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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the skill of high frequency precipitation analyses" *by* A. Kann et al.

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Dear Editor and second Referee,

We would like to thank the second referee for his contribution to improve the quality of this manuscript. The referee addresses some major issues that should be elaborated more detailed: The use and quality of radar data, the synoptic description of case studies and conclusions.

RC: Although the scientific work exhibits a sound use of the scientific operation methods, some important aspects are missing in my opinion. This holds especially true for the fact that radar data is used and case studies are performed. The necessary scientific diligence is sometimes missing in the way that a more thorough examination is needed when dealing with radar data. One has to point out the limits of the radar

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measurements and also discuss the findings in the context of critical data. In other words: Some of the following questions should at least be slightly touched: Especially is the assumption of Rayleigh-scattering valid. Why does the radar underestimate the precipitation? Is the used Z/R relation valid or does it change in an excessive way. Where is the radar sites located in comparison to the examined precipitation? How is the resolution degrading, when sampled at a farther distance? Is there some over-shooting of the more distant precipitation? Is there some beam blockage? How is the attenuation evolving?

AC: A detailed description of the radar data processing, the calculation of the "max-CAPPI" product, a discussion about radar-related measurement errors and a visibility map are included in Section 2.1.

RC: The conclusions are also sometimes not clearly deduced. A more thoroughly discussion about how conclusions show up is needed. The relations between the findings and the process of the convective precipitation should be - at least- a bit more addressed. To put in other words: What is going on there (esp. in the case studies), what ideas do you have, what might be the reasons for the results? The description of the synoptic situations is not very clear and sound. There are some major weaknesses and inaccuracies in the text. A more thorough and correct description is strongly advised since the results strongly depend on the special synoptic situation.

AC: The case studies, the synoptic description and the discussion is extended and elaborated much more detailed.

RC: The presentation quality offers some weaknesses. First of all the pursuit of common theme is missing. It is sometimes a patchwork of paragraph, especially in the introduction. The discussion and remarks are also sometimes a little bit short and there are some repetitions.

AC: The introduction is re-designed in order to better follow the common theme.

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4. Specific comments

RC: Page 11606 Abstract: mentioning what statistical methods are used would be appropriate

AC: Besides bias, mean absolute error (MAE) and root mean squared error (RMSE), the skill scores Equitable Thread Score (ETS), True Skill Score (TSS) and Frequency Bias Index (FBI), which are commonly used for validating precipitation, are computed. The abstract is shortly amended.

RC: Page 11606 line 2: maybe one or two more examples, not just hydrological modelling

AC: Two more examples are added, thunderstorm warnings and reference for spatial NWP verification.

RC: Page 11606 line 8: what station network? -> weather station network

AC: Yes, weather station network. Added to the manuscript.

RC: Page 11607 line 2: “It” maybe beginning with a capital letter after the colon

AC: Corrected in the manuscript.

RC: Page 11607 line 5: Insert a paragraph after “2011):” for better readability.

AC: The paragraph is inserted.

RC: Page 11607 line 16-20: “The WegenerNet consists of 151 stations. . . Kann et al. 2011).” This description is repeated 3 times (on page 11611 and 11619). It would be better just placed in the “The WegenerNet” section.

AC: The paragraph describing the WegenerNet is eliminated from the introduction section.

RC: Page 11607 line21-24: In the listing precipitation character is missing, which has probably the most pronounced influence.

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AC: Yes, of course. The precipitation character is added to the listing.

RC: Page 11607 line 24: skill scores of what? Skill scores of the synoptic conditions?
AC: Skill scores of verification. Added to the manuscript.

RC: Page 11606-11608 whole Introduction: The Introduction is somehow hard to read. The composition is somehow fragmentary. The common theme is missing. It seems just a as a listing of different aspects, which are not properly connected. It is not shown how they are related or play a role in the overall QPE and verification process.

AC: The introduction has been re-organized to follow the common theme and slightly amended.

RC: Page 11609 line 11-12: "The focus of the present study is on the rapid-INCA analysis" This is to imprecise. Try a little more clear formulation.

AC: Rewritten to: However, the focus of the present study is on the rapid-INCA analysis procedure, not on nowcasting.

RC: Page 11610 line 8-9: Please strongly !! comment on the max-CAPPI approach. A citation of this unusual approach would be helpful.

AC: The MaxCAPPI (Maximum Constant Altitude Plan Position Indicator) product is provided for each radar station, which is calculated from three-dimensional radar volumes by projecting the maximum value within a vertical column to a two-dimensional image plane. A detailed explanation is added to the manuscript.

RC: Page 11610 line 11-12: "as precipitation estimates of the radar may underlie important systematic errors (amongst others due to topographic effects)". This is an important point in radar QPE, so one should comment on this a bit more or at least specify some more important error sources (like ground clutter contamination, Bright Band, beam broadening, anomalous propagation, mixed precipitation and validity of the used Z/R relation, . . .)

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AC: The data are ground clutter corrected by Doppler processing and mulit-temporal/multi-parameter statistical filters. But no further correction on the beam is done, so that radar data derived products can be influenced by measurements errors, such as bright band, signal attenuation, scan strategy, radar miscalibration, radome wetting, and errors due to non-meteorological echoes. Further details about error sources in radar QPE are discussed in the manuscript.

RC: Page 11610 line 19: ”to a superior precipitation distribution” – superior? This is – in my opinion -judgment which is at this point not valid.

AC: The wording ‘superior’ is replaced by ‘better’.

RC: Page 11610 line 25 – page 11611 line 2: “In areas with low radar quality, the combination algorithm assigns large weights to the station interpolation. The radar derived QPE contributes with small-scale convective cells which were not captured by TAWES stations of ZAMG.” - Specification where the areas are and a comment on this is needed. Why is the radar quality low?

AC: This is described now in detail in the radar section of the manuscript.

RC: Page 11611 line 11: “and measure the main parameters air temperature” the specification main is not relevant. Keep it simple and clear. Just: “and measure air temperature,. . . “ would be in my opinion better.

AC: The word ‘main’ is eliminated from the manuscript.

RC: Page 11611 line 9-10: “which is a region with high weather variability and sensitivity to climate change (Kabas et al., 2011a).” -> Is the sensitivity to climate change relevant for the findings of this paper?

AC: We agree that the sensitivity to climate change is not relevant for this paper. Therefore this part of the sentence is removed from the text.

RC: Page 11611 line 21: “The QCS is run hourly and checks for each of the 151 sta-

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tions the availability and correctness as well as the technical and physical plausibility of the measured data in eight quality-control (QC) layers" – An example of the correctness check method would be nice. Was the wind influence taken into account?

AC: OK, some further description of the QC layers (one table and a paragraph of text) is added. The check of precipitation data is taken as example. Wind influence on precipitation is not taken into account by the QCS since it is not very relevant in this foreland region (Szeberényi 2014). Szeberényi, K.: Analysis of WegenerNet Precipitation Data and Quality Evaluation for Case Studies and Climatologies. Sci. Rep. No. 58, document WCV-SciRep-No58-KSzeberenyi-Mar2014.pdf, Wegener Center Verlag, Graz, Austria, available at: http://www.wegcenter.at/wcv/ (last access: 15 Jan. 2015), 2014.

RC: Page 11611 line 27: Here one can introduce the abbreviation IDW, not on the next page (11612line 22).

AC: Yes, we missed that. Actually we found that IDW should be introduced even earlier (at page 11609, line 25). We put it there now.

RC: Page 11611 line – 27 to page 11612 line 2: Relevant for this paper?

AC: We think that the fact that the gridded data (5 min data) are summed up to various weather and climate data products (page 11611 line 27 to page 11612 line 2) is relevant because these products can provide a quick overview of the precipitation situation, e.g. at the data portal one can take a quick look at the spatial distribution of the hourly and daily precipitation sums of the case studies mentioned in the paper. We therefore prefer to keep this sentence.

Page 11612 line 11: "(precipitation data at 5 min resolution)" should also be specified in 2.3.

AC: This specification is also added to 2.2 (note: 2.3 is the chapter where the specification is already written).

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RC: Page 11613 equation (1): Should be bigger or equal then. What is the unit of h_n? An identification that the equation is a numerical value equation would be valuable.

AC: The equation is corrected, the unit of h_n is millimetre (added in the manuscript).

RC: Page 11613 + 11614 Synoptic situations: These descriptions of the synoptic situations are a little bit too short and are - in my opinion - not correct enough. A more thorough comment on the convective setup (instability for example) and possible trigger mechanism would generally be more adequate and needed. What were the CAPE values? What amount of shear (esp. DLS) was present? Also a figure with the synoptic situation and an information in the text of the maximum amount of precipitation during the each event would be appropriate. Keyword examples: - August 3rd: approaching trough – convergence line – moderate instability – DLS: 15-20 m/s - August 15th: eastward progressing trough, sufficient DLS for organization, possible squall line. - August 19th: Did the cold front cross the country? Sufficient DLS of 10- 15 m/s, region in the vicinity of a LLJ. - September 1st: The atmospheric instability does not directly lead to thunderstorms (a bit unfortunate formulation) compare for example the depiction in "Johns, R. und C. Doswell, 1992: Severe local storms forecasting. Wea. Forecasting, 7, 588–7612". In this case we had high LL moisture, some instability (CAPE) and a large scale lifting mechanism. Thunderstorm initiation near warm front in an environment of strong DLS.

AC: The main objective of the paper has been determined to center around the properties of the rapid INCA model, and the authors therefore considered details on the synoptic situation of less interest. However, we agree that sound information on the weather situation and the convective setup is not only interesting to readers but also important for a comprehensive overall picture of the selected cases and the performance of the method itself. Therefore a panel of images illustrating the synoptic situation is added to the manuscript, and the description of the four events is extended significantly.

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Page 11614 section 3.2: a comment on the data quality (reliability) for each case would be good, since we are dealing with radar data. And more important: The general description is again a little bit sparsely. It could be a bit more detailed, in the way to describe the evolution of the region of precipitation.

AC: The general description as well as comments on the data quality are added to the manuscript.

RC: Page 11614 line 10: rapid-INCA analyses of what? Possibly precipitation sums?

AC: Yes, precipitation sums. Added to the manuscript.

RC: Page 11614 line 12: 2 and 3 mm per 5 min are below the Wussow criterion -> This is a conflict with page 11613 line 5

AC: Yes, true. However, it was decided to keep this case due to its interesting synoptic features although it does not reach the Wussow criterion.

RC: Page 11614 line 13-14: “The precipitation cells on 15 August 2011 are gradually intensify with time to 6mm per 5 min.” I think that the cells are also expanding.

AC: Yes, the sentence is rewritten including also “expanding”.

RC: Page 11614 line 14-15: “On 19 August 2011, a heavy precipitation cell moves slowly across the domain,.” The center of the cell more or less touches the domain. It crosses an edge.

AC: The sentence is rewritten accordingly.

RC: Page 11614 line 15-17: “and on 1 September 2011 extremely high maxima are reached (>10 mm/5 min) before the precipitation cells leave the WegenerNet domain to the south-east” As I see it: The maxima are reached a little bit outside the domain.

AC: The wording ‘maxima’ is replaced by precipitation amounts.

RC: Page 11614 line 23-25: “Generally, both the onset and evolution of rapid-INCA

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precipitation amounts follow the WegenerNet observations." What about September 1st?

AC: This is mainly due to the overestimation of radar QPE caused by uncorrected hail signals (see also comment below).

RC: Page 11615 line 1-2: Comment on why is there a slight overestimation of radar derived QPE. Possible error sources?

AC: On 01 September 2011 mainly one intense thunderstorm with severe hail at the ground is observed crossing the target region. The rainfall overestimation in this case may be attributed to uncorrected hail signals while the signal attenuation is negligible.

RC: Page 11616 line 4 Maybe insert a paragraph after "local convection" for better readability.

AC: Done.

RC: Page 11616 line 11-12: "Another reason might be the tendency to miss heavy precipitation events with rapid-INCA." This is an interesting finding, which also should be found in the Conclusions.

AC: Done.

RC: Section 3.4: A little bit more comments or a discussion on the findings would be valuable. What are the possible error sources?

AC: Section 3.3 and section 3.4 are combined and a thorough discussion about error sources is added.

RC: Page 11617 line17-19: "In contrast, interpolated rain gauge measurements exhibit a better agreement to observations in the vicinity of the two TAWES stations than elsewhere." Well, this is kind of trivial. Maybe point this out.

AC: This is pointed out by refining the wording.

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RC: Page 11617 line27: In this context it would be nice to know the location of the radar stations. Is there any beam blocking? Is there a slightly better performance of radar QPE in the Northwest and Southeast? AC: With the closest radars are as far as 100 km and 125 km, the large scan volumes in the areas of interest reduce the spatial variability which can be resolved in the radar measurement. The minimum visible height of 2000 m above ground adds further estimation errors for ground precipitation. But local differences cannot be attributed to local beam shielding effects (a Figure 2b is added). The manuscript is complemented accordingly. RC: Page 11618 line 4: "Clearly," really? Other color coding in the figure would make it more clear.

AC: "Clearly" is replaced by 'obviously'.

RC: Page 11618 Line 16: Just an annotation: Well the TAWES station data contains just two stations.

AC: It' true that only two TAWES stations are located in the WegenerNet domain.

RC: Page 11618 Line 20-22: Larger errors for samples including light precipitation might be a radar issue (with the radar having more problems to sample light precipitation).

AC: The coarse resolution of the radar data and the reduced visibility in the target region are likely to be reasons for the underestimation of light precipitation in rapid-INCA analyses. The manuscript is complemented accordingly.

RC: Page 11618 Line 25-28: "During heavy precipitation events, the interpolated rain gauge measurements usually overestimate the spatial precipitation amount and yield better scores than the radar derived QPE which usually underestimates the precipitation field" There would be a comment on the variability of the ZR relation useful.

AC: As long as no hail effects are involved in the measurement, it is likely that convective rainfall intensities are underestimated in the radar QPE due to the fixed Marshall-Palmer relation, which is used to convert radar reflectivities to rainfall intensities. It has

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been concluded in several studies, that different rain types would need different Z-R relationships (Austin, 1987, Atlas, 1999, Steiner et al. 2003). The Marshall-Palmer relation yields good results in stratiform rain, but can fail in convective rain (Foote, 1965, and following from the findings in Austin, 1987, Steiner et al. 2004). The manuscript is complemented accordingly.

RC: Page 11619 Line 6: "convective season in 2011 (1 April 2011–30 September 2011)" this information should better be communicated earlier in the paper.

AC: This information is included in chapter 4 (long term verification).

RC: Page 11619 Line 9-11: This information about the WegenerNet is written for the third time in this paper. Which is a bit too often (See comment Page 11607 line 16-20).

AC: We agree, this information about the WegenerNet has been eliminated from the conclusion.

RC: Page 11619 Line 27- page 11620 line 2: Maybe it is a result of a different precipitation character with a different drop-size-distribution and thus the use of an inappropriate Z/R relation.

AC: A general discussion is included.

RC: Page 11619-11620 general comment on the conclusions: Again the conclusions are a little bit short.

AC: The conclusion is extended, also and specifically with respect to the impact of the radar data uncertainty to the precipitation analyses.

Figures:

RC: Figure 1: Include the radar locations.

AC: There is no radar located within the zoomed map. See Figure 2 for WegenerNet position relative to closest radars.

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RC: Figure 2: Where is the WegnerNet in this Figure. Are the TAWES stations measuring in 5 min or 1 min intervals. In section 2.1 it say that the 1 min measurements are aggregated to 5 min sums.

AC: The WegenerNet is included in this figure. The TAWES stations are measuring in 1 min intervals, but data are transferred in 5 minute frequency.

RC: Figure3: Where is the WegenerNet and where are the radar stations. Are the points the TAWES stations? A graphic with the Difference between the rapid-INCA precipitation analysis and the radar derived QPE would make it more clear.

AC: This figure is redesigned.

RC: Figure 4: The interesting part is very small. One has got a lot of problems to identify the WegenerNet region. Again, an indication of the radar location would be helpful. Maybe an indication of the dates within the graphics would help too.

AC: Figure 4 contains now a zoom to the interesting area. Dates are within the graphics.

RC: Figure 6: Unit of the precipitation rate is missing. Maybe a colored graph would be better – related to the readability.

AC: Both suggestions are included.

RC: Figure 8: What parameter is shown. Indicate that those are precipitation measurements.

AC: The caption of this figure explains that interpolated TAWES station measurements (mm/15min) are shown. In the context of this paper, TAWES station measurements are always related to precipitation measurements (and not any additional parameter that might be measured at stations).

RC: Figure 11 Include information about the threshold used for this figure.

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AC: Information is included (threshold=0.5mm/5min).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 11605, 2014.