Earth Surf. Dynam. Discuss., 3, C370–C375, 2015
www.earth-surf-dynam-discuss.net/3/C370/2015/

Earth Surface Dynamics
Open Access
Discussions

ESurfD
3, C370–C375, 2015

Interactive Comment

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

S. M. May et al.

mays@uni-koeln.de

Received and published: 19 October 2015

We thank reviewer 2 for the critical comments/suggestions. We will consider these comments in the revised version. The major and minor comments given by reviewer 2 will be addressed in the following.

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.

Author response: We agree with reviewer 2. Since we cannot present empirical data on e.g. the coefficient of static friction or lift, we – as most previous studies – generally rely on the use of coefficients taken from literature. However, some authors give certain ranges (with min and max values) for these coefficients, the use of which apparently results in different flow velocities required for the transport of the clasts: Several authors have also used 0.7 for the coefficient of static friction (Noormets et al., 2004; Paris et al., 2010; Nandasena et al., 2011); Benner et al. (2010), give values of 0.6 or 0.65-0.8 as possible values for static friction, which only slightly differ from 0.7. However, Nott (2003) refers to the empirical study of Fukui et al. (1963) and introduced friction factors between 0.82-1.02. When applying min and max values given in Nott (2003), the results (i.e., flow velocities required for the transport of the clasts) change by the order of 0.2 m/s. For the coefficient of lift we used 0.178, similar to several previous studies (Nott, 2003; Noormets et al., 2004; Paris et al., 2010; Nandasena et al., 2011). However, Benner et al. (2010) state that every value between 0.05 and 0.2 may be realistic, with the maximum value being close to the one used in our study and in the previous studies. When applying the minimum value given in Benner et al. (2010) (0.05), then flow velocities would increase by 0.5-0.7 m/s for rolling/sliding, and up to ∼20 m/s for saltation/lifting. We thus think that, for the estimation of minimum flow velocities, the values for coefficient of lift used in our study are conservative values. However, as to the coefficient of drag (Cd), we introduced boulder-specific values in the revised version: Noormets et al. (2004) refer to Fig. 3 in Helley (1969) illustrating the relationship of the shape of a clast transported by flowing water (i.e., expressed by the Corey shape factor) and the Cd it experiences. Accordingly, different shape factors are calculated for ESA 7 and ESA 9 (0.73 and 0.55), and thus different Cd values may be inferred when following Noormets et al. (2004). Applying these boulder-specific shape factors and taking the boulder-appropriate Cd factors from Fig. 3 in Helley (1969) (ESA 7: 0.85-1.15, ESA 9: 1.4-1.8), minimum flow velocities necessary for the transport of ESA 7 increase by 1.5-2.5 m/s, for ESA 9 to 0.3 and 1.2 m/s. We will incorporate the most important aspects of this discussion in the revised text to clarify the choice of the various coefficients. Since boulder-specific values for Cd can be inferred, we will include these new values in the revised version.

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.

Author response: We agree that the definitions of a, b and c axes should be consistent throughout the paper, and that the denominations “longest, second longest and shortest axis” may be misleading since they are disregarding the position of the boulder to the flow. We have thus changed the definitions given in line 2, page 12, to “width” and “height”. However, in all cases, the longest axis of the investigated clast referred to length (a-axis), the second longest axis was the width of the boulder (b-axis), and the shortest axis was the height of the boulder (c-axis). The values used for the boulder axes in the Nandasena equations are thus in the correct relation to force balance.

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.

Author response: The calculation of flow velocities for joint-bounded ESA 5 using Nandasena’s equations are indeed related to uncertainties. We have mentioned that JBB scenarios tend to produce overestimated values, and that “discrepancies may for instance be related to the overestimation of strain forces between the block and the strongly karstified reef body, or to the underestimation of the waves’ impact and lift forces approaching the cliffs and their associated jets (Hansom et al., 2008)”. On the other hand, one may also assume that ESA 5 was already submerged during the initiation of motion due to storm surge and wave setup and resulting elevated water levels, and vertical jets/wave splash-up may have thus had a reduced influence; little is known

ESurfD

3, C370–C375, 2015

Interactive Comment

about flow velocity amplification along cliffs under submerged conditions. However, for this reason, the value derived for ESA 5 by the JBB scenario is not used in the following discussion; we have used the minimum velocities calculated for ESA 7 and 9 for comparison with the survivor video and the recently published models of infragravity waves.

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.

Author response: We agree that the discrepancy is beyond the range of uncertainty if we compare it with Roeber and Bricker and others. We are currently re-evaluating our model to explain/reduce the discrepancies between our model and the previously published ones, and we will incorporate the re-evaluated model results into the revised version of our manuscript. However, despite the mentioned discrepancies, the conclusions inferred from the previously published models are similar to the ones presented here – flow velocities (as a result of pressure and wind driven storm surge and wave setup) remain clearly below those required to transport the clasts at ESA. We thus feel certain that the main conclusions of our paper will remain the same, even if based on the re-evaluated 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.

Author response: We thank reviewer 2 for this comment; although the wave height is differing from the previously published models (re-evaluation currently under way), the timing of maximum significant wave heights in our model is generally in agreement with the generation of the infragravity waves video captured at ~6 a.m. PHT (local time), and thus with the occurrence of max. wave heights at Hernani: Figure 8a shows max. significant wave heights at the timing of the highest wave development, which started – according to our model – at ~5.10 a.m. PHT (depicted in Fig. 8a) and lasted until ~6 a.m., while highest flow velocities (due to pressure- and wind-induced setup) occurred delayed (Fig. 8b,c). This is in agreement with the information given in Roeber and Bricker (2015), stating that modeled offshore wave heights dropped rapidly after 6.a.m., "even though the pressure- and wind-induced setup persisted. Since the pressure and wind-driven setup lagged the offshore sea state, only a short time window existed for the surf beat to reach its most destructive form" (Roeber and Bricker, 2015: 8). However, due to the comparably low resolution of our model, in our opinion it is not necessary to present a further subfigure showing wave height distribution in the same area as Fig 8c at the timing of the highest wave development; no additional information would be provided. We will nevertheless add a note/further explanation on the timing of max. wave heights (see above). We will also add some information on local wave characteristics into the revised manuscript.

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.

Author response: We agree with reviewer 2 that our text implies a relation between the flow direction illustrated by the Delft3D model and the boulder trajectories, and we agree that this sounds contradictory since the high flow velocities must be explained by the occurrence of infragravity waves. However, we have to point out that there is a coincidence of the modeled flow vectors with the transport direction of the boulders, which we cannot fully explain. We thus try to clarify/rephrase this section in the revised

ESurfD
3, C370–C375, 2015

version, along with mentioning a possible deflection of infragravity wave-driven reef-top currents (see next comment).

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 borelike 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.

Author response: We agree, and we will mention/point out this specific question. Deflection along the cliff coast may potentially play a role in the direction of infragravity wave-driven water currents on top of the reef platform.

References used (in addition to those cited in the original text)

Fukui, Y.; Nakamura, M., Shiraishi, H., and Sasaki,Y ., 1963. Hydraulic study on tsunami: Coastal Engineering in Japan, 6, 67-82. Helley, E.J., 1969. Field measurement of the initiation of large bed particle motion in blue creek near Klamath, California. U.S. Geological Survey Professional Paper 562-G (19 pp.). Bricker, J. and Roeber, V.: Mechanisms of damage during Typhoon Haiyan: storm surge, waves, and "tsunami-like" surf beat, E-proceedings of the 36th IAHR World Congress, 28 June–3 July, 2015, The Hague, the Netherlands, 2015. Benner, R., Browne, T., Brückner, H., Kelletat, D., and Scheffers, A., 2010. Boulder Transport by Waves: Progress in Physical Modelling. Zeitschrift für Geomorphologie, 54, Suppl. 3, 127-146.

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