

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

Anonymous Referee #2

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Dear Authors, dear Editor, I have reviewed the aforementioned work. My conclusions and comments are as follows:

1. The scope of the paper is to conduct an evaluation and verification of high resolution and frequently updated precipitation analyses which are based upon the merging of measurements from automatic ground weather stations and radar data into the “rapid-Integrated Nowcasting through Comprehensive Analysis” (rapid-INCA). The Quantitative precipitation Estimation (QPE) by means of this merging is compared to the independent observations of a very dense weather station network (WegenerNet).

The comparison and evaluation is conducted on the one hand by means of four case studies, which are characterized by heavy convective precipitation. On the other hand a long term evaluation is conducted for the convective season of 2011. The results show that there is an overall significant underestimation of the precipitation and that

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there is a high dependence on the station data quality.

2. Overall ranking

My advice: The paper needs a major revision.

3. General evaluation

Scientific significance

The core topic of this paper has a definite significance in the scientific field of Quantitative precipitation analyses and estimates, since it addresses the still open and up to date problem of verifying, evaluating and comparing special multi-sensor precipitation estimates. Furthermore the evaluation of the skill of high frequency precipitation analyses is addressed. This is of value, since so far mostly hourly and daily precipitation sums have been handled.

Scientific quality

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 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 overshooting of the more distant precipitation? Is there some beam blockage? How is the attenuation evolving?

The conclusions are also sometimes not clearly deduced. A more thoroughly discus-

C5709

sion 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.

Also a more pronounced description about the different QPE methods is advised in a way, that a more thorough discussion about the spatial pattern and temporal evolution of the precipitation analyses should be conducted (see specific comments Page 11614 section 3.2)

There is a little drawback about the stations density of the TAWES stations in the Wegener domain. There are just two stations which are more or less the basis for the difference of the rapid-INCA precipitation analyses to the radar QPE. Because of this point, some conclusions seem a little bit trivial. Nevertheless, the WegenerNet data is, on the other hand, very valuable and of high quality. And – as I wrote in the beginning - does this paper show a good quality in applying the statistical verification methods, which is a very good basis for a further improvement of this paper.

Presentation quality

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. Nevertheless is the language clear and of good style.

Conclusion: The authors show a good scientific work and are on a very good way, but a little more effort is needed to make this paper ready for publishing. It is mainly a little bit short. Thus I would advise a major revision.

C5710

4. Specific comments

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

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

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

Page 11607 line 2: "It" maybe beginning with a capital letter after the colon

Page 11607 line 5: Insert a paragraph after "2011):" for better readability.

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.

Page 11607 line 21-24: In the listing precipitation character is missing, which has probably the most pronounced influence.

Page 11607 line 24: skill scores of what? Skill scores of the synoptic conditions? Please specify.

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 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.

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.

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

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

C5711

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, ...)

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

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?

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.

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?

Page 11611 line 21: "The QCS is run hourly and checks for each of the 151 stations 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?

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

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

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

C5712

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.

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.

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. Page 11614 line 10: rapid-INCA analyses of what? Possibly precipitation sums? ;)

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

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.

C5713

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.

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.

Page 11614 line 23-25: "Generally, both the onset and evolution of rapid-INCA precipitation amounts follow the WegenerNet observations." What about September 1st?

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

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

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.

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

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.

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?

Page 11618 line 4: "Clearly," really? Other color coding in the figure would make it more clear.

C5714

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

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).

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.

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

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).

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.

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

Figures:

Figure 1: Include the radar locations.

Figure 2: Where is the WegenerNet 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.

Figure3 Where is the WegenerNet and where are the radarstations. 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.

C5715

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.

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

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

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

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