



Interactive
Comment

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

Anonymous Referee #2

Received and published: 18 July 2015

This paper provides a new estimation of sea-level rise impacts in Poland. It represents a substantial contribution to the understanding of natural hazards and their consequences as it brings new data. The authors also clearly present difficulties arising from regional to national scale assessments of sea-level rise, and present an adequate list of related uncertainties. They also use damage functions for a large number of assets. Therefore, I think that this study can be useful as the regulation requires such assessments at least in Europe, and also provide a concrete example of quantification of coastal risks (=hazards*vulnerability*exposure) as sea-level rises.

I feel that the main weaknesses of the manuscript are: (1) the fact that uncertainties are just listed and not really quantified, although they must be very large; (2) a static vision of sea-level rise and the dynamic response of coasts (sedimentation, erosion);

C1293

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



(3) some intrinsic contradictions between the main conclusions (abstract) and result presented.

These weaknesses could be overcome by rewriting the article and extending the discussion section (in particular pages 2515 and 2516) into a real review of the main sources of uncertainties, how they are addressed in the scientific literature and in other countries similar exercises, and providing some quantified assessments of the uncertainties to the extent this is possible. Overall, this represents important revisions to the manuscript.

Detailed comments:

Comments on the conclusions: In the the abstract, the author write: "The results revealed that even by using a static "bathtub fill" approach the amount of land, population or assets at risk has been significantly revised down. Sea level rise or storm surges are unlikely to reach intensity required to cause significant damage to the economy or endanger the population. The exposure of different kinds of assets and sectors of the economy varies to a large extent, though the structural breakdown of potential losses is remarkably stable between scenarios."

I think that the authors should reconsider this statement:

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. Therefore, I strongly encourage the authors to address this temporal dimension of sea-level rise impacts in their final article.

2. In addition, the authors say that the impacts are lower in their analysis than in previous exercises. However, the difference is only a factor of 2 to 4, which is to my opinion lower than the uncertainties of the results. If the authors could provide order of magnitude of the uncertainties of their results, they could conclude on this point, but it is not possible at present.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

3. In addition, the DIVA model is based on a different approach (Bruun rule), whereas the present paper does not consider erosion. Hence, it is not surprising that the results are different.

4. In addition, the stability among scenarios seems not consistent with the conclusions (page 2518 lines 10 and following).

This section of the abstract and the related text should be rewritten.

Other comments

Section 2.2.1: Digital elevation model The term "accuracy" seems used here instead of "precision". I recommend to clarify what represent the values provided for the precision (?) of the DEM (RMSE?).

Line 15 page 2500: I suppose this refers to the accuracy of the positioning of assets

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.

Lines 5 a,nd related pargaph 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.

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). A framework to investigate the economic growth impact of sea level rise. Environmental Research Letters, 7(1), 015604.

Results: the authors should be careful in providing an appropriate number of significant digits in their quantified results (e.g., in table 2: Wastelands and other lands 12 337PLN: the 337PLN are just noise).

Figure 6 should also indicate the surface area for each bin (not only percentage).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



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

NHESSD

3, C1293–C1296, 2015

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C1296

