Hydrol. Earth Syst. Sci. Discuss., 12, C5898–C5902, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C5898/2016/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Climate change increases the probability of heavy rains like those of storm Desmond in the UK – an event attribution study in near-real time" *by* van Oldenborgh et al.

Anonymous Referee #3

Received and published: 4 January 2016

The manuscript presents an analysis of the heavy rainfall event in Scotland and Northern England associated with the storm Desmond in December 2015. The aim of the study is to find whether the probability of occurrence of these type of events may have increased as a result of anthropogenic climate forcing. The authors apply three methods: one based on analysis of observations, trying to find a trend in the parameters of the Generalised Value Distribution; the second method is based on global simulations with the global model Ec-Earth over the last 120 years, applying the same statistical framework; the third is based on the analysis of a large ensemble of simulations with a regional climate model driven by either all forcings or only natural forcings. All three methods point to a role of anthropogenic forcing in the probability of occurrence of

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these type of events.

In my opinion, the manuscript is worth publishing in HESSD, but in general I often found the writing inaccurate and not well structured, so that the manuscript would definitely benefit from an editorial revision. I have indicated some passages with which I had to wrestle, but in general I would recommend going through the whole manuscript, keeping the readability of the whole in mind.

1. One preliminary question is the motivation of the study. The authors highlight that one of the novelties of the study is the attribution of an extreme event in almost real time. However, what is the scientific value of this type of attribution ? Why not wait until all station and other meteorological data are available ? If the benefit is the 'journalistic' value, I think this argument is weak for a publication in a scientific journal. I am not suggesting that the study should not be published here, but the motivation should be made stronger.

2. A recent study has found that global models, and probably regional models as well, may clearly overestimate the change in precipitation due to increased radiative forcing (Fildier abd Collins, 2015 , GRL, doi:10.1002/2015GL065931). This is related to a deficient representation of atmospheric absorption in most models. Of course, this result may be preliminary , but I think it should be discussed here, as it directly impacts the conclusions of this study.

3. (Otto et al., 2016). is a submitted manuscript. I am unsure about what the citation rules in HESSD are in this regard.

4. 'the return time, f1 /f0 = τ0 /τ1 . However, these are calculated to answer subtly different questions: how much the probability changed due to the observed trend for the ob- servations, due to all forcings in the coupled model and due to anthropogenic forcings in the large ensemble.

This paragraph becomes clear later, when the authors get into the detains of the three

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methods. At this state in the manuscript it is rather confusing. Which 'trend' is meant ?

5. 'Low-frequency variations also play a minor role here. The largest uncertainties arise from the random weather, which affects all three methods equally.#

Why do low-frequency (I assume internal variability) plays a minor role ? I would actually tend to think that the opposite is more correct: the influence of internal variability for extremes would be larger than for the mean climate. At any rate, this assertion needs some justification.

6 ' A preliminary indication was −1obtained from the ECMWF analysis, which gives about 28 mm day for Northwest −1England and 31 mm day for South Scotland. '

What is the approximate area (in squared km) to which this numbers refer ?

7. These block maxima were fitted to a Generalised Ex- treme Value function (GEV) scaled with the low-pass filtered global mean temperature'

which block maxima ? This is the first time that this expression appears in the manuscript ? The reader would appreciate being more specific.

Also, the expression 'scaled with the global mean temperature' is very vague. I could not find any technical description in the manuscript about how this (linear?) scaling is done, and this is an important point in the analysis. Is the mean of the GEV re-scaled ? are all parameters fitted with a model including the mean global temperature as a co-variate, as for instance in Kharin and Zwiers, 2005, doi: 10.1175/JCLI3320.1) ?

8. Figure 2 needs a better explanation. At first sight, the quantiles lower than 50% cannot be identified. Also, the caption should indicate how large the area is, as the probability distribution of daily rainfall very strongly depends on the size of the area. Where is the horizontal line ? I cannot see any ('The results are shown in Fig. 2 for the two regions. The horizontal line denotes the preliminary indication for precipitation in these areas.')

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9. The caption of figure 3 is also very confusing. does sigma denote the uncertainty in the estimation of mu or the standard deviation of the GEV ? How was mu estimated, by maximum likelihood or by the method of moments ? What is a Gumbel plot (I see just return values) ? What is the climate of 2015 (or of 1931) ? I guess that the blue (red) lines show the return values simulated after the estimation the parameters of the (transient ?)GEV distribution , using the GEV_values for 2015 ? If this is true, the reader would appreciate being more specific.

10. The Northwest England region shows no trend in the maximum daily precipitation over October–February, with a 95 % uncertainty margin on the change in return times of these extremes of a factor 0.3–2.1 (1 indicates no change).

How are these changes computed ? does it refer to the implied change through the whole period ?

11. 'The resolution is T159, this is about 150 km, too low to re-'

The reader would again appreciate being more specific. Not all readers will be acquainted with climate model jargon

12. 'The differences are mainly in the response to the aerosol and greenhouse gas forcings of the climate model used, which may differ somewhat from the real world. Very low frequency natural variability could also cause the results to diverge.'

My guess is that the main differences could stem from the climate sensitivity of the model, generally stated, either understood of temperature sensitivity or precipitation sensitivity, rather than from the prescribed forcings.

An important point related to this would be to show the simulated trends of mean precipitation in this area and check whether they agree with Figure 3a , i.e. no trend in mean precipitation)

13. As in the EC-Earth results the return time of an event of the magnitude estimated from the high-resolution ECMWF analysis, without bias corrections, would be very high,

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with a return time of about 1600 years and a 5–95 % confidence interval of 1000 to 2500 years under actual climate conditions. The confidence interval represents the sampling uncertainty after bootstrapping.

So how was the bias corrected ? Or is it explained in the following paragraph ? I found it confusing.

14. 'We checked that the different SST patterns in 2012/2013 and 2013/2014 indeed did not make an appreciable difference. The natural forcings, that were included in the coupled climate model but not here, also have a small influence, as argued in the introduction.'

I am totally confused. The ensemble of regional simulations did include the natural forcings: one, in addition to the anthropogenic forcings; the second, in isolation. I have no clue what this paragraph actually means.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 13197, 2015.