Our thanks to both reviewers, which -with their comments- helped us to improve the quality of this work. Below, we provide the detailed replies (**R/.**) to each of the comments.

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## ANONYMOUS REVIEWER #1

1. The presentation of the results and the discussion that follows are rather superficial and could be substantially improved. Let's be honest! Given the large number of links, it should come as no surprise that the measurement and representativity errors constitute the major source of uncertainty. The idea of assessing the relative error associated with mapping is new but the methodology used to tackle this issue could be further improved. For example, other interpolation methods (e.g., universal kriging and splines) and network topologies (e.g., various subsets of the considered network) should be considered before drawing any hasty conclusions.

**R/.** In this work, the use of Ordinary Kriging (OK) enables not only a consistent comparison of our results against those presented by Overeem et. al. (2013) [*Overeem, A., Leijnse, H., and Uijlenhoet, R.: Country-wide rainfall maps from cellular communication networks, P. Natl. Acad. Sci. USA, 110, 2741–2745, doi:10.1073/pnas.1217961110, 2013*], but also a simple and straightforward way of disentangling various sources of uncertainty in rainfall maps derived from microwave link measurements. The OK approach also works as a simple interpolation technique highly suited for the geographical conditions of The Netherlands (and its climate), under the conditions further explained below in our reply to comment #15.

We realize that alternative interpolation methodologies could yield lower mapping uncertainties/errors; however, a comparison of different interpolation methods was considered beyond the scope of the current research. Such a comparison could indeed form a good starting point for future research along this line.

We agree with the reviewer that one would expect the contribution of mapping to the total uncertainty to be small given the high density of the link network in The Netherlands. We have carried out some additional analyses on the effect of the local link density on the uncertainty. See our reply to comment #9 for details.

2. The title of the paper is somewhat misleading: it gives the false impression that this is a general and exhaustive analysis of the different error sources involved in microwave link rainfall estimation. In reality, however, the authors provide a case study for the Netherlands and only consider two main sources of errors (i.e., measurement and mapping). A better phrasing that is more aligned with the content of the paper would help.

**R/.** The title of the paper will be changed to: “**Measurement and interpolation uncertainties in rainfall maps from cellular communication networks**”.

Our analyses involve data from an entire cellular communication network. As such, it complements our previous detailed treatment of the various physical error sources affecting rainfall estimates from individual microwave links only:

- *Leijnse, H., R. Uijlenhoet, and J.N.M. Stricker, 2008: Microwave link rainfall estimation:*

*Effects of link length and frequency, temporal sampling, power resolution, and wet antenna attenuation. Adv. Water Resour., 31, 1481–1493, doi:10.1016/j.advwaterres.2008.03.004.*
- *Leijnse, H., R. Uijlenhoet, and A. Berne, 2010: Errors and uncertainties in microwave link rainfall estimation explored using drop size measurements and high-resolution radar data. J. Hydrometeor., 11, 1330–1344, doi:10.1175/2010JHM1243.1.*

Note that The Netherlands and Israel are currently the only countries in the world where such data are available to the research community at a country-wide scale. In our opinion, we present more than one case study, since the analyses are based on 12 days of country-wide data.

3. There is a general confusion between "measurement" errors and "link-radar representativity" errors in the paper. Often, the term "measurement error" is used to denote both types of errors (e.g., p.3301, ll.6-7 and p.3302, ll.1-2). At other instances (e.g., p.3305, ll.3-6), the "link-radar representativity" is grouped with the mapping errors. This absolutely needs to be clarified to avoid any confusion.

**R/.** We agree with the reviewer that there is some confusion in the paper about representativity errors, and that this should be clarified. The aim of the paper was to separate mapping errors from the other sources of error, whereby we assume the gauge-adjusted radar rainfall fields to be the ground truth. Because our mapping methodology takes line-averaged rainfall intensities and treats these as point-scale rainrates, such errors (which could be called spatial representativity errors) are included in the mapping error. The term "measurement error" that we use throughout the paper includes all other representativity errors. We will modify the text in several places in order to clarify the issue:
- On p. 3295, lines 15-17, we will replace the sentence "In this way … or temporal sampling." by **"The simulation allows us to separate mapping errors from other errors."**.
- On p. 3296, lines 6-10, we will remove the two sentences "Radars sample a … microwave link measurements.".
- On p. 3296, line 26, we will add **"The path-average link rainfall estimates are assigned to the point at the center of the link, so that these point data can be used in the OK interpolation. This conversion from line-scale to point-scale data is part of our mapping method, and hence errors resulting from this conversion are part of the mapping uncertainty."**.
- On p. 3299, line 15 we will modify "in space and time" to **"in time".**
- On p. 3302, lines 5-10, we will modify the sentence "The remaining scatter … such as weather radars)." to **"The remaining scatter can be attributed to the interpolation methodology (including the assignment of line-average rainfall intensities to the link's center point), the spatial variability of rainfall, and the effect of other factors such as the variable and limited density of the link network (more links in urban than in rural areas)."**.
- On p. 3305, lines 3-5, we will remove the sentence "We converted our analyses … microwave link measurements.".

4. Some additional details about the variogram used to krige the rainfall fields (LINK, partSIM and fullSIM) are required. Please specify if you used a single variogram for all three cases and all time steps or if some kind of estimation/adjustment was performed. If the kriging of the link data was performed using a climatological variogram, please mention it. Also, it might be worth mentioning what happens to the interpolation in case the variogram has to be estimated from the link data.

**R/.** We used a single semivariogram model, namely the spherical model with parameters derived by van de Beek et al. (2011) [*van de Beek, C. Z., Leijnse, H., Torfs, P. J. J. F., and Uijlenhoet, R.: Climatology of daily rainfall semi-variance in The Netherlands, Hydrol. Earth Syst. Sci., 15, 171–183, doi:10.5194/hess-15-171-2011, 2011.*] based on rain gauge data. This is an isotropic and climatological model, which indeed was used in all the kriged rainfall fields from the LINK, partSIM and fullSIM data. No adjustment or semivariogram fitting was done for any of the three types of data.

We described and detailed the characteristics of this model in the last two paragraphs of Section 2.3 (Rainfall maps).

Below, in reply to comment # 15, we now indicate why we chose this model and not one fitted model from link data, namely because of the difficulty to systematically retrieve one consistent model for 15-min rainfall depths. In order to implement the remaining suggestions of the reviewer, the beginning of the last paragraph of Section 2.3 will be rephrased as follows: "**For the LINK, partSIM, and fullSIM datasets, 15-min rainfall maps were obtained as follows: first, the spherical semivariogram parameters were computed and downscaled for the given day of the year. Hence, a single semivariogram is applied to all 15-min time steps within that given day. The nugget was defined as 10% of the sill. Second, rainfall depths...**".

5. What about a simulation approach? If you know the variogram, you can generate artificial rainfall fields with similar spatial structures. This could be used to study the importance of the interpolation method and of the network topology.

**R/.** In our application of OK we have restricted our analyses to average fields (i.e. expected values). To study the effect of interpolation method and network topology we will introduce in the revised manuscript the concept of microwave link density per pixel, and compare such values against the error metrics ($r^2$ and CV). In that way we can assess the influence of the network topology (i.e., its density) on the OK method, as suggested by the reviewer.
Please see our detailed analysis in our reply to comment # 9.

6. What about intermittency? Is intermittency the reason why on p.3300 ll.5-6 you restrict the comparison to points with at least 0.1 mm accumulation? Please specify the underlying assumptions and comment on the effects they might have on the results (i.e., bias, CV and non-stationarity).

**R/.** The reason to select only those paired-rainfall depths for which the gauge-adjusted radar value (considered to be the ground-truth) exceeded 0.1 mm, was to only consider hydrologically significant rainfall depths. In other words, all radar rainfall depths below 0.1 mm were considered as no rain. This allowed us to be consistent with the inter-comparison we carried out among the three datasets we based our analyses on, namely LINK, partSIM, and fullSIM.

In page 3301, lines 19-22; we indeed gave a hint of what would happen to the metrics (more specifically to the relative bias), had the 0.1-mm threshold not been applied: "If all paired rainfall accumulations would have been used (and not only those in which at least the radar rainfall depth exceeds 0.1 mm) one would expect the relative bias to be exactly the same for all aggregation levels, because both aggregation and computation of the bias are linear operators (Eq. 1)".

As a matter of fact, had we decided not to apply such a threshold, the relative bias would have been substantially reduced (almost to an unbiased situation), the CV would have drastically increased, and the square of the correlation coefficient would have improved by 30, 16, and 10% respectively for the LINK, partSIM, and fullSIM datasets. This comparison between metrics is shown in the table below, only for the case of 15-min rainfall maps ($A$ = 1 km$^2$).

| | **0.1 mm Threshold** | | | **NO Threshold** | | |
|---|---|---|---|---|---|---|
| | LINK | partSIM | fullSIM | LINK | partSIM | fullSIM |
| **rBias** | -14.3% | -13.0% | -9.3% | 1.9% | -0.5% | 0.9% |
| **CV** | 1.216 | 0.871 | 0.748 | 3.2813 | 2.332 | 2.002 |
| **$r^2$** | 0.366 | 0.605 | 0.709 | 0.477 | 0.700 | 0.779 |

The differences between the two cases of “thresholding” are mainly attributed to the size of the sample over which the metrics are computed. When the 0.1 mm threshold was applied to the radar rainfall depths, there is a reduction in 86.1% in the number of pixels used for computing the metrics compared to the case of no threshold. If we look at the expression for CV (Eq. (2)), and assume that the bias is close to 0, then the effect of adding zeroes to both link and radar rainfall data is that the CV increases with sqrt($N$). Given the 86.1% data reduction this means that the CV is expected to decrease by a factor of 2.68 if all values that are removed are indeed zero for both datasets. The fact that the reduction factor is 2.70, 2.67, and 2.68 for LINK, partSIM, and fullSIM, respectively, means that the differences in bias are caused by low rainfall intensities.

7. p.3301, ll.15-17, *We see that the biases are hardly reduced and therefore conclude that the underestimation noted earlier must be almost entirely due to errors introduced by the incomplete spatial sampling*.
I would be more careful with this statement. The observed differences can also be the result of a sub-optimal interpolation method. In this case, the major issue is not the fact that you have incomplete sampling but the stationarity assumption behind ordinary kriging (i.e., constant mean and variance). In other words, the fact that partSIM has only a slightly lower bias than LINK may also be because ordinary kriging is not the best interpolation method in this case. The point I try to make here is that the choice of the interpolation method and the assumptions behind it matter, especially in networks with highly variable densities. Maybe if you had used another interpolation method, the differences in bias between partSIM and fullSIM would not have been that large...

**R/.** As we explain more in detail in our response to comment # 15, for the conditions and constraints of this work, we assumed stationarity. The fact that we only used one method of interpolation allowed us to determine the relative contribution to the global error. It seems that it can be attributed to incomplete spatial sampling. Note, however, the table in comment # 6, which shows the dependence of relative bias on the chosen threshold(s). Hence, we believe that underestimation cannot be directly attributed to incomplete spatial sampling.

On p. 3301, lines 8-17, we will remove the entire paragraph “The main question we focused... by incomplete spatial sampling.”.

8. p.3303, ll.25-26, *We found that link rainfall retrieval errors themselves are the source of error that contributes most to the overall uncertainty in rainfall maps from commercial microwave link networks*.
It's more correct to say that the major error is due to the retrieval and/or the representativity error between link and radar, with no way of knowing which contributes most. Also, you forget to say that this result is based on the assumption that the variogram of the rainfall field is known a priori. If you had no radar nor gauge data, the variogram would have to be estimated directly from the (incomplete) link data, which adds another dimension to the problem.

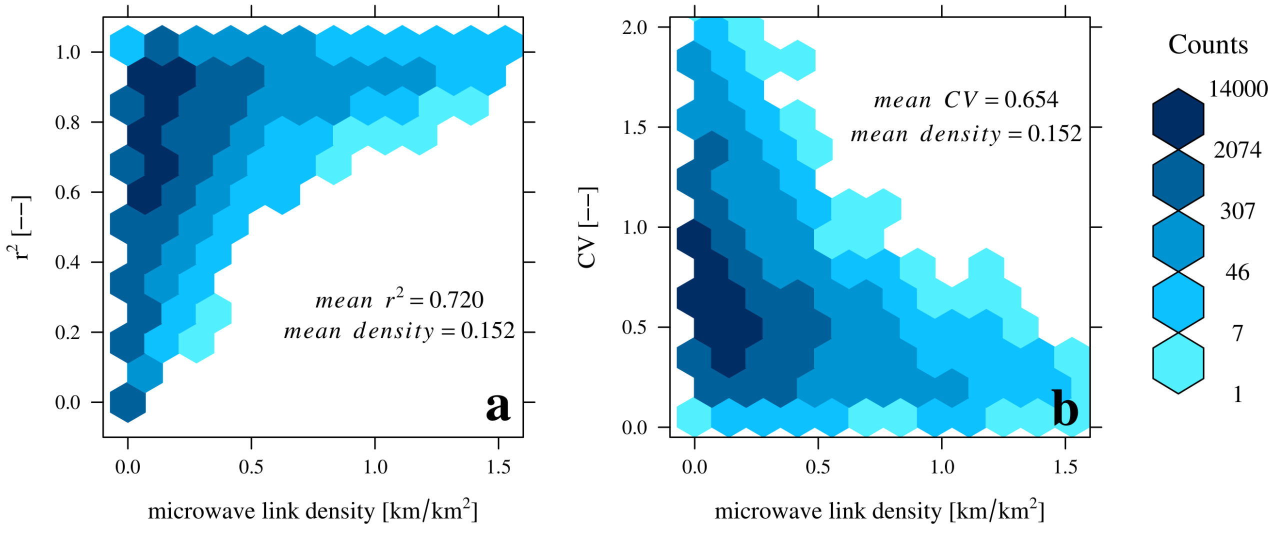
**R/.** In our response to comment # 15, we explain our reasons to use a model semivariogram and not an empirical (fitted) version.

We maintain that measurement errors are the source of uncertainty that contributes the most to the overall error, given our two-category classification and how we used one reference framework (radar info as ground-truth) to estimate the relative error contribution of each category: measurements and mapping. Note that the term “measurement error” that we use throughout the paper includes almost all types of representativity errors (see comment # 3).

The sentence will be rephrased as: ***“We found that measurement errors themselves are the source of error that contributes most to the overall uncertainty in rainfall maps from commercial microwave link networks.”***.

9. More generally, it would be interesting to see how the relative contributions of measurement errors and mapping errors change as a function of the number of links, their density or any other characteristic related to the network's topology. Intuitively, the mapping error is going to increase with decreasing link density. I understand that this is a difficult question to answer. But at least, the authors could discuss it a little bit more.

**R/.** A detailed exploration of the relative contributions of measurement and mapping errors as a function of link density, as the reviewer suggests, was thought as a follow up for this work. The idea was to explore more in detail the regional contribution to the error/uncertainty distribution of areas with higher and lower link densities, i.e., cities and rural areas respectively. This was meant to be done by applying the same methodology but only using subsets of the Dutch link network (hence not the entire network). Nevertheless, as a first exploration of this suggestion in the current work, we created two more scatter density plots in which we present the dependence of the metrics $r^2$ and CV on the microwave link density (see figure below), for every pixel in all 15-min time steps in the 12-day data set for the fullSIM case ($A$ = 1 km$^2$). We selected this data set because it is the only one in which the link network is fully operational among all 15-min time steps. The microwave link density (map) was computed for every pixel as the cumulative length of all link paths contained within a 13x13 pixel square area divided by the corresponding area size.



In the revised version of the paper, the above figures and the paragraph below will be included: **“From the figure above it can be seen that a higher density in the link network guarantees good correlation between the estimated values of rainfall and the ground-truth. From the left panel (a) it can be concluded that lower link densities also contribute (and in large**

**proportion) to higher correlation coefficients. This means that without considering errors in link measurements, these latter being the largest source of uncertainty in country-wide rainfall fields, the network density and the mapping methodology considered here are, respectively, high and good enough to retrieve accurate rainfall fields at such country-wide scales (at least in The Netherlands).”**.

10. Is a relative bias of 15%, a CV of 121% and a coefficient of determination of 0.37 at 15 min acceptable for practical applications in hydrology or not? If not, what could and should be done to overcome these issues and improve the overall accuracy of rainfall maps derived from microwave links?

**R/.** That depends on the catchment characteristics, in particular the catchment’s response time (e.g. Berne et al., 2004 - [*Berne, A., Delrieu, G., Creutin, J.D., Obled, C.: Temporal and spatial resolution of rainfall measurements required for urban hydrology, J. Hydrol., 299, 166-179, doi:10.1016/j.jhydrol.2004.08.002, 2004.*]). A detailed investigation of these issues is therefore beyond the scope of the current work and will be dealt with in future contributions.

For practical hydrological applications (distributed models) it is always better to have an unbiased rainfall input (or close to such situation) than one with no uncertainty but a large bias. This is because a bias will systematically propagate throughout the whole hydrological model. In practical applications, rainfall field inputs will also contain NO-rain values (intermittency), and their metrics would certainly be improved in comparison to the statistics cited by the reviewer, which have been obtained after applying a 0.1 mm threshold (see Table in reply to comment # 6). Although the CV increases (larger uncertainty; see the discussion in our reply to comment #6), the relative bias substantially decreases (from 15% to 2%), leading to a nearly unbiased situation ideal for hydrologic (rainfall-runoff) models.

Exploring exactly how the measurement errors in microwave link rainfall retrievals propagate through hydrological models, is beyond the scope of the current work. How link networks compare to other networks (radar, gauges, satellites) when their rainfall retrievals are used as inputs in hydrological models is ongoing work.

11. Section 3 (Results) is very short. It could easily be merged with Section 4 (Discussion).

**R/.** We decided to keep sections 3 and 4 (Results and Discussion) separate.

12. It would be nice to mention the main result in the abstract as well, and not just in the conclusion.

**R/.** We will add the following sentence at the end of the abstract: “**Errors in microwave link measurements were found to be the source that contributes most to the overall uncertainty**”.

13. p.3292, l.19 ... *that is, the physics involved in the measurements such as wet antenna attenuation, sampling interval of measurements, wet/dry period classification, drop size distribution (DSD), and multi-path propagation.*
The sampling interval and the wet/dry classification are not exactly related to the physics of the problem. It's more a sampling and signal processing issue. Please reformulate. In addition, you could include the dry weather baseline attenuation in the list of uncertainties.

**R/.** The “dry weather baseline attenuation” suggestion will be incorporated, and the sentence rephrased like it was originally stated in the abstract: “... **(1) those associated with the individual microwave link measurements such as wet antenna attenuation, sampling interval of measurements, wet/dry period classification, dry weather baseline attenuation, drop size distribution (DSD), and multi-path propagation;...**”.

Lines 5 to 10 in page 3303 will be also rephrased to be consistent with the change above: “**In general, these errors can be attributed to different sources like wet antenna attenuation, sampling interval of measurements, wet/dry period classification, dry weather baseline attenuation, drop size distribution (DSD), multi-path propagation, interpolation methodology and algorithm, the availability of microwave link measurements, and the variability of rainfall itself across time and space**”.

14. p.3296, ll.8-10. *Simulated rainfall depths are based on radar data; hence, they largely reduce the sampling differences between radar and microwave links measurements.*
This sentence is confusing. Are you referring to the weighted averaging of the radar data with respect to the link path? Or am I missing a crucial point here? Please clarify.

**R/.** The reviewer is not missing any crucial point here; indeed the related sentence refers to the problem in comparing gauge-adjusted radar rainfall measurements (i.e., measurements taken in a volume in the atmosphere at 1500 m altitude adjusted by point measurements on the ground) against microwave rainfall retrievals.

We agree with the reviewer that this sentence can cause confusion. We will therefore remove it (see also our reply to comment #3).

15. p.3296, ll.25-26, *Kriging is ideally suited for interpolation of highly irregular-spaced data points.*
This statement needs to be nuanced a little bit. Kriging is a good (linear) interpolation method that takes into account the spatial structure of the data but also comes with its own limitations. In particular, ordinary kriging assumes second-order stationarity of the process. Thus the mean and variance of the process are assumed to be constant. In reality, however, rainfall often turns out to be spatially heterogeneous and non-stationary. Typically, the stochastic relation linking the rainfall at two separate sites depends not only on the relative distance separating the two sites but also on surrounding topographic features and their location with respect to the flow of weather. By applying ordinary kriging, you assume that there are no trends and heterogeneities in the field. This should be clearly mentioned in the text as it is a strong hypothesis.

**R/.** We indeed assumed the anisotropy and stationarity restrictions that Ordinary Kriging (OK) implies. Still, we, like Overeem et. al. (2013), used the OK approach as a simple and straightforward interpolation technique. For the OK to be applied, we indeed made several assumptions, isotropy and stationarity included. We based these assumptions on the geographical conditions of The Netherlands. Its relative small area and flat topography allows for meteorological events (like rain) to be (statistically) homogeneously distributed across its land surface.

As suggested by the reviewer, the related sentence will be rephrased as follows: “**Kriging is ideally suited for interpolation of highly irregularly-spaced data points. Nevertheless, this method comes with its own limitations, and a number of assumptions should be made for the method to be valid, e.g., isotropy and statistical stationarity. These assumptions are further explained in Sect. 6.**”; and a new paragraph will be included in the Constraints and Recommendation section explaining more in detail the above reasoning behind these assumptions.

This is the new paragraph to be included: "**Apart from its simplicity and the 30-year rainfall dataset on which it is based, we also chose the isotropic spherical semivariogram of van de Beek et al. (2011), because a consistent semivariogram model estimated from link data was not feasible for 15-min rainfall intensities. Isotropic semivariograms assume equal spatial dependence in all possible directions. Rainfall is generally a phenomenon that exhibits anisotropy in time and space (Lepioufle et al., 2012; Velasco-Forero et al., 2012; Guillot and Lebel, 1999; Amani and Lebel, 1997). Nevertheless, it is reasonable to assume isotropy for The Netherlands given its relative small area and flat topography. OK assumes the mean to be constant and unknown within the region of interpolation. When this unknown mean presents substantial changes over short distances, the assumption of statistical stationarity is no longer valid. Universal Kriging, Kriging with External Drift, and Regression Kriging (RK) are more sophisticated interpolation techniques that incorporate trends to account for non-stationarity (e.g. Schuurmans et al., 2007). The performance of these geostatistical techniques to retrieve link rainfall maps was beyond the scope of this research.**"

16. Please reconsider the color scales in Fig 6 and Fig 7. Red is perceived as a bright color and should therefore be associated with large values (and vice-versa for green). Also, a significant fraction of the population has problems differentiating between red and green tones. Blue-red, green-purple or shades of gray are common alternatives.

**R/.** We are aware of the particular (and uncommon) color scale we used in Figs. 6 and 7. The departure point of this work/paper is the previous work by Overeem et. al. (2013) [Overeem, A., Leijnse, H., and Uijlenhoet, R.: Country-wide rainfall maps from cellular communication networks, P. Natl. Acad. Sci. USA, 110, 2741–2745, doi:10.1073/pnas.1217961110, 2013]. The reason we decided to implement this color scale was to bring some continuity to the plots/maps presented in Overeem et. al. (2013). Therefore, readers (especially those familiar with Overeem et. al. (2013)) would be able to visually compare the maps presented in both papers, and easily see (or perceive) the improvement in rainfall maps.

17. In general, it would be nice to have a more consistent use of color scales throughout the paper.

**R/.** The reviewer in his previous comment (# 16) expressed the color-blind issues that affect a significant fraction of the population. He/she also suggests blue-red, green-purple or shades of gray as common alternatives to be used in color scales. We are also aware of such issues. That is why in figures not related to any previously presented by Overeem et. al. (2013), i.e., Figs. 2 and 3, we indeed used the blue-red and green-purple color scales suggested by the reviewer.

18. p.3290, l.25 *These rainfall maps were compared against ...*
Not sure which rainfall maps you are referring to. The 3500 ones mentioned on l.23 or the simulated ones from l.24? Please clarify.

**R/.** "... ~3500 computed rainfall maps..." refers to maps obtained from real and simulated link rainfall depths. In the paper, the following sentence on 1.24 "Simulated link rainfall depths were obtained from radar data." indicates that the simulated link rainfall depths from which the rainfall maps were computed are based (or were obtained) from radar rainfall depths.

The related sentences will be rephrased as follows: "**Simulated link rainfall depths refer to path-**

**averaged rainfall depths obtained from radar data. The ~3500 real and simulated rainfall maps were compared against quality-controlled gauge-adjusted radar rainfall fields (assumed to be the ground truth).**”

19. p.3292, l.2 the reference to Messer et al., 2012 should be put into parentheses.

**R/.** Thanks. When the manuscript was submitted, the related reference was indeed into parentheses. The parentheses were removed during the editing process. This concern is probably related to referencing-rules (or edition standards) established by HESS/Copernicus. We will make sure this is corrected in a revised version of the manuscript.

20. p.3292, l.25 I'm not sure if the term physical errors is appropriate here. Maybe “measurement" or “sampling" would be more appropriate.

**R/.** The word “physical” will be replaced by “measurement” yielding: “**Only the overall effects of measurement and interpolation errors were addressed here, but not all measurement errors separately.**”

21. p.3293, l.10, The parentheses in (2011) are not really necessary.

**R/.** Thanks. The parentheses will be removed. The sentence will read: “**which is spread across the months of June, August and September 2011.**”

22. p.3294, l.17 *(2) there are gaps in the network, <u>without link data</u> at all ...*

**R/.** The sentence will be changed to: “**... (2) there are gaps in the network, because of complete absence of link data or low data availability.**”

23. p.3295, l.10 ... *the performance of the link network <u>assuming that all links provide perfect measurements</u>* ...

**R/.** The sentence will be changed to: “**... (1) to evaluate the performance of the link network assuming that all links provide perfect measurements of path-averaged rainfall at the 15 min interval;...**”

24. p.3300, l.21, *Figure 4a, d and g <u>show</u> the relation between the actual link...*

**R/.** Thanks. The sentence will be changed to: “**Figure 4a, d and g show the relation between the actual link and radar rainfall depths, for the three cases of spatiotemporal aggregation.**”

25. p.3303, ll.1-2 *In other areas, the nugget of the employed variogram has a similar effect of reduction on large errors.*
This sentence is not clear. Please reformulate.

**R/.** The sentence will be changed to: “**In areas with lower link densities the nugget of the**

**employed variogram has a similar reducing effect on large errors.**”

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**ANONYMOUS REVIEWER #2**

While the scientific content is very good, I agree with most points Reviewer #1 raised. The paper can certainly be improved by adding a more detailed description of the sources of error listed in the introduction, and their implications on data quality.
Overall, I suggest that the paper could be published after some additions are made to the methods and discussion sections. It is currently very short and could gain in clarity this way. Also, the main findings of the study that the main source of uncertainty are the link rainfall retrievals themselves is not reported in the abstract.

**R/.** Most of the changes suggested by reviewer # 1 will be implemented. Thus, the suggested changes of reviewer # 2 with regard to reviewer # 1 have already been taken into account (see replies to reviewer #1).

Finally, while this is an interesting alternative to use already existing infrastructures, discussion is needed on the usefulness of these network-based rainfall data for hydrological modelling and flood forecast.
More precisely, while the study focuses on 12 days, can we foresee using link rainfall in an operational way in the future? and what are the key improvements required to reach this stage? This would make an interesting point of the applicability of this method to measures rainfall in places which may have a well-developed cellular network but lack a radar and/or an extensive gauge network.

**R/.** The applicability of rainfall fields from link networks as input in hydrological modeling, has been discussed in our response to comment # 10 from reviewer # 1.
To foresee their operational applicability in the near future is something really difficult to predict. Although it is true that the spatiotemporal resolution of link measurements of rainfall falls within the requirements for hydrological modeling of urban catchments (Berne. et al., 2004), the largest hurdle is to overcome the restrictions by most of the cellular providers with regard to data availability (i.e. their data sharing policies).

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