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

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We appreciate the effort and comments of the anonymous reviewer upon which we were able to realize that we have not presented our research question and results in a sufficiently clear manner. We have now improved the clarity of our presentation, specifically with respect to highlighting the novelty of our approach and results. Below, we respond to each of the points raised (reviewer's comments are included in quotation marks):

"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

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Discussion paper

that common sense is sufficient to reach to the same conclusion 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."

We agree with the reviewer that proving that damage estimates are strongly affected, when the water level reduction is considered, is nothing new and is, to some extent, common sense. In our paper however, we quantify this effect within global sea-level rise impact assessments, which has never been done before. The papers that the reviewer cites above (some of which are also cited in our manuscript) have not addressed this issue. Specifically:

- the cited studies do not assess flood damage, they primarily focus on the flood hazard and/or model single events

- none of the studies cited are global

- none of the studies include a range of physical and socio-economic scenarios (with the exception of Seenath et al. 2016 who only use two sea-level scenarios)

Our paper addresses those points by performing a sensitivity analysis using the range of published water-level reduction factors. Our findings are new as no study has demonstrated, among others, that the range of uncertainty due to the omission of this factor in global scale impact assessments is larger than that introduced by sea-level rise scenarios; that this omission can affect the spatial patterns and distribution of impacts; and that the inclusion of this factor can lead to a shift in the timing of impacts.

"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."

In principle, this point is well taken, but we disagree with the conclusions. Generally, this point relates to an old debate between local scale and global scale modellers and judging from the reviewer's comment, the reviewer probably belongs to the former camp. The essence of this debate is: If you zoom into a specific unit (here coastal profile) of a global model, the results can differ from those of local analysis. However, on broader scales these differences tend to average out as local effects are not as dominant.

Irrespective of this debate, the reviewer's statement regarding our working assumption (i.e., "continuously increasing profiles") being wrong is not supported by the data. We have explicitly addressed this point in the manuscript and our results show that this is a perfectly valid assumption, with respect to the resolution of the elevation data. We must also note that the caption to Figure 1 explicitly refers to the "coastal profile" and not to the "beach profile", hence defining the scale of interest. Indeed, areas potentially flooded can extend several hundreds of km further inland than suggested by the reviewer, again making clear the potential scale of the features under investigation. Accordingly, the comment regarding coastal characteristics not being resolved in a 100m (or higher) resolution DEM is directly related to the scale of the study – this is the highest resolution that has been used in global studies and that is also used in some of the studies cited by the reviewer (e.g. Ramirez et al. 2016), where it is termed as hyper-resolution). The reviewer is thinking about processes which currently cannot be resolved at global scales as it is simply not possible (computationally) to use data of much higher resolutions, such as those employed in the local studies, e.g. Breilh et al. (2016), that the reviewer cites. More importantly, such data for coasts currently do not exist at global scale!

“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).”

We fully agree with the reviewer regarding his comments on global river flooding – however, we believe that it is not good practice to simply equate coastal flooding with river/inland flooding. At the coast we are able to bring to the analysis a collation of observations (Table 1) which are specific to coastal wetlands. The significance of these observations are discussed in more detail below. The representation of processes that the reviewer refers to is currently the reality of global studies on coastal impact assessment. Lack of consistent global-scale coastal data and processing limitations when conducting the large number of model runs required for impact assessment (due to the different scenarios and time steps) have not yet allowed for additional complexity and there are currently no other global studies accounting for further processes (this is exactly what we are trying to do in this paper). We are also currently working on those aspects and hope to be able to address some of the above points in the near future.

The reasons for this gap are discussed in the manuscript (but also in other published work, e.g. Vafeidis et al., 2008; Hinkel et al., 2014) and are related to the lack of consistent data at global scale and to the computational costs for conducting the large number of model runs (due to the different scenarios and time steps) that are required for comprehensive impact assessments. Some of the studies that the reviewer cites (e.g. Seenath et al. 2016) also agree on this point and do not dismiss the bathtub method, pointing out that it can provide good results for some locations. Further, Seenath et al. correctly mention that hydrodynamic models also face several limitations when applied at broader scales.

“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 paper examples on how to relate the land use classes and the water level reduction they drive.”

Our literature search aims to inform our sensitivity analysis. We are of course aware of Manning friction coefficients, which have been widely used to characterise these processes, although the range of values of Manning’s ‘n’ (0.035 – 0.15 for floodplains) serves as an indication of the uncertainties involved. However, using such coefficients in a global study is far from trivial due to a range of issues, such as: the availability of consistent high-resolution global land use data, particularly for coastal areas where land-use maps show the highest errors; the empirical nature of the coefficient values, which greatly inhibits its use for global scale studies; the lack of information/values on different types of surfaces (e.g. urban surfaces at settlement or city scale) at global scale. Finally recent studies clearly show that water attenuation can have significant spatial variation even along the same land-use type (e.g. Stark et al. 2015). More fundamentally, the Manning’s n approach may not be the most appropriate method to account for energy and momentum losses due to vegetated features. This is because the actual resistance to flow is not only through bottom friction but also from the form drag of plant architecture (stems, branches and leaves), particularly when the vegetation is emergent. Furthermore, there is the additional effect of reduction of surface winds in the presence of vegetation. Thus Table 1 presents a wholly new compilation not reported in any basic textbook, as far as we are aware. We report field studies of actual water level reduction rates, alongside numerical modelling efforts, to strengthen our analysis. Our study exactly emphasises in a quantitative manner the need of better understanding on how different types of land use attenuate water levels.

“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"

We have addressed these points in our responses above and we believe that our findings are new and relevant for those working in the field of global coastal impacts and vulnerability. The reviewer may not be aware that for coastal sites at a global scale there is limited information available. Regarding the issue of validation we would like to repeat that our study presents a global sensitivity analysis (which is a form of validation); is based on a well published and evaluated global modelling framework; and is limited by validation issues that are common to global assessments that involve future projections. Finally we would like to stress that the main aim of the paper is to quantify uncertainty in global coastal impact assessments due to not accounting for water level attenuation. Our results show that uncertainties in impact assessment resulting from this omission are of a similar range to uncertainties related to total sea-level rise and provide an assessment of the large spatial and temporal variations that may result. For this purpose, our methods can provide important results to inform future adaptation policies at regional to global scales. It is still a question to be answered whether and to what extent a better representation of the physics would provide significant further insights in this context, particularly when considering the data limitations at global scale, and we are currently conducting research to explore this question.

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