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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

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Reply to Anonymous Referee #1

Language

COMMENT: 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.

REPLY: The paper will be submitted to native speakers for language revision.

Structure

COMMENT: Structure: The paper suffers from structural problems, and this needs to be straight- ened 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.

REPLY: The technical part of section 5.2 has become subsection 2.1. Only the results will be presented in section 5.2, with explicit reference to the method described in 2.1.

Tables and figures

COMMENT: There are too many tables and figures, and I would suggest romev- ing 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 titles to the figures.

REPLY: All titles in figures have been removed. Some figures and tables have been

removed.

Discussion

COMMENT: 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? How can this results be used in impact studies for example.

REPLY: These paragraphs have been added at the end of section 5.3:

"The IPCC 2007 A1B scenario simulations predict for Northern Europe above 55°N that high extremes of precipitation are likely to increase in magnitude and frequency, especially during winter times (Meehl et al., 2007). This is consistent with our findings for western Norway, although the relative increase in frequency does not differ from winter to summer.

It is important to point out that LM-based downscaling is a technique which can be applied to any possible GCMs. The quality of it's results depend directly on the quality of the GCM data that it is dowscaling. LM can be considered fairly sure to improve the results in a significant way on the upwind side of the mountains at mid-high to high-latitudes, where thermal convection is not a significant influence on extreme precipitation. However, LM must not be used for assessment of lee-side stations. The most significant drawback of the technique is the risk of using it uncorrectly. Although the downscaling is quite simple and transparent, there might be need for a considerable effort to study whether the region of interest is suited to LM-downscaling."

Specific comments

COMMENT: 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?

REPLY: Typo, corrected to IPCC 2007.

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

REPLY: Cubash and Meel reference added to reference list

COMMENT P7541, line 1: Exactly what about precipitation is a "great difficulty" for scientists. I think that you need to be more specific.

REPLY: Text changed to: "Precipitation strongly and visibly influences human life, and has always been a primary subject of meteorological studies. Precipitation results from a chain of different physical processes. When a precipitation event takes place, it is often difficult to identify: i) which processes are at work, ii) the temporal and iii) spatial scales at which these processes work and iv) how big part each of them play in the event's formation and evolution."

COMMENT 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?

REPLY: This part has been changed and some information about IPCC AR3 and AR4 findings has been added. The paragraph now reads:

"The need for local assessments of precipitation has grown in recent years due to the increase in precipitation extremes and the widespread awareness about findings of the IPCC 2007 AR4 Report on climate change (Solomon et al., 2007). The Report predicts that, in a future warmer climate, mean precipitation will increase at tropical and at high latitudes; and that precipitation extremes will increase more than the mean in most tropical, mid and high latitude areas (Meehl et al., 2007). Already in the IPCC 2001 AR3 Report, General Circulation Models (GCMs), the most commonly used tool for climate predictions, show an increase in precipitation due to an increase in greenhouse gases (Cubash and Meehl et al., 2001). It is suggested that changes in extreme precipitation

are easier to detect and attribute to global warming than changes in mean annual precipitation (Groisman et al., 2005). "

COMMENT P7542, line 15, "run over a limited domain", I would suggest changing this to "run over a limited spatial domain".

REPLY: Changed as per reviewer's suggestion.

COMMENT P7545, line 15. 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? AND COMMENT 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).

REPLY: The text has been changed to clarify the two steps in the study, namely: we select only OP events (they happen only with RH greater than 85%) and then among the OP events we find the large/extreme ones (the 99th percentile of OP events).

COMMENT 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.

REPLY: We have performed Pareto distribution tests on four model control simulations to determine the thresholds for extreme OP. The thresholds depended on the model run we used, and their values were located in the interval between 99%-ile and 99.9%-ile of OP. The results suggested that the choice of 99%-ile was in some cases too low while 99.9%-ile was always too high. Therefore we have now chosen 99.5%, and all the results in the paper have been changed accordingly. See Coles, Ch.4.3.1 "Modeling Threshold Excesses" on threshold selection, in particular page 80: "Above a level

u_0 at which the asymptotic motivation for the generalized Pareto distribution is valid, estimates of the shape parameter ξ should be approximately constant, while estimates of σ_u should be linear in u, due to (4.9)". (4.9) is shown in page 79, same section.

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

REPLY: This section now reads: "The largest relative increase in extreme OP intensity happens on the coast. The coastal stations, however, exhibit the weakest absolute values. See for example Table 4, which refers to the Flora - Gloppen cross-section; and Table 5, which groups all 74 stations in coastal, fjord, inland and mountain stations according to their proximity to the coast and elevation."

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

REPLY: Figure 7 has been removed.

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

REPLY: The use of two different applications might give more reliable results, but we have not actually performed an analysis on climatological data that can confirm this. Thus, we have removed this second method altogether from the paper. The paper is meant to present a new methodology and possible applications in a simple way. There is no need to confuse the reader with something that is unnecessary to the goal of the paper and which gives too similar results in this application.

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

REPLY: Part of section 5.2 has been moved to section 2, as mentioned above.

COMMENT P7549, line 5. Discussion Paper Equations 5 and 6 are almost identical, and I would suggest to use only eq 6.

REPLY: Only equation 6 has been kept.

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

REPLY: Corrected.

COMMENT 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.

REPLY: Part of this has been addressed at the end of section 5.3, see above in the reply to the comment to Discussion. In section 6, we have substituted lines 19-20 with:

“Downscaling climate scenarios with LM seems to open up interesting possibilities for insight. In climatology, for instance, it can be used as a tool to evaluate GCM performance in some areas:

i) the use of standard deviation of relative values instead of absolute ones makes it easier to compare models and point out those underperforming;

ii) the transparent nature of LM makes it easy to see in which respect the models diverge and what impact that has on the downscaled results.

In weather forecasting, LM downscaling can be useful for present and future studies. Some examples:

i) for the present by providing indications to the role of some variables to influx that might have been overlooked;

ii) for the future, since LM downscaling points out how the relationship of these variables to precipitation might change, by indicating which variables have to be monitored

in case need should arise to change the parameterization and settings of the forecasting models.”

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

REPLY: Table 5 has been kept, Figure 7 has been deleted.

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

REPLY: The reviewer has advised to cancel Table 9 or Figure 11 since they share the same information. Table 9 was deemed important because it includes the standard deviations of the different components. This information will be preserved by including it in Figure 11.

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

REPLY: Table A1 has been removed as suggested by reviewer.

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

REPLY: Figures 3 and 4 have been combined.

COMMENT P7573, Figure 5. I did not understand how the two dotted lines differed, could you please explain it more carefully.

REPLY: The text now reads: “The upper dotted line shows the mean of the thirty 99.9%-iles of yearly OP intensities for the 1971-2000 time slice. The lower dashed line shows the 99.9%-ile of the whole 1971-2000 time slice.”

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

REPLY: Figure 6 has been significantly changed to allow for better reading.