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Interactive comment on "Threshold effects of hazard mitigation in coastal human–environmental systems" *by* E. D. Lazarus

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This manuscript is a well-written and coherently organized contribution to discussions concerning the coupling of human interactions to environmental processes. The catalyst for this contribution is the paradox that although society seems to gain more insight into the fundamental dynamics of natural disasters from both the physical and human system perspective, the financial cost of disasters is rising. The author describes three hypotheses to explain this effect. The first is that the natural forcing is increasing in strength. The second is that the economic forcing is increasing. The final hypothesis is that mitigation filters small-scale events resulting in increased large events. The author then provides a toy-type model to illustrate how mitigation in the form of threshold-guided prevention of disturbances can alter the damage distribution

when driven by a consistent natural forcing. I recommend publication of the manuscript after addressing a few issues below – mainly for clarification. The manuscript warrants publication because it provides a much-needed context for future work on coupled human-environmental systems. While this work, particularly the model itself, does not contribute to answering the paradox it frames the issue in a way that clearly illustrates how follow-up efforts can help to resolve the impending problem of rising natural disasters.

With respect to needed clarifications, first I suggest that the author define more clearly marked bounds between the hypotheses. Specifically, the second hypothesis associated with rising value of vulnerable human agency needs to be clearly distinguished from the third hypothesis. To be distinct the rise in value must be uncoupled from the natural system. It many cases, particularly along the coastline, the increasing value of property is a direct result of human mitigation. If storms and erosion took their natural course the value of coastal property would be much different. To the extent that damages rise along a coastline because of increased property value, this then falls into hypothesis three. Hypothesis two must be clearly stated as a rise in value unrelated to mitigation.

My other suggestion for clarification is to point out very specifically that the presented model does not represent hypothesis three. The model can be thought of as an ordinary differential equation for wall strength: dWs/dt = -(p+S-(WsWh)). Because the deterioration rate, p, is a negative exponential function of Ws the rate of change of wall strength is a strong nonlinear function of wall strength. The smaller the wall strength the much more rapidly it deteriorates. The much more rapid reduction in wall strength leads to larger damage events. So from the outset the model has built in a distribution of changes to wall strength that have very high probability of large changes. Therefore, when the manager decreases the threshold needed to fix the wall, the system filters to high damage. Unlike hypothesis three, the large damages do not increase because of the filtering. In the present model the distribution remains the same, the manager just cuts off certain portions of the distribution. In hypothesis three, the act of cutting off the distribution changes the probability of large events. I ask the author to just clarify this point in the paper.

As pointed out by the other review, I would also ask the author to clarify the final statements in the manuscript about coupling strength. I too am not sure that coupling is reduced as described. It is very important to distinguish the variables that the author is referring to in the coupled arguments.

A few other minor clarifications:

1) Figure 5 caption states that the plots show time series of wall strength and storm damage. It appears to me that the green lines are actually the positive fixes to wall strength and not all of the damage events. Damage events would be D=S-WsWh and those seem to occur more often than indicated by the green lines.

2) Figure 5 shows what is referred to as parameter space plots with trajectories. The axes of these plots are not parameters they are model variables. As such these seem to be phase space plots. However, they are not even really phase space plots because damage is just a function of Ws. The only dynamical variable in the model appears to be Ws. I recommend removing these plots to reduce confusion.

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