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.

van Oldenborgh et al.

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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

C7113

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 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.

We are grateful for these thoughtful remarks, which improve the legibility of the paper considerably. In hindsight, we should have taken a bit longer to write the article well. For the next events we will attempt to have a template ready and keep the points made here in mind. (See eg our next attempt, http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-308/hess-2016-308.)

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 I am not suggesting that the study should not be published here, but the motivation should be made stronger.*

   The motivation is simply that the demand for an analysis like this is strongest in the week or so after the event. We think we can give a scientifically valid answer within that time frame. A later analysis can of course be more precise, but does not meet this specific demand. We think there is value in having the preliminary analysis documented in a peer-reviewed article.

   We have added the following text at the end of the introduction to address this concern: 'This preliminary analysis has been made in near real-time to answer

C7114

questions that arose at the time of the event. Later papers are expected to give better estimates of the return time and impact of climate change on the event, but within the uncertainty margins of this fast analysis.' Note that the first sentences of the conclusion also addresses this point.

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.*

We are not sure this article is relevant as it considers changes in global mean precipitation, whereas we study local extreme precipitation. The latter is not constrained as much by the atmospheric energy budget as it is far from equilibrium. The most relevant relation in this case is simply the Clausius-Clapeyron scaling of the moisture content of the atmosphere. The turnover rate of this moisture is not relevant as the amount of precipitation is set by the local weather dynamics and not the global energy balance.

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

If the article is still not accepted by the time the manuscript is published in HESS we will remove this citation.

4. *'the return time, $f_1/f_0 = \tau_0/\tau_1$. However, these are calculated to answer subtly different questions: how much the probability changed due to the observed trend for the observations, 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 methods. At this state in the manuscript it is rather confusing. Which 'trend' is meant?*

We assumed the reader would be familiar with these framing issues. A reference to the 2016 Stott et al, WIREs Clim. Change review has been added for readers that are not.

In this case the word 'trend' denotes a change in the PDF over time. We clarified the text to 'the fitted trend for the observations'.

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.*

Added 'natural' to 'variations'.

Added 'precipitation extremes are not significantly correlated to the Atlantic Multidecadal Oscillation (AMO) or Pacific Decadal Oscillation (PDO) at $p<0.1$ over 80 years of observations.' The connections with ENSO are also not significantly different from zero. We are not aware of any other low-frequency variability that could influence extreme precipitation in this region.

6. *'A preliminary indication was obtained from the ECMWF analysis, which gives about 28 mm day for Northwest England and 31 mm day for South Scotland. ' What is the approximate area (in squared km) to which this numbers refer?*

These areas are defined on the UK Met Office web site, which we now refer to at first mention. They are about 200×200 km, so contain many grid points.

7. *'These block maxima were fitted to a Generalised Extreme 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.*

This refers to the 'daily and two-daily maxima occurring over the period October–February' two sentences earlier. Deleted 'block'.

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)?

Only the position and scale parameters vary, and their ratio is kept constant Added 'The cumulative distribution function is

$$F(x) = \exp\left[-\left(1 + \xi\frac{x - \mu}{\sigma}\right)^{1/\xi}\right],$$ (1)

$$\mu = \mu_0 \exp(\alpha T'/\mu_0)$$
$$\sigma = \sigma_0 \exp(\alpha T'/\mu_0)$$

,

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.')*

The quantiles lower than 50% are all zero and have been deleted from the key. The reference was incorrect after a new figure was inserted at the last moment has been fixed. We also took the opportunity to choose clearer colours.

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.

The caption has been greatly expanded. The method of estimation (maximum likelihood) is now mentioned in the text.

A Gumbel plot just refers to the X-axis having a double log scale so that a Gumbel distribution (GEV with $\xi{=}0$) is a straight line. We assume that this is standard terminology in extreme event analysis.

The definition of 'current climate' and 'climate of 1931' is now made explicit.

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?*

Added 'between 1931 and 2015'. These changes can be read of from the figure as ghe intersect of the blue lines (red lines) with teh horizontal line. The uncertainty is estimated using a non-parametric bootstrap, which is now also mentioned in the text.

11. *'The resolution is T159, this is about 150 km, too low to . . . ' The reader would again appreciate being more specific. Not all readers will be acquainted with climate model jargon.*

We tried to make this a bit clearer: 'The resolution is T159, this is about 150 km, which is too low to represent the mountains that show the highest precipitation in Fig. 1'

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.*

This is exactly what we wrote. We did not want to use the term 'climate sensitivity' as this is usually meant as the equilibrium response to a doubling of CO_2, whereas the trend up to now is mainly the transient response to both CO_2 and aerosol 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)

Fig. 3a does not show the trend in mean precipitation but the trend in the maximum of winter daily precipitation. This comparison is implicit in the comparison between Figs 3 and the model results. In practice the extremes indeed scale with the mean in this region, but there is no strong theoretical justification for this as far as we are aware and we did not think that a test whether mean precipitation trends agree would strengthen the results.

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, 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.*

It is explained in the following paragraph. We tried to lessen the confusion by adding a sentence 'However, this return time is again not a realistic estimate.' at

C7119

the end of this paragraph.

We did not attempt to correct this bias due to lack of information at this moment. Using the data that became available in January it was found that the return time of this event was in fact somewhat below 100 years in these regions.

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.*

We expanded the paragraph to be clearer: 'Again, the question addressed with the atmosphere-only large ensemble method is slightly different from the other two methods. Here we ask how much the probability has changed given the influence of prescribed anthropogenic forcings and the observed SST patterns. We checked that the different SST patterns in 2012/2013 and 2013/2014 indeed did not make an appreciable difference, confirming that the influence of specific SST patterns is likely low for this event. The other difference is that in these experiment the natural forcings are kept constant between the factual and counterfactual climates, whereas in the observations and coupled experiments they vary over time. However, these forcings have a small influence, as argued in the introduction, so that the results should be comparable between the different methods.'