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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The use of genetic programming to develop a predictor of swash excursion on sandy beaches" *by* Marinella Passarella et al.

Anonymous Referee #1

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I have read the manuscript with pleasure and a lot of interest and I think it is an important contribution. The manuscript is also well written and the presentation of the results is good. Therefore I am very favorable to its publication in your journal I only have some minor comments/suggestions which I feel would improve the manuscript. The authors compiled a new wave run up dataset, by extending the already broad Stockdon et al dataset with other measurements. Following they apply machine learning to conclude that the latter performs better in predicting wave run up heights. Machine learning in general have been already shown to be very capable predictors of wave runup. One of the earliest examples can be found here (https://link.springer.com/article/10.1007/s10236-011-0440-5) and maybe the most re-

cent before the present work is Abolfathi et al 2016, with all studies reporting very good results. Since empirical run up formulas are simplifications of the actual processes deriving site specific 'recalibrations' of existing wave run up formulas has been considered a recommended practice. That way at least the effect of some unkown parameters is reduced. I would recommend the authors to mention that somewhere when they discuss previous studies, since among the recent ones mostly Stockdon 2006 aimed to propose a universal parameterization. Wave setup is part of run up and is driven by wave breaking. The latter is controlled by the nearshore beach slope (and not of the beachface only) a parameter which most times remains unknown, among others. Moreover, infragravity motions and wave setup are not the same thing but they could be confused in some field measurements. The authors could elaborate on these aspects when they discuss infragravity parameterizations. The weak point of machine learning techniques is that their predictive skill is limited to the conditions covered by the parameter space of the training dataset. GP is superior in that aspect to ANNs, since the final product is a relationship that is based on parameterizations which were derived considering the physical processes. At the same time it is not meant that the coefficients estimated will result in reasonable results beyond the range of the training dataset. In this case the training dataset is quite extensive but given that most of the global coastline is not included, it is not for granted that the solution could fail in other parts of the world. All this is not criticism, I just think the authors should discuss the above points. In addition I believe that it will be helpful for the reader to provide information about the range of input parameters for which the formulas are valid (maybe in form of a table).

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