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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.

Anonymous Referee #2

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I thank the authors for their reply which helped to clarify some aspects of their work. However, in their reply, they do not fully address the points raised in my previous comment. I take this opportunity to clarify them further.

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.

The authors write that I claim that their methodology is questionable, without explaining why. In my comment, I have identified at least three aspects which in my opinion needed to be clarified. The authors wonder if my concern was about LISFLOOD or HIRHAM, which, as they notice, are well documented state of the art hydrological and climatic models. I have nothing against LISFLOOD or HIRHAM, however, the authors should notice that their documentation does not make them invulnerable to the various sources of uncertainty involved in the modeling process.

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.

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.

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.

With respect to the authors final comment "We regret the reviewer seems to think his or her disinterest in the recent advances in climate modeling should be the norm for the wider hydrological community" I want to point out that the authors should demonstrate more respect for the reviewers work.

References:

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