

Interactive comment on “Extreme weather events in the Sneeuberg, Karoo, South Africa: a case study of the floods of 9 and 12 February 2011” by R. C. Fox and K. M. Rowntree

Anonymous Referee #1

Received and published: 7 September 2013

General comments

The paper examines rainfall characteristics prior to, and during, two flood events in the Sneeuberg of South Africa, on 9 and 12 February. The main analytical aspect of the work is assessing the difference in rainfall characteristics between several datasets, firstly a relatively dense network of rain gauge data, and secondly several satellite-derived estimates. The paper finds that the satellite-based estimates vastly underestimated the intensity of the rainfall events leading to the 2011 floods, as well as their spatial pattern, and warns against indiscriminate use of such data for local studies relating to flood management. This is a valuable lesson, since indeed many studies

C4710

are found in the literature that take such data without a thorough examination of their applicability. In that sense, the paper provides a valuable contribution.

However, overall, in its current form, I would not recommend publication in HESS, for the reasons outlined in this review. My main points of concern relate to four aspects. Firstly, I find the structure and setup of the paper difficult to follow, which means that the focus is often not clear. Secondly, the paper does not provide enough technical detail on the methods and data used. Thirdly, I miss a discussion of the main findings in the context of other scientific work carried out in this field, including how this work extends scientific knowledge in a general sense. Fourthly, I miss a critical discussion of the methods used in this paper, their limitations, and recommendations for further research based on these. Each of these is discussed more specifically in the following specific review comments.

I do believe, however, that the content of the paper could in principle be considered for publication in HESS, if major revisions are undertaken to address these main points, and the specific points below. One suggestion that I have been thinking about, is whether the authors have considered including an examination of how the differences in the climate data lead to differences in hydrological response? This could be examined by forcing some hydrological model (perhaps just a simple rainfall-runoff model) to examine how the simulated peak-flows are affected by the different input datasets. This would be valuable, since the paper does purport to be looking at the significance of these findings for water managers.

Specific comments

1. Overall structure of paper. I find it difficult to follow the overall flow of the paper. The introduction is very long, and it is not revealed until the third page what the paper is actually about. Instead, the paper starts with a one page description of the flood events of 9 and 12 Feb 2011. This is followed by half a page stating that past studies have shown that rainfall intensities have increased in the region in the last 50 years

C4711

(with no clear link to the first paragraph). Then, there follows have a page describing some studies on sedimentology in the basin, the relevance of which is not clear to me. Following this, there is a section stating that the area contains many rain gauge stations. Only after that do we learn that the aim of the paper is to examine how this gauge information differs from satellite data, with specific reference to the floods of 2011. After this, there follows a paragraph listing some other papers that have looked a gauge data versus satellite data, although this does not contain any details of what they found.

I would suggest to let the reader know much earlier what the overall theme and aim of the paper is. For example, a possible setup could be to start with one paragraph stating that serious flooding occurred in 2011 due to high intensity rainfall, and that the intensity of rainfall has increased over the last 50 years (as shown by studies xxx, xxx, xxx). I would then suggest that the details of the floods be moved to section 2, which would be better called "Study area and the floods of 2011". This would allow the authors to be more focused in the introduction. Following this introductory paragraph, the authors could then immediately state that it is therefore important to improve the understanding of what causes these floods, and that one important aspect for that is to have a good understanding of rainfall processes. This then leads to the aim of the paper, i.e. to examine the differences of the different rainfall datasets for this. Section 2, in its current form, is a combination of study area and local rainfall data. However, the description of the local rainfall data would, in my mind, fit better in a "Methods and data section" (which is currently missing).

Section 3 could then be the "Methods and Data" section. It would be useful if this described all methods and data, which are currently fragmented over sections 2, 3, and 4. For example, it could be structured as: 3a) local rain gauge data; (b) satellite-derived data; and 3c) statistical analyses (i.e. current section 4).

The current conclusion section in fact reads to me like a discussion, and I believe it would be better to call it this, and provide only a short "Conclusion" that gives the main

C4712

findings only. Or, at least, call this section Discussion and Conclusions.

2. Related to above point on introduction, I feel that the main aim/objective/research question of this paper is not made clear. It would be useful to formulate this very clearly, e.g. "The main aim of this paper is.....". Bullet-points or similar may help to make it very clear for the reader what the main aim is.

3. In the abstract, the authors state that "These analyses add to our understanding derived from existing rain gauge information". However, in the paper I miss a discussion of how these findings add to that understanding. Indeed, what is the current understanding? A review of this should be included (see also later points).

4. Also in the abstract, the authors state that intense rainfall events have become more frequent in the regions in the past 50 years, and in the manuscript they provide evidence from literature for this. However, in the abstract they also state that "... it is likely that these trends will accelerate". However, In the manuscript I found no mention of this, and so it is not clear to me on what evidence this statement is based. This should either be left out or clarified in the manuscript with reference to the relevant literature.

5. It would be beneficial to add a table summarising the main parameters of the local gauge stations data (e.g. name, attitude, years of data, number of missing days, measuring technique etc.).

6. On p. 10815, the authors provide regression equations of cumulative rainfall between stations. Is this based on daily or yearly data? Please clarify.

7. More importantly, whilst the authors show the strong linear regressions, they report that this was carried out to identify possible break-points. However, in this case a more specific change-point detection analysis should be performed (of which there are many possible methods in the literature), rather than only an ordinary least squares regression.

C4713

8. In the section on the Giovanni data, please also tabulate the main important points about the data, and include links to sources (if possible).
9. Current section on data treatment. I find this section to be much too lacking in technical details. It would not be possible to reproduce the results based on the information provided. For example in each subsection, the authors state that the datasets "...were analysed..." but give no detail of what this analysis entailed. I feel that this section would need substantial improvement, c.q. clarification, to be considered for publication in HESS.
10. The results are sometimes described a little vaguely, which can lead to misunderstanding of them. For example, on page 10818 the authors state that "The high rainfall on 12 February was concentrated over Genora and Quaggasvlei". However, whilst reference to figure 5 indeed shows that these stations had the highest rainfall on that day, the rainfall at others stations was also relatively high.
11. At several places, the authors refer to the "regional average" based on the gauge-data (e.g. p. 10819, line4; figure 5; and several other places: please clarify what this means. Is it the average of the stations used? Or is it the average of the entire region, using spatially interpolated data based on the rain-gauge data?
12. On page 10819, line 14-16, the authors state that whilst they show recorded hourly data for just one stations, there is "...no reasons to think that the storm profiles would be very different at other points in the catchment". On what do the authors base this? In fact, storms can be extremely localised, and so without any explanation, I find this comment unsubstantiated. In fact, it also seems to contradict what the authors state themselves later in the paper, for example on page 10820, lines 16-17 "This is quite possibly due to the very localized pattern of heavy rainfall on 12 Feb...".
13. In the context of the findings shown in Fig 6, my last comment is not trivial. Here, the authors show that the hourly satellite-derived data show much lower peaks than the gauge-derived data during storm events. However, given the localised nature of

C4714

storm events, this may well be expected of the satellite-data, which are not point data, but area average. This should be critically discussed. Moreover, given this spatial discrepancy, I am not surprised to see that the satellite data of the storm events show longer durations and lower peaks than the (point) rain-gauge data (Figs 6a v 6b) (although the magnitude of the difference in peaks is indeed interesting and raises many questions!). Given that the gauge data show higher peaks but shorter duration rainfall than the satellite data, it would be interesting to look at differences in the cumulative rainfalls over each entire storm event.

14. Related to last comment, it would be easier for comparison if figs 6a and 6b were plot on 1 figure (a split y-axis could be used to account for the difference in absolute values).

15. Section 5.3: again, unclear if the regional averages are simply averages of the gauge-stations, or an aerial average after some kind of interpolation. Please clarify. Moreover, the authors compare the mean and variance of annual precipitation amounts between datasets. Please provide also data on the statistical difference (or otherwise) between these data. Again, a table would be useful here with the main statistical characteristics of each dataset.

16. As mentioned earlier, I feel that there needs to be a discussion section where the findings are placed in the context of past research, and explaining how the current research expands this knowledge. For example, in the last paragraph the authors do list several other papers looking at differences between gauge data and satellite data. A discussion of these studies would be more suited in this discussion. Also, it should be discussed how they are similar to, or differ from, the current research. Although I am not an expert on this literature field, a quick literature search returned many papers that could be relevant (e.g. Looper & Vieux, 2012; Grimes et al., 1999; Grimes et al., 2003; Roca et al., 2010, amongst many others).

17. As stated earlier, please include a critical reflection of the limitations of the work in

C4715

the discussion.

18. Finally, please provide more information on the figure captions so that these are understandable without having to refer to the main text.

Minor technical points 1. P.10811, line 17: add 'of' between 'The impact....these floods..."

References

Grimes et al., 1999. *Journal of Hydrology*, 222, 93-108.

Grimes et al., 2003. *Journal of Hydrometeorology*, doi:10.1175/1525-7541(2003)004<1119:ANNATR>2.0.CO;2.

Looper, J.P., Vieux, B.E. 2012. *Journal of Hydrology*, doi:10.1016/j.jhydrol.2011.05.046.

Roca et al., 2010. *Journal of Applied Meteorology and Climatology*, doi:10.1175/2009JAMC2318.1.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 10809, 2013.