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Discussions

Interactive comment on "Data driven components in a model of inner shelf sorted bedforms: a new hybrid model" *by* E. B. Goldstein et al.

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Goldstein et al. describes a new predictor for the near bed reference concentration using a machine learning technique (genetic programming) which is driven by an extensive set of measurements. The performance of the new predictor is illustrated by a comparison with the empirical formula's proposed by Nielsen (1986) and Lee et al. (2004). This new predictor, together with a recently obtained formulation for the ripple height (Goldstein et al., 2013), is blended into an existing numerical model describing sorted bed form formation and evolution. Two types of bed forms are found to emerge with this new hybrid model which are linked by the authors to sorted bed form modes apparent from previous analytic work and field observations.

The manuscript is well written and logically describes the interesting endeavor to im-

prove the performance of an existing model by using a "hybrid" model; in order to reproduce the occurrence and characteristics of sorted bed forms. However, several aspects of the manuscript seem to be not yet thoroughly analyzed, while other elements appear (to some extent) incorrect. As such, the manuscript (presently) describes a rather unconvincing analysis. For this main reason, I can not support the publication of the manuscript in present form and recommend a major revision which suitably addresses the comments below.

Sincerely,

Tomas Van Oyen

Major comments:

A. It appears from the manuscript that the authors claim that the new sorted bed form model outperforms the sorted bed form model described by Coco et al. (2007a). However, the manuscript does not adequately demonstrate this point. In particular, it is not clear how figures 7 and 9 of the manuscript differ from the results presented by Coco et al. (2007b).

For instance, what is the difference between Fig. 7 (manuscript) and figure 10 of Coco et al. (2007b)? Now, it appears that both only differ in the sense that the bed forms develop more slowly in the new model. Please clarify extensively the difference.

To some extent, the manuscript seems to suggest that the resulting bed form wavelength, emerging from the model, becomes stable; also in the case that a unidirectional current is considered (thus without the artificial reversing of the current). As this is a major caveat in the model of Coco et al. (2007a); this result would be a major breakthrough. Is this the case? If it is, please prove it rigorously and highlight this result in the abstract, conclusions, etc .. (it would be a major step forward so highlight it!). If not, please discuss this model characteristic critically.

In addition, it is not clear to me how the model characteristics (focusing on the phase shift between the bed undulation and the location of the coarse grains) presented in Fig. 9 (manuscript) differ from those depicted in figure 13 of Coco et al. (2007b)? Please clarify substantially in the revised manuscript. I understand from the manuscript that the bed forms depicted in Fig. 9 (manuscript) are generated mainly because the coarse grains are not mobile enough to be transported. This is in contrast with the result obtained by Coco et al. (2007a) which indicate that no patterns develop when no coarse sediment is put into suspension. Please investigate and discuss why with the new model, you do obtain bed form appearance in this case while previously no patterns where observed. Is this just because in the model of Coco et al (2007), there is a critical value of shear stress below which there is not reference concentration, while the new formulation does not consider this?

Finally, in order to convincingly demonstrate the model improvement, it is necessary to present also the resulting patterns with the previous model, considering the same conditions.

B. The model is claimed to be an improvement as it now favorably compares with field observations. However, the presented discussion is incomplete and does not take into account the entire observational picture; as such, the manuscript in present form appears to be misleading.

I realize that observed and reported characteristics of sorted bed forms in the field at distinct locations are not persistent, and therefore it is difficult to compare model results with field observations. However, stating bluntly that there are two types of sorted bed forms observed, one with coarse grains in the troughs of the bed undulation and one with the coarse grains shifted downcurrent of the trough is incorrect. In fact, both Goff et al (2005) and Ferrini and Flood (2005) report that within the same location, the phase shift can alter, suggesting that both an upcurrent and a downcurrent shift occurs (in addition, Goff et al (2005) refers also to observations reported by Schwab et al (2000) which describe an upcurrent shift). These observations should also be taken

into account in the discussion of the model performance.

In addition, the “mode” which emerges with coarse grains in the through, is related with an upcurrent migration of the bed forms. I know that reported observations of migration of sorted bed forms can often be questioned. Nevertheless, to the best of my knowledge, upcurrent migration has not yet been observed. Hence, it appears that the model does not provide a fair description of this aspect of the bed forms occurring in the field. In the revised version of the manuscript, please provide a discussion on this feature of the model outcome with respect to field observations.

Please note that the points described above do not mean that the described model results can not be considered as a step forward. However, also the caveat's of the model need to be highlighted. Otherwise the manuscript is highly misleading (providing only a “good-news-show”); which, in my opinion, impedes the scientific progression on the subject.

Minor comments:

1. The manuscript, as presented, seems to put forward that the improved results are obtained due to the newly derived formulation of the reference concentration. However, also a new the ripple prediction is implemented. Is it possible to untangle both effects on the results?

2. The new predictor is argued to be preferable to that of Oehler et al. (2012), as the latter is not smooth. I can follow this reasoning, however, it seems only fair to compare also the performance of this model with the field observations, and to discuss the outcome with respect to the GP predictor performance.

3. The sorted bed form model considers both a steady current as well as wave action. However, the reference concentration is only related to wave action. This could be appropriate for several locations. On the other hand, Guttierez et al. (2005) suggest a significant correlation between the near bed shear stress (controlling the sediment mo-

tion) and the occurring wind-driven current. Please clarify and discuss the introduced approach, also in the light of the observations of Guttierez et al. (2005).

4. The emergence of “mode 1” is an interesting feature which has now been obtained by increasing the coarse grain size. Is this the only “route” towards this “mode”? I would expect that decreasing also the hydrodynamic conditions would result into the generation of this pattern. Please investigate and discuss also this possibility and maybe others.

Specific comments:

p. 532 l. 8: “This newly . . . predictors” → “We demonstrate that this newly . . . predictors”

l. 16: “However, . . . modeling.” A bit a strange sentence. Please rephrase

p. 533 l. 3: “of the accumulation of error as .. is (1)” → “the errors accumulate and (1) ”

p. 535 l. 7: “with only a slight bathymetric relief . . . ”. This statement does not describe the full range of sorted bed form observations, e.g. Goff et al. (2005), Aubrey et al. (1982). In particular, sorted bed forms where the ratio between the bottom undulation and the mean water depth is 1/4 can hardly be described as related to only a slight bathymetric relief. Please adjust the manuscript suitably taking into account these observations.

l. 8 – 11 and also at other places in the manuscript: “Unlike bedforms . . . Van Oyen et al. 2010, 2011)”. In my opinion, it is misleading to put Van Oyen et al. (2010, 2011) behind that sentence. In fact, these two manuscripts point out that bed features with characteristics resembling the sorted bed forms observed in the field can also be triggered by bathymetry-flow interactions (taking of course a grain size mixture into account) in addition to the sorting feedback stipulated by Murray and Thieler (2004). Hence, they do not straightforwardly support the “sorting-mechanism”. Please describe correctly the findings of these studies.

l. 24 – 28: “Sorted bedforms show . . . Flood, 2005). Van Oyen et al (2010,2011), ... two distinct configurations. ”. As discussed above this statement does not correctly reproduce field observations. Please adjust. In addition, note that Van Oyen et al. (2010,2011) introduces two modes in order to distinguish between the processes governing the generation of sorted bed forms; i.e. features resulting mainly from interactions between the hydrodynamics and the bottom elevation (“topography- driven mode”); and the sorted bed features which are driven by bed roughness – flow interactions, in addition to the influence of the bathymetry (“roughness-driven mode”). In this sense, the bed features emerging from the presented model are both roughness-driven modes. Please consider this point while revising the manuscript as it is confusing.

p. 536 l. 18: The use of the variable d_0 for the wave orbital diameter at the bed is a bit confusing.

p. 537 l. 5: “because three data sets” → “because the three data sets”. Also, I guess the dataset collected in a microtidal estuary in a mean water depth of 1.7 m does not correspond to a sorted bed form field?

l. 8 - . . . : This information provided in this section is a bit similar to that described in Goldstein et al. (2013). Maybe, it is an idea to refer to that paper for additional information, in addition to the content described.

p. 539 Equation (1): I guess the difference between p and b is summed over all points and therefore a summation sign is missing.

p. 542 l. 9: The performance of the model deteriorates for very low concentrations. Is there a reason for this? Maybe because not critical shear stress for initiation of motion is considered? Please investigate and discuss in the manuscript.

p. 543 l 15: Equation 2: the flux of suspended sediment needs to be integrated from some level to the free surface, please specify in the equation.

p. 544 l. 19: Please provide some indication of the impact of assuming Usig = Uw

Equation 9: $C\_0 \rightarrow C\_0,s$

p. 561: Figure 1 has been noted in Coco et al. as courtesy of VIMS. Perhaps this is also unnecessary here?

p. 562: I saw a similar figure in Oehler et al (2012). Perhaps you need to refer to this work, if you reproduce the figure here?

p. 567: This figure needs to be much bigger and of better quality (even zooming in on the screen did not allow to capture fully what happens.)

Interactive comment on Earth Surf. Dynam. Discuss., 1, 531, 2013.