Hydrol. Earth Syst. Sci. Discuss., 7, C4499–C4503, 2011
www.hydrol-earth-syst-sci-discuss.net/7/C4499/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "South African Weather Service operational satellite based precipitation estimation technique: applications and improvements" *by* E. de Coning and E. R. Poolman

Anonymous Referee #1

Received and published: 1 January 2011

Review de Coning and Poolman (SAWS)

This paper describes rainfall estimates and their evaluation for use in flash flood guidance in southern Africa. The rainfall estimates are based on satellite technology and regional atmospheric model outputs.

The paper in general is quite lengthy in its descriptions of products, models and evaluations carried out in other studies but give relatively little attention the part having (to my mind) most scientific value: the performance evaluation of the different products (HE compared to the combination product, HE quasi-convective+Unified model stratiform

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rainfall). The paper therefore reads more like a technical report than a scientific paper. With some (perhaps significant work) this could be improved. I recommend the authors to reduce the first part and make more work of the evaluation, which is to my mind interesting enough for the region to report in this journal. A suggestion, if used for flash flood warning, also the potential reduction in lead time due to the additional processing time of the combination product could be used to evaluate the different products.
My main comment in the performance evaluation of the products is that it is left too much to the subjective opinion of the reader to judge the quality of the rainfall estimates from the spatial figures provided. In the last part of the paper, finally some statistics are reproduced for the respective products but there are quite some limitations on these statistics, being:

- they are derived from a very small sample of days of the full available time series.
- it is unclear why only ten days were used (and why specifically those ten days) for performance assessment.
- Commonly used verification statistics are lacking (e.g. contingency scores such as hits, misses, false alarms). I understand that such estimates are not evidently made at the pixel basis, but they could for instance be made at the provincial scale, or at the scale wherein the rainfall estimates are to be used (E.g. catchment scale). Also, the difference in performance over different time scales wherein the products are to be used for flash flood guidance (hourly, 6 hourly, daily basis) could be shown to improve the section on performance.

Furthermore, the structure of the paper could be enhanced. Section 2, 3 and 4.1 describe available methods and materials, Section 4.2 and 5.1 seem to belong to a 'problem description' section (showing deficiencies in the HE rainfall estimates, and the needs for operational rainfall estimation in flash flood guidance systems) and may

HESSD

7, C4499–C4503, 2011

therefore be combined and may have a more suitable place shortly after the introduction section of this paper. Sections 5.2-5.3 describe the methods and results.
Finally a short comment on the abstract: it is somewhat lengthy. The problem description can be much shorter (extensive rainfall measurements throughout southern Africa are hard to obtain, and it is insufficiently clear how satellite rainfall estimates and regional atmospheric models can add to rainfall estimation at large scales). The abstract is also unclear in what this paper describes. l. 11-13 mentions a description of applications of the HE and the paper mentions as application the South African Flash Flood Guidance, however I have seen no application throughout the paper in a Flash Flood guidance system. Instead examples are shown and potential improvements are demonstrated. Then in l. 16-19, the scientific content seems to be described, a new precipitation product, base don HE and UM together. I'm not sure if the goal of the paper is to present a novel method to rainfall estimation and to explain its enhancements compared to earlier products, but if so I suggest to reword this to put emphasis on the second description of the target of the paper.

Summarizing, I think there is potential in this paper but it will need significant work. I recommend major revision. The descriptions of the products, models and past performed evaluations can be shortened. The evaluation of the products should be significantly improved to reach a scientific level.

Detailed comments:

p. 8838, l. 9 explain what a 'Unified Model' is.

p. 8839 l. 5 the data used for forecasts 12 hrs ahead depends on the hydrological conditions in the area. Perhaps add 'flash flood forecasts at small spatial scales in southern Africa' to clarify this.

p. 8840 l. 3 This seems a suitable place to describe the scope of this paper. I couldn't find a clear description of scope in the introduction.

HESSD

7, C4499–C4503, 2011

l. 6 Sect. 3 → Sect. 4

p. 8845 l. 13 “Rainfall totals over 1 to 6 hours should be most reliable while 24h totals might be too high.” This sounds somewhat strange: if a 24-hr average is too high, the 1 to 6 hour estimates should have similar biases. Second, on longer time scales, noise averages out (for example, if independent in space and time and normally distributed, noise averages out by a factor $\sqrt{n}$, n being the amount of samples being averaged). So please explain this a bit further if it is really true.

p. 8846 l. 2-3. I do not understand point (b). Is some form of smoothing applied to generate the products?

l. 26-27 “. . .only by southern African . . .” a word is missing, countries? Weather Centres?

Section 4.2 and later sections: some provinces are mentioned. Perhaps add a map showing the provinces of South Africa?

p. 8850 l. 14-15 Here the bias correction for HE is described. However, as mentioned by the authors, HE reproduces mostly convective events. If only HE is to be used, this bias correction seems to be appropriate, however, if combined with the UM stratified precipitation, a bias correction should be made comparing HE with convective precipitation only. How is this taken care of?

p. 8851 l. 5-6. “..attempting to identify those periods in which the rain rate approximates that expected from stratiform rainfall”. This is true, but this should also apply for the bias correction of HE but then rainfall, expected to have its origin in convective events (see point above). How is this taken care of?

l. 21 how was the 150 mm threshold selected? Please explain this.

l. 24-l.27. “If the maximum . . . stratiform rainfall field”. Why should only the maximum of the two be used? Should they not be weighted averaged??

HESSD

7, C4499–C4503, 2011

p. 8852. l. 1. Why only 10 days? Why not the full hindcast period and provide rigorous statistics on the full January 2008-December 2009 period (e.g. for rainfall averaged per province)? Why specifically these 10 days?

section 6. I would put more focus on the product performance evaluation.

All figures with sub-figures: the subplot letters a, b, c and d are lacking in all figures with subplots.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8837, 2010.