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Discussions

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

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We thank the reviewer for the time and care taken to review our manuscript. In the following sections, we respond in detail to each of the review comments, and outline how we intend to address them in the revised manuscript. For clarity, we include each of the reviewer's original comments in italics, with our responses in regular text.

General comments:

Comment: 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.

Respond to general comment: We thank the reviewer for the positive comments, and are pleased that the reviewer finds the paper interesting, well-written, and scientifically novel enough to merit publication. The reviewer suggests that some more detail is required on some of the methods, and in discussing some of the methods. This is a useful point, and we will be sure to address this in the revised manuscript. We feel that by addressing this point, and the specific points below, the revised manuscript will be greatly improved.

Response to specific comments:

Main comment: 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 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.

Respond to main comment: Many thanks for the suggestions and for directing us to these papers. Indeed, we agree that it would ultimately be very useful to work towards a full Monte-Carlo analysis of risk based on the uncertainty range of all drivers. However, at the present we do not have the data available to perform such an analysis. The aim of the current paper is not to provide a full certainty analysis. Instead, we further develop a relatively simple flood risk model for a megacity in a delta region, using Jakarta as a case study. In doing so, we also want to use the model to see how risk develops, given the best scenarios that we can find for the different risk drivers. In this case, there are more climate scenarios available, compared to scenarios of the other drivers that we used. We could indeed simplify the climate change scenarios, by selecting just a limited range of scenarios/model combinations. However, this would imply having to choose those model/GCM selections a priori. Alternatively, we could simply report the mean (or median, or similar) change in future risk over the full range of climate change scenarios or models. However, as we show in the manuscript, this would not provide very useful information, since (as shown in Figure 8) the median change in risk due to climate change would then be reported as a small decrease (under both the low and high sea level rise scenarios). However, Figure 8 provides much more rich information on the large range of uncertainty around the different projections. We feel that including such information provides a lot of information of use to practitioners and scientists alike. We agree fully with the comment that the land subsidence and exposure scenarios have no uncertainty associated with them since we just have one scenario for each of these. We will provide a better reflection on this in the revised manuscript and make the resulting limitations very clear.

Comment 1: 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.

Respond to comment 1: Many thanks for this important remark. In the revised manuscript, we will report values based on sea level rise scenarios from AR5. We are now carrying out the necessary work to simulate these results.

Comment 2: 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.

Respond to comment 2: Thanks for the comment; indeed reviewer #1 also requested more clarification on this point. It seems that we were not clear enough in our original manuscript. We will provide more information in the revised manuscript. In the meantime, a short explanation is provided here. We assumed a constant subsidence rate for the future. This assumption is based on projections carried out by Deltares, in close collaboration with the National Bureau of meteorology (BMKG) and Jakarta Office of Public Works (PU). The current rate is based on investigations by Abidin et al. (2011) over the period of 1982–2010. The decision to assume a linear rate of subsidence is supported by investigations in several other cities over longer time-periods, e.g. Tokyo for 60 years (Endo et al., 2001), Tokyo lowland for 20 years (Aichi, 2008), and Bangkok for 20 years (Phien-wej et al., 2006). Other cities have shown that land subsidence can indeed be reduced rather rapidly once groundwater extraction is reduced. In Tokyo, for example, the government implemented a gradual groundwater extraction policy for 13 years by preventing the creation of new wells and regulating groundwater extraction in the central districts of Tokyo to an absolute minimum (Tokunaga, 2008). As a result, groundwater potential recovered quickly, particularly due to high recharge rates in the region, and the land subsidence stopped in several years. In March 2015, the Ministry of Public Works (PU) of Indonesia issued the "100-0-100 sanitation policy", which means that the government aims to provide 100% of water supply needed by Jakarta by 2019. If the target is achieved in time, we therefore expect that land subsidence would reduce quickly after 2019. Hence the assumption to continue land subsidence until 2025 in the model.

Comment 3: 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.

Respond to comment 3: In the revised manuscript we will clarify methods. The bias correction of the original daily precipitation data was not carried out in this project or paper. We used the bias-corrected data from the ISI-MIP project (Inter-Sectoral Impact Model Intercomparison Project), described in Hempel et al. (2013). We will add a brief description of this procedure in the revised manuscript; however, for details we will refer the reader to the original paper. The description of how we then derived the 100-year return period of extreme precipitation from the former dataset will be provided in greater details (as this is the part of the analysis that is new here). We will also clarify the method used to develop the stage-damage functions, as also requested by reviewer #1.

Comment 4: It would be nice to have a physical visualization of the hazards, vulnerabilities, exposure, and resulting risk over the catchment, to be placed around page 4448.

Respond to comment 4: Good point. We will produce a figure showing an inundation map (representing hazard) for 1 return period, e.g. 100 years (it is not feasible to show all return periods for reasons of space constraints). We will also show the land use map used in the analysis to represent exposure. The stage-damage functions used to represent vulnerability are already shown in Figure 3. We can also add a map of the resulting risk.

Comment 5: 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.

Respond to comment 5: Many thanks for the suggestion. We would prefer to keep this detailed discussion here, since it is used to discuss our results in a broader context. However, we agree that it would be useful to also include a condensed paragraph on this issue in the methods section where we define the scenarios; we will add this to the revised manuscript.

Comment 6: The discussion is very good and really helps balancing the paper. Especially I like the paragraphs on page 4453 and bottom of 4456.

Respond to comment 6: Thank you for the compliment. Here, we describe how the results could be used, even given the large uncertainties we see. To help avoid that readers expect a full uncertainty analysis, we will clarify in the introduction that that is not aim of the paper, and make an explicit reference to the discussion section where we describe potential uses.

Detailed comments:

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

Respond: Good point. We will add a succinct (1 or 2 sentence) description of what the model does in the abstract.

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.

Respond: We agree fully. Dikes and levees (and other structural methods of hazard reduction) remain a key aspect of flood risk management. The point we want to make is that recent decades have seen more focus on also addressing the other elements of risk (exposure and vulnerability). We will rewrite this sentence to clarify and avoid ambiguity.

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

Respond: We will provide the reference and a short description in section Methods on how the stage-damage functions were derived.

P4443, L25: Sentence is too complicated.

Respond: Agreed. We propose to amend to: "The most important single change in the hydrological and hydraulic situation since 2007 has been the completion of the Eastern Flood Canal (Banjir Kanal Timur, BKT). This canal diverts flood waters away from the eastern side of the city. It was not included in the former schematisation of SOBEK, but is included in the new schematisation used in this paper."

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/3/C2252/2015/nhessd-3-C2252-2015-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 4435, 2015.