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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Water-level attenuation in broad-scale assessments of exposure to coastal flooding: a sensitivity analysis" *by* Athanasios T. Vafeidis et al.

Anonymous Referee #1

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I have just finished reviewing the paper and I can provide my comments. The paper is very well written and the topic is definitely hot and of interest to a broader audience. However, I am afraid that this is where my positive comments about the manuscript end. The authors use DIVA, a large-scale coastal impact assessment tool to assess the effect of landward water level reduction to the estimated impacts. The addition of this feature to DIVA is definitely an improvement for the tool per se, but I struggle to find how the work brings new knowledge to the community; which should be the case when a new paper is published. The authors report that the estimated damages are strongly affected when the water level reduction is considered, but this is absolutely nothing new. Apart from the fact that common sense is sufficient to reach to the same conclu-

sion there are several previous works proving that (Breilh et al., 2013; Ramirez et al., 2016; Seenath et al., 2016; Vousdoukas et al., 2016). All the above studies show how the static approach overestimates flood extents and some even demonstrate how this affects estimates of number of people affected. Among the above papers there are also large-scale applications (i.e. European scale), which could be the only 'new' element introduced from the manuscript. Even the reduction rates considered constitute no contribution since (i) they have been published elsewhere (ii) and no testing/validating takes place at least to provide some recommendations about suitable estimates to the community. At this stage it is important to highlight that the way the authors deal with coastal flooding is perfectly identical in terms of implementation to a case of inland flooding. The authors just use a DEM and forcing water levels along the coast which is identical to what the inland flooding community does with dynamic models long time ago. There are quite a few global river flooding impact assessments which use dynamic models and demonstrate that the time to leave the bathtub approach has arrived long time ago(Alfieri et al., 2017 and references therein; Winsemius et al., 2016). So starting from the point that all hydrologists and coastal engineers/geomorphologists know that bathtub has the only advantage of being simple in its implementation, the question is what could be done better. Even neglecting all the large-scale implementations of hydrological models, and considering that a simplified approach would be the way to go, the authors seem to neglect basic principles as well as all the progress recently done by the community. - All the arguments about the continuously increasing profiles related to Figure 1 are wrong. Figure 1b is much closer to what most beach profiles look like in nature than Figure 1a. Beaches have dunes, berms, dykes seawalls, etc which are not resolved by a 100 m DEM, but that doesn't make them non-existent. I consider that this point only is enough to reject the work since it raises serious concerns about the methodology followed. - The authors resolve to searching the literature for reported water level reduction rates while the answer is in the basic textbooks. Flooding intensity is attenuated by bottom friction and the community has been estimating Manning friction coefficients depending on land use class for decades. There are also text book

examples on how to relate the land use classes and the water level reduction they drive. The Flood Index Approach is based on the same principle and is very computationally light (Dottori et al., 2016). - Breilh 2013 did an exhaustive comparison of inundation approaches and proposed among others the Volume Integration approach which respects the volume of water contributing to the flood event, something the present effort omits. - As mentioned above, all the discussion until now relates to coastal flooding exercises which in terms of implementation are similar to river flooding ones. Most particularities of coastal flooding are omitted; i.e the effect of waves manifested as wave setup, runup and overwash (Matias et al., 2008; McCall et al., 2010), as well as the interaction between storm surge generation, wave propagation and flooding (Bertin et al., 2014). Projects like MICORE and RiscKit have introduced a lot of new developments on that direction which at least should be mentioned.

Given all the above I have serious concerns about the work being published at this form. The approach is outdated, the paper brings no new knowledge and there is no validation to support any claims. The authors should at least demonstrate that their approach is valid based on some kind of validation against observations. They could also bring new knowledge to the community by recommending testing such an approach and testing against others, highlighting in which cases it could be valid, proposing calibration parameters etc

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