference. The temporal filters used to fill the initial gaps in the MODIS snow products (see section 2.3.2 page 10 lines 4-11) led us to consider that these data were more valid at the 10-day than at the daily scale. However, it should be noted that evidence for significant differences between the various satellite-based datasets can be found for any space and time indicator despite the 10-day time scale. This shows that the 10-day time scale is consistent in the comparison.

Modifications to manuscript:

Based on your comment, we have added a few lines in section 3 (page 10, lines 25-32) to better explain why we chose the 10-day time scale for analysis.

“It is noteworthy that the use of a 10-day time scale rather than a daily time scale may conceal some of the differences among the datasets, notably by eliminating any insights into their capacity to capture individual events and higher intensities. However, our choice was based on the inconsistencies we expected between gauges and daily measurements of SPPs as a reason to (i) use a different daily time window aggregation than the local one (8 am to 8 pm) for SPPs delivered at daily scale, (ii) the spatial inconsistency between point-gauge measurement and average grid-cell measurement (Tang et al, 2018), and (iii) the temporal filters used for gap filling of MODIS snow products, which led us to consider that these reference data were more valid at a 10-day scale than at a daily scale.”

Referee's comment:

2 - Focusing only on the calibration of the hydrological model provides little added value to the direct comparison with rainfall. I agree that a comparison with discharge data is warranted, as they provide a useful independent data set that makes it possible to evaluate potential biases of the precipitation product over a catchment area. This is potentially useful, because of the inherent weakness of comparing point rain gauge

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data with pixel-average SPEs. However this benefit reduces with increasing size of the rain gauge network. At the same time a hydrological model-based evaluation has other issues, such as errors in the model structure and ET estimates. None of this is discussed in detail.

Authors' response:

Indeed, as you say, assessments relying on discharge are warranted not only because they are independent datasets, but above all, because discharges incorporate precipitation over a catchment. It is known that precipitation gauge networks frequently do not fully capture spatial precipitation features, even more frequently in regions where data are scarce. The purpose in our study is to highlight the complementarity of point gauge comparison and the sensitivity analysis of hydrological modelling (see section 5.2 “Gauges versus hydrological modelling-based assessment”, page 27 line 20 to page 28 line 7). We used model calibration to report on hydrological model sensitivity to different SPP forcing data. Independent validation of the hydrological model is beyond the scope of this paper, as our aim was not to assess the ability of the model to reproduce streamflow responses in detail nor to be transferred under climate variability in the basins studied. Similarly, our purpose is not to evaluate and discuss model structure or ET estimates. Consequently, the same ET estimates and hydrological model were used to assess all the SPPs considered by using a specific calibration for each dataset.

Modifications to manuscript:

We believe your comment was due to the fact our objectives were not clear enough. As mentioned above, they have been clarified in the revised version. We have also added further details in the discussion (see section 5.2 page 29 lines 8-12).

“Similarly, other ET estimates could have been used, especially those based on remote sensing techniques to compensate for the limited availability of temperature gauge data. However, we would like to underline that our purpose was not to evaluate or

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discuss the model structure or ET estimates, but to highlight the complementarity of point gauges and integrated hydrological modelling. In this context, the same ET estimates and the same hydrological model were used to assess all the SPEs through the specific calibration of each dataset.”

Referee's comment:

3 - The decision to exclude elevation as a co-variable in the interpolation process nor to analyse the elevation-dependence of SPE performance explicitly, seems a wasted opportunity. Indeed, one of the main advantages of the study region is to understand the performance of the satellite products as a function of topographic characteristics. The cited study that shows that hydrological models are not very sensitive to elevation dependent rainfall interpolation is in my opinion not a valid argument. In the case of SPEs there are good arguments to expect an elevation-dependent performance.

Authors' response:

As explained in section 2.2.2, pages 7, lines 13-15, we decided to not take the potential elevation-dependency into account in the interpolation of in-situ precipitation data (unlike temperature). This was justified by the fact that the available gauges are all located on the flat part of the four study basins, which made it impossible to show a potential relationship between elevation and precipitation in these basins. However, it should be noted that the actual elevation-dependency potential accounted for in the SPE was implicitly evaluated through hydrological modelling of this topographically complex region.

It is also worth mentioning that, as reported in many studies, SPPs are generally less accurate over mountainous regions (see discussion on this point in section 5.4, page 30, lines 20-27). Two publications referring to the same region already reported on SPP accuracy as a function of elevation (Ochoa et al., 2014; Satgé et al., 2017b).

Modifications to manuscript:

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The decision to exclude elevation as a co-variable in the interpolation of in-situ precipitation has been reformulated in section 2.2.2, pages 7, lines 13-15, to clarify our purpose with respect to this point. “Because the gauges are mainly located in the flat land part of the basins, it was not possible to provide evidence for an effect of elevation on precipitation distribution. Consequently, no orographic effect was accounted for in the interpolation of the point precipitation observations”

Referee's comment:

4 - I am afraid that I fail to see the purpose of the evaluation using remotely sensed snow cover data. I agree that solid precipitation is a major issue in SPEs that needs to be studied further, and that the study region would be an excellent opportunity to do so. I also agree that SPEs may be used to model river basins with snow cover, where such issues may propagate. But the implementation presented here does not generate any significant insights in either of these issues and I am really left wondering how can be learned from the presented results.

Authors' response:

Considering snow-cover as a reference to evaluate SPP consistency is a promising new way to differentiate rainfall and snowfall over unmonitored regions. It is part of the effort to cross-validate remote sensing datasets. In this context, a recent study reports on the possibility of using remote sensing soil humidity datasets to assess SPEs potential (Massari, Crow and Brocca 2017). In the case of the Altiplano, we found no evidence of significant differences in performance between the different precipitation datasets (Pref and SPPs) using the snow modelling approach. This was a bit disappointing and is further discussed in section 5.4. (page 31 line 1-5). The absence of clearer differences may be explained by the presence of permanent snow-covered areas whose seasonal dynamics are hard to capture from optical imagery (MODIS), because there is more variation in depth than in areal extent. The regional seasonal snow cycle captured by MODIS is consequently weak and erratic. However, the results

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reported here do not invalidate the proposed protocol. In other regions with other snow conditions, it is a promising way to assess SPPs thanks to the sensitivity of snow modelling in reproducing snow cover dynamics as observed from MODIS snow-products. We believe that many readers will find the proposed method useful for their studies.

Referee's comment:

Because if these issues, I think that the current manuscript has only very limited scientific significance beyond the local scale, in the sense that none of the conclusions are sufficiently solid to gain significant insight in how the products may perform in other regions.

Authors' response:

Indeed, as stated in the introduction, the consistency of SPPs varies with the region because it depends on (i) land cover (emissivity, surface temperature), (2) topography, and (3) precipitation intensities. It is therefore correct to say that the conclusions on the SPPs drawn from this study cannot be extrapolated to other regions of the world. However, as mentioned above, the main scientific interest of the study is the protocol we propose to assess SPP consistency. Indeed, the aim of this protocol is to show how the current assessment of SPP potential can be influenced by the method used (single indicator, single time window), and, as a consequence, to recommend the use of different indicators to provide consistent reports on SPPs potential/limits. Finally, by identifying and elucidating these issues, the paper also reports on the extent to which SPEs can be used in poorly monitored basins to support water resources management.

Modifications to manuscript:

As a result of this comment (and of an earlier comment you made), we realized that the objectives of our study were not clear and we have consequently considerably modified the introduction part 1.4 as follows:

"From the previously established state of the art, this paper investigates the influence of

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selected indicators and time windows on assessments of the space-time consistency of SPPs. The comprehensive protocol relies on different indicators: gauge observations; (ii) observations of streamflow using sensitivity analysis of a lumped hydrological model in different catchments; and (iii) snow cover observed from satellite imagery via sensitivity analysis of a distributed snow model in an unmonitored mountainous area, applied to four time windows. The aim of using different indicators was to evaluate whether the efficiency of the SPPs varies with the assessment method, whereas different time windows are used to evaluate a potential variation in SPP performance over time. The Lake Titicaca region was selected as study area because it includes all the specific features considered as potential limiting factors for SPPs (high mountain massifs, large water bodies and snow covered areas) to evaluate the potential of SPPs in an extreme context in terms of the sensors' limitations with respect to the orographic effect (i.e. mountains) and high temperature/ emissivity contrast (i.e. Lake Titicaca and a snow-covered region). It also offers the opportunity to provide feedback on the use of SPPs over poorly monitored regions."

We have also modified part of the original abstract from lines page 1 lines 25-32:

"... whereas the SPP's ability to reproduce the duration of MODIS-based snow cover resulted in poorer simulations than simulation using available precipitation gauges. Interestingly, the potential of the SPPs varied significantly when they are used to reproduce gauge precipitation estimates, streamflow observations or snow cover duration and depending on the time window considered. SPPs thus produce space-time errors that cannot be assessed when a single indicator and/or time windows is used, underlining the importance of carefully considering their space-time consistency before using them for hydro-climatic studies. Among all the SPPs assessed, MSWEP v.2.1 showed the highest space-time accuracy and consistency in reproducing gauge precipitation estimates, streamflow and snow cover duration."

We also made modifications in section 3 page 10 line 21-23:

C12

“Each assessment step was analyzed according to three 4-year time windows (2000-2004, 2004-2008, 2008-2012) and one 12-year time window (2000-2012) in which a hydrological year corresponds to a period starting on 1st of October to the following 30th of September. The aim of the proposed protocol was to investigate the influence of the selected indicator (gauges, streamflow modelling, snow modelling) and time window to assess the SPPs space-time consistency. More details of the proposed protocol are presented in the following sections.”

We also made some modifications to the conclusion page 32 line 4-8:

“SPPs present space-time errors that cannot be assessed when only one indicator and/or time window is used. Indeed, the use of a single indicator is not representative of SPP performance (SPPs may be ranked differently depending on the indicator used) and may conceal part of SPP potential and/or limitation. Similarly, the use of a single time window for SPP assessment may also conceal part of SPP potential and/or limitations (SPPs may rank differently depending on the time window used).

Referee’s comment:

At the same time, the paper is very long and contains a lot of information that is readily available in the relevant literature and does not need to be repeated here. In fact, I think that the manuscript could easily be condensed into 1/2 or even 1/3 of the current length. This would make it much sharper and easier to assimilate the presented information.

Authors’ response:

We can understand why you feel the paper is very long. However we address readers in both the hydrology and remote sensing communities, so shortening it by half or even by one third does not seem reasonable. We believe it is important to provide sufficient detail so it can be understood all.

Modifications to manuscript:

Without more detailed information on what you think should be shortened, we have

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limited our reductions to a few sentences or parts of sentences. An exhaustive list is given below:

Page 1, line 23, we deleted: “by the hydrological model tested”

Page 2, lines 4-5, we deleted: “After some adjustment over Lake Titicaca, MSWEP v.2.1 should thus be preferred for the regional hydro-meteorological survey.”

Page 2, line 29, we deleted: “as precipitation estimates were found to be very accurate”

Section 2.1 Study Area. The following paragraph has been deleted:

“Runoff from the lake basin and direct precipitation over the lake contribute approximately 53 and 47% of the lake water supply, respectively (Roche et al., 1992). However, studies on the regional water balance are more than 30 years old (Carmouze et al., 1977; Carmouze and Aquize, 1981; Lozada, 1985) and may not be representative of the current situation. Indeed, a recent study over the Altiplano (López-Moreno et al., 2015) found a temperature increase of between 0.15 and 0.25°C decade⁻¹ between 1965 and 2012, while Heidinger et al. (2018) reported an increase in the intensity of precipitation extremes over the period 1965-2010 in the same region. The temperature is expected to continue to increase until the end of this century while precipitation may decrease by 10 to 30% depending on the climate projections (Bradley, 2006; Minvielle and Garreaud, 2011; Urrutia and Vuille, 2009). There is thus a need to update the Lake Titicaca water balance to support efficient water management to adapt to this changing situation. However, the transboundary, economic and remote context means the sparse hydro-meteorological monitoring network prevents a consistent analysis. Indeed, precipitation presents high spatiotemporal variability due to the geomorphologic and tropical context which is poorly represented by the available monitoring network. With nearly-global scale coverage, SPPs are a promising alternative to monitor regional precipitation in space and over time.”

And replaced it by:

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“The Lake Titicaca is drained by the Desaguadero River to the south (Fig. 1) which contributes up to 65% of water inflows into Lake Poopó (second largest Bolivian lake) (Pillco and Bengtsson, 2010). An accurate Lake Titicaca water balance for monitoring purposes is therefore crucial to support efficient water resources management in the Altiplano. However, the transboundary, economic and remote context means hydro-meteorological monitoring is sparse. Thanks to almost global scale coverage, SPPs represent a promising alternative to monitor regional precipitation in space and over time, and offer unprecedented opportunity to achieve efficient regional water resources management.”

Page 8 line 4, we deleted: “for precipitation estimates”

Page 8 line 11, we deleted: “This is why we aimed to analyse SPPs at a 10-day time step for which we assumed that differences in daily time windows were negligible.”

Page 11 line 11, we deleted “To avoid overloading the analysis with less suitable SPPs”

Page 17 line 5, we deleted: “Figure 5a is the Taylor diagram obtained from SPPs at the regional scale for the 2000-2012 period.”

Page 18 line 3, we deleted: “Figure 5b presents the performance of the SPPs at the regional scale for the three 4-year time windows considered: 2000-2004, 2004-2008 and 2008-2012, in the form of a Taylor diagram.”

Page 19 line 6, we deleted: “Figure 6 shows the performance of the SPPs for three 4-year periods (2000-2004, 2004-2008, and 2008-2012) in terms of CRMSE.”

Page 21 line 6, we deleted: “Figure 7 shows the efficiency (in terms of NSE scores) of the hydrological model in reproducing streamflow at the outlet of the four tested basins (Ilave, Katari, Keka, and Ramis) using Pref and SPPs as input data over the period 2008-2012”

Page 24, line 1, we deleted: “Figure 8 compares the snow cover duration (SCD) observed by MODIS and the durations simulated by the snow model using Pref or all the

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SPPs as input data over the different periods.”

Referee’s comment:

However, in addition I think that it needs further analysis to increase the scientific significance and provides a more integrated and purposeful evaluation, as opposed to the current combination of methods, which feel disjoint and ad-hoc. In my opinion this is would have to be a (very) major revision.

Authors’ response:

As explained above, our objectives were not clear. Regarding the protocol, each step of it provides complementary information on SPP space-time consistency, which is crucial information for their use and enhancement. We hope that the changes made in the manuscript and the additional information reported will clarify the objectives and relevance of the present study.

Referee’s comment:

- I think that it is a missed opportunity not to include IMERG. Of course IMERG does not go as far back in time as the other products. But given that the temporal analysis does not yield much of a trend, I don’t think that that is a major issue. At the same time, the added value of the GPM data probably makes it currently one of the most relevant products from a water resources management perspective.

Authors’ response:

In fact, we already evaluated GPM based SPPs (IMERG and GSMAP) in a previous study (see Satgé et al, 2017). IMERG only covers the period from 2014 to the present. As assessment based on different time windows is one of the main points of the proposed protocol, it would be impossible to include IMERG. We would also like to point out that the temporal analysis yields much more than a trend. Actually, when looking at potential of SPPs as forcing data in hydrological model, the ranking of SPE performance varied considerably depending on the time window considered (Figure 7). To

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give a rapid example, we cite part of the text in section 4.2 "Space-time consistency of SPEs compared with streamflow simulations" (see page 22 lines 8-15):

"However, as shown in Fig. 7c, the SPE hydrological ranking in the 2008-2012 period changed drastically over time. For example, for the Katari catchment, MSWEP v.2.1 led to the best streamflow simulations for the 2004-2008 and 2000-2012 but not for the 2000-2004 period, for which TMPA-Adj v.7 forced streamflow simulations had a higher NSE score of 0.85. Additionally, CMORPH-BLD v.1 potential fell drastically over the 2000-2004 period with a negative NSE score, whereas it produced the most realistic streamflow simulation for the period 2008-2012. In the Keka catchment, for each time window, the best streamflow simulation was obtained using different SPPs. CHIRP v.2, PERSIANN-CDR and CMORPH-BLD v.1 resulted in the highest NSE scores over the various sub-periods analysed, with respectively 0.73 for the period 2000-2004, 0.83 for 2004-2008 and 0.77 for 2008-2012"

Modifications to manuscript:

We realized that this information needed to be recalled in the discussion part 5.3 "Space-time SPP consistency" and we have therefore added a few lines about this in the discussion part 5.3 page 29 line 23 to page 30 line 1.

"For each assessment indicator considered (i.e. gauges, streamflow and snow cover duration), the performances of the SPPs varied depending on the time window considered. These variations can be easily observed when SPPs are used as forcing data for hydrological modelling, in which case the ranking of SPPs observed for each of the 2000-2004, 2004-2008, 2008-2012 and 2000-2012 time windows changed significantly (see Fig. 12b and 12c)."

Referee's comment:

- The language is often rather imprecise, which may lead to misinterpretation. I did not have the time to make an exhaustive list, but a couple of examples include:

C17

Authors' response:

We agree. We have responded to each of your comments concerning the language and the text has been corrected by a native speaker.

Referee's comment:

p12/8: "overloading": I don't think that you can "overload" research with "unnecessary" results of "useless" SPEs. All this sounds rather unscientific. It is fair that a subset of products is used to reduce the workload, but ideally on a scientifically sound and transparent basis.

Authors' response:

Agreed, we changed the text: "To avoid overloading the analysis,.. " — > "To filter less suitable SPEs,.. " "Useless" — > "less suitable"

Referee's comment:

p12/10: "Pref is influenced by the interpolation process": This is rather euphemistic as Pref is the direct result of the interpolation process.

Authors' response:

Agreed. This sentence has been removed.

Referee's comment:

p13/13: "regional" -> "spatially averaged"

Authors' response:

Done. This term has been corrected throughout the text.

Referee's comment:

P32/21: "globally": do you mean over the study region? This is surely not global? Some other specific comments:

C18

Authors' response:

Here we mean "in general". We deleted the word "globally".

Referee's comment:

p8/6: no orographic effect: I don't follow the reasoning here. Why would accounting for the orographic effect not allow for an objective comparison?

Authors' response:

Agreed. See above our answer to your comment #3 on the decision to exclude elevation as a co-variable in the interpolation process.

Modifications to manuscript:

Based on your previous comment, we have changed the text concerning the use of elevation as a co-variable in the precipitation interpolation process (see section 2.2.2, page 7 line 13-15). "Because the gauges are mainly located in the flat land part of the basins, it was not possible to provide evidence for an effect of elevation on precipitation distribution. Consequently, no orographic effect was accounted for in the interpolation of the point precipitation observations".

Referee's comment:

p11/13: "useless SPEs": "useless is quite a strong word. Surely, they are not useless for some applications. Perhaps a more scientific formulation can be found?"

Authors' response:

Done. We have replaced 'useless' by "less suitable"

Referee's comment:

p12/13: "when more than 80% of daily values records were available": This is not very conservative and may lead to large errors. I suggest to take only 10-day periods that have complete daily records.

C19

Authors' response:

We agree that this statement was poorly explained. Actually, only 10-day amounts with more than 80% of common daily data available were used and each 10-day amount was computed using only daily amounts available for Pref and all SPPs in order to compare exactly the same thing.

Modifications to manuscript:

In response to your comment, we have changed the text to make it clearer (see section 3.1 page 11 lines 11-14). "For each of the 69 0.05° grid-cells, the 10-day records were only computed when more than 80% of daily values were available from all precipitation datasets (Pref and SPPs) for exactly the same date. Next, mean spatially averaged 10-day precipitation series were computed from Pref and all SPPs by aggregating the values from all 69 grid-cells."

References:

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