

## ***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 #7**

Received and published: 19 January 2016

The submitted paper investigates to what extent climate change has altered the odds of a rainfall extreme like the one observed during storm Desmond in the UK. The authors use three alternative approaches to assess the likelihood change of the observed event due to external factors. The authors submitted the manuscript within a remarkably short period of only 4 days after the event occurred to demonstrate that real-time attribution statements are possible in the time period during which media interest in the event is still high. Thereby they imply that such scientific near-real-time assessments are possible and meet scientific standards even of peer-reviewed publications.

Ironically my interpretation after having read the manuscript is quite the opposite. The

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authors made an impressive effort in doing a comprehensive analysis and submitting a manuscript in such a short period. I applaud for comparing three different methodological approaches. However, frankly speaking, I do not see much robust added value beyond a simple generic Clausius-Clapeyron argument that any climate scientist could have given to the media while the event was unfolding. The whole method intercomparison framework seems to imply a rigorous scientific assessment of an accuracy that goes far beyond such a general thermodynamic argument. But does it really do so? I am not convinced, maybe because, at most, the manuscript meets the standards of a blog article rather than the ones of a scientific paper. I recommend revisions and I am convinced that the manuscript has the potential to make a valuable contribution to the literature. But more work is needed and more generally I consider the exercise for a rigorous real-time event attribution (within days after the event) that adds substantial value beyond simple arguments on existing literature to have failed. I detail my comments below.

#### Definition of event

Despite a whole section on the event definition, it remains unclear what definition is finally used in the attribution statements. The event thresholds used seem to even differ between the three methodologies. The precipitation totals mentioned in the text dramatically vary between some 341 mm in 24h to less than 30mm/d used in Fig. 6. I understand the scale difference between in-situ measurements and gridded observations and models, nevertheless, I think a common event definition informed by observations should be used in all methods. It is unclear why one should have confidence in the precipitation totals of the ECMWF 24-hour forecast for such an exceptional event. Even though the change in likelihood may not be particularly sensitive, the event definition should not be arbitrary. If in this exercise the availability of observations only days after the event was the limiting factor, the whole real-time attribution effort should maybe first focus on improving the immediate availability of observations rather than investing all resources and efforts in developing complex modelling frameworks. For

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this event, the manuscript needs to be revised using a proper observational estimate of the event once it has become available and using the same event definition throughout all methods. For future real-time event attribution exercises I recommend waiting for proper observational data. I simply do not see the point of implying accurate scientific event attribution before even the observational estimates have become available. The argument that for the attribution statement it does not matter how extreme the event was, does not foster public confidence in the scientific rigour of such real-time attribution exercises.

### Observational analysis and internal variability

The observational analysis is interesting and valuable but both the methodology and the findings require further documentation. You analyse southern Scotland and north-western England separately and find different observed trends in heavy precipitation. Are you suggesting that the difference relate to systematic differences in the forced precipitation response or due to internal variability? I suspect it is primarily the latter and internal variability could have easily masked the heavy precipitation response even at these relatively long time scales. This should be tested based on the EC-EARTH initial condition ensemble. By how much do the trends at these time scales differ between different realizations? I suggest to use a large enough area or to pool enough stations in order to increase the signal-to-noise ratio and reduce the effects of internal variability. Attribution based on individual station series or single gridpoints that are heavily influenced by internal variability does not make sense. The choice the area size required to make robust statements may be informed by the EC-EARTH ensemble. However, simply averaging the likelihood ratio for South Scotland and Northwest England as done here seems arbitrary.

### Clausius Clapeyron

You argue that changes in precipitation extremes go beyond a simple Clausius-Clapeyron scaling and changes in atmospheric circulation may play an important role.

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You should test this and quantify how much of the changes in the likelihood ratio can be explained by a simple CC scaling of the precipitation extremes with the regional or large-scale warming. Do the large ensembles of weather@home and EC-EARTH really show a robust forced response in atmospheric circulation? Such large ensembles would be the ideal testbeds to address this question. As stated in the introductory paragraph I suspect that much of the changes found here are accounted for by a simple CC scaling.

## Model evaluation

The authors mention but do not perform any model evaluation for this region. I understand that such an evaluation is not straightforward and bias adjustment would be particularly challenging. However, if you expect people to have confidence in these results beyond simple thermodynamic arguments some evaluation for the regional precipitation climatology of the two models is needed.

## Confidence interval

The strongest point of the paper is the comparison of three methodologies. However, it is unclear how you get from the three confidence intervals of 1.3–2.8, 1.1–1.8 and 1.05–1.8 to the 5–80% or even the best estimate of 40% highlighted in the abstract. It seems that you only used the weather@home results. Why would you have more confidence in this than the other approaches?

## Minor comments

13199: L24: you do not introduce what  $f_1$  and  $f_2$  stands for. Is it equivalent to  $p_1$  and  $p_2$ ?

13199: L27-29: “In the limit that the trend is completely due to anthropogenic forcings these coincide. In the UK in winter this is as far as we know a reasonable approximation.” That’s an odd statement. At least in the observations the role of internal variability will be substantial.

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13200: L22: rephrase sentence

13201: L24-25: As stated above I agree that spatial pooling or aggregation is essential here but the way the averaging of two regions is motivated and done here is confusing.

13203: L3: 4-5% is about what CC scaling would give you, right?

13204: L13-15: Comparing only two years is a very poor test of the influence of SST variability. Even if I assume that the conclusion still holds for other years it should be tested with SST variability from about a decade or so.

13206: L12-13: I understand what you mean but it is confusing to argue that the anthropogenic forcing matters but the SST patterns does not if in weather@home you ultimately reflect the anthropogenic influence by an SST masking. I think what you mean is that intra-decadal SST variability does not matter but multi-decadal SST changes do matter. References: It gives a bit an odd impression if more than two third of the references are from the authors or their teams. Particularly if the paper is about a subject like heavy precipitation where there is a vast body of literature.

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

**HESSD**

12, C6212–C6216, 2016

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