

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: 26 February 2015

Subject

In their manuscript “5 year radar-based rainfall statistics: disturbances analysis and development of a post-correction scheme for the German radar composite”, A. Wagner and colleagues present an analysis of errors that are present in the radar composite product RX and suggest a “post-correction scheme” in order to reduce these errors (please note that in the following, I will prefer to use the term “post-composition correction scheme”).

The study attempts to detect and correct dominant errors, mainly caused by static
C309

ground clutter, partial beam blocking, and by the fact that the radar beam samples at different altitudes (depending on beam elevation and distance from the radar location). Error structures are mainly established by observations from 16 radar sites in polar coordinates (PX product), then transferred to the composite grid, and applied to the composite data (RX product). In addition, the authors address inconsistencies in overlapping areas, and also apply a rain gauge adjustment technique.

Scope

The paper does not contain any hydrological analysis or perspective which would generally be expected for a publication in HESS. However, I am aware that HESS has been publishing manuscripts which exclusively focus on radar-based precipitation estimation, and have only expressed an implicit relevance for hydrological applications. Therefore, I think the paper fits fairly well into the scope of HESS.

Significance

As for the scientific significance of the manuscript, I have mixed feelings. It is, in fact, hard to address errors and artifacts after variables from a radar network (reflectivity or precipitation) have been composited on a joint Cartesian grid. Data from a single radar station in polar coordinates allows for the application of more efficient and more targeted correction procedures. *From a practical perspective*, it would thus make sense to provide a workflow that can be used to reduce errors after the actual composition took place. This might be helpful for users of composite data who do not have access to the original “raw” data or the capabilities to process these. However, we need to admit that such a post-composition correction will never be more than a kludge. *From a research perspective*, it is kind of a dead end: you know that you could do better in terms of precipitation estimation and quality control if you had access to the “raw” radar data in polar coordinates. So, the problem is not a scientific one, but rather one of data accessibility and the capabilities of radar data processing.

Hence, I would like to maintain that the potential scientific significance of “post-composition correction schemes” is limited. Nonetheless, I acknowledge that there might be users, in research or applications, who could benefit from an efficient and straightforward post-composition correction procedure. Unfortunately, I have a couple of serious concerns that are specific to the procedure suggested by Wagner and colleagues in their present manuscript. I would like to point out the major ones, though not exhaustively, in the following section.

Major methodological concerns

The “hybrid” nature of approach: I already pointed out that it would be good to have an efficient (straightforward) procedure to enhance the quality of precipitation estimates on a composite grid. This is because local radar data are sometimes unavailable (with *local* I refer to observations from one radar station in native polar coordinates). However, the authors mainly use local data in order to investigate the error structure of observed reflectivity. Then they transfer this error structure to the composite grid. But if local data was available, other correction procedures could and *should* be applied. Then, the quality of the local radar observations can be used e.g. as a weighting criterion for composition (see e.g. Peura, 2010, but there are many more). I am aware that the PX product is only a qualitative product. But why not use the DX product then? I really have to say that the entire procedure appears unnecessarily twisted.

Very specific to the situation in Germany: Juggling with the RX/PX/DX product terms already implies that the entire procedure is highly specific to the data situation in Germany. This makes transferability to other “environments” virtually impossible. I think that this is a problem for an international journal such as HESS.

Is the data up-to-date? Why restrict the analysis to data from 2005-2009? I can understand that such studies and their publication necessarily imply substantial time lags, but six years appears very much. This raises another concern: As I heard, the German Weather Services now routinely performs a “re-analysis” of radar data with

C311

the most up-to-date RADOLAN processing chain. I would expect that this also involves an update of RX data. Have the RX data in this study been produced with the latest, or at least a recent, RADOLAN re-analysis? The evaluation of “outdated” RX data would appear unfair. Speaking of RX: Why did the authors not use the RY product which, as far as I understand, involves a better quality control and is not produced by using the “Push” mechanism. Altogether, I have the strong impression the the data used in this analysis is not up-to-date.

Inconsistent analysis periods: Why is the analysis with the PX data conducted with data from 2000-2006? Why not use the same period as for the RX data analysis? Is this to guarantee independency? Why should it be independent?

Inhomogeneity: The authors do not present a solution to deal with inhomogeneities in the data time series (relocations of radar, changes in processing, changes in scanning strategy, changes in calibration, ...).

Usefulness: The verification (or “evaluation”, as the authors put it) is not sufficient to demonstrate the usefulness of the suggested approach. This is mostly a matter of temporal scale: The authors compare the corrected and uncorrected radar-based precipitation estimates to rain gauge observations in order to compute the RMSE. This is done for *annual* precipitation depths. But is annual precipitation depth really a variable that I would want to quantify by using radar observations? I strongly doubt it. The authors argue with “climatological investigations” (see e.g. conclusions). In my opinion, though, looking at (mean) annual rainfall is not a priority in “radar climatology” *at all*. Radar climatology is a highly topical research field that aims at identifying statistical properties of precipitation mostly at short duration (typically for convective storms), taking into account the spatial dimension (in addition to intensity and duration). This is also what the authors mention as their motivation in the introductory section. I think that demonstrating an error reduction at an annual scale is not sufficient to justify the effort. This points to an overall weakness of the manuscript: it is not clear for which applications the correction procedures would actually be useful. Given that HESS aims

C312

at the field of hydrology, there should at least be an *implication* for which kind of hydrological analyses the product is recommended (as compared to e.g. an interpolated field of annual precipitation depths from rain gauges).

Flawed verification: Apart from the fact the the evaluation could not really demonstrate the added value, I have the impression that the verification approach is methodologically flawed. First, the difference between the so-called "application period" and the "validation period" does not become clear. Second, and more important, it appears that rain gauges that had been used for the rain gauge adjustment were also used for the computation of the RMSE. The authors confirm this assumption in ll. 3 of p. 1787. This approach makes the verification basically pointless, even if the authors did only use the median rainfall over the radar domain in order to derive adjustment factors. Verification of adjustment procedures requires rigorous cross-validation techniques. And the visual comparison of precipitation patterns in Fig. 11 does not proof the validity of the approach. The authors did not even validate the procedure for the interpolation of rain gauge observations (which would also require a cross-validation). Finally, the contribution of the single correction steps should be demonstrated: not only the "end product" should be verified, but the intermediate products along the procesing chain in order to show the adequacy of each step - in particular the correction for altitude and spokes (for clutter, it will be more difficult).

Lack of comprehensibility: A serious problem that extends throughout the paper is that the applied methods are not adequately described. I reckon that this problem *might* be resolved. However, the presence of inadequate and fuzzy documentation is striking, and it is not acceptable. Just a few examples: p. 1773, ll. 12-13; p. 1776, ll. 18 ff.; p. 1782, ll. 21 ff. This shortcoming basically applies to the documentation of all the correction steps in the entire section 4.1. And in a similar way, it also holds for many of the figures (e.g. usage of inadequate color scales in figures 2, 4, 6, and 11; dotted plots in e.g. figures 3, 7, 8 where at least transparency would have been appropriate).

C313

Recommendation

There are *a lot* of other concerns about the methodology and the presentation quality which I did not elaborate on above. I really acknowledge the attempt of the authors to come up with a useful approach for "post-composition correction", as a service to users who do not have access to raw radar obseravations, and I sincerely hope that my above statements did not come across as harsh. However, I see an intrinsic limitation in the scientific significance of such a "post-composition correction". More importantly, though, I have serious concerns regarding the specific analysis and the methodology suggested by the authors, and they could not convincingly demonstrate the usefulness of the approach.

I was really struggling to see whether a major revision could solve the entirety of these shortcomings. But I think it can't. Therefore, I am sorry to say that I do not recommend this manuscript for publication in HESS.

Literature

Peura, M., 2010: The living composite, Proceedings of the Sixth European Conference on Radar in Meteorology and Hydrology (ERAD2010), Vol. 1 (Advances in Radar Applications), pp. 350–354.

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

C314