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Hydrology and Earth System Sciences Discussions

Interactive comment on "On the benefit of high-resolution climate simulations in impact studies of hydrological extremes" *by* R. Dankers et al.

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Response to the interactive comment by Anonymous Referee #2

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R. Dankers, L. Feyen and O.B. Christensen, 15 June 2009

We thank reviewer #2 for taking the time to further clarify his or her position. Like before, we will address the points that are being made below.

R#2: The title of this paper is "On the benefit of high-resolution climate simulations in impact studies of hydrological extremes". With respect to the topic under investigation, I have pointed out some limitations of the methodology that the authors have used in their work, concluding that using their approach it would be very difficult if not impossible to prove their hypothesis. In their comment, the authors answer that their objective "is to explore differences in the LISFLOOD simulations, brought about by differences in the horizontal resolution that is used in the driving climate model". Having clarified the objective of their paper, I argue that the title is misleading, and I suggest a change in title which more clearly reflects the real objectives of their study.

We can see the point of the reviewer here, and propose to change the title to: "On the effect of resolution of regional climate simulations in impact studies of hydrological extremes"

R#2: In my comment, The first question that I have raised in my previous comment is whether the higher resolution climate model gives a better representation of reality than the lower resolution climate model. With this question, I did not mean to be offensive. I am not an expert in climatology, and I just wanted the authors to better substantiate their statement. In the field of hydrology, nobody can claim that, generally speaking, a higher resolution hydrological model gives a better representation of reality than a lower resolution model. If I read that in the field of climatology the opposite is almost given for granted, I am at least suspicious. Moreover, from their paper, this appeared like an assumption, rather than like established knowledge.

Once more, also in the field of regional climate modelling this is not 'taken for granted', but has been explored in many studies; for references, see our previous response or see e.g. the paper in Nature by Christensen and Christensen (2003). The key thing to understand is that hydrological and regional climate models generally operate on a different scale. By increasing the resolution of HIRHAM from 50 to 25 or 12 km, physical processes such as convective precipitation or the effects of orography and a heterogeneous land surface can be described in more detail. Hydrological models such as LISFLOOD, on the other hand, are usually run at a grid scale that already allows for the main sources of spatial variability in the landscape, such as topography or land use – LISFLOOD for example usually runs at a grid of 1 or 5 km. However, if LISFLOOD were to run at a 50 km grid and were to assume only 1 elevation value for the entire grid cell, it would be unlikely that it would be able to realistically simulate snowmelt in a mountainous catchment that has large topographical differences. Likewise, if we were to simulate spatial patterns in soil moisture at plot or field scale, a 5 km grid would clearly be insufficient and we may expect some improvement from moving to a higher resolution grid, if sufficient input data would be available at that scale. We propose to provide a more complete discussion on this point in the manuscript and to include more references to the literature.

R#2: The second question raised the point whether the LISFLOOD model is sensitive to precipitation patterns, at the particular scale of investigation. This has not been investigated by the authors, and cannot be given for granted. As I suggested, an easy way to test if the model is sensitive to precipitation patterns, is to average the higher resolution climate input (12km) at lower resolutions (25 and 50km). If the model does not produce any sensible difference of performance, clearly there is a problem. If the model shows any sensitivity, they will have the green light to proceed to the next stage.

We did not perform the analysis proposed by the reviewer for two reasons. Firstly, hydrological processes do not scale linearly with precipitation. This is not the case in reality and also not in spatially distributed hydrological models that are *designed* to take spatial differences into account. Averaging the high resolution precipitation field to a lower resolution, as the reviewer is suggesting, is therefore bound to affect the results. This is especially true in mountainous regions where differences in the spatial

distribution of the precipitation input lead to differences in snow storage and the timing and rate of snowmelt. In upstream catchments smaller than the larger grid scale it is even impossible to obtain the same results since the precipitation input is not the same. Many previous studies – including the ones cited by the reviewer – have shown that an improved (more accurate) representation of the precipitation over an area leads to an improved simulation of river discharge at the catchment outlet. But please keep in mind that we use a spatially distributed hydrological model and are not just looking at the average response at the outlet of some large river basin.

Secondly, we believe that our study does not actually require an investigation like this. Our aim is to see if there is an 'asymmetry' in the (hydrological as well as climatological) response, and there are two possible outcomes: either there is a difference, or not. In the latter case, clearly it doesn't matter if one uses higher or lower resolution climate simulations as input into a distributed hydrological model such as LISFLOOD. In the first case, however, the difference *can only be due to* the differences in the climate model resolution and this is something to keep in mind in climate impact studies of hydrological extremes. In our study we do obtain such differences, as can be seen in Table 1: when using the 12-km HIRHAM simulation we get more river grid cells showing an increase in the 100-year return level and less showing a decrease, compared to the lower resolution climate simulations.

R#2: The third point that needs to be analyzed is whether using real data, running their model with a higher resolution rainfall input provides better simulations. Here the authors complain about a lack of references in my previous comment. In this respect, the literature shows that using real observed data (with all the uncertainty that involves the operation of 'measuring', hence I am not talking about virtual, or hypothetical experiments with artificial data), in general models perform better for better estimates of rainfall totals over the catchment. That is, if rainfall is measured at one or at ten raingauges within the catchment, a model would generally perform better when the information of ten raingautes is used. However, using the information measured at ten *raingauges, it appears to be difficult to take advantage of the additional information of rainfall pattern. That is, total being the same, it appears to be difficult to make use of the information of where the rainfall falls within the catchment. Here I summarized the general state of knowledge that appears from the literature, and the authors may want to read the key works of Obled et al (1994), Smith et al (2004), and Andreassian et al (2001), where they will find plenty of references. If the authors cannot prove that given a more realistic rainfall input the model produces more realistic output, clearly there is no point in carrying the analysis beyond this stage.*

In essence, the reviewer is arguing that once the precipitation total over a catchment is known with sufficient accuracy, any additional information on the spatial pattern of rainfall will not improve the simulation of river discharge at the catchment outlet. In our response, we would first of all like to point out (once more) that we are not just looking at extreme discharge levels at a number of outlets of large river basins, but analysing the spatial patterns in the hydrological response at sub-basin scale, in principle at every 5 km grid cell within the European domain.

Secondly, a key issue in the argument of the reviewer is that one needs to obtain an accurate estimate of the total rainfall over the entire catchment, and for this it is imperative to have an adequate sampling of, and realistic representation of, the spatial patterns in precipitation, especially in mountainous areas or regions with a strong atmospheric feedback from the land surface. This is why almost all studies, including the one by Obled et al. (1994) mentioned by the reviewer, find increased hydrological model performance with increasing density of the rain gauge network (for more references, see our previous response). Note, though, that the conclusions of Obled et al. (1994) apply to a very small rural catchment (71 km^2) comparing the use of 5 and 21 rain gauges at the catchment outlet – our simulations are at a somewhat larger scale. Improvements that may be expected from moving from a 50-km to a 25 or 12 km regional climate simulations are of a different order, e.g. an improved location of precipitation over mountainous areas with the added consequences for the simulation

of snow storage and melt.

Smith et al. (2004), who are cited by the reviewer to support his or her argument, in fact show that using a spatially distributed hydrological model and taking spatial patterns in precipitation into account does improve the simulation of river discharge at the catchment outlet over a lumped model, but this depends on the basin characteristics as well as the degree of spatial variability in the precipitation. Obviously, we are not arguing that there is no 'dampening' effect of the catchment at larger scales – as a matter of fact, that is one of the outcomes of our paper. At smaller scales, though, we do see important differences, disproving the argument of the reviewer. Nevertheless, we would be happy to discuss this in more detail in our manuscript.

References

Christensen, J.H., and O.B. Christensen: Severe summertime flooding in Europe, Nature, 421, 805-806, doi:10.1038/421805a, 2003.

Obled, C., J. Wedling, and K. Beven: The sensitivity of hydrological models to spatial rainfall patterns: An evaluation using observed data, J. Hydrol., 159, 305-333, 1994.

Smith, M. B., V. I. Korena, Z. Zhang, S. M. Reed, J. J. Pan, and F. Moreda: Runoff response to spatial variability in precipitation: an analysis of observed data, J. Hydrol., 298(1-4), 267- 286, 2004.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2573, 2009.