

## ***Interactive comment on “Climatology of daily rainfall semivariance in The Netherlands” by C. Z. van de Beek et al.***

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The authors would like to thank the reviewer for the comments. Our replies are

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listed below.

*“Year-to-year variations of the fitted spherical variogram parameters have been shown to exist, but they are found to be limited. On average, the simple cosine parameterizations of the variogram sill and range have been shown to perform well.”. Of course variations exists and they are limited, could be it otherwise? Perform well? According to which metric defining “well”?*

These variations are shown in Fig. 9 Comparing the year-to-year standard deviations in Fig. 9 to the means presented in Fig. 7 yields that the maximum year-to-year variation is approximately 50% of the mean.

1)2088: lines 12-13. “The goal of this study is to produce a simple equation to estimate the daily rainfall variogram as a function of the time of year”. Actually, the Authors present an equation for the 90 days moving average and present any analysis regarding the fluctuations of the value registered on one particular day with respect the 90 days average. Moreover they do not care to communicate to the reader if they consider the values of the variogram parameters on one particular day (e.g. April 18th) as represented by 90 days moving average centered on the particular day or else (e.g the day is the left border of the 90 days window?)

In our study we consider climatology and not an individual day in a certain year. Using this climatology it is possible to focus on peculiarities of individual rain events as we did in a case study between March 2004 and March 2005. As for our averaging method we used the center value of this averaging period to find the the corresponding day. This way there is equal weight on both sides of the day of interest. We also tried using a 60 day averaging window and a triangular weighting function instead of equal weights for each day, but both did not have significant impact on the parameters we found for our climatological study. We will add a longer description of the method as

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well as a mention of the 60-day averaging and triangular weight.

2)2090: lines 5-8. *“Assuming stationarity and isotropy of the rainfall field, which is not an unreasonable assumption on the daily scale”. Do the Authors have some convincing evidences supporting this statement? If yes, please supply these evidences. I am not aware of any such evidences. Did the Authors verify (in the limits of the available statistics) that the variogram (x,y) is indeed dependent only on the difference x-y (stationarity) and that it is also isotropic? The lack of such analysis makes me feel that “reasonable” means actually “convenient” since stationarity and isotropy allows the description of the rain field by two parameters (range and sill of the function (h)) instead of 528 variogram values (one for each couple of locations). Moreover, even if one assume stationarity and isotropy for convenience, an analysis of the deviation from this condition should be made in order to assess the “accuracy” of this approximation.*

We are well aware of the anisotropy of rainfall, but:

- The temporal scale of fluctuations of anisotropy of rainfall in the Netherlands is rather small and will largely disappear after averaging over a 90 day period
- we want to reduce the number of parameters to keep the model simple so the parameters are statistically understandable

You are right that it is convenient, but the shape of the Netherlands and the density of the dataset do not allow for a good isotropy analysis. For future work a more dense dataset and/or inclusions of Belgian and German data might offer more insight into the effect of anisotropy. Based on the lack of any significant orography in the Netherlands and the small size combined with the 90-days averaging we judged that it was a safe to assume stationarity. We will discuss these reasons in more detail to make things more clear for the reader.

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3)2093: lines 13-17 *It is impossible to see the 40 days delay with the scale used in Fig. 3. What is the significance of this delay? Is it important for the conclusion of manuscript? I think this part could be dropped. Otherwise add figure and text to explain better this part.*

Agreed, we will drop this as it only adds confusion and is indeed of no importance to the conclusions of the manuscript. We discussed possible causes shortly in our reply to Anonymous Referee 2.

4)2094: lines 15-16 *As the previous point, it is impossible to see with resolution of Fig 4a. An additional figure would be necessary to show the variability of the annual minimum and maximum over the 30 years.*

We would like to point to Figs 7 and 9 where 30-year averages and standard deviations are shown.

5)2095-2096-2097 *The application of the sqrt-sqrt transformation is completely arbitrary (why this and not another one). What is really accomplished? Figure 6 (units of x-axis are missing) does not convey any message to me.*

We went for a pragmatic approach. For example a log-transform would have given a problem with zeros, whereas a box-cox transform would have yielded extra parameters and complexity. As we want to keep the model simple we went with the sqrt-sqrt transform, which did yield good results.

We agree that Fig. 6 does not contribute anything significantly and we will remove it.

6)2097: lines 21-25. *The explanation of the “approximate” 6 months periodicity of Fig. 9A is unsatisfactory. Yes in Fig. 7B the “peak of the climatological fit is slightly later in the year and lower than the actual fit”, and “the minimum in this figure the climatological*

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*fit actually comes earlier". So what? It does not automatically implies the occurrence of a 6 months periodicity.*

You are correct in this and we should simply present these figures as the errors that can occur during year-to-year fluctuations and not mention periodicity. The errors for the sill are considerable and for range fairly small. We will calculate max deviation % and average differences % for sill and range and present them in the text (which effectively means dividing Fig. 9 by Fig. 7). This corresponds to your first comment.

*7)2097: lines 25-end and 2098: lines 1-3. Is this discussion necessary? What accomplishes?*

We agree and we will replace this with the discussion from point 6.

*8)Short range analysis: I think this entire section could be omitted as it is not clear how general the results relative to this case-study (March 2004 – March 2005) are.*

These results are not general. However, the case study is illustrative, and we will therefore leave it in the manuscript. The specifics of this case are influenced by factors that are inherent to the datasets used (i.e. ratio of number of KNMI gauges and UU-WUR gauges, and limited length of the UU-WUR dataset).

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