

Interactive comment on “5 year radar-based rainfall statistics: disturbances analysis and development of a post-correction scheme for the German radar composite” by A. Wagner et al.

Anonymous Referee #1

Received and published: 23 March 2015

I would like to thank the authors for their detailed response to the report.

Irrespective of the effort they took for that response, I have to admit that I am not convinced at all. In their first sentence, the authors state that they *"wish to thank the reviewer for her/his constructive remarks and will try to implement them."* Looking at the detailed responses, though, almost each of them aims at leaving the analysis and the manuscript essentially unchanged!

In the following, I would like to respond to some - not all - of the authors' statements.

The authors start with emphasizing the role and the significance of the error analysis

C646

provided in the first part of the paper. They state that *"it seems to us that the referee exclusively considers the correction scheme, which covers only half of our paper."* This is surely not the case. But the lack of validity of the error analysis inherently affects the validity of the post-composition correction scheme.

The authors continue with justifying the need for such an error analysis and a corresponding correction scheme as *"not all potential users might be aware of pitfalls and shortcomings."* I would like to maintain that there is no need to convince me that such an analysis would be useful. In my original report, I already stated that "[it] might be helpful for users of composite data who do not have access to the original 'raw' data or the capabilities to process these." So I do not doubt the importance to communicate the systematic errors of radar-based rainfall estimates to the potential (and diverse) user community. What I doubt, though, is the validity of the presented approach.

One of my concerns was that the verification at an annual time scale is not helpful for potential users. On pp. C621-622, the authors now argue that *"as a possible customer of the usage of our correction scheme, insurance companies for example are interested in hazard maps, for instance to know regions of enhanced hail risk or severe precipitation."* The authors basically argue that it is important to remove systematic errors from the RX product in order to increase the quality of the RX product, and, subsequently, CONRAD products. Their assumption is that any systematic error they identify at an annual time scale will also apply at short time scales relevant for heavy rainfall. I strongly doubt that because e.g. VPR effects are not persistent in time. If the authors think otherwise, they need to provide evidence. If they think that their correction scheme improves the RX product at a sub-daily time scale, they need to verify this. Everything else is speculation.

The authors state that *"we are aware that the proposed scheme is not a substitute for common correction algorithms on single radar images [...]; but it has clear advantages regarding the correction of systematic limitations."* The authors make a similar statement on p. 1769 II. 12-15 of the manuscript. This way, the authors basically suggest

C647

that local radar data are not suited to identify and correct systematic errors. I think the opposite is true: before compositing on a joint grid, you can and should remove systematic errors from the local radar data.

I would like to clarify that my reference to Peura (2010) was only an example on how quality information could be used for composition. Quality-weighted composition can include any kind of quality information, instantaneous and long-term.

Another major concern (also shared by the second referee) was the use of the PX data instead of DX data. The authors state that "*higher resolution data would certainly be desirable for detailed investigation, but our data base would be very scarce if more than six classes were analyzed.*" I have to admit that I do not understand why the data base should become scarce if DX data were considered. I think DX data are also archived with the DWD - so why not use them? And using the qualitative nature of the PX product in order to justify the need to use PX data is circular reasoning. There are surely other ways to analyse errors from quantitative (DX) data.

The authors also state that "*the PX-product and RX-product are both projected in Cartesian coordinates. So the transfer of detected spokes and pixels affected by clutter from one product to the next is easier.*" I don't think that is a reasonable argument to state that it is "easier". And it surely is more appropriate to detect clutter signals in polar coordinates before gridding the data on a Cartesian grid. And, as referee 2 stated, the authors should be aware that the RX product is not created from the PX product.

Another concern was the analysis period. In their response, the authors state that "*the main reason for the analysis of the time-span 2005 to 2009 was to use a homogeneous data basis with minimal changes in scan strategy, availability of radar systems or maximum detected range. In 2010, the maximum range of single radar sites within the composite was extended from 128 km to 150 km (p. 1790, l. 22). This significantly influences the size and the amount of overlapping areas. Additionally, the radar systems were replaced and partly relocated in the following years with a subsequent*

C648

modification in scan strategy and quality control, creating a break in data homogeneity." This answer only confirms my doubts about how this approach can account for inhomogeneous data. What the authors basically state is that their analysis cannot be transferred beyond the years 2005-2009. This dramatically limits the significance of this study. On p. C627, the authors also state that "*there are several parameters that may cause additional inhomogeneities. The section p. 1790, ll. 14-24 addresses this issue.*" The corresponding section, however, only *mentions* the problem - it does not address it.

The authors state that "*the RX data is widely used and it is the basis for the CONRAD composite data. So it is not meaningful for us to analyse the RY-product.*" I guess there are also users of the RY product. As it is the explicit motivation of the RY product to provide higher quality than the RX product, I do not see reasonable arguments to ignore it (even if the authors use RX for CONRAD).

Regarding my concern of "*inconsistent analysis periods*", the authors respond that "*consistent analysis periods might look nicer, but we see no real problem here. Our approach depends on safe statistics which is directly linked to the amount of measurements used as the longer timespan provides more measurements.*" I am not aware of the term "safe statistics". Surely, though, it is quite casual and not convincing to state that there is "no real problem here" and that consistency is only a matter of "looking nice". The authors need to support this by evidence.

With respect to the criticised "*lack of usefulness*", the authors respond: "*We agree with referee 1 that radar climatology often aims at identifying statistical properties of precipitation at short duration. But here we are interested in the total of precipitation, intense precipitation or hail patterns on a longer temporal scale. We believe that a statistical analysis of mean precipitation patterns is an equal part of a climatology besides the investigation of precipitation at short duration. A verification on a shorter temporal scale is not aimed at nor possible under this approach. The comparison of annual rain amounts with rain gauges is our focus.*" I am sorry to say that this statement is not con-

C649

vincing at all. What do the authors mean by "*total of precipitation, intense precipitation or hail patterns on a longer temporal scale*"? They should not mix up long-term precipitation totals and "intense precipitation" or "hail" events. The concept of climatology of heavy rainfall events implies that you analyse the frequency of heavy rainfall events over a long period. Looking at annual accumulations does not have any significance for what the authors refer to as "intense precipitation". Simply stating that "*a verification on a shorter temporal scale is not aimed at nor possible under this approach*" is not enough. It is not sufficient to use heavy rainfall for legitimising the analysis, but then to deny verifying the added value for the analysis of heavy rainfall events.

So, as stated above: I acknowledge the authors' efforts to come up with a detailed answer. Unfortunately, the responses could not rebut any of my major concerns. So my opinion on the manuscript remains essentially unchanged.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 1765, 2015.

C650