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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The simultaneous occurrence of surge and discharge extremes for the Rhine delta" *by* S. F. Kew et al.

Anonymous Referee #1

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An interesting paper, demonstrating a potential engineering application of climate model data. However, the authors seem to have adopted a complex approach, based mainly on model data, when a more direct approach based on measured data could probably have done the same job.

The authors imply, both in the abstract and in the main text that the barrier's fitness for purpose is usually considered based on the assumption of independence between sea surge and river discharge. It seems unlikely that any major project designed in the last 50 years would make such an assumption. I suggest, unless there is quotable evidence that the design was based on independence, it be made clearer that this is a hypothetical comparison. As it stands, the barrier designers may feel that the paper is questioning their professional competence.

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It would be a more interesting paper if the authors were able to say what their conclusion would imply for the barrier. For example, if there were a slight dependence between sea surge and river discharge, how would this affect the probability of simultaneous occurrence at the barrier and would this make any difference to its standard of service. Related to this, it would be more interesting to focus on the n-day duration, and the relevant lag with surge or wind speed, that would tend to cause the "simultaneous" occurrence implied in the paper's title, and which could potentially be used to comment on implications for the barrier.

Perhaps the ensemble modelling I have been involved with is different to that used in the paper, but doesn't it involve the same sequence of forcing events just with small changes to model parameters. If so, ensemble modelling would deliver a number of representations of the same periods of time and the same storms, and therefore break the assumption of independence between the years of data analysed in the paper. A little more explanation of why this is not so would be enough.

Related to that, looking for slight dependence between one model parameter and another parameter derived from the same model seems of doubtful validity, as it would presumably just reflect the assumptions underlying the model. Again, a little more explanation of why this is not so may be enough to allay this concern. Combining this and the previous point, could it be that the same storm is picked up repeatedly, one per ensemble run, as this would approximately correspond numerically with the level of dependence detected.

I found it difficult to see the intended information content in some of the figures, particularly Figure 4. Presumably in the finished paper, the figures will sit within the text, but I would suggest some example interpretations are added so that readers can follow through the detail of the calculations and conclusions drawn. I suggest also that all figures of one type are plotted on the same scale. At present, most of the figures are on different scales.

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I suggest that the conclusions be prefaced with a comment to the effect that these are tentative conclusions drawn from numerical model data. Anyone reading only the conclusions might think that they are based on more solid information.

More minor comments are given in the attached marked-up copy of the paper.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C63/2013/nhessd-1-C63-2013-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 111, 2013.