Hydrol. Earth Syst. Sci. Discuss., 6, C3316–C3319, 2010
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Future extreme precipitation assessment in Western Norway – using a linear model approach" *by* G. N. Caroletti and I. Barstad

Anonymous Referee #1

Received and published: 10 February 2010

Review

I found this paper very interesting and potentially very useful for evaluating GCMs on a local scale, especially in areas where orographic precipitation is the dominant process for heavy precipitation events. The methods applied are valid and worthy of publication. However, the results need to be better presented. The papers suffer from a poor structure which makes it hard to follow. There are also too many figures and tables (some showing the exact same information). My recommendation is that the paper is accepted for publication after a major revision.

Language: The paper needs to be looked over in terms of language. It is not very clearly written, and I would advise that a native English speaker went over the text. This is especially the case for the Introduction.

Structure: The paper suffers from structural problems, and this needs to be straightened out to make it clearer. I do not like that Section 5.2, which is clearly a technical section comes after some of the major results. It would make more sense to have this as subsection 2.2.

Tables and figures. There are too many tables and figures, and I would suggest romeving some of the figures and merging other which are similar. Some figures and tables show exactly the same thing, and in those cases you should have either a table or a figure, not both. Also, the quality of the figures can definitely be improved. There should be no titels to the figures.

Discussion. I would have liked some discussion about the added value of the downscaling in comparison with the GCM precipitation. What are the possible drawbacks with the technique? Ho can this results be used in impact studies for example.

Specific comments

P7540, Line 4. The Authors mention the IPCC 2003 report, and also reference this in the abstract, but I am not aware of any 2003 report. Which one do you mean?

P7540, Line 4. Cubash and Meehl, 2001 is missing in the reference list.

P7541, Line 1. Exactly what about precipitation is a “great difficulty” for scientists. I think that you need to be more specific.

P7541, Line 3. Here you mention the IPCC 2007 report, but without reference. Also, there is no mention on what he report states about precipitation, which I thought would be the interesting bit?

P7542, Line 15, “run over a limited domain”, I would suggest changing this to “run over

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a limited spatial domain".

P7545, Line 27. Here you mention how you define extreme OP events, but it seems to me that the 85% is the limit for an OP event, and not an EXTREME event, is that right?

P7546, Line 9. You here mention 99th percentile as an extreme event, but I would strongly argue that it cannot be seen as an extreme event. The right term would be "a large event". Extreme events are more seldom occurring than the 99th percentile. This comment applies to the whole paper. I was also sometimes confused if you were talking about OP intensity or precipitation intensity , it would perhaps help the reader to be very clear what is discussed at all times.

P7546, line 3. Here you mention OP events, without the term extreme, which are events where the RH is above 85%, is that correctly understood? (see earlier comment on this).

P7547. Lines 21-25. This section is not clear to me, please rephrase this.

P7547. Line 25. Table 5 and Figure 7 show the same information, one should be excluded.

P7548. Line 14-24. I do not really understand the argument behind applying two different variants of the method. What is gained by using a larger spatial domain? The results are somewhat fudged, but what are the benefits? Can you show that the results are more robust?

P7548. Line 17-18. I do not understand what you mean with this sentence. What does "bad precision" mean in this context?

P7549-P7550. This whole section 5.2 should be moved to section 2, since it is a method development.

P7549, Line 5. Equations 5 and 6 are almost identical, and I would suggest to use only eq 6.

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P7550, Line 3, “rewrite Eq. (6) using Eq (7)” should be “rewrite Eq. (7) using Eq (8)”

P7552, Line 17-20. Here the authors talk about possible applications, but they are not being specific what insights are possible. Is the method thought of as a analysis tool for GCM or to improve precipitation forecasts.

P7560, Table 5, This information is in Figure 7. Keep one of them, not both

P7564, Table 9. This information is basically shared with Figure 11, keep on of them.

P7565. Table A1. I find this table not very interesting and it can be removed.

P7571-7572. I would suggest combining Figure 3 and 4 to one figure. It is not necessary to show the baseline period, please remove those bars (goes for all figures where this is present).

P7573. I did not understand how the two dotted lines differed, could you please explain it more carefully.

P7574. Fig 6. The left hand side of the figure is too small. The legends and labels cannot be read.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 7539, 2009.