Nat. Hazards Earth Syst. Sci. Discuss., 3, C2244–C2251, 2015
www.nat-hazards-earth-syst-sci-discuss.net/3/C2244/2015/

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.

Y. Budiyono et al.

yus.budiyono@vu.nl

Received and published: 13 November 2015

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 comment:

In this manuscript, authors presented flood (river) risk assessment in Jakarta for current and future period considering multiple drivers i.e., climatic change, land use change and land subsidence. The paper is based on a previous study by the authors, Budiyono et al. (2014), in which current river flood risk in Jakarta has been assessed by using

Damagescanner-Jakarta risk model. The improved version of the previous model is used to simulate future flood risk here. According to the assessment, the most dominant driver for increased flood-risk in future (2030) is land subsidence. Overall, the presented research is innovative and timely. The major conclusions are highly relevant for flood risk management in Jakarta. Despite the paper is already in good quality, I found several limitations (see below). In order to publish the paper in NHESS, I suggest 'major revisions' taking into account following comments.

Respond to general comment: We thank the reviewer for the thorough review of our manuscript. We are pleased that the reviewer finds the research to be innovative and timely, and the conclusions to be highly relevant for flood risk management in Jakarta. Next to these positive comments, the reviewer states a number of limitations that need to be addressed before publication in NHESS. We thank the reviewer for these very useful suggestions. In the following paragraphs, we outline how we intend to address these points in the revised manuscript. We believe that doing this will greatly improve the quality of the manuscript.

Response to specific comments:

Comment 1: Authors found that direct economic damage for current flood risk is USD 143 million p.a.. They compared the results with previous estimate of Budiyono et al. (2014). However, I do not find any comparison with official flood damage record of a historical flood event. For a reliable estimation of flood damages, this comparison is essential.

Respond to comment 1: Thanks for the useful remark. Indeed, such a comparison is very important. In our previous paper (Budiyono et al., 2015), we included a Table (Table 1) of estimated damages caused by floods in 2002 and 2007, namely USD1.5 billion and USD0.9 billion respectively. These estimates were made by the National Development Agency Planning (Bappenas). For reasons of brevity, we chose not to restate these numbers in the current paper. However, the reviewer makes a good point:

NHESSD
3, C2244–C2251, 2015

it would indeed be easier to make the comparison explicit also in the current paper. Therefore, in the revised manuscript we will compare our new modelled damages with the estimates for 2002 and 2007 from Bappenas. The 2002 and 2007 floods had a magnitude of approximately 50 years. Our Damagescanner estimate of damage from a 50 year flood is USD1.4 billion in the previous schematization and 579 million in the new schematization. This is lower than the reported damages in 2007. It should be noted also that the 2002 and 2007 floods both occurred prior to the completion of the Eastern Flood Canal, which provides protection to large parts of the east of the city. A thorough description and comparison will be provided in the revised manuscript.

Comment 2: The major conclusion of the paper is that land subsidence is highly responsible for increased flood risk in future. However, future land subsidence is calculated based on a strong assumption that current rate of subsidence remain same for the future period. Based on such a strong assumption, providing a quantitative value (173% increased risk due to only land subsidence) might be misleading for decision maker. I understand that authors already discussed their assumption. However, authors should clearly mention that in realistic scenario of land subsidence, this estimation might change significantly.

Respond to comment 2: Indeed, we assumed a constant subsidence rate for the future. This basis of topography map used digital elevation model (DEM) is carried out by Deltares, in close collaboration with the National Bureau of meteorology (BMKG) and Jakarta Office of Public Works (PU), based on the rate by Abidin et al. (2011) ivestigations 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

NHESSD
3, C2244–C2251, 2015

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) in 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 policy target is achieved in time, we expect that land subsidence would reduce quickly after 2019. Hence, the assumption to continue land subsidence until 2025 in the model is most reasonable for Jakarta. We acknowledge that these considerations were not described in the original manuscript, and therefore we understand fully the comment of the reviewer. In the revised manuscript, we propose to describe this aspect in much more detail, and also to state more clearly in the discussion section how our assumption potentially affects the results.

Comment 3: Assessing flood risks for current period, authors have used a land use map of 2002. How a land use map of more than 12 years old can represent current land use? There are plenty of sources for developing land use map.

Respond to comment 3: We agree that the land use map of 2002 is rather old, given that we are now in 2015. However, for this study, we decided to use the official land use maps issued by the authorities in Jakarta, in order to ensure relevance also for practical studies. The most recent of such maps is the land use map of 2009 (which was issued in 2012). We could therefore have used this map, but we note that the land use map of 2002 has consistent land use classess with earlier maps of 1980 and 1995, which could in the future offer potential for the kind of land use modeling that the reviewer refers to in his/her comment 4. We will make the reasons for this selection clear in the revised manuscript. Further, we note that the land use classes in the 2002 map are more in line with those in the 2030 land use plan (compared to the land use classes in the 2009 land use map), which makes the comparison with future land use more consistent. Nevertheless, based on the reviewer's comment, we have also carried out an additional simulation, using the 2009 land use map, whereby the land use classes

NHESSD
3, C2244–C2251, 2015

have been reclassed to be consistent with those for 2002. This resulted in estimated flood risk of USD 186 million p.a. This is higher than the estimated risk using the 2002 map (USD143 million), suggesting that land use change over the period 2002-2009 may indeed have led to an increase in risk. For the revised paper, we will calculate risk using the land use maps for 1980, 1995, 2002, and 2009, and assess the change in risk over time. This will be used to provide a simple extrapolation of how flood risk may change in the future due to land use change, assuming the land use plan 2030 is not implemented (see also response to comment 4 below). In theory, it would be possible to develop our own land use map for current conditions, for example using using LiDAR and Quickbird data. However, to produce 12 land use classes that are consistent with the government issued land use maps would be prohibitive within the scope of this study. Moreover, as stated at the beginning of this response, we want to use the official land use maps issued by the authorities in Jakarta, in order to ensure relevance also for practical studies.

Comment 4: For assessing flood risks of 2030, authors have used official land use plan 2030, which is ideal scenario (not a realistic scenario) of land use. In order to be consistent with other drivers (land subsidence, climate change), simulation based land use scenario is more appropriate.

Respond to comment 4: Many thanks for the suggestion. We do agree that it would be very useful to use a simulation-based approach for land use modeling in Jakarta, to provide a "realistic" future land use map, as well as the idealised scenario. We have searched extensively for such a model in Jakarta, but it is currently not available, and we feel it is beyond the scope of the current study to develop such a model. We did investigate the possibility of using data from global models, such as that of Güneralp and Seto (2013) and Seto et al. (2012). This model was recently used to develop probabilistic land use maps of Indonesia, at a spatial resolution of 1km x 1km, by Muis et al. (2015). However, given the scale of that study, the maps only show the likelihood of each cell becoming urban, and do not have any differentiation between different

NHESSD
3, C2244–C2251, 2015

urban land use classes. For the revised manuscript, we proposed to use a much more simple approach to at least give a first-order impression of how risk may change due to land use change, based on extrapolations based on past land use. We will calculate risk using the Damagescanner, based on the following land use maps: 1980, 1995, 2002, and 2009 (reclassed to be consistent with the maps of 2002 and previously). We will then use the change in risk over this timeframe to project a possible change out until 2030. Whilst simple, this does provide a similar level of consistency as the approach used for land subsidence.

Comment 5: The term 'vulnerability' has multiple notions and in this paper authors have considered only physical based depth-damage functions. Authors should mention this limitation taking into account several important literature such as Cutter and Finch (2008); Cutter et al. (2013); Gain et al. (2015) and Giupponi et al. (2015).

Respond to comment 5: Indeed, the term vulnerability has a whole range of uses within the field, as the reviewer correctly points out. Here, we use the term as it is defined in the Glossary of UNISDR, namely "The characteristics and circumstances of a community, system or asset that make it susceptible to the damaging effects of a hazard". We will make this clear in Method section of revised manuscript. In terms of operationalizing that definition of vulnerability, the reviewer is correct that we only use depth-damage functions, which (partially) represent physical vulnerability. We will add a short discussion on the limitations of this approach, as suggested. In the revised manuscript, we will use the term "depth-damage function", instead of "vulnerability curve".

Comment 6: In my opinion, development of vulnerability map is a complete black-box. After reading the manuscript it is not immediately clear how expert meeting following Fuzzy Cognitive Mapping was used to generate vulnerability curve, along with the method developed by Budiyono et al. (2014)

Respond to comment 6: Our original description of the method to represent vulner-

NHESSD

3, C2244–C2251, 2015

ability appears to be unclear. We did not develop a vulnerability map as such, but rather a set of depth-damage functions to represent (physical) vulnerability for different land use classes found in the land use map. We will clarify this in the Method section of revised manuscript. The reviewer also states that the method used to develop the depth-damage functions is not clear; we thank the reviewer for bringing this to our attention. The procedure is described in detail in the paper of Budiyono et al. (2015), and so we chose to just refer to that paper here. However, we will be happy to provide more detail in the revised manuscript in the Method section.

Comment 7: Authors did not consider changes in the vulnerability for the future period. I understand that this is lot of works. Authors should at least include the work by Mechler and Bouwer (2014) on vulnerability changes.

Respond to comment 7: We agree that potential changes in future vulnerability are very important, and this is a field that is now starting to receive attention. It is a good suggestion to include a citation to the work of Mechler and Bouwer (2014) in the discussion. We will do this, as well as referring to the recent work of Jongman et al. (2015), who found that vulnerability may have been reducing in some developing countries in recent decades. However, at this moment we simply do not have the detailed understanding of how future vulnerability may change in Jakarta (not even the sign of change), and so for the current paper we will leave this out but discuss clearly the limitations of doing this.

Comment 8: Authors have nicely described the implication of the study in the context of Jakarta. However, authors should provide a synthesis about the novelty of the approach to the general readers.

Respond to comment 8: Thank you for the compliments and advice. We will provide a short synthesis on the novelty of the approach as in the risk resulting from the new schematization to show the effectiveness of eastern canal, the risk in the face of global warming and how the two parameters gamma distribution can be used to represent the

NHESSD
3, C2244–C2251, 2015

wide uncertainty and how the average value is used to observe current policy.

I have no technical comments. Respond: Noted, thank you.

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