

## ***Interactive comment on “Rapid attribution of the May/June 2016 flood-inducing precipitation in France and Germany to climate change” by Geert Jan van Oldenborgh et al.***

**Geert Jan van Oldenborgh et al.**

oldenborgh@knmi.nl

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We'd like to begin this response with a brief note of thanks to Professor Shepherd for his comments. Our team is constantly striving to do the best possible science and we take his comments to heart. It is our sincere hope that this point by point response helps provide a better case for how we approach rapid attribution. I summarize these show that, to the best of our knowledge, the attribution techniques are state of the art, even comparing to slower analyses. The use of preliminary data does not affect the outcome much. There is a real demand for such quick scientific attribution information, which we try to provide in an as transparent and rigorous framework as possible. The framework we use at the moment—open review in this journal—may not be optimal,

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but we are looking into alternatives whilst rejecting non-transparent options.

*Reviewer 1 has thoroughly identified a number of reasons why this paper is nowhere close to being acceptable as a primary scientific publication, so I won't belabor them but will simply say that I agree:*

*(i) Authors weren't prepared to wait to get access to quality-controlled data;*

We now have the quality-controlled data from Météo France and can compare these with the initial estimates. The maximum 3-day precipitation over the Seine basin in the Météo France Safran reanalysis dataset is 61 mm/3dy and for the Loire 57 mm/3dy. Compared to our initial estimate of 55 mm/3dy and 47 mm/3dy respectively, based on real-time data, we underestimated the rainfall by 10% to 17%. This is well within the uncertainty range given in the original submission and confirms the validity of our previous results, as we show in the reply to reviewer 1. It should be noted that the Risk Ratio is not very sensitive to the severity of the event in general, and here even less as  $\xi \approx 0$ . So, contrary to the claim, taking real-time data did not materially affect the analysis.

Observations were also used for model validation and trend detection, but as these exclude the event itself they are entirely based on quality-controlled data.

*(ii) Authors weren't prepared to take the time to explore mechanisms for the change in risk, and in particular whether dynamical changes might have been important (which is ironic, since some of the same authors have recently argued that one must do this in event attribution);*

First, the change in risk analysed here is due both to circulation changes and thermodynamic changes as the models used in this study simulate this and the observations entail both effects. We have not decomposed the change in risk to thermodynamic and dynamic factors, as this is not a first-order concern to the intended audience (and we never claimed this must be done, but what we did say in the article we think the

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commenter referred to, Otto et al. (2016), is the opposite: that **not** reporting the combined effects of thermodynamic and dynamic changes could be problematic). It is an interesting scientific question, which will no doubt be taken up, but does not impact the total change in risk that we wanted to communicate. This is quite standard in the peer-reviewed literature: Schaller et al. (2016) is one of only a few papers that we are aware of where the dynamic component is computed separately. We are just publishing an in depth methodological paper on separating the two effects (Vautard et al., 2016), but this was after the current paper and not yet routine enough to include in a rapid analysis.

*(iii) Although the public concern was about flooding and HESS is a hydrology journal, the authors weren't prepared to take the time to assess the flooding.*

As far as we are aware, there is only one single paper in the peer-reviewed attribution literature that downscales the precipitation to flooding, Schaller et al. (2016), which took more than two years of our time. All other peer-reviewed papers on floods, including all six in the most recent BAMS special supplement, analyse rainfall and do not use hydrological modelling. We judged that an attribution of the rainfall is state of the art at this moment and carefully indicated this in the title. This is of use to the readers of the analysis before the attribution of the floods, which will probably be undertaken but again will probably take a few years.

We did take into account the hydrological situation as much as possible with current tools, by defining the rainfall event in so that a direct link to the impacts (floods) were maintained. In this case by using only the late-spring season so that the relation between extreme precipitation and floods would stay constant, by taking the average over the river basin and by summing over the time scale most relevant to the floods.

We are planning to extend our rapid attributions to flood properties by coupling the output of the climate models to hydrological models. However, this will take a few years of development and testing in slow analyses before the results are robust enough to

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include in the rapid ones.

The author of this comment did not mention the strong points of this study, which are rare even in peer-reviewed extreme event attribution articles published on much longer time scales.

1. A careful event definition that does not draw a box around the extreme but sets seasonal, spatial and time boundaries dictated by the impacts.
2. Explicit and quantitative evaluation of the ability of the models used to represent correctly the phenomena being attributed. For France, we reject one of our ensembles of simulations because the PDF does not resemble the observations, even after a multiplicative bias correction. For Germany, we reject most ensembles as these models cannot resolve the relevant spatial scales or have an incompatible PDF. In the end, all models used have a resolution that is high enough to be able to represent the extreme rainfall, in contrast to many peer-reviewed attribution studies that do not include model evaluation or use models that do not resolve the event under study (e.g., Min et al., 2011).
3. Use of multiple high-statistics ensembles of high-resolution models. The results of different climate models that are suitable for the analysis still can differ greatly, especially when simulating summer. A multi-model ensemble gives an indication whether the model uncertainty is larger than the uncertainties due to the weather variability. We are not aware of many studies that use multiple high-statistics ensembles of high-resolution models.
4. An explicit synthesis of the results for France into a consistent attribution statement, and the conclusion that this cannot be done with current information for Germany.

*This particular paper has to be judged on its own merits. However, similar issues arose in the previous HESS submission from this team on the Desmond*

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*Storm* (<http://www.hydrol-earth-syst-sci-discuss.net/hess-2015-534/>). The issues would seem to be endemic to the rapid-attribution framework, because they result from the severe time constraints. Thus I would like to take the opportunity that this open review process provides to raise some broader issues about such studies, picking up on the final sentence in Reviewer 1's report.

*I am concerned that such studies are a disservice to the scientific community. Peer review is the foundation of science, but is done on a voluntary basis. As a former journal editor I am acutely aware of the enormous effort provided gratis by editors and reviewers. To ask them to assess papers that are written so hastily, with so many details left unaddressed, is simply not fair. Time constraints imposed by the media are not a sufficient reason to rush the process of preparing a paper that should meet the standards of rigour expected of an original scientific publication.*

There is no inherent contradiction between rigour and speed. The results of this analysis are as rigorous as most attribution papers, as we indicated by the list above. The main problems with the Desmond paper were insufficient validation and a rushed presentation of our results. We improved upon both these aspects in the current paper, with an explicit model evaluation and an exposition that allows complete reproduction of the results (all the time series are available from [climexp.knmi.nl](http://climexp.knmi.nl) and W@H), again in contrast to much of the published literature. We aim to be as transparent and rigorous as possible.

*Perhaps more importantly, I am also concerned that they are a disservice to the public, and to the public perception of science. There are two aspects to this. The first is that the public wants to know how the extreme event (in this case flooding) was affected by climate change. What they get is a quantitative answer to a different question, based on some proxy for the event (in this case, precipitation over a large region). As the authors are fully aware, the event definition has a very large effect on the quantitative answer, especially when the latter is expressed in terms of return time. So whilst there is a quantitative answer, it is not serving any local resilience need because it is not about*

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*the event that captured people's attention. I would call this pseudo-quantification.*

As we mentioned above, we worked hard to make sure that our event definition corresponds as closely as possible to the flood in seasonal, spatial and temporal extent, given the current state of the art in event attribution. We did discuss the flooding on a level similar to the UK analysis the commenter refers to. We hope to move on to a quantitative attribution of runoff or flood levels in the near future. In the only case that we know of that did this, Schaller et al. (2016), it was found that the main factor that changed the attribution significantly was the snow response (less snow in the current climate gave smaller floods). This does not play a role in late spring floods on the Seine and Loire, so we expect there to be agreement between the attribution of the meteorological variable and the hydrological variables. Follow-up studies will show whether this is indeed the case.

*The second aspect is that the Discussion paper looks like a scientific publication, and I suspect the public are unaware that it is not a peer-reviewed publication. I was asked by a reporter (a science writer at Associated Press, so mainstream) to comment on the third paper in this series, on the Louisiana flooding (<http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-448/>). He said it was embargoed! I had to tell him that it was not embargoed, but openly available on the HESS web site, and that I would not wish to comment to a reporter on a study that had yet to undergo peer review. He clearly did not understand the distinction. The paper on the Desmond Storm mentioned above was downloaded over 1000 times, but in the end did not survive peer review. I feel very uncomfortable about this situation.*

The Associated Press journalist mentioned, in the third paragraph of his story <http://bigstory.ap.org/article/3af97f8e1e0c40baab77fec391dce2a/noaa-global-warming-increased-odds-louisiana-downpour>, emphasised that our analysis was not yet peer reviewed. We have made a point to be very transparent about our process and we have found that journalists do indeed understand the distinction and are careful to report that our results have been submitted for peer

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review.

It is important to note that, at the end of his story, the AP reporter writes: 'Most outside experts – including six who contributed to the National Academies of Science report that looked at climate attribution studies – praised the science and results. The national academies panel chairman, retired Admiral David Titley, a Pennsylvania State University meteorology professor, said the Louisiana study followed the guidelines the academies set out and uses observations, models and physics to come to its conclusion. "It's an excellent study," said Columbia University climate scientist Adam Sobel, who was on the academies report team. "These are top established researchers and the GFDL model is one of the best in the business for this purpose. The methods are appropriate and very thoroughly and clearly explained as are the assumptions necessary to draw the conclusions." ' We conclude that there are also many scientists that agree with our approach that did not comment on this HESSD paper.

Concerning the Storm Desmond paper, we have repeated that analysis with all information half a year later, and found indistinguishable results. The numbers that were quoted from it have not changed with all the new information and analysis later. We are documenting this in a follow-up paper in HESS to take away the commenter's 'uncomfortable feeling'. The current paper incorporates the criticism levelled at the Storm Desmond paper: a more complete methods section and explicit model evaluation.

*It seems to me that these rapid assessments submitted to scientific journals are falling between two stools: apparently offering more than what can be said within a few days based on the meteorology of the event and accepted physical principles concerning climate change, but not sufficiently thorough to be a rigorous scientific analysis. The US NAS extremes report (DOI: 10.17226/21852) recommends "provision of stakeholder information about causal factors within days of an event, followed by periodic updates as more data and analysis results become available".*

Indeed, and we try to do as much as possible in the first week or two. Our experience is

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that we can compute credible risk ratios in that time frame, which are more informative than making general remarks that may or may not apply in the specific situation. The current paper, with explicit model evaluation, five large high-resolution ensembles and a careful synthesis is already more robust than most of the peer-reviewed literature on extreme event attribution. No doubt in a few years' time our approach will become operational and need not be published. However, at this moment this is not yet the case, as evidenced by the comments on this paper.

We plan to follow up with a more detailed analysis using all information then available, and will investigate as part of that analysis whether the initial computations reported here were accurate. A first look could already be performed at revision time of this paper, carefully noting which information was added later.

*A good model for this is provided by the UK Met Office, who after the January 2014 UK flooding issued a technical report (with CEH) the following month ([http://www.metoffice.gov.uk/media/pdf/1/2/Recent\\_Storms\\_Briefing\\_Final\\_SLR\\_20140211.pdf](http://www.metoffice.gov.uk/media/pdf/1/2/Recent_Storms_Briefing_Final_SLR_20140211.pdf)). This provided a thorough discussion of the event from a synoptic perspective, with some preliminary discussion of possible links to climate change. It also discussed the flooding, not only the precipitation. It bore the imprimatur of the issuing organizations (and was presumably internally reviewed), so carried considerable weight. The publications in the peer-reviewed scientific literature came only much later, following detailed analysis. That sort of staggered approach seems much more consistent with the NAS recommendations than the paper under discussion here.*

We agree with the commenter that another publication model would be more appropriate than the one we have been using up to now. We are working on this. However, we do not agree that the model discussed above is appropriate. The lack of an open, transparent review process may have been one of the factors that only much later it was found that none of the mechanisms proposed in their section "Weather and climate change drivers" is visible in more than a century of observations (van Oldenborgh et al., 2015; Wild et al., 2015) and the two drivers found in Schaller et al. (2016), ther-

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modynamics and a shift towards more westerly extreme circulation types, were not mentioned.

Our main concern is to publish correct results, and an open and transparent procedure greatly enhances the possibility that results hold up to later scrutiny. Quantitative model evaluation, a multi-model result and a careful synthesis are according to the NAS report necessary to obtain reliable results, and in this paper we perform all of these and thoroughly document the methods and results. The external feedback we have received to date suggests that the science community and the public are served by a timely presentation of these results.

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