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Interactive comment on "The impact of particle shape on friction angle and resulting critical shear stress: an example from a coarse-grained, steep, megatidal beach" *by* N. Stark et al.

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The paper describes an interesting set of experiments on a peculiar range of sediments found on a dynamic coastline. Two main messages emerge: there are very interesting but very poorly understood processes going on in poorly sorted sand-gravel sediments under waves, and we can learn something from a combination of tilting tray tests and geotechnical direct shear tests. This is therefore very novel work of great interest to sedimentologists, fluvial and coastal geomorphologists and engineers. However, the paper is not ready yet for publication as important literature is not incorporated and basic analyses that the data seem to allow were not presented. I therefore recom-

mend major revisions and encourage the authors to share the data and description of this fascinating and almost alien sediment sorting behaviour in the literature. Below I elaborate on the literature of interest and the analyses required.

There is a large body of literature in the fluvial sedimentological community and in the Powders and Grains community on the angle of repose including mixture effects and particles with shapes other than spherical. This literature is not referred to but is useful. See for instance http://proceedings.aip.org/resource/2/apcpcs/1542/1 and also the review in http://dx.doi.org/10.1029/2011JE003865. Also there have been a number of papers in WRR (Water Resources Research) and JGR on the beginning of motion of particle size mixtures such as recent work of Wu, work by John Buffington and Montgomery, Peter Wilcock, Stephan Vollmer and others. These works analytically derive threshold of motion curves (Shields curves) where angle of repose is explicitly incorporated and particle size sometimes as well. There is also a lot older work including Paul Komar and Li 1986 in Sedimentology that is of interest. Finally there is some work on the dilation of water-worked beds (maybe a paper by Lynne Frostick and perhaps some work by John Wooster et al on infiltration of fines into a gravel bed).

There is something that I don't understand but appears relevant for this paper: the friction angle derived from the direct shear test occurs under a normal stress of up to 64 kPa. Now in the natural situation of transported sediment the submerged sediment is actually at the bed surface. A normal stress of about 50 kPa means that a sample with unit surface area is subjected to a weight of 5000 kg. With a typical sediment+pores density of about 1500 kg/m3 this translates to a sliding sediment layer of more than 3 m thick, which is a considerable avalanche. How is this comparable to the friction or, rather, pivot angle of moving sediment particles ON a bed surface? Perhaps the material properties in these very different conditions can be compared and indeed I think so, but it is not yet well explained and argued here and I don't know why it would be comparable and I don't know references where this is discussed. A table with the different mixtures, friction angles and observed behaviour would be very convenient to have as a summary of sections such as 3.1.1.

Some systematic data analyses are lacking. The angularity of the sediments are clearly known, so why is this not plotted against the friction angle? Shape is easily parameterised by some sort of shape characterisation (see Folk 1966 in Sedimentology and I think there also was an extensive review in 2012 or 2013 in Sedimentology on particle shape description classifications).

Furthermore, figure 3 has the time series with horizontal displacement and the shear stress. Usually, in such tests, the peak shear strength just before 'failure' is plotted against the different normal stresses. A linear fit to these points results in an apparent cohesion (intercept) and the angle of internal friction (the slope of the line). This simple analysis has not been done but the data presented in the paper allows the authors to do this for the different sediments. Given the main objective of the paper, a direct comparison of these values for the different sediments would be most useful. We found, by the way, in a similar setup of the same size that for low normal stresses this analysis is rather inaccurate and the data is rather sensitive to sample preparation and sample inhomogeneity including larger particles being dragged along the shear plane, and, as the authors here report, even particles being destroyed. However, with least squares linear regression the effective uncertainty of the angle of repose should be straightforward to quantify.

Also, figure 7 is a dimensional plot with interesting trends. I think the experimental data of the direct shear tests and the angle of the tray allow you to do without much effort some very interesting analyses. In the first place, how does the angle of internal friction derived from the shear tests compare to the intercept of a curve fitted to the date in fig 7 (where intercept is a minimum angle)? And, if the angle on the horizontal axis is normalised by that intercept, does the data collapse? Would it be possible to collapse the data if the velocity on the vertical axis is normalised by settling velocity in water?

Was this velocity in fig 7 in each experiment constant in time anyway? I would expect

not. There probably is a static friction angle at which the material starts moving, but once in movement only a lower angle is required to keep it moving: the dynamic friction angle. When the material is placed on a tray with a larger angle, wouldn't you expect ongoing acceleration? In Pouliquen's work there is interesting discussion on this effect (Phys Rev E and other granular community literature). So perhaps the intercept in this plot is the most interesting information: the static friction angle determined in a different way independent of the direct shear box test.

Was porosity measured? This would be a proxy for the number of particle-particle contacts: the more poorly sorted a mixture, the lower the porosity (Frings et al. in Sedimentology and WRR) and the more contacts. This could contribute to material strength and I would expect that poorly sorted sediment is more difficult to dilate, because dilation immediately leads to kinematic sorting with the smaller particles percolating down into the mixture. Porosity can be estimated directly from particle size distributions, which would be good to have in this paper anyway. From this reasoning, an increase of friction angle for mixtures is expected, and the behaviour of mixtures reported elsewhere and discussed in this paper on page 1195 are therefore unexpected and intriguing.

Are the mixtures in the experiments bimodal or unimodal? Bimodal experiments show very different behaviour (see work of Peter Wilcock et al. in J. of Hydraul. Engineering and elsewhere).

Beginning of discussion: friction angles of 42-46 degrees are indeed high but have been reported in the literature for other angular and irregular sediments. Carrigy (1970 in Sedimentology) and JRL Allen in his large 1984 book (reports on highly elliptic rice and spaghetti included in there) for instance. The rest of the discussion is highly interesting in pointing out that mixtures reporting elsewhere have reducing friction angles.

The phases in the conceptual sketch (fig 8) are interesting, but what happens on the seabed during the events of interest? The paper starts of by describing the general situation but the discussion does not come back to this. However, it is of great interest.

There has been a lot of work on sand-gravel mixtures in rivers, including the peculiar bedforms and sorting trends and imbrication, all of which may be highly relevant in this environment. Either remove the environment altogether or say more about it later (if you can - perhaps not enough is known to say anything).

Interactive comment on Earth Surf. Dynam. Discuss., 1, 1187, 2013.