

Interactive comment on “The effect of ripple types on cross-shore suspended sediment flux” by S. R. Kularatne et al.

Anonymous Referee #1

Received and published: 16 June 2014

This paper presents an analysis of flow, suspended sediment concentration, and bed morphology collected at 15 different beaches in Australia. Single-point suspended sediment concentration measurements were made with an optical backscatter sensor (OBS) at a fixed height (5 cm) above the bed. Flow velocity was measured – also a point measurement – with an electromagnetic current meter at 25 cm height, collocated in the cross-shore direction with the OBS. Building upon an earlier study of ripple types based on the same data set (Doucette, 2000), the authors use these data to explore the question of cross-shore suspended sediment flux. As the authors rightly point out, there have been few studies of the effects of ripple type on the net cross-shore sediment transport, and the purpose of the paper is to explore this question specifically for the suspended sediment flux in the incident swell band (i.e. between 9 and 20 s period). However, despite agreeing that the question is of interest, I nevertheless have

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a number of concerns regarding this acceptability of this manuscript.

Major Concerns

1. The results are based on 17 min data runs at different locations, “one run at each location in the cross-shore” (Doucette, 2000, p.46), 3 to 5 cross-shore locations per beach, and each location at different depth on the beach profile (Table 1 in this MS, and Table 1 in Doucette, 2000). Thus, the results are based on only one 17 min run at each cross-shore location, with the instruments in a fixed position relative to the ripple profile for each run (barring rapid ripple migration). For orbital scale ripples (which would include vortex ripples, one of the ripple types discussed in the MS), one wonders if it might not be the case that the net suspended sediment flux would – in principle – depend on the cross-shore position relative to the ripple crest of a sensor at a fixed height, even in symmetric oscillatory flow. This possibility seems to be suggested by the authors themselves as an explanation for the variable sign of the net flux over cross ripples, which contain an orbital scale component:

“the different positioning of the instruments relative to the primary and secondary ripples might have caused the variability in the direction of the cross-shore sediment flux.” (p. 231, also Table 3)

In the 2-dimensional case, the time-averaged sediment flux balance for incompressible flow at a fixed height can be written as

$$\langle uc \rangle_x + \langle u'c' \rangle_x = - \langle wc \rangle + \langle w'c' \rangle + CW_s|_z$$

where: $\langle \rangle$ indicates the ensemble average; the upper case, lower case, and primes respectively indicate time-mean, wave, and turbulence quantities; the x,z subscripts denote partial differentiation in the horizontal and vertical; W_s is the particle settling velocity; and it has been assumed for the sake of simplicity that the concentrations are low enough that sediment can be treated as a passive scalar with a concentration-independent and flow-independent settling velocity. If this equation is integrated in the

vertical from bed level to a height at which the sediment concentrations are zero, the r.h.s. becomes equal (effectively) to the rate of change of the bed level, which would be zero for non-migrating, equilibrium ripples. In this case, the vertically-integrated horizontal flux divergence would vanish, and if the turbulent flux were small, then the vertically-integrated wave flux would be independent of position along the ripple profile. However, for measurements made at a fixed height – i.e. not in a vertical profile that would admit vertical integration – and at an arbitrary single point along the ripple profile, the horizontal divergence of the ensemble-averaged wave flux would be non-zero because the other terms are non-zero in principle, making even the sign of this flux rather difficult to interpret. If one then adds the possibility of ripple migration, and 3-d ripples, the interpretation becomes still more complicated.

The above argument – that the sign of the horizontal suspended sediment flux in the wave band at a fixed height and a fixed position relative to the ripple profile is difficult to interpret – is born out by the detailed measurements of suspended sediment flux over full-scale vortex ripples reported by van der Werf et al. (2007). Shown in their Figure 13 are vertical profiles of the flux at different positions relative to the ripple crest. At heights above the bed of order the ripple height – i.e. comparable to the 5 cm height in the MS – the flux changes sign at some of the positions.

2. Suspended sediment flux (in the incident wave band) and bed state (i.e. ripple type including flat bed) are both very tightly connected to the characteristics of the wave forcing. It has been demonstrated that ripple type in sandy nearshore environments is determined primarily by wave energy, the 3rd-order moments (skewness and asymmetry) being relatively unimportant (Hay and Mudge, 2005). Based on this result, one might hypothesize that the direction of net suspended sediment transport – in the wave band – over rippled beds might be determined mainly – or at least partly – by the skewness/asymmetry of the cross-shore wave motion near the bed. The authors' discussion of their results for the different ripple types and the summary in Table 3 are consistent with this hypothesis for at least 3 of the 6 cases. However, no quantitative

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measures of skewness and asymmetry are provided in the paper. This seems to me a major weakness.

3. The presentation and discussion of the results is highly qualitative, which is unsatisfactory. I would like to see, either tabulated or in graphical form and preferably both, the integrated swell-band sediment flux – i.e. the co-spectrum integrated over the swell band – for each ripple type and each location. Pursuant to the above-mentioned absence of quantified measures of the degree of wave non-linearity, these fluxes – possibly normalized by the incident wave energy – should be compared to the wave skewness and asymmetry for each ripple type among the different locations.

4. The first term within parentheses on the l.h.s. in the above equation, u_c , is what the authors have measured (or would be if the co-spectrum had been integrated over the swell band to obtain the numerical value). As indicated in Table 3 of the MS, there is apparent consistency in the sign of this term for the different ripple types, with the exception of cross ripples. This consistency might indicate that by making measurements at different beaches for the same ripple type (and presumably therefore at different positions relative to the ripple crest, though this is not stated in the MS), this consistency might be used as an argument in support of the sign of u_c at the fixed sensor height being roughly uniform over the length of the ripple profile. However, since neither the values for the flux nor the skewness or asymmetry of the wave motions are presented, the reader is left wondering whether the sign of the observed flux has nothing to do with the bedforms and everything to do with the higher-order statistics of the wave motions. This is unsatisfactory.

5. The interpretation of the phases from cross-spectral analysis requires that the 95% confidence level of the squared coherency be determined, so as to identify at which frequencies the phases are reliable, as has been standard practice for many decades (see Thompson, 1979, and references therein). This not done in the MS, which is unacceptable (see also the next point). [N.B. The authors have computed the 95% confidence intervals for the phases, but this is a very different thing.]

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6. The last 3 panels in Figures 8, 10 and 12 – i.e. the panels showing cross spectrum (presumably meant to be the magnitude of the cross spectrum?), the coherence (is this coherence or squared coherence?) and the phase – are not even referred to in the text, and should be deleted. Of the corresponding panels in Figure 7, only (f) and (g) are discussed (see p. 226) but the text addresses a difference between the wave-band flux and the flux at low frequencies, which is outside the stated scope of this paper. Thus, these panels could also be deleted along with the related text without loss of relevant content.

7. An important point relates to the statement in the Introduction at the top of p. 217: “suspended load transport is dominant over rippled beds”. Despite the list of citations in support of the statement, it is doubtful that this statement holds generally. For example, Masselink et al. (2008) found that vertically-integrated suspended fluxes and bedload transport estimated from ripple transport rates were very similar. Thus, I think the authors would be well advised to change the statement to read “suspended sediment transport MAY be dominant over SOME rippled beds”, and the Masselink et al. paper should be cited in an additional sentence as a counter example. N.B. The Aagaard et al. (2013) paper – cited in support of the statement – is missing from the References at the end of the paper. If this is the Aagaard, Greenwood and Hughes (EarthSciRev 124, 32-50, 2013) paper, then I believe citing it as supporting the statement would be incorrect. As indicated in their abstract, AGH2013 is about suspended sediment transport. The only occurrences of “bedload” produced by a word search of this AGH2013 paper are on p. 48. The first 2 such occurrences are in the Discussion, and refer to the Masselink et al. (2008) results. The 3rd and last appears in the Conclusions, where AGH state “Further, the role of bedload in nearshore bar development and migration is still a moot point”. It would seem best not to be overly definitive in statements about the relative importance of bedload and suspended load transport in sandy wave-dominated nearshore environments.

Recommendation

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In the opinion of this reviewer, the MS would have to undergo major revision and be sent out for re-review before it could be accepted for publication. In the comments above, several suggestions have been made for improving the paper, in particular to make it more quantitative, and more focussed. However, unless I am missing something, the issues raised in Point 1 would seem to be especially serious. Perhaps the authors will be able to provide a solid set of counter arguments. If not, then an approach similar to that outlined in Point 4 would be required.

References

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Interactive comment on Earth Surf. Dynam. Discuss., 2, 215, 2014.

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