Hydrol. Earth Syst. Sci. Discuss., 12, C7126–C7135, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C7126/2016/

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.

van Oldenborgh et al.

oldenborgh@knmi.nl

Received and published: 30 June 2016

Reply to Interactive comment #5.

General comments

The motivation for this paper is the widespread flood impacts following Storm Desmond in the UK in December 2015, which led to questions as to whether climate change played a role. The point of the paper is to provide a methodology that can be used to answer such a question in the days following the event. While I can understand the mo-

tivation for a methodology that can provide robust answers rather than just speculation when an event is still in the public eye, it is extremely important that this methodology is well justified in the peer-reviewed literature prior to those robust answers being publicised.

Unfortunately I cannot recommend it for publication in its current state. A clearer explanation of the three methodologies is required, as well as a better placing of these methodologies and the case study within the existing literature, and a discussion of the limitations of these methodologies. I have provided guidance for this within my specific comments.

We thank the reviewer for the helpful comments. With hindsight we see the shortcomings of the manuscript in these aspects (also pointed out by the other reviewers), which are due to inexperience and a self-imposed time constraint when we did the study. We have addressed these in the revision of the current manuscript and in our procedure for following events (cf. http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-308/) and hope the improvements make the study acceptable for publication.

Specific Comments:

- *This paper focusses on the precipitation amounts during Storm Desmond, rather than the flooding itself. Given that the media attention is on the flooding, it is extremely important to declare the difference between the results of a precipitation attribution to an attribution the flood impact. Given the importance of antecedent conditions for [most] flood events, an analysis of river flows would have been appropriate, and could easily have been carried out using the 'observational analysis' methodology, but of river flow rather than rainfall gauges.*

  We agree completely that a flood attribution would have been more useful. However, it was beyond our reach at the time. On the observational side, we attempted to obtain river flow data through CEH, but these were not yet available.

On the modelling side, our models have not yet been coupled to hydrological models so we could not do the model analysis on flood levels or river runoff either. We are working on both problems (see eg Schaller et al, Nature Climate Change, 2016 for a slow attribution study that does this), also for the rapid time scale, and hope to have these capabilities in place in a year or two.

In the meantime, we try to make clear in the title, abstract and introduction that we only attributed the rainfall and not the flood itself. As the rainfall is the main driver of the floods, and other factors such as soil saturation are similar in most years in this season, we considered this a good first approximation.

- *The methodology is difficult to follow, with some things appearing in the Figure captions that are not referred to directly (e.g. Fig 3 a,b,c,d), and methods such as the scaling of the block maxima with the low-pass filtered global mean temperature needing to be clearly explained, and where possible, referenced.*

  The figure captions have been completely rewritten and greatly extended to make them clearer. The scaling of the block maxima is now documented in detail:

  'These maxima were fitted to a Generalised Extreme Value function (GEV) scaled with the low-pass filtered global mean temperature anomaly T' (GISTEMP, Hansen et al, 2010), a proxy for anthropogenic climate change. The cumulative distribution function

$$\begin{aligned} F(x) &= \exp\left[-\left(1+\xi\frac{x-\mu}{\sigma}\right)^{1/\xi}\right], \qquad (1) \\ \mu &= \mu_0\exp(\alpha T'/\mu_0) \\ \sigma &= \sigma_0\exp(\alpha T'/\mu_0) \end{aligned}$$

  is fitted to the observations using a maximum likelihood method varying α, μ_0, σ_0 and ξ. Uncertainties are estimated with a non-parametric bootstrap. The results

  are shown in Fig. 3 for the two regions. The horizontal line denotes the preliminary indication for precipitation in these areas.'

- *On p13200 the authors mention that 'low-frequency variations play a minor role here'. This is clearly wrong, as the NAO is well known to influence precipitation in NW UK, e.g.*

  - *Burt T P and Howden N J K 2013 North Atlantic Oscillation amplifies orographic precipitation and river flow in upland Britain Water Resour. Res. 49 3504–15*
  - *Svensson, C., Brookshaw, A., Scaife, A. A., Bell, V. A., Mackay, J. D., Jackson, C. R., . . . & Stanley, S. (2015). Long-range forecasts of UK winter hydrology. Environmental Research Letters, 10(6), 064006.*

  The authors should comment on how the NAO signal has been accounted for in their analysis and influenced the results they have found.

  The NAO of course matters greatly for precipitation in this area. However, this is only at short time scales. The probability of extreme precipitation in NW England in December is higher if the NAO is higher in December, but it is not significantly higher if the NAO is high over the whole season studied, October–February, simply because the main decorrelation time scale of the NAO is well below one month.

  In this study we attempt to separate the probability of extremes occurring in two components: trend and chance. The randomly varying weather component of the NAO falls squarely in the chance category, and is therefore implicitly included in the calculations as such.

  There are two possible complications to this simplified picture. One is that the NAO may have a trend component due to global warming. As the reviewer is well aware, a lot of research has gone into this since the high NAO values of the

1990s. The trend was found to be small, both in models and in observations up to now ($r = -0.05$ for the correlation between the October–February Gibraltar-Iceland NAO from CRU and smoothed global mean temperature 1880–2014). Anyway, this component would have been included as a dynamical contribution to the trend in this study, and hence affected the risk ratio in the attribution statement.

The second, more worrying, complication could be that the NAO (or another relevant mode of climate variability) has a low-frequency component. This would mean that our error estimates are too small, as these assume all years are independent. The sentence quoted above addresses this possibility. Concerning the NAO, the October–February averaged NAO index has lag-1 yr autocorrelation zero, and the spectrum does not show significant low-frequency variability beyond the integrated effect of the high-frequency variability. The month-to-month autocorrelation and seasonal predictability do not affect our seasonal maximum analysis.

Other low-frequency modes that could have caused dependences between different years are the AMO and PDO. However, we show that high precipitation in the area of interest has very low correlations with these two modes, so they do not affect the results.

Added text: '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.'

- *In terms of the area averages, I think there needs to be more discussion (based on the literature) of how area averages can reflect localised extremes, and on Figure 1 the areas used for North West England and Southern Scotland areas should be outlined.*

On the first point, as far as we are aware there is no literature on whether Areal Reduction Factors are changing in time. From a meteorological point of view, there is no reason to assume that the secondary lows that cause the kind of precipitation extremes studied here would change in spatial scale (as opposed to summer convective precipitation). Taking the ARFs constant implies that the Risk Ratios that are estimated in his study are also valid for smaller scales.

We have added the following text to the paragraph that defines the areas: 'Precipitation averaged over smaller areas such as the basins of the rivers that flooded, and indeed point data at rain gauges, are assumed to have similar changes in the probability of extreme precipitation due to global warming. The extremes themselves do vary with spatial scale, but the ratios of extremes at different scales are assumed to be constant in time. For large-scale winter precipitation events such as storm Desmond we know no evidence that would contradict this assumption.'

On the second point, we have added these areas to Fig. 1.

- *There is no discussion of why a GEV fit has been used, or whether it provides a good fit to the data. For Figure 5 the GEV fit doesn't look good, with the residuals not normally distributed at the extreme end of the distribution. In addition, the y-axis could be on a log scale to make the plot easier to decipher.*

We added text: 'These maxima were fitted to a Generalised Extreme Value function (GEV), as is appropriate for block maxima (Coles, 2001). The GEV depends on time by scaling it with the low-pass filtered global mean temperature anomaly . . . '.

In all fits the observations are well within the uncertainty bounds (which are wide because these are cumulative plots, in which the points are highly dependent). A normal distribution does not fit the data at all, as expected on theoretical grounds, see attached figure.

We disagree that making the Y-axis logarithmic makes the plot easier to decipher.

The standard convention for Gumbel plots is to use a Gumbel X-axis and a linear Y-axis, this transforms a Gumbel distribution (GEV with $\xi = 0$) into a straight line. As the shape parameter ξ is usually small, also in this case, this gives almost straight lines.

- *In p13202 line 25, the authors state that the 1 in 100 year event is used, which is roughly the return time of the event in the observations (presumably the Eskdalemuir series, though this is not stated) and from ERA-Interim (though again, the reference for this hasn't been provided).*

  The choice of this return period is critical. In Figure 5, no lines are drawn to guide the reader to the 1 in 100 year event, and clearly if the observed 2015 value (for which there is a line) was used then the uncertainties are so large at this extreme end of the distribution that it is possible (though not the most likely explanation) that climate change actually reduces the likelihood of such an event occurring. I would suggest using the return period for the South Scotland and North West England precipitation totals when it becomes available, if it hasn't already.

  The return times for South Scotland and Northwest England are now available, but because of the time of day that storm Desmond passed relative to the observing time of 9:00 UTC, the rain is spread over two days in these series. Return times for the two-day sums are less than 7 years.

  A better match for the models in the ERA-interim estimate over the larger box, which turned out to be 25.1 mm. This gives a return time of about 20 years in the EC-Earth ensemble, so we did not lower the initial estimate from the ECMWF analysis enough. In the EC-Earth ensemble the assumptions underlying the GEV fit make the risk ratio independent of the return time. The Weather@Home analysis does not make this assumption but the ratio also appears to be quite independent of the return time.

  We have updated the article with these data, clearly marked as added in the

revision.

- *I'm concerned about the use of the Eskdalemuir time series because it is the 'only one publicly available'. Is this the use of only this due to the motivation of producing this analysis in real-time? Have the authors attempted to obtain data from some of the gauges in Cumbria? Is the lack of suitable data now the main limiting factor in performing attribution in near real-time?*

  Yes, we have asked for data from the UK Met Office. Since the underlying station data are obtained from different networks operated by various agencies and have to undergo quality control, the area averages are only publicly released the following month. The station data are also unavailable until that time.

  We find that the lack of suitable data is indeed a problem, but this is improving as more real-time station datasets and analyses become available and more and more meteorological offices move to an open data policy. Indeed, often relevant record-high values can be obtained from the meteorological offices right after the event as they are communicated to the press and public.

- *For sections 4 and 5, I would like to see some discussion of to what extent models that don't include orographic effects can provide meaningful results when comparing return periods for this kind of orographic rainfall event, especially where NAO has been shown to double the orographic enhancement (Burt and Howden, 2013).*

  We refer to the above paragraph on the role of the NAO and other high-frequency climate variability. In this analysis these are just noise, which we attempt to separate from the signal, which is the trend due to anthropogenic greenhouse gas and aerosol emissions. The weather variability is higher in the mountains. As the NAO does not have a connection with global warming up to now, the trend is unaffected. The end effect is that orographic effects decreases the signal/noise, hence missing orographic effects probably cause an underestimation of the uncertainty in the coarse-resolution models but does not effect the central value. We should redo the study using the recent 12-km RCM runs that were used in the attribution of the extreme rainfall in France and Germany at the end of May 2016 to investigate the magnitude of the effect.

Added: Another difference is the lower orography in the EC-Earth model due to its relatively coarse resolution. This will lower variability somewhat as orography amplifies the effect of circulation (Burt and Howden, 2013) and hence lead to an underestimation of the uncertainty. For the large area chosen we expect the effect to be small for this winter situation. Higher-resolution models are needed to ascertain this. It should be noted that we also do not know the orographic situation of the stations that form the Northwest England and South Scotland time series and whether these are a good representation of the areal average.

Technical Comments:

- *The labelling of the Figures, especially in section 3 is wrong.* This has been fixed, thanks.
- *P13205, L7: I think this is written the wrong way round?*
- *P13206, L17-19: Also need to mention the antecedent conditions, which also contributed to the flooding.*

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

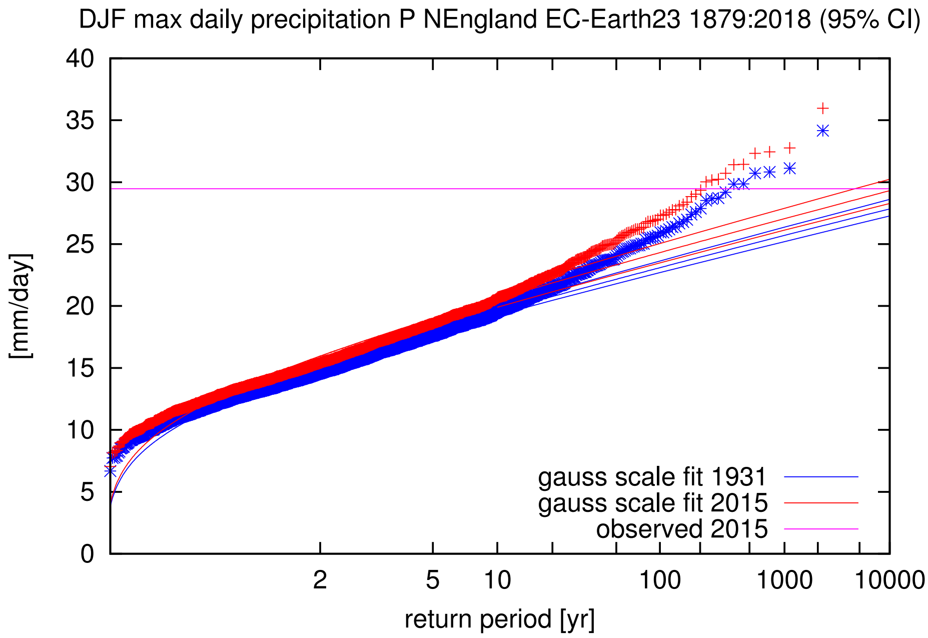

Fig. 1. Fit of EC-Earth precipitation to a normal distribution