This manuscript tackles an interesting subject, the effect of shells on the development and stability of small-scale current ripples. It also investigates the initiation of motion of mixed sand-shell sediment beds. The rationale for the research and the aims and objectives are clear and the methods are described reasonably well. However, the results are not described in sufficient detail to allow me to verify if the interpretations fully support the data. The manuscript lacks what I regard as essential descriptions of the development of the ripples from the initial flat bed, as well as the development of the shell clusters. The development of these clusters contradicts with the statement that the shells were immobile. It is also unclear how the ripple data in Fig. 4 were calculated. Assuming these are mean values, which time period and how many ripples were used to calculate these averages. The tiny histograms are not helpful in this. The authors should include time-series of ripple height and length, so to provide stronger evidence that equilibrium ripples formed at all shell contents. This is important because this would strengthen the argument that the presence of shells leads to smaller equilibrium ripples rather than that the shells delay ripple development because of reduced availability of movable sand due to bed partial bed armouring by the shells, thus resulting in final non-equilibrium ripples of increasingly smaller size as shell content was increased.

Moreover, an explanation of the shape of the TKE curves, and changes in their shape, in Figs 5 and 6 are wanting, and the trends in BBS(cr), u(cr), and ks in Figs 7 and 8 need to be fully explained. I also feel that the authors brush over the fact that their armour layer covers only a small portion of the bed (seemingly much less than half the bed in Fig. 3). Is this really an armour layer in the common meaning of the term? The trend in ripple asymmetry is also not explained.

The relative roles of TKE, bed roughness, and bed stabilisation in bed shear stress trends and ripple size need to be further explored. This would strengthen the auithors' rather speculative conclusions and render their work more applicable. I believe that their data would allow the authors to take this extra step.

I feel that part of the wider implications Section 4.4 is too 'hand-waving', with too many statements that are insufficiently well explored or distract from the main aim of the work (e.g. oscillatory flows)

The writing needs attention throughout, but especially in the Discussion section. The text is also repetitive in a few places.

In summary, this manuscript addresses a novel and timely subject, but it needs further analysis to convincingly show that the interpretations are supported by the data. This may be a matter of describing the data in more detail and strengthening the conclusions.

Further comments

Line 62-64 – The ripples described by Baas et al., 2000, Baas & De Koning (21995) and Lichtman et al. (2018) are not around 1 m in wavelength and 0.01 m or more in height, but of the order of 100 mm long and 10 mm high. These are 'current ripples' according to the classification scheme of Ashley et al. (1990, J. Sed. Petrol, v.60, p.160-172), whereas the dimensions given here are in the 'dune' category. This is an important distinction, because current ripples are much more than just "tiny dunes". Their interaction with flows is fundamentally different, reflected in fundamentally different size predictors, for example.

L.68-70 – Lichtman et al. (2018) and Malarkey et al. (2015) did not study "particulate organic matter", but extracellular polymeric substances (EPS), which are cohesive, non-particulate organics.

L.77-78 – What is mean by “general profile”?

L.97-98 – Please rephrase, because most rocks fragments are siliciclastic, so this sentence does not make sense. In which way are shells “hydraulically somewhat more similar to siliciclastic particles”? Please explain.

L.101 – Shells cannot be dead. Replace with “empty shells”?

L.107 – What is meant by “bedform … conditions”? Do you mean bedform stability or perhaps bedform dimensions?

Fig. S1 – What do the colours mean in the graphs?

Equation 4 – Is *ks* the total bed roughness (based grain friction and form drag)?

L.221 – Please specify “sufficient amount”? This is crucial as, for example, Shields and van Rijn have shown in the past.

L.231-234 – Please be more specific: “strongly controlled” how? “all affected” how? “drastic change” how?

L.235-236 – What is meant by “these ripple parameters”? Which parameters?

L.237 – What is “rather immobile”? Were they immobile or not?

L.238 – How does Fig. 3 show disappearing ripples or ripples migrating around clusters? Move “Figure 3” to end of previous sentence?

L.256-264 – Is it necessary to describe the trends in so much detail? To me, the graphs tell the story sufficiently well. This section can be removed, and the R^2-values and p-values can be added to the graphs in Fig. 4.

Figure 4 – How many ripples are the average heights, lengths, asymmetries and migration rates based on for each shell content? When were these parameters measured? At the end of each run? Some of the scatter may be caused by the dynamic nature of rippled beds, with heights and lengths changing all the time even at equilibrium conditions (see Baas, 1994). Ideally, the ripples should be measured at multiple times during equilibrium conditions to reduce data scatter. I would also like to see time-series of ripple height and length: (a) to see when the ripples at low shell content reached equilibrium compared to the control; (b) to check if the ripples at the high shell content were still growing after 4 hours or not. This is important, because the reason for the decrease in ripples height and length could not be related to shells as such, but to the larger size of the shells and fragments (similar to adding gravel to sand).

Figure 5 – The colours of the data points are difficult to distinguish from one another. Please modify.

Figure 5 – Please explain the shape of the TKE curves. And why does the shape change with increasing shell content? I agree that 50% shells has the highest TKE, but is there a trend with increasing shell content from 0 to 50%

L.303-319 – This is a repetition of the results, and therefore unnecessary. In fact, this text reads better (even though it is somewhat convoluted) than the text in the results section. I therefore suggest that the authors move this text to Chapter 3 and merge it with the existing text and focus Chapter 4 on interpretations and discussion.

Section 4.1 – This section does not fully explain the observations and several statements are incomplete or unclear. What kind of structures (L.322)? What is anchoring of shells (L.323)? The text jumps too suddenly from gravel to shells. What does "enhances the erosion threshold" mean (L.323-324), larger or smaller critical shear stress? What is the difference between individual and loose shells (L.325)? If the shells were immobile, how did they form clusters? I am missing a description of how the clusters and armouring layers are formed. Figure 3 does not show an armour layer in the traditional sense, since most of the bed consists of sand. Can the authors rule out that the ripples were small to absent at the highest shell contents because the shells behaved as 'heavy' clasts that were difficult or impossible to move by the flow (thus behaving similar to very coarse sand or gravel clasts)? The smaller ripple sizes may therefore point to progressively slower ripple development rates, with 4 hours being insufficient to form equilibrium ripples at 20% and 50% shells. As mentioned above, this manuscript needs a detailed description of ripple development based on the available video footage. How do the authors explain physically that an enhancement of mean near-bed flow slows down the ripple migration rate? Shouldn't higher velocities increase bed material transport rate and therefore ripple migration rate? The trend in ripple asymmetry in Fig. 4c is not explained.

Section 4.2 – I find the interpretations of the trends in critical bed shear stress and critical velocity too speculative, and there are again unclear statements and comparisons with literature. L.346: Why is this now a gradual decrease, whereas in Section 3 there was no trend? This first paragraph essentially described the data again, which could be considered unnecessary. The second paragraph (L.348-357) raises more questions than it answered to me. Using a simple weighted mean of Shields-derived critical bed shear for 97.5% pure sand (D_{50} = 352 μm; $\tau_{b,cr}$[sand] = 0.2 Nm^{-2}) and 2.5% shells (D_{50} = c. 20 mm; $\tau_{b,cr}$[shells] = 18 Nm^{-2}) yields $\tau_{b,cr}$[mixed sand-shells] = *c.* 0.7 Nm^{-2}, which is close to 0.8 Nm^{-2} in Fig. 7a. Therefore, the increase in critical bed shear stress from 0% to 2.5 % can be explained by the increase in grain size, which is probably similar to what the authors call "stabilising the sediment". Do the authors need to also invoke increasing *ks* and near-bed TKE to explain the increase in critical bed shear stress? In fact, the authors sit on the fence too much, I feel. Would it be possible to do additional analysis and determine the relative contribution of stabilisation, TKE and bed roughness? This would make the paper much stronger, less speculative. Speculation continues in the interpretation of the trends at shell content above 2.5%. What is meant by "deflected over the shells" and how does this reduce the disturbance of the boundary layer? Even if this is a valid explanation, why does the critical bed shear stress first decrease and then increases again. The authors do not explain this. I may be missing something, but didn't Friedrichs et al. find exactly the opposite to this study? L.358-366: This appears to be a convolute way of saying that up to 10% shells the decreasing ripple size leads the falling bed roughness and above 10% shells the increasing shell content leads the rising bed roughness.

L.369-370 – What kind of debris, fragments and particles? Malarkey et al. (2015) did not study debris etc. but EPS, which is non-particulate.

L.388-389 – This statement is only valid for ripples where all particles can be moved. The shells were not moving, so is it surprising that the migration rate was lower? Fewer sand grain were exposed in the mixed sand-shell beds, so the answer here probably is that the ripple migrated slower because less sand was available for bedload transport.

L.403-416 – I do not see what this information adds to the narrative. What is meant by "the flow hat is characteristic of our study typically consists of diurnal or semidiurnal tides, instead of unidirectional flows"? Bidirectional tides are entirely different from unidirectional flows. If his study really intended to simulate tidal flows, the use of unidirectional flow experiments needs to be fully justified in the introduction and methods. Ripples in oscillatory are also entirely different, so the rather vague statements on wave-dominated environments add nothing substantial to the paper in my opinion. The other statements in this paragraph can be removed or they should be explored/ explained in much more detail, e.g. the potentially interesting sentence on L.410-412.

L.424 – The most commonly used Wentworth scale states that 0.352 mm sand is medium-grained sand.