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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "River flood risk in Jakarta under scenarios of future change" *by* Y. Budiyono et al.

Anonymous Referee #2

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Overall the paper is interesting and well written. The authors have clearly outlined a series of papers with marginal improvement on the same topic and case study, but the novelty is sufficient to merit publication. My main concern is a lack of detail and discussion of methodology as outlined below.

Main comments: In general I think that the paper is in-balanced in the way the drivers are described. Precipitation and sea levels are described using an assessment of the overall uncertainty (although assuming that the models are independent, which is highly questionable). Land subsidence and land use is described using a single projection without uncertainties. This leads to underestimation of the overall uncertainties reported and discussed in the paper, since probably the largest uncertainties are related to these two drivers rather than the climatic changes foreseen. Novel papers

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within this type of assessment can be found in Veerbeek and Zevenberger (2013) and Urich and Rauch (2014) while Zhou et al (2012) gives and example of discussing how some drivers are ignored. Further, extreme precipitation and sea levels are often correlated as described in e.g. Pedersen and Arnbjerg-Nielsen (2012) and Zheng et al (2014). Also the concept of autonomous adaptation is ignored, except for a remark that it is assumed that regardless of scenarios it is assumed that there is no damage for return periods below one year. Given that there has been quite a reduction in the risk in the last decade an assumption of continuous reduction of risk in the form of risk reduction is also feasible. So the projections are highly simplistic and the description of the drivers must be aligned with each other. I would suggest discussing the processes outlined above and then use a projection for precipitation and sea level that is as simple as the other drivers. A full monty with uncertainties on all projections would of course be very nice to see indeed.

The scenario for sea level rise from the IPCC report in 2007 was recognized to be too low already when published and with new scenarios presented even already in 2008 and with further improvements in the AR5 report published in 2013. It is therefore highly questionable to use the report from 2007 to construct the scenarios for the study.

The projection for land subsidence seems a bit odd with a questionable assumption of no subsidence after 2025. Please discuss and/or justify this assumption, especially since this is related to the main findings of the study.

There are several places where the methods are poorly described and where it cannot be derived what the authors have done. The most clear example is the description of extreme precipitation. There are quite a few bias-correction methods available, but it cannot be derived how you have obtained the results. Hence the derivation of the 100 year return period based on the short time slices you have used is completely unknown.

It would be nice to have a physical visualization of the hazards, vulnerabilities, expo-

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sure, and resulting risk over the catchment, to be placed around page 4448.

The discussion on page 4449-50 is important, but something I would prefer to have as a preamble for defining the scenarios in the introduction or methods section.

The discussion is very good and really helps balancing the paper. Especially I like the paragraphs on page 4453 and bottom of 4456.

Detailed comments P4436, L3: Please outline what the Damagescanner-Jakarta can do rather than assuming the code being known to potential readers.

P4437, L8: There is no opposition between flood risk management and implementing dikes and levees. The method is outlined in the textbook by Chow et al (1988), way before any of the references the authors cite.

P4442, L20: Please provide reference for the FCM method.

P4443, L25: Sentence is too complicated.

References

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