

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.

T. Shepherd

theodore.shepherd@reading.ac.uk

Received and published: 26 October 2016

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;
- (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);

[Printer-friendly version](#)

[Discussion paper](#)



(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.

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 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.

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

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

Printer-friendly version

Discussion paper



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.

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”. 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). 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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-308, 2016.

Printer-friendly version

Discussion paper

