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Natural Hazards and Earth System Sciences
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# *Interactive comment on* "The role of the reef-dune system in coastal protection Morelos (Mexico)" *by* Gemma L. Franklin et al.

**Anonymous Referee #1**

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Review of "The role of reef-dune systems in coastal protection in Puerto Morelos (Mexico)" by Franklin et al. This paper presents an analysis of the combined impact of reef and dune degradation on determining storm impact. In general I found the paper interesting and conclusions primarily supported (and very timely given recent events), but the analysis a bit lacking. See my detailed comments below, but my general recommendation is that this paper needs a major revision prior to publication. The numerical simulations conducted by the authors can provide much more information about what is causing the observed runup extremes and it would be good to delve a bit deeper into what is going on.

Specific comments 1. Pg 3, lines 5-15. A majority of this information is not relevant, e.g. annual temperature and rainfall do not impact the runup.



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Discussion paper

2. Pg. 4, line 10. Please specify which model.

3. Pg. 4, line 13. A high r2 does not indicate model performance unless coupled with the regression slope. The r2 only tells you how well a model reproduces the variance.

4. Section 3. I do not see the point in including the flume experiments in this paper. You are essentially calibrating the model on an unrelated data set for a reef/beach profile that was not made to replicate your field site. Essentially you are just showing that SWASH works on reef profiles which has already been shown (Zijlema, 2012, Buckley et al., 2014). Additionally, and while it is unfortunately the case, showing that the model is calibrated at one site does not mean it is calibrated at all other sites. As a result, my preference would be to entirely remove the discussion and comparison with the flume results and use the extra space to further develop the results as they result to the field site. Also I found the discussion of the runs with and without the reef crest confusing.

5. Figure 1. Can the inset be made a higher resolution and zoomed out a bit to provide more geographic context?

6. Pg. 6 line 5. How long is each simulation run for? This is important in determining the validity of the statistics which include long waves.

7. Page 6 line 26. Extreme runup is defined inconsistently.

8. Page 6. Line 30. I am confused about the definition of Rlow. The setup is the average runup so why is this no Ravg? Also it would be helpful to remind the reader that here Z is the tidal level.

9. Page 6 line 32. As is sort of acknowledged in the discussion, not including surge is a huge limitation of the approach. As the depth of reef submergence directly effects the short wave transmission across the reef the surge is critical in determining the runup (in addition to the fact that the surge adds to the water level from which waves runup). Could you not include this for the simulations suing the hycom model? I find this a major limitation of the current study. High surge also acts as a proxy for the reef

degradation, and thus neglecting surge probably causes your results to underestimate the occurrence of over toping.

10. I think the results section could be considerably beefed up. By using a phase resolving model you allow for a lot of information on the runup dynamics to be gleaned. As has been demonstrated in the available literature reef/lagoon systems can often act as open basins and thus have the potential to enhance/trap IG energy.

11. I like the inclusion of the dune height in the analysis but wonder if treating the dune as an un-erodible feature underestimates the overtopping.

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