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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 May 2009

Reviewer #2 judges our paper negatively, but does not substantiate his claims. We address his points below.

R#2: In my opinion the outcomes of this paper had to be expected, since its purpose is a mission impossible.

The purpose of our paper (as mentioned in the text) is to explore differences in the LISFLOOD simulations, brought about by differences in the horizontal resolution that is used in the driving climate model. We fail to see why that would be a mission impossible.

R#2: In addition, the methodology used is questionable.

The reviewer claims that our methodology is questionable, but does not explain why. Is the LISFLOOD model not a widely used and well-documented hydrological model (see e.g. De Roo et al., 2000; Feyen et al.,2007; Feyen et al., 2008; Van der Knijff et al., 2008 and references therein)? Is HIRHAM not a state-of-the-art regional climate model that has been tested in many different regions and studies (see e.g. Beniston et al., 2007; Christensen et al., 2008; Fowler et al., 2007; Frei et al., 2006; Jacob et al., 2007; May, 2007)? Is the statistical analysis not sound (see Criss and Winston, 2008; Coles, 2001; Katz et al., 2002)? Unless the reviewer is able to prove otherwise, we think our methodology is sound and well-founded in mainstream hydrological and climatological research.

R#2: As a result, their conclusions are highly speculative. On the other hand, the paper is well written and clearly formulated. I think that the authors, before trying to assess the impact of higher resolution climate scenarios on flood simulation, should perform a "feasibility" study of their work, given the large sources of uncertainty involved. First of all, does the 12 km scenario of the RCM give a better representation *of reality then the 25km, and of the 50km scenario? According to their work, this is only assumed (line 25 page 2585).*

A higher resolution climate simulation can be expected to yield a better representation of, for example, orographic precipitation patterns due to the higher level of detail (as explained on page 2585, lines 7-12). This is well established in regional climate modelling (see e.g. Christensen et al., 1998; Christensen and Kuhry, 2000; Hagemann et al., 2009) but is also easy to imagine when comparing, for example, a 50-km and a 12-km elevation model of the Alps. Even if the reviewer is not aware of the relevant literature in the field of climate modelling, he or she could have looked up our paper in the Journal of Hydrology that we refer to on page 2576 (Dankers et al., 2007). In this paper we compared precipitation patterns as simulated by HIRHAM in the Upper Danube basin with observations and showed that the higher resolution simulation yields more realistic precipitation patterns.

The statement on page 2585 relates to the differences in the climate change signal that we get in the three different model experiments, and should not be taken out of context. An analogous statement could be: if the 50-km experiment is assumed to provide more realistic model input, then the 12-km experiment results in an overestimation of future flood hazard.

R#2: Second point. Is the LISFLOOD model sensitive to resolution of climatic input? For example, would model results chance when the 12km HIRHAM scenario is averaged at 25 and then 50 km? If not, the study is hopeless.

We fail to understand the relevance of this argument. The lower resolution HIRHAM simulations are not an average of the 12 km simulation. They are independent model runs and in all cases the HIRHAM output was regridded to the 5-km grid of the

hydrological model. LISFLOOD is 'sensitive' to the resolution of the climate input as far as the information in this climate input is different. This is what we are investigating in this paper.

R#2: Third point. If the LISFLOOD model is run with higher resolution observed data, would it perform better? The authors should be aware that several studies in the field of hydrology have tried to prove the usefulness of higher spatial resolution of precipitation data, and only a few of them could prove their advantage. In most case, an opposite conclusion was drawn. In fact, the catchment acts as a low pass filter on the spatial heterogeneity of input data.

A difference in the resolution of an observation-based precipitation dataset is not the same as a difference in the resolution of a climate model run. Nevertheless, if a higher resolution observation-based dataset is created with a larger number of stations, then it can be expected to give better results, because the information content is better. Many studies have shown how hydrological model performance decreases with raingauge network density (e.g., Anctil et al., 2006; Brath et al., 2004; Dong et al., 2005) and that inadequate representation of the spatial variability of precipitation can be responsible for large modelling errors (Bárdossy and Das, 2008; Dorninger et al., 2008). Likewise, a higher resolution climate simulation can be expected to yield a better representation of the climate system because the information content is better – for example, a more detailed representation of the land surface – resulting in a better simulation of the spatial variability of precipitation and precipitation extremes (again, see Dankers et al., 2007).

R#2: In paragraph 3.2 the outputs of the hydrological model are compared using a Nash and Sutcliffe coefficient. However, the differences shown in the hydrograph of Figure 3, most likely, reflect the differences in the dynamics of the three different *input scenarios, rather than on their resolution. The same can be said for figure 6, which in my opinion shows that the input scenarios tend to become more similar when averaged on a larger area, but does not say much about the hydrology of smaller vs. larger basins.*

Indeed the differences shown in Figure 3 (or even more so in Figure 5) reflect differences in the dynamics of the three different climate simulations, and these are the result of the different resolution used in the climate model runs. We have mentioned explicitly in the text (p.2577-2578) that the three climate simulations that we employed used the same boundary conditions and that the only difference between them is the horizontal resolution that was adopted. But perhaps the reviewer has a different explanation how the same climate model, using the same boundary conditions, can result in the different hydrographs shown in Figures 3 and 5 if this is not related to the different horizontal resolution?

Figure 6 shows that the weather mechanisms that cause extreme discharge levels in large river basins are also captured by lower-resolution climate simulations. At smaller scale, this is not necessarily the case. We think this is of direct and practical relevance for other climate impact studies.

R#2: As a result, all the conclusions drawn for what concerns the hydrology are in my opinion speculative, and of little interest to the hydrologic community.

We believe our results are sound and the reviewer has not been able to prove otherwise, nor does he provide any references to studies that would underpin his argumentation. We regret the reviewer seems to think his or her disinterest in the recent advances in climate modelling should be the norm for the wider hydrological community.

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