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Interactive comment on "New estimates of potential impacts of sea level rise and coastal floods in Poland" *by* D. Paprotny and P. Terefenko

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We would like to thank the referee for his comments. We have revised the manuscript following the reviewer's suggestions, where necessary. The response to each of the comments is presented below, using the reviewer's grouping into "main weaknesses", "detailed comments" and "other comments". The revised manuscript is also attached.

Main weaknesses, #1: "the fact that uncertainties are just listed and not really quantified, although they must be very large". We have accordingly rewritten the discussion, creating section 4.1 "Uncertainties" to incorporate more detailed information. As far as was possible, we provided quantification for inaccuracies originating in the digital elevation model, topographic objects database and damage curves, as well as assumptions

such as the static "bathtub" method and uniform sea level rise along the coast. We added uncertainty bounds to Table 4 and added Figures 9 and 10 to the discussion.

#2: "a static vision of sea-level rise and the dynamic response of coasts (sedimentation, erosion)". As we now note in the discussion, a method adequate to the resolution and scope of the study is currently lacking, and the link between sea level rise and the response of the coast is still under investigation.

#3: "some intrinsic contradictions between the main conclusions (abstract) and result presented". The comment is related to the four "detailed comments", as follows:

1. "sea-level will not stop rising by the end of the 21st century. Therefore, it is just a matter of time that the impacts of sea-level rise become more significant". The abstract was tweaked to specify that we meant that "sea level rise or storm surges are unlikely to reach intensity required to cause significant damage to the economy or endanger the population" in the context of the 21st century. Naturally, in a very long perspective the sea can reach enormous levels, as it did in the past, but going beyond the next century seems to be a stretch given the numerous potential pathways of future climate change.

2. "the authors say that the impacts are lower in their analysis than in previous exercises (. . .) the difference is only a factor of 2 to 4, which is to my opinion lower than the uncertainties of the results". Uncertainty range was added to Table 4, as well as Figure 9. We see that there is still a big difference to other estimates, especially regarding population and assets. We also added an additional scenario from Zeidler's study for better comparison.

3. "the DIVA model is based on a different approach (Bruun rule), whereas the present paper does not consider erosion". Notwithstanding the doubts of the applicability of Bruun's rule, it is unlikely that the inclusion of dynamics in DIVA-based studies made much difference for the results, at least in case of Poland. As we note in the discussion now, due to the use of a coarse global DEM that misses most coastal barriers the

impact of erosion on the results is probably minuscule.

4. “the stability among scenarios seems not consistent with the conclusion (page 2518 lines 10 and following)”. We don’t think there is inconsistency, as it is clearly stated that the “stability” refers to the structural breakdown, not the magnitude of the event.

Other comments:

1. “Section 2.2.1: Digital elevation model The term "accuracy" seems used here instead of "precision"”. The text was modified to clarify that we meant the mean error of the DEM.

2. “Line 15 page 2500: I suppose this refers to the accuracy of the positioning of assets”. That is correct; the text was modified to clarify that.

3. “Lines 23 and following paragraph page 2501: this section is confusing at one may think that the increments used are lower than the precision of the DEM (?). I suggest clarifying.” It is clear now from section 2.2.1 that the precision of the DEM is not lower than the increments.

4. “Lines 5 and related paragraph page 2502: there is a strong assumption in this approach: it is assumed extreme water levels are even along the coast of Poland, which is most probably not the case. I suggest discussing this point in section discussion.” The point was added to the discussion in section 4.1.

5. “Section 2.4: I suggest clarifying what types of costs are considered here (direct tangible costs) e g using the framework of : Hallegatte, S. (2012)”. We used the paper suggested by the reviewer to clarify the kinds of costs covered by our study in section 2.4.

6. “Results: the authors should be careful in providing an appropriate number of significant digits in their quantified results”. The “insignificant” digits were left were the numbers came directly from the sources of data and rounded were necessary when they were our own calculations (e.g. in Table 2 build-up area value was rounded to the

nearest thousand, as we estimated those values, as opposed to other land use types that were taken from official statistics).

7. “Figure 6 should also indicate the surface area for each bin (not only percentage).” The graph was added to Figure 6, as suggested.

Additionally, we have corrected some typos and language mistakes throughout the work and corrected the references list.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/3/C1522/2015/nhessd-3-C1522-2015-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 2493, 2015.