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Interactive Comment

Interactive comment on "Block and boulder transport in Eastern Samar (Philippines) during Supertyphoon Haiyan" *by* S. M. May et al.

Anonymous Referee #2

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<General comment> This paper presents solid evidence of giant boulder transport on a reef-lined coast of Eastern Samar during Super typhoon Haiyan. The authors carefully estimate physical properties of each boulder and discuss minimum flow velocity required for its movement in different modes using existing formulas. Based on wave modeling, they show that the boulder transport cannot be explained with the phase-averaged wave model, suggesting that infragravity waves play a key role in the transport processes. The paper overall is well written and their findings will be a benchmark for future studies on reef platform boulder transport. However, their results of wave modeling show a significant discrepancy from previous results by others and there are some points that they should address and explain better. Therefore, I recommend publication after the following comments are addressed.

<Specific comments>

1. P.747, L10: How did the authors determine the coefficients of drag, lift forces and static friction for each boulder? The coefficients should be different especially between round-shaped and slab-shaped boulders. The static friction should also be different on the beach and terrace behind it. Please discuss uncertainty associated with the choice of these coefficients.

2. P.750 L2: b and c in equations (1)-(3) are defined as the second longest axis and the shortest axis. I think they are not appropriate definitions as the force balance would become independent of the boulder direction to the flow. The choice of b and c may significantly affect the minimum velocity especially for the elongated boulder such as ESA9.

On the other hand, a, b, c are defined as length, width and height in 4.2. The definitions should be consistent throughout the paper.

3. P.750, L16: Is it appropriate to apply Nandasena's equations for ESA5? It was originally located at the cliff edge where flow velocity could have a vertical component locally, or wave splash-up could exert impact force on it.

4. P. 751, L3:The maximum significant wave height of 4-6 m off the coast is too small for one induced by the super typhoon with extreme winds. Many others commonly estimated the value between 15 and 20 m. Roeber and Bricker estimated it as 19.7 m off Hernani. This discrepancy is beyond the range of uncertainty of wave hindcast model.

5. Figure 8c: When was the maximum wave height resulted at ESA site? I suggest an additional figure of wave height distribution in the same area as Fig 8c at the timing of the highest wave development. There is no information provided on local wave characteristics and how much waves were underestimated by the phase-averaged model.

6. Figure 8c: The velocity field developed along the coastline looks mostly due to storm

surges and there seems to be very small contribution from the wave-induced velocity. The authors emphasize the agreement of the flow direction and boulder trajectories implying that the boulder transport is attributed to the flow (P.752, 13). This sounds a bit contradictory to the later discussion on the importance of infragravity bore-like waves which is lacking in the model. Please explain more on this.

7. 6.2: I agree with the authors that the extreme flows on the coast cannot be explained without the presence of infragravity waves, which were also illustrated by Roeber and Bricker (2015) and Shimozono et al (2015). A question arises as to whether the bore-like waves similar to one observed in Hernani can be generated in shore-parallel direction because the large-scale boulder transport occurs along the shore. It may be worth mentioning this point.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 739, 2015.