

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

Received and published: 13 November 2017

Having received the last response from the authors I would like to make my final comments in the interactive discussion..

- Our paper does not simply “highlight the importance of accounting for the effects of water level attenuation” but actually quantifies the uncertainty related to these effects in global impact assessments. Importantly it shows that differences resulting from the use of different RCPs are of the same magnitude as differences resulting from the lack of accounting for water-level attenuation.

Reviewer: This is partially true. To quantify the importance of water level attenuation one would need to have a proper estimate of water level attenuation. Instead the

C1

authors use results from the ‘tilted bathtub’ (as some like to call it) but this by far over-simplifies most of the processes related to flooding. So this point is not valid, as the study is comparing two things that are both wrong.

- As the reviewers mention, the need to account for such effects is well known to the community but not implemented due to computational constraints. We agree and would also like to add data constraints to those limitations, which are at least equally important and which we now discuss in detail in the manuscript.

Reviewer: Fair enough..

- Our paper is the first study that addresses exactly these limitations for global coastal impact assessments under a full range of sea-level and socio-economic uncertainty. The study by Vousdoukas et al., (2016) is not global (contrary to what the reviewer suggests) and only assessed exposure for a limited set of physical scenarios (no socio-economic) and a single event. - The main reason why no other study to date has carried out a global impact assessment using a hydrodynamic model is, besides the input data volume, the large number of runs that one would need to conduct for the analysis. Using a hydrodynamic model in our study would have involved at least 60 runs (20 time steps for three physical scenarios), with the number increasing greatly when additional scenarios (including socio-economic ones) are considered. The reviewers might be unaware of the above data and processing limitations for impact assessments when suggesting the use of hydrodynamic models (implemented e.g. through the use of specific Manning values). - We have conducted a sensitivity analysis (which is a form of validation), exactly because our study is global and involves future projections for a range of scenarios. - Including land use information (that both reviewers suggest or imply) in our assessment (e.g. for deriving Manning values) is not possible as there are currently no global consistent land-use scenarios available. Contrary to what the reviewers suggest, (as we discussed in our response to the first reviewer), such data are not readily available. Even existing current global land-use data (not scenarios) suffer from severe limitations (e.g. resolution, relevant land use classes) when it comes to

C2

representing narrow coastal strips. On a more general note, we would like to point out that the use of hydrodynamic models does not always guarantee improved results, as explicitly stated by Vousdoukas et al. (2016). The bathtub model has so far been the only option for global impact assessment as it can produce reasonable results for many locations and under specific conditions (as clearly stated in all the references cited by the first reviewer); and is representative of maximum potential impacts. Importantly, it is questionable whether a more detailed representation of the physical processes (in this case flooding) is the most urgent requirement of impact assessments as there are many sources of uncertainties (data, physical scenarios, socio-economic scenarios, damage estimations) that may as well be of equal importance. Most of the papers that have been cited by the reviewers support this argument by underlining the above uncertainties. Our work is a first step towards developing a simple model for assessing flooding at global scales, by quantifying one important source of uncertainty. Importantly it suggests the possibility of emulating physical processes through the application of simple correction factors and a typological approach.

Reviewer: In all these arguments there are several statements which are either partially true, or partially relevant. For example, there are global land use datasets. They may have issues with accuracy and resolution but claiming that using a fixed coefficient for water level reduction is better than considering land use maps is at least a strange argument. Both reviewers do not question that the study is the first global assessment of the kind, we just both doubt about the validity of the findings. Any implementation of bathtub (tilted or not) or other simplified model on the existing global DEMs can be sufficient to address relative changes in future flooded areas, affected people, or damages. But the applicability of the method kind of stops there. Trying to quantify the uncertainty comparing to shaky flood models and without any validation is far fetched. Especially because the community has showed that we can approximate better the problem of coastal flooding. Sensitivity analysis is not validation. No other study has carried out a global COASTAL impact assessment using a hydrodynamic model or at least a more complex method, but there are several ones on river flooding. This goes

C3

back to my previous comment on which the authors gave an incomplete reply. For sure coastal flooding is different than river one. In my opinion the benchmark is set by the studies of Bertin 2014, or McCall 2010, but these cannot be applied in large areas. Still anything beyond any bathtub solution would be a step forward. If the river community does it at global scale (CaMaFlood, GLOFRIS, GLOFAS) how can the authors claim it is not possible for coastal?

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2017-199>, 2017.

C4