Response to referee #1 (F. Maussion) on "Spatio-temporal assessment of WRF, TRMM and in situ precipitation data in a tropical mountain environment (Cordillera Blanca, Peru)" by L. Mourre et al.

We sincerely thank Dr Maussion for its constructive comments on our paper, and we have tried to take them into account as thoroughly as possible. The main point raised in its review relates to the choice of the rainfall products that were selected as typical of the gauge product family, on the one hand, and of the model product family, on the other hand. We thus address this essential issue first, and we then provide point by point answers to the other, more technical, comments.

1. General discussion on the quality of the chosen products

A first general comment on the quality of the chosen products is that we do not pretend that they are the best possible candidates for representing their respective family. We only think that in each case they are reasonable choices, so that their main characteristics can be considered as typical of the family to which they belong.
Our second comment is that the goal of the paper is to assess the main differences between these products so as to draw the attention of the reader on their possible bias and shortcomings, keeping in mind that we claim that no rainfall product in this region can be considered as a reference at all scales. Thus it would be very difficult to state which product is the best within a given family.
Third, we must recognize that it is indeed a natural request to provide the reader with some information on the spread of the characteristics of the products belonging to the same family, in order to be able to judge whether the differences between the typical products of each family are significantly larger than the differences between products of the same family.
And finally the remarks and questions of the two reviewers are extremely relevant and we hope to provide here the answers they deserve.

1.1 Critical evaluation of the kriging products.

Reviewer comment. *The algorithm relies on the calibration of a statistical interpolation model, with the addition of the topography as a further predictor. As acknowledged by the authors, this does not work very well since the precipitation maxima are found at the mountain tops. The authors however do not discuss another (to my opinion, more important) short-coming of this method: the direction of the air flow. It is well known that the leeward and windward sides of orographic barriers have opposed precipitation patterns. The shortcomings of KED are best shown in Fig. 05: the north-eastern part of the domain (towards the Amazon Basin) should be much wetter, as shown by TRMM and WRF. Here the effect of topography is overestimated by the KED model.*
This might not be too problematic within the Rio Santa basin thanks to the reasonable number of stations on each side of the basin. Outside of the basin the KED results should be interpreted with great caution. I would argue that the omission of the air flow directions is the major reason why the daily-evolving variogram performs best for the cross-validation (Table 5): it contains indirect information about the air flow through the current precipitation patterns that day (K-DE works also better than KED-M for the daily values).

As the reviewer points out, the north-eastern part of the domain (towards the Amazon Basin) is much dryer in KED outputs compared to what it is in WRF and TRMM. This underestimation is primarily due to the lack of sampling in this area which does not allow catching the rainfall effect of the moisture influx from the Amazon Basin. Our belief is that no statistical method could really make up for this lack of measurements. KED is an efficient interpolator when a simple external drift – as the altitude – can be found. But when it comes to incorporating in the interpolation such a changing and complex variable as the air flow, we cannot rely on a simple linear method. This is why physical models are a necessary complement of ground measurements, especially in those regions of great spatial variability associated with a complex topography.
The fact that rainfall is significantly underestimated in the Amazon Basin is not a major concern for us, since the focus of the paper is on the Santa catchment. However we added a comment in the new version in order to acknowledge this problem (in Section 2.1 of the new version).
It is a reasonable assumption that DE (daily evolving) variogram contains information about the spatial structure of precipitation related to the air flow direction, explaining why this type of variogram gives better results; but in fact it is a more general statistical matter that DE variograms are more tied up to the observed structure of the daily rain field than an average variogram.

<u>Reviewer comment.</u> Furthermore, when comparing the KED products to the other products at the station locations (Figs. 3, 4, 7) the authors should not use the full-model products but the cross-validation ones. For example, the frequency diagrams at Corongo should not rely on the observations at Corongo for the calibration. Maybe the authors did this already it is not clear from the text. This might well mitigate the good results of KED in these analyses.

This is absolutely right and in the new version we use the ground cross-validation products rather than the ground full-interpolation products thus removing the methodological bias associated with the latter.
While the results are very similar for the 27-km resolution, a change in the upper part of the distribution is observed at the 3-km resolution (Figure 4; Figure 3 in the previous version of the paper), resulting in a slight decrease in performance with respect to the HSS index in figure 5 (figure 4 in the previous version of the paper), and also concerning the precipitation above 5 mm d^{-1} in figure 3 (figure 4 in the new version). This shows that the empirical distribution of the ground rainfall products is indeed very sensitive to the network sampling. When there are no observations within a grid mesh, the smoothing out produced by the interpolation process tends to minimize the occurrence of strong rainfall while overestimating the occurrence of rainfall (187 days in the cross-validation product against 153 days for the full sample product). On the other hand as for the ground full-interpolation products, the WRF and KED products remain significantly different with a likely strong overestimation of heavy daily rainfall by WRF.

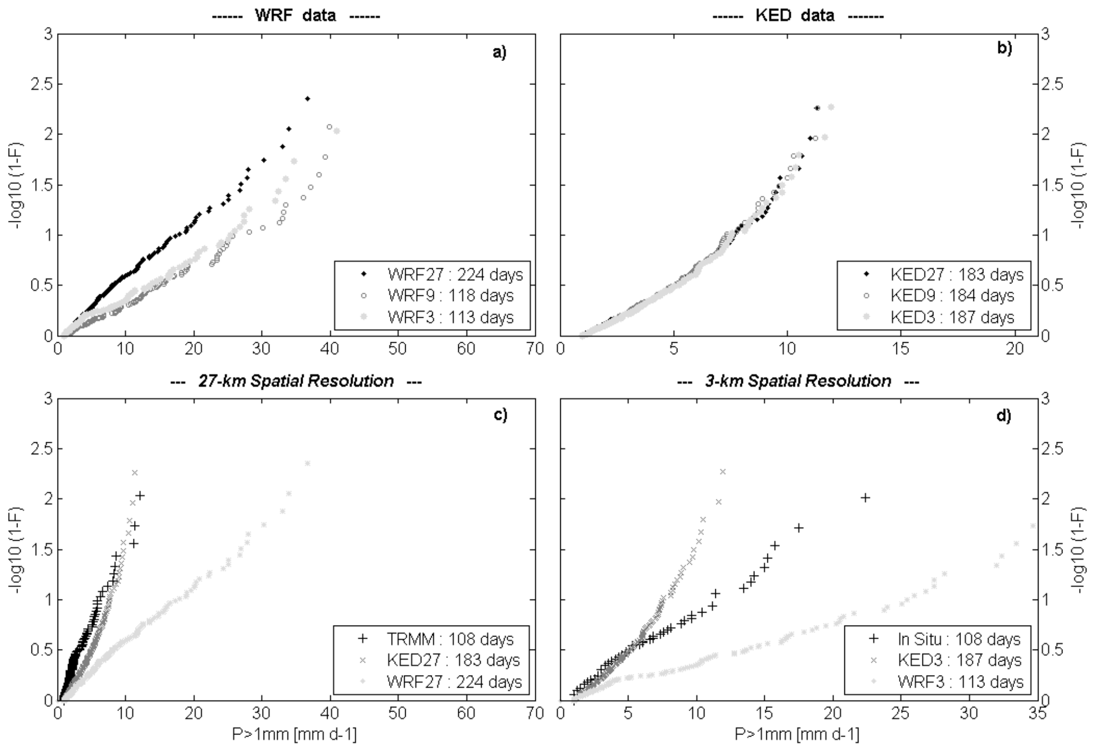

Figure 4 (Figure 3 in the previous version): Frequency diagram of Corongo (station n°2) of daily precipitation data > 1 mm d-1 for WRF outputs (a) and KED products (b) at three different spatial resolutions, and for all products at 27 km (c) and 3 km spatial resolution (d). Numbers in the bottom right corner indicates the number of days with precipitation > 1 mm d^{-1} for each dataset.

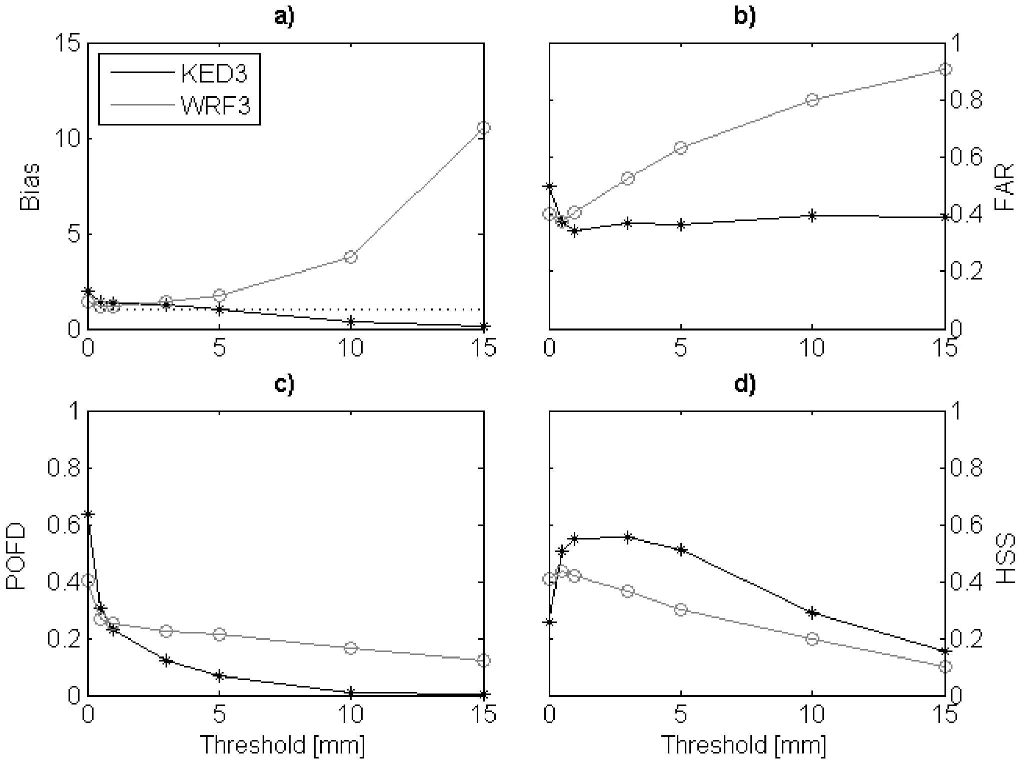

Figure 5 (Figure 4 in the previous version): Daily precipitation indices: BIAS (a), False Alarm Rate (b), Probability Of False Detection (c) and Heidke Skill Score (d). Calculated for KED3 (black) and WRF3 (gray) against rain gauges precipitation data located in the Sierra area. Scores have been evaluated for several daily precipitation thresholds: 0.1, 0.5, 1, 3, 5, 10 and 15 mm.

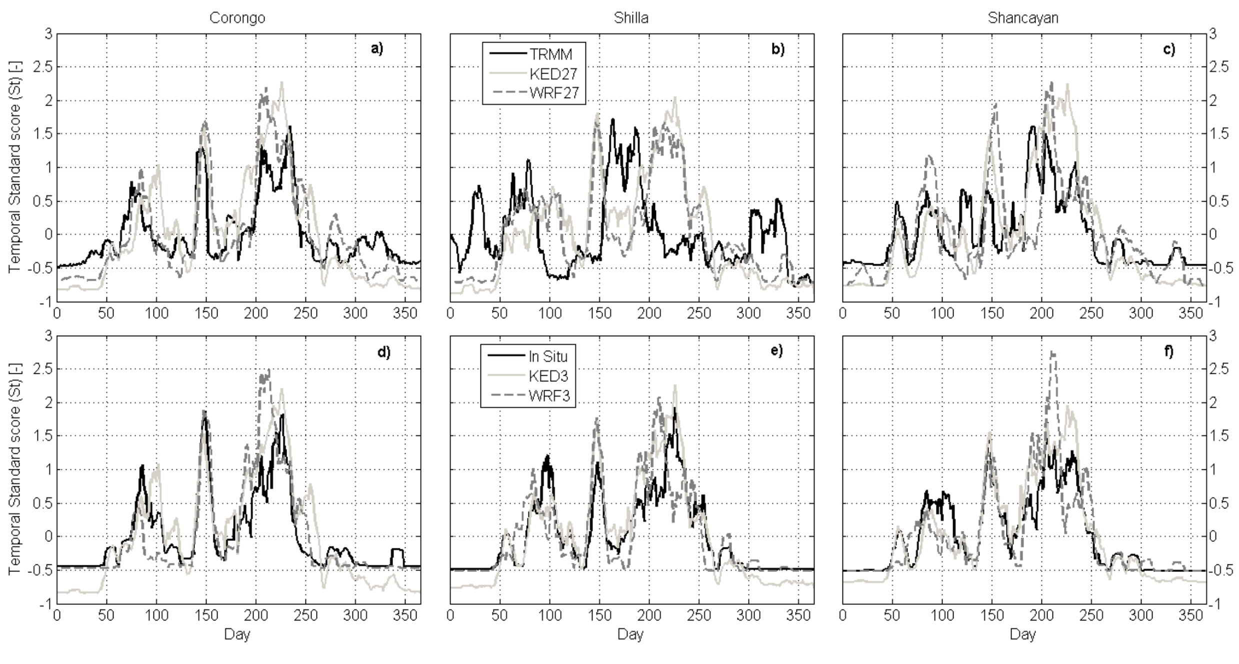

Figure 8 (Figure 7 in the previous version): Temporal Standard score of running means of daily precipitation amounts over 10 days for three stations along the Rio Santa valley, for 27 km (a-b-c) and 3 km (d-e-f) spatial resolutions. Gray line is for KED, dotted line for WRF, and dark line either for TRMM (upper panels) or in situ (lower panels). Day 1 corresponds to the 1st of August 2012.

1.2 Description and purpose of the WRF simulation

The authors rely on one single WRF simulation, but WRF is sensitive to the parametrizations and forcing data used. From the literature and my own experience I don't expect that the conclusions of the present study will be *affected substantially (over-estimation of precipitation, for example, is a known feature of the model), but this limitation should be acknowledged in the discussion. I also have a few concerns about some aspects of the simulation design. I do not formally ask for new simulations, but I suggest the authors to consider the possibility of adding a small number of sensitivity studies (see specific comments below). This would help to strengthen their conclusions by also adding some recommendations for scientists wishing to conduct modelling studies in the region.*

Various WRF configurations have indeed been tested in the region. Several of them gave results very similar to the simulations presented in the paper, especially from the rainfall distribution point of view. We are therefore confident that the WRF rain field retained for comparison with the other types or rainfall product is fairly representative of rain fields produced by regional atmospheric simulations in the region. It would have overloaded the paper to enter into the details of these various configurations; however, in order to provide the reader with some background on this important issue, we added this paragraph in Section 2.5:

« *The over-estimation of the precipitation is a frequent bias in numerical models (e.g. Mearns et al 1995), particularly in complex orographic regions. Preliminary tests of sensitivity with various WRF parameterizations (including different cumulus schemes, cloud microphysics, planetary boundary layer and land surface options) have been done in the tropical Andes at a 27 km horizontal resolution; a clear over-estimation of precipitation was observed with all these configurations and over all the domain, including the high mountain areas. The biases found with other configurations were almost similar to those of the one selected here in terms of the precipitation spatial distribution, and with quantitative differences more pronounced in the eastern slopes of the Andes and in the Amazon region rather than in high mountain zones like the Cordillera Blanca. The configuration finally retained for this study (Table 3) has been selected because (i) it minimizes the positive precipitation bias in the tropical Andes above 3500m, and (ii) it simulates correctly the spatial distribution of the precipitation in the region, including the zones of maximum precipitation situated in the Amazon basin and in the eastern slopes of the Andes (Fig. 2), when compared with the TRMM data. At 3-km resolution, the Noah-MP option was found to decrease the precipitation over-estimation in the Cordillera Blanca and show a more realistic snow distribution when compared with previous observations.* »

We provide below some additional material for the reviewer. As mentioned, WRF was tested at a 27 km horizontal resolution with six different parameterizations, including different cumulus schemes, cloud microphysics, planetary boundary layer and land surface options, for the month of February 2013. Results of this WRF-Ensemble mean (WRF-EM) is displayed in Figure R1 below. Over almost the entire domain and particularly over the Cordillera Blanca, the standard deviation computed among the six monthly means (Fig. R1a) is clearly smaller than the bias computed with the WRF-EM minus TRMM (Fig. R1b).

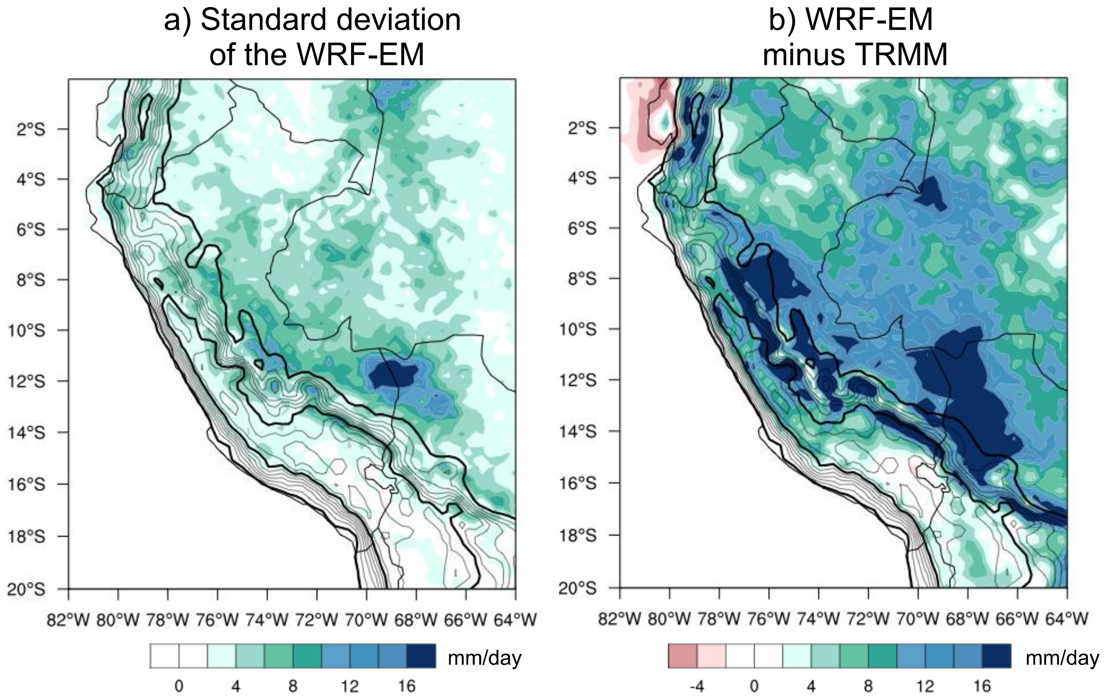

Figure R1 (not included in the manuscript): a. Standard deviation of the precipitation (mm/day) computed *from the WRF-Ensemble mean (WRF-EM), six configuration of WRF-27km with different parameterization options, for the month of February 2013. b. Precipitation bias (mm/day) computed with the WRF-EM minus TRMM for the month of February 2013.*

At the 3km resolution, we tested three options for the surface scheme Noah-MP during the month of February 2013. Results of this WRF3- EM are plotted in Figure R2 below, comparing monthly precipitation of WRF3-EM with in situ data. In almost all the domain, the standard deviation of the model simulations (Fig. R2a) is clearly smaller than the difference between the ground measurements and the model mean (Fig. R2b).

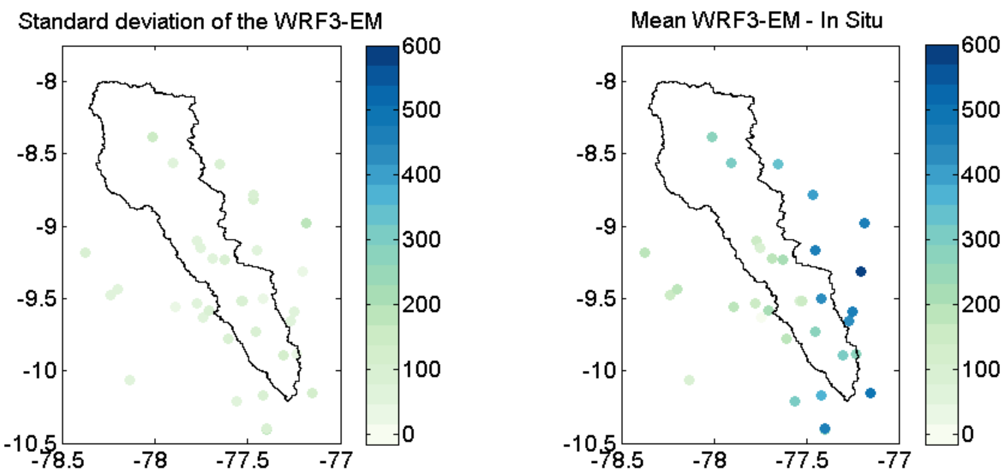

Figure R2 (not included in the manuscript): a. Standard deviation of the precipitation (mm month^{-1}) computed from the WRF3-EM (three configurations of WRF-3km with different surface scheme) for the month of February 2013. b. Precipitation bias (mm month^{-1}) computed with the WRF3-EM minus in situ data for the month of February 2013.

Shorten the text to focus on the essentials:

The manuscript could attract more readers with a clearer and more concise writing. There are some repetitions throughout the manuscript, the introduction material is sometimes only indirectly related to the study.

Following the advice of the reviewer several modifications were made in order to avoid repetitions and to better focus the paper (abstract, introduction, comparison of the ground products, conclusion).

2. Specific comments

Abstract: It is a matter of taste but I suggest to avoid using paragraphs and to shorten it. I suggest to remove certain details ("largely due to operational constraints", "glacial area", "Thompson microphysical scheme", . . .) and to avoid repetitions ("– ground based, satellite derived, RCM outputs –" repeated afterwards anywhere, "here", etc.).

As proposed, certain details had been removed in the new version of the paper.

P6639 you write: "The driving question of this study is to identify and compare the precipitation data sets that can be used for properly characterizing the water balance over catchments of the region, from the sub-daily and daily temporal scales driving flooding to the decadal and multi-decadal scales". You should be more careful here because you actually don't (and can't) address all these scales (data availability, computational cost of WRF, etc.). I would welcome a more concrete formulation of the study's objectives.

A more specific formulation is used in the revised version:

"The driving question of this study is to identify and compare the precipitation data sets that can be used for properly characterizing the water balance over catchments of the region from sub-daily to yearly temporal scales."

P6639 "the precipitation produced by climate models" → I suggest to use a more precise formulation ("climate models" cover a wide range of models). I would also refrain to use the term "RCM" for your WRF simulation: a one year long simulation data forced by analysis data cannot be termed a "climate simulation". Some studies prefer "Mesoscale Atmospheric Model" (MAM), or simply "Numerical Weather Prediction model" (which is, after all, *what WRF was designed to be).*

Agreed: a one year study is obviously not a climate simulation; "RCM model" was replaced by "WRF model", throughout the paper.

P6645, WRF simulation: More information is needed here:

• Rationale for the choice of the first domain boundaries. Domains 2 and 3 are close to the western boundary of domain 1, which is usually not a good thing for the consistency of the boundary conditions (the influence of the large-scale driving data is much larger at the boundaries).

Domain 1 includes all the tropical Andes, as it was designed to be part of a project including the entire region. The western boundary conditions were verified and the critical zone is not included in the domain 2.

• For future studies, consider using reanalysis products instead of FNL analysis data to avoid time-consistency problems, as discussed by Maussion et al. (2014).

We thank the reviewer for the suggestion. The FNL analysis were used for practical reasons, as they are more easily and rapidly available than other WRF forcing data. However a sensitivity test was performed with ERA-Interim reanalysis as forcing data, and the over-estimation precipitation bias was found to be stronger in the Cordillera Blanca region than with the FNL analysis.

• Did you use a spin-up for your simulation?

Yes. The simulation starts at the beginning of April 2012, but in order to perform the comparison on a 1-year hydrologic year corresponding to the period where in-situ data are available, the 4 first months were finally not used and considered as a spin-up period. This information has been added in Section 2.5 of the new version.

Did you consider using some kind of nudging during the simulation time? I suspect that the good temporal performance of WRF (Fig. 7) can be attributed to the small simulation domains and to the fact that the nested domains are all close to the boundaries.

Nudging was not used in our WRF simulation. The model is forced at the boundary every 6 hours. Domain 2 is close to domain 1 at the western boundary. However, the moisture flux mainly comes from the Amazon Basin, on the eastern side of the Andes (Garreaud, 2009) and the seasonal variability of precipitation is more strongly associated with regional processes from the East than from the West (except in case of a strong ENSO year).

• From Table 3 it seems that you have used a cumulus parametrization for all three domains. If this is the case you should justify it. There are arguments against using cumulus parametrizations for spatial scales well below the resolutions they have been designed for (so called "gray-scales", see e.g. Arakawa (2004) or the introduction by Grell and Freitas (2014) for more references).

In theory, current cumulus parameterizations (CP) were developed for grid scales from 32 to 48 km (Arakawa, 2004), and have shown to work well also for a 12-km grid resolution (Wang and Seaman, 1997). Below a 5-km resolution, the influence of CP is not clear yet, and sensitivity tests should be performed for each configuration and region of study. However, in real cases, using a CP at high resolution is useful in complex orography regions so as to improve the air column stability of the model. In addition, Gililand and Rowe (2007) have shown that it is not always appropriate to assume that model simulations using small grid spacing (< 5 km) will not need a CP. While in an idealized case with a 3-km grid scale configuration they found that a CP is not needed, in a real-case study they demonstrated that a CP is needed in order to accurately represent the small sub-grid scale processes that occur with isolated, discrete convective cells. In their example, at 3 km WRF without CP was not able to resolve all the precipitation, in a region where precipitation comes from cumulus formation.

In the tropical Andes, the most common precipitation bias of regional climate models is an overestimation in the higher mountains. Grell and Freitas (2014) show that the no-CP WRF parameterization at 5-km horizontal resolution better reproduces precipitation in the monsoon areas of South America when compared with TRMM. However, in the tropical Andes it is clear in their Figure 11 that this no-CP configuration also shows the strongest overestimation bias, when compared with activated cumulus parametrization configurations. However, more sensitive simulations should be pursued in order to confirm the use of a CP or not in Cordillera Blanca at 3-km.

These precisions were added in the text (Section 2.5).

P6652, L12 "WRF overestimates rainfall, probably due to errors in the NCEP-FNL forcing": the discussion about this "overestimation" is not new. From all possible reasons, the "wrong boundary conditions" would not be my first choice. In the absence of more detailed information (further forcing data experiments? Other WRF parametrizations experiments? Underestimation of rain gauges?), I suggest to remove this sentence.

We agree with the reviewer that the overestimations of rainfall in WRF cannot be attributed only to the large scale boundary forcing, and that a larger spectrum of runs would be required to deal properly with this question. As this is not on the scope of our study, the sentence was removed.

Conclusions about TRMM: I wonder if these are not an over-interpretation. Most of these conclusions are general and not directly related to the material presented in the paper.

While we did not carry out a detailed study on the TRMM product used here, the effect of ice scattering on the ground on the quality of the TRMM product for high altitude regions has been documented in previous studies (see e.g. Yin et al., 2004, the Tibetan Plateau). Considering that some studies directly use TRMM data as inputs to an hydrological model (Lavado et al., 2009; Andres et al., 2014), we believe that those remarks are relevant to our paper, which aims at providing some overall perspective on the rainfall products that can be used for forcing hydrological models in this region.

3. Technical Corrections

All the modifications related to the technical comments made by the reviewer were included in the revised version of the paper. Some of these modifications are more specifically commented below.

P6642 *"one hydrological year (August 2012 to July 2013)". Unless there are good reasons to call this "hydrological year" I would suggest to simply call it "year" (most HESS readers would expect an hydrological year to span Oct. to Sep.).*

In this region, the driest months are June – August with rainfall sometimes reappearing in August. We thus think that it is appropriate to specify that our period of study roughly corresponds to a hydrological year

P6643 *"summertime"→ austral summer. Are temperatures really higher during austral summer in the region?*

Although variations of the seasonal temperatures are less than 1°C in the region (Juen et al., 2007), temperatures are slightly higher during austral summer and can modify the 0°C isotherm line altitude (considering a lapse rate from other studies (Juen et al., 2007; Carey et al., 2012; Schauwecker et al., 2014) the displacement is between 125 to 155 meters).

P6642*, "In-situ data" The authors gathered an impressive number of stations for the region. I think that some readers will be interested to know what is the "availability" of these data. If they are available, state where they can be downloaded. If they are not publicly available, say it too (this will spare some searching time for the curious reader).*

The SENAMHI data were available thanks to our collaboration with this institute and particularly with Waldo Lavado. They are not freely available.

The UGRH data were also available thanks to our collaboration with this institute and are not freely available.

The UNASAM will normally soon be available at http://www.ciiaders.com/.

P6646, L10 *"daily scale which is the corner scale for the comparison carried out in this paper": you also provide an analysis of diurnal cycles.*

The daily time scale is the only one for which all datasets are available and at which we can compare all the different sources of precipitation data. The diurnal cycle analysis is only carried out on the in situ data and the WRF3 outputs. That is why we considered that the daily time scale is the corner scale of our study.

P6650, L18 *"WRF precipitation areal averaging effect is the only one that is not similar at all stations inside the Rio Santa watershed, and this complex problem, beyond the scope of this study and probably related to the internal thermodynamic of the model, will not be addressed here.": I don't understand this sentence.*

The point we wanted to make is that the differences between the WRF27/WRF9 distributions and the WRF3 distribution vary in nature from one station to another, which raises some questions on the model behavior.

However, since this point is not illustrated in our paper (distributions are shown for one station only in Fig.4 –Fig. 3 in the previous version of the paper) and since it would require additional space to comment it while it is not central to the paper, we chose to remove this comment.

P6652, L5 *there are approx 13 grid points of 27km resolution in the catchment. Random sampling errors are very likely: how did you compute the area-averaged precipitation for the catchment? Did you use sub-pixel masks, or did you consider the center coordinates of the grid points?*

This is a very relevant comment that allowed improving the quality of our computations. Initially two watershed masks (at 27 and at 9-km spatial resolution) were considered. In the revised version proposed here, a new watershed mask was computed at a resolution of 90 meters in order to reduce the sampling errors in the mountainous areas. This 90-meters resolution mask was used to re-calculate the water precipitated over the catchment for all the products (each 90m*90m pixel is given the value of the coarser pixel to which it belongs). This modifies the results, as can be seen from the new Table 4.

Table 4: Precipitation data used in this study, with their spatial and temporal resolution, and the accumulated amount precipitated over the Upper Rio Santa watershed during hydrological year 2012/2013. WRF and KED (corresponding to kriging data with external drift – daily evolving variogram) are at 3 different spatial resolutions (27, 9 and 3 km). TRMM is the TRMM3B42 product.

Product	**Spatial resolution**	**Temporal resolution used in this study**			**Annual precipitation over the watershed [m]**
		Hourly	**Daily**	**Yearly**	
In situ	Punctual	x	x		-
KED27	27×27 km²		x	x	0.83
KED9	9×9 km²		x	x	1.01
KED3	3×3 km²		x	x	0.95
WRF27	27×27 km²		x	x	2.91
WRF9	9×9 km²		x	x	1.95
WRF3	3×3 km²	x	x	x	1.97
TRMM	27×27 km²		x	x	0.57

P6654, L1 *"But we have to keep in mind that it corresponds to precipitation averaged for 3 km grid cells that could include lower area in this zone of strong altitudinal gradients.":in the "real world", yes, but not in the "WRF world". There is no subgrid topography in WRF and it is a common misinterpretation: the grid cell height in the one "true" height in WRF and the solid precipitation estimations of WRF are based on this altitude only.*

The reviewer is right, WRF has no subgrid parameterization and the altitude of the grid cell is the true one for the selected resolution. However, when using WRF precipitation as inputs for glacio-hydrological modeling with a finer spatial resolution for the topography, not taking the precipitation gradient within the original WRF pixel will have some impacts on the quality of the simulation. Nevertheless, we removed this sentence that could be misunderstood in this context.

P6654, L16 *"as ice on the ground scatter energy in a similar way as precipitation drops in the atmosphere (Maussion et al., 2011)": Let's try to avoid reference chains. Maussion et al. (2011) (wrongly) attributed this sentence to Yin et al. (2008) who in fact referred to their earlier study (Yin et al. 2004).*

We give this citation back to Yin et al. (2004).

Fig. 8*: specify which time is used (LT?). Station observations: the hours of day where precipitation is not observed*

at all seem unlikely (e.g. from 4H to 9H at Shancayan). Any explanation?

Local Time (LT) is used in this figure. Most of the UNASAM stations measure little rainfall during the second part of the night. However the scale of figure 8 (figure 9 in the new version of the paper) was misleading. The threshold of detection with the tipping bucket gauge used is 0.254 mm. As can be seen from the additional figure R3 presented below several isolated tipping occurrences can be recorded in the early morning, but it is very rare to observe more than 2 tippings during one hour for this part of the day and few days recorded rainfall during those hours.

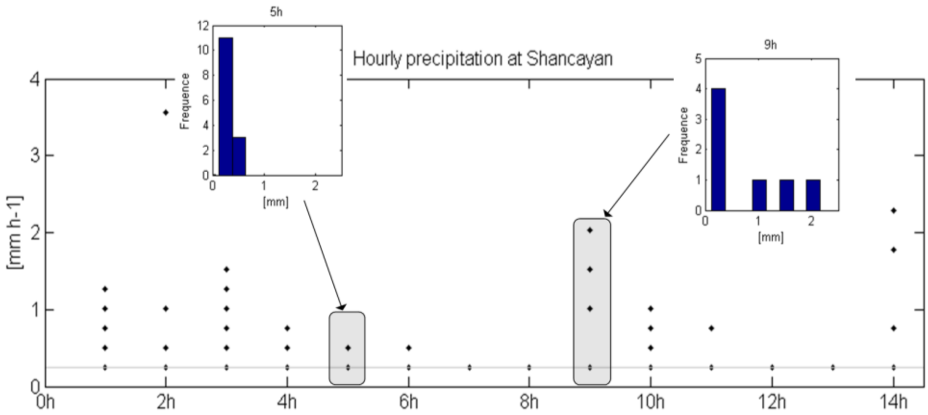

Figure R3 (not included in the manuscript): Values of hourly precipitation at Shancayan between 0h and 14h for the hydrological year 2012/2013 (1st August 2012 to 31st July 2013). The gray line corresponds to the threshold detection of the tipping bucket gauge. Note that each black point is representative of several measurements.

P6657 *"at this 9 km resolution, non-hydrostatic effects are significant and since convection is partially solved in the model more realistic precipitation quantities are produced": I don't understand this sentence.*

Applying WRF in Portugal at 9 and 27 km spatial resolution, Soares et al. (2012) concluded that the non-hydrostatic effects of WRF where relevant for resolution of 10 km or higher, and that at this resolution, convection is partially solved. However, this sentence, which is based on a single source, was removed.

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