Reply to the comments:

In blue: reviewers' comments
In red: authors' reply

Reviewer #1, Dr. S. D. Rosen:

General Comments and Remarks

The article of Hiroshi Takagi and his collaborators approaches an important subject, namely the risk associated with subsiding coastal sites under potential flooding of a type similar to that induced by tsunamis. The authors investigated thoroughly a coastal section presently protected by thin coastal dykes and bring a potential relatively cheap solution for reducing the flooding risk to the local population via plantation of mangroves. While the proposed solution may be adequate for a temporary protection of a number of 10-20 years, we believe that it will not provide protection in the long run under the foreseen climate change induced global sea level rise.

A number of specific remarks are presented below and in section b in Table 1 is provided a list of technical and typographical corrections suggestions to the article contents.

We thank the reviewer for the great number of very productive comments and suggestions, which would enable us to significantly improve our manuscript.

1. The nick-naming of the dyke-break induced flooding as tsunami for greater awareness of public is understandable, but because it is misleading due to its prolonged flooding, in this reviewer's opinion, it should not be accepted. Instead, a plain nick name such as "dyke-break extreme flooding" or at least "dyke-break induced tsunami like flooding" would be preferable. If my opinion is accepted all terms in the text should be corrected accordingly.

As described in Lines 24-31, p.2, we coined the term *dyke-break induced tsunami* in order to clearly illustrate to members of the public the danger and phenomenon that could be caused by the rupture of a coastal dyke. Local people seem to be unaware of the dangers posed by this type of sudden violent flood events. For example super strong typhoon Haiyan in 2013 caused a massive storm surge, claiming more than 6000 lives in the Philippines, even though the meteorological agency in the Philippines issued a typhoon warning with a potential *storm surge* height up to 7m a day before the landfall. According to the authors' post-disaster survey, however, a number of local inhabitants could not realize what would happen due to *storm surge* as many of them had only just heard the term for the first time. Many people expressed the view that it would have been better for authorities and media to describe it by a simpler vocabulary such as a *tsunami*.

In this regard, the term *flood* (Indonesian: *banjir*) is unlikely to evoke the real danger that would be caused by a dyke-break event, since local inhabitants may imagine a gradually increasing persistent inundation, particularly in Jakarta. Based on this consideration, we coined *dyke-break induced tsunami* to get people to easily imagine the serious consequences that could arise from the break of a dyke. Also, we expect that the usage of this term may be acceptable, as *dam-break induced tsunami* or *landslide-induced tsunami* are similarly used to describe the danger caused by a sudden movement of a large water mass. The *dyke-break induced tsunami* is considered similar to those expressions. Nevertheless, we do understand the concern raised by the reviewer. Thus, we will clarify this point more carefully in the revised manuscript.

2. It would be advisable that the authors mentioned sea water desalination as a counter action potential solution against land subsidence induced by underground water withdrawal.

We agree. A recent NHESS article by Budiyono et al. (2016) describes so-called "100-0-100" sanitation policy issued by the Ministry of Public Works (PU), Indonesia. We will refer to their paper to emphasize the importance of mitigating underground water withdrawal in the revised manuscript.

Budiyono, Y., Aerts, J. C. J. H., Tollenaar, D., Ward, P. J. (2016) River flood risk in Jakarta under scenarios of future change, Nat. Hazards Earth Syst. Sci., 16, 757-774, DOI: 10.5194/nhess-16-757-2016

3. A fast and significant subsidence rate has been indicated for the recent past years. It is not clear on what basis the same rate is maintained for the coming 10 years as well as for further time states. The subsidence would depend on the soil type of the underground and the thickness of the pervious layers, so it is not necessary correct to extrapolate the same sinking rate for the

future, unless the pervious soil and its thickness give base to this assumption, fact that is not stated.

As the reviewer suggests, the land subsidence appears to be the most difficult issue in projecting floods in a rapidly developing coast such as Jakarta. Since publicly available data is very limited in the country, it may not be easy to project the subsidence rate by a theoretical method (*e.g.* Terzaghi consolidation theory). Thus, we simply assumed that land subsidence will continue at the present rate for a while (at least 10-15 years), resulting in a 2-meter subsidence in the studied area, based on the subsidence rate over the last couple of year shown in Fig.1 and other published articles on subsidence in Jakarta. We consider this projection does not really overestimate things, given the recent rate continues unabated at present.

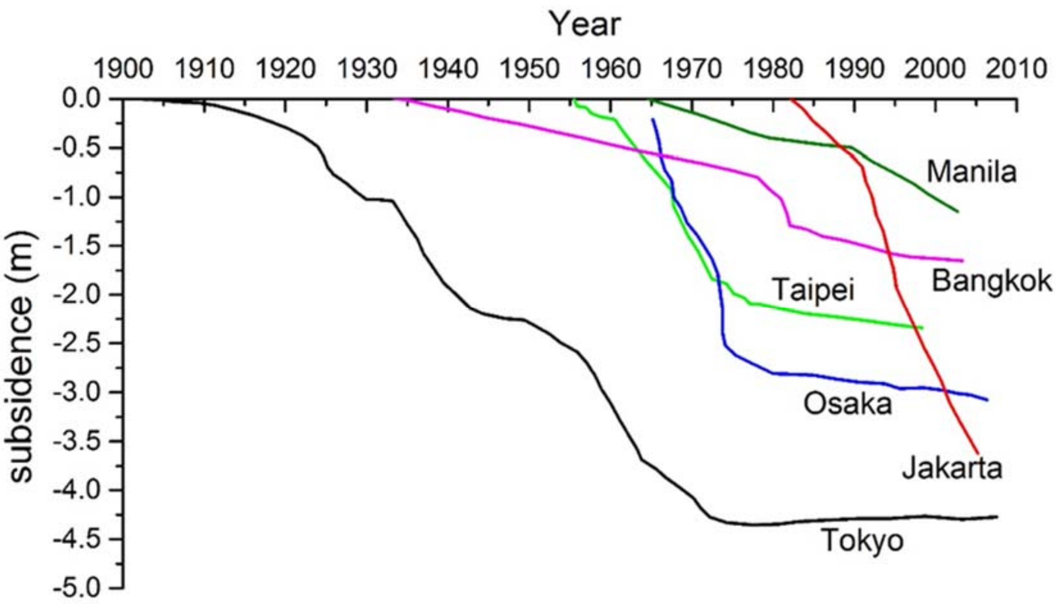

Figure 1: Land subsidence in Asian megacities

4. It is not clear also if the plantation of mangrove forest will be able to provide the expected protection in future. The present water depth in the proposed plantation area is indicated as 50 cm and that the plants grow at approximately as the present sea level rise. However, new publications (e.g. Dutton et al., 2015; De Conto and Pollard, 2016; Hansen et al., 2015; Mengel et al., 2015;) indicate a feasible faster and larger sea level rise globally (up to 1m by 2050, 2-3 m by 2100), in which case the mangrove will not be able to grow at the same rates and provide

the expected protection from dyke-break rapid flooding. Perhaps an engineering alternative could be to adopt the Dutch concept, of building wide sand dunes at the waterline, requiring resettling of population and its activities back to higher and more remote places from the lower sea/water front areas. Another theoretical option might be migration to higher places (Roberts and Andrei, 2015) or that adopted by Miami City in USA (Weiss, 2016).

Thank you very much for raising this very important issue associated with the accelerated pace of SLR. The embankment will end up being submerged under mean sea level, resulting in the loss of its function against potential coastal floods. We will refer to those articles suggested by the reviewer and discuss engineering issues to be solved for creating the mangrove forest and these alternative solutions in the revised manuscript.

5. The criteria proposed following the classification given by Pistrika et al. as well as the one proposed by Wright et al., (2010) seem very problematic as explained further below. Also the data brought by Suga et al., 1995) indicating a safe velocity limit of up to 0.8 m/s in a water depth of about 0.8 m, whereas at speed of 1 m/s was the highest safe limit walking against the current. Based on a research beach bathers survey study carried in Japan with in order to determine safe recreational conditions, that paper stated a safe limit of current speed of about 0.15m/s for knees deep water flow (about 0.5m depth), beyond which bathers could not walk normally or remain stable. Unfortunately, I was not able to find this article published in the "Coastal Engineering in Japan", in the 1980's. In the present article, the authors selected to use a depth velocity product criterion to determine safe passage of pedestrians in a flooded area. The present paper describes a flooding in the Philippines where people could cross a flooded street in a water depth of 0.6 m (knees depth) and while a 0.6 m/s current flow was present. The information provided about the persons particulars is very limited and about which type of street (paved, unpaved, etc) was crossed in the flooded area. This seems already dubious as it is not clear how one was able at the time to measure the current speed, which, if it was 0.6 m/s (based on the Japanesse paper I mentioned), should have been done by a tall, heavy and strong person with perhaps even some cable support from being carried away. A velocity-depth product of 1.0 m^2/s seems already unsafe, if we consider that this product can be due to various scenarios, such as: a depth of 1m and speed of 1.0 m/s (2 knots); a 1.8 water depth in a 0.55 m/s current (1 knot); or a water depth of 0.6m in a current speed of 1.67 m/s. These all lead to same velocity-depth product of 1 m^2/s. A more rigorous approach is the work of Cox et al., 2010, quoted by Pistrika et al., which in this reviewer's opinion is a very important one. We believe it would be appropriate that the authors quote the following text taken from Cox and al., 2010, or

at least refer to it and give a summarizing figure from that publication, copied further below as Figure 1. Since the Cox et al. report is more recent and of broader coverage, and since it refers in greater detail to the various types of persons and ages and floor bottom conditions, even if the Japanese article was right for the wave induced current under open coast conditions with waves and sandy sea bottom, we estimate using the Cox et al. report would be more adequate for use in the present article.

We thank the reviewer for reminding us to those important work which we didn't quote in the first draft of our manuscript Although we consider the report of Wright and his collaborators provides meaningful insights for safety criteria during floods, we do also agree that the criteria cannot be represented by only one single value, given uncertainties associated with various factors. The reviewer's suggested article seems to give a more comprehensive and conservative criteria by taking into account *e.g.* a height and mass product, which enables to understand what would happen to either children or adults. In the next revision, we will also introduce reference to Cox et al. (2010) in a way such that:

"Wright et al. (2010) proposes a depth-velocity product of 1.0m^2/s as the safe limit for pedestrians. *However, a plot of the relationship between human's instability and flow regime appears to be scattered by multiple factors such as surface material; subject actions -either standing or moving-, experience and training, clothing and footwear and physical attributes including muscular development and/or other disabilities; the definition of stability limit (i.e. feeling unsafe or complete loss of footing). Thus, the depth-velocity product criteria suggested by Wright et al. (2010) could become optimistic for some adverse conditions. Regarding physical differences between adult and child, Cox et al. (2010) suggests that for children with a height and mass product of between 25 and 50, low hazard exists for flow values of the depth-velocity product* < 0.4 *m^2/s, with a maximum flow depth of 0.5 m regardless of velocity and a maximum velocity of 3.0 m^2/s at shallow depths (D* < 0.2 *m).*"

b. Table 1 List of technical and typographical corrections suggestions to the article content Fig. 2

We will reply to those suggestions listed on the table in the revised manuscript.