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Interactive comment on "Observations of the effect of emergent vegetation on sediment resuspension under unidirectional currents and waves" *by* R. O. Tinoco and G. Coco

Anonymous Referee #1

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General Comments

This paper presents results from a laboratory investigation of the effects of emergent, bed-anchored, mimic vegetation stalks on turbulence intensity and suspended sediment concentration in wave-forced and current-forced conditions. As indicated by the authors in their informative Introduction, there have been very few previous studies of flow-vegetation interactions with a mobile bed, so the experimental design implemented here as well as the results themselves will be of interest to the wider community. As the authors also rightly point out, the lack of such studies means that the additional feedbacks involving mobile bed adjustment to fluid forcing in the presence of vegetation have not received much attention in the literature, especially for wave forcing. The

mimic vegetation stalks are rigid, smooth cylinders. The wave forcing is (stated to be) monochromatic. Possibly the most interesting result, and one appropriately highlighted by the authors, is the pronounced decrease in the threshold of sediment suspension for dense versus sparse arrays of stalks.

Specific Comments

1. The concentration measurements were made at a single height, 5 cm above the bed, a height which is a significant fraction of the 16 cm total water depth. It can be argued that the authors' use of the terms sediment resuspension "threshold" and "critical velocity" for sediment resuspension is, if not incorrect, at least misleading. My reasoning is the following. Though not explicitly discussed by the authors, the dynamical role of smooth-sided round cylinders protruding vertically from a mobile bed is in my opinion, at least in part, to inject vertical vorticity into the nearbed flow, and thereby provide a mechanism for raising particles from the bed higher into the water column than would otherwise be the case. Thus, in the absence of cylinders and therefore a strong source of vertical vorticity, particles would be resuspended to lower heights. With a single sensor at a fixed height quite high in the water column, it seems quite conceivable to me that, when cylinders were absent and/or at very low densities, material actually in suspension – but lower in the water column – would have passed under the sensor undetected. Consequently it is not obvious to me that the critical velocities estimated by the authors can be directly compared to the values predicted by the standard semi-empirical formulae (i.e. Table 3 vs Table 4).

2. In a related vein, the authors state on p.13 that their data in Figure 12 for the waves-only, no cylinder case are consistent with the 0.112 m/s Komar and Millar estimate for the critical velocity for sediment resuspension. To me, the "threshold" would be defined by a sharp increase in concentration: i.e. an abrupt change in slope in the concentration-velocity relation. I look at Figure 12 and see critical values for n =0 closer to 0.3 and 0.2 m/s at x = 0 and x= 3 m respectively, which are a factor of 2 or more higher than 0.112 m/s. At first I thought there must be a typographical error in the text,

but it seems not, as Table 3 does indeed indicate an observed value of 0.12 m/s for n = 0. This 0.12 m/s estimate seems to me inconsistent with the data. The authors need to clarify/explain exactly how they obtained their threshold estimates from the data.

3. The authors state that their experiments were carried out with monochromatic waves but the surface elevation time series' in Figure 13 indicate that the waves were definitely not monochromatic: i.e. they were either highly skewed or distinctly sawtoothed, depending upon cylinder density and location relative to start of the patch. In either case, the waves necessarily contain phase-locked harmonics and are therefore not monochromatic. I suggest "regular" or possibly "regular periodic" would be the correct term.

4. Also on the topic of the character of the wave forcing, I understand from the text on the bottom of p. 10 that the waves shoaled after leaving the paddle due to the presence of a ramp leading to the sand bed, resulting in significant wave transformation even in the absence of cylinders. But, as indicated by the data in Figure 13, there are also very significant differences in the character of the waves depending upon whether or not cylinders were present: i.e. for n = 0 the waves were highly skewed, for n = 250/m2 they were highly asymmetric. I would like the authors to comment on the possible effects these differences in the 3rd-order wave statistics might have had on their observations of sediment resuspension, and the implications if any for wave-vegetation-sediment interactions in nature.

5. Several aspects of the experimental design need clarification. In Figure 2 the cylinders appear to be attached to a base plate beneath the sand. I would like to know much more about how the cylinders were emplaced, and the order in which things were done, in order to have a better grasp of the initial condition of the bed prior to each run. a) How was the sand surface prepared between the trials with different cylinder densities and runs at different flow speeds/wave heights within a trial? Was the surface re-flattened between trials, or between runs? b) Were the cylinders removed and replaced between runs, or were the cylinders installed at the highest density, and

then selectively removed to produce progressively lower densities? c) Were replicate trials – i.e. trials with the same cylinder density but different random configurations – carried out?

6. The cylinders were arranged in random patterns. Is there evidence in the literature indicating that the species being mimicked in these experiments – mangrove roots and fan worms – are similarly arranged at random in nature?

Technical Corrections

(i) I was puzzled by the use of the term "agitator" running at frequency fp (p. 8, top), whereas fp is indicated elsewhere as the "pump" frequency (Fig. 5), which would consistent with the "p" subscript. Figure 5 also indicates that the flow speed is proportional to fp, so it makes physical sense that it would be a pump, whereas agitator suggests a device used to introduce turbulence into the flow. If "agitator" is really what is meant, please clarify how it differs a pump. Else, if "pump" is what is meant, please use this term instead.

(ii) On p. 9, the phrases "large alterations" and "significant scour" are used to describe the deformations in the bed resulting in the decision to stop a trial. This terminology is woolly, and too many of us are guilty of its use, myself included. Here however, the authors' interest is in the sediment response to forcing in the presence/absence of vegetation interaction, of which bed deformation is a major component. Could the authors not therefore be more specific about what "large" and "significant" mean in this context?

(iii) p. 10 and later: "transversal" should be "transverse"

(iv) p. 10, middle. The authors discuss the transverse standing wave induced by flow past the cylinders. Wouldn't the concentration peak be expected to pass the sensor twice per cycle, and therefore appear at twice the modal frequency in the Scc spectrum?

(v) p. 11, bottom, and later. The text states “To look at the correlation between the recorded velocities and concentrations, the frequency spectra are calculated. . .”. This is incorrect. To investigate “correlation” between two time series one either computes the cross-correlation function, or the cross-spectrum.

(vi) p. 12,line 6-7: “generated within the cylinders” should read “generated within the cylinder array”

(vii) p. 12, line 9: “that observed” should read “who observed”

(viii) p. 12, line 15: Suggest changing “the sole responsibles” to“solely responsible”

(ix) p.15, line 9-10. This sentence does not make sense. As stated in the first half of the sentence, there is a “clear distinction” which clearly depends on cylinder array density, so why would it be interpreted as “density independence” as stated in the second half?

(x) Figure 5 caption. Please state the value of x at which these measurements were made. Also, there appear to be error bars within the symbols. Please state what they represent (i.e. are they possibly related to 5c above?).

(xi) Figure 6 caption. State the value of x at which these measurements were made. Also, it would be of interest to see these results for both values of x.

(xii) Figure 7 caption. Modes 1 and 2 are indicated here, but these are the frequencies of modes 2 and 3 given in the text on p.10. Please correct. Also, Sww is indicated in the caption, but Svv is the label on the Figure. Please correct.

(xiii) Figure 7 and Figure 15 captions. Multiplying BOTH Svv and Scc by 10 to improve visualization as stated makes no sense. This could be accomplished by changing the y-axis limits.

(xiv) Figure 15 caption. Please state what the green line is.

(xv) Figures 3, 4, 9 and 10. Please state what the red line is. Also, I had to zoom in on these figures to be able to appreciate/see the scour and bedforms indicated by

the green arrows. This will be fine I expect in electronic versions of the final paper, but will people reading the printed version be able to appreciate/see these changes? I suggest that the authors consider cropping the photographs vertically to remove the water surface, the upper 50% of the cylinders, and the sediment subsurface, none of which are of much interest, and arrange the resulting images vertically in a page-wide stack so that the changes to the bed are revealed more clearly. Otherwise, unless blown up, these images are not very informative.

(xvi) Table 1. Please add a column listing the values of aD.

(xvii) Table 2. Why list these parameter values for Hm > 0.05, when the results from these runs are excluded from the analysis, as stated on p. 9?

(xviii) Table 3 and 4 captions. Please state the value of x for which these numbers were computed.

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