Response to Reviewer 1
Goldstein et al. 'Data driven components in a model for inner shelf sorted bedforms: a new hybrid approach'

Reviewer comments in plain text
Author comments are in BOLD
ESurfD Manuscript text is in *italics*
Added Text is in ***Bold Italics***

We thank Referee #1 for taking the time to read and review our manuscript. She/He offers some excellent suggestions. We address each specific comment below:

Anonymous Referee #1

Review of ' Data driven components in a model for inner shelf sorted bedforms: a new hybrid approach'. General comments: This is an interesting manuscript that combines a data-driven approach to determine reference concentration of suspended sediment and a model for inner self sorted bedforms. The approach used in the data-driven component is novel. It is interesting to see the Genetic programming used here detects the dependence of the reference concentration of suspended sediment on a modified form of Shields Parameter. The manuscript is worthy of publication in Earth Surface Dynamics, however after moderate revision. Please note that I ticked 'minor revision' option as there wasn't a 'moderate revision' option.

Specific comments:

Section 2 – Even though more detailed of the data sets used in the manuscript are available elsewhere the authors should include important information about the datasets (e.g. sampling duration, sampling frequency, height above bed, water depth, etc.) in the manuscript, preferably in a table.

This is a good point, and we will include a table

Section 2.2 – Explain reasons for using 40 centroids for the GP prediction. You should do a sensitivity analysis to select optimum number of centroids, unless you have done so in Goldstein et al. (in press).

The reviewer brings up an excellent point. This issue was addressed qualitatively in Goldstein et al. (2013; CSR): As we mention in the paper, we use the similar number of centroids in this study as the previous study.

There exists no methodology for determining the optimum number of centroids, especially since 'optimum' needs to be precisely defined. We believe this question deserved a significant study of its own, so instead used the qualitative guidance from Goldstein et al. (2013; CSR) and we tackle the question of # of centroids in a more quantitative manner in a different paper that is now submitted.

Section 5- One of the main reasons for the differences in results between Coco et al (2007a) and the results this manuscript can be the differences in sediment and hydrodynamic conditioned used. To make a direct comparison between the performances of the two models, you should use same conditions. I strongly recommend to re-run the new model using same conditions to that in Coco et al. (2007a) model.

This is an excellent point. The new Hybrid model has several substantial differences to the Coco et al. (2007a) model. As such, using the baseline hydrodynamic and sedimentological conditions of the Coco et al. (2007a) model there is no sorting in the hybrid model. We now make this clear:

Line 1; page 549

"…The "hybrid" version of the sorted bedform model is able to reproduce the sorting feedback using new parameterizations built from data. The sorting feedback hypothesized by Murray and Thieler (2004) is robust to changes in the mathematical description of the processes in sediment transport and hydrodynamics on the continental shelf, and hybrid model results are comparable to previous modeling efforts (Murray and Thieler, 2004; Murray et al., 2005; Coco et al., 2007a). ***The behavior of the hybrid model and the Coco et al. (2007a) model under identical hydrodynamic forcing is different because there are quantitative differences between the mathematical description of sediment transport processes. For instance using the baseline conditions of the Coco et al. (2007a) model the hybrid model produces no sorted bedforms. This is a direct result of changing the C_0 predictor from the Nielsen (1986) formula (which over-predicts sediment transport; Figure 6) to the new GP derived C_0 predictor. Changes to the sediment transport formulas prohibit us from directly comparing the three models under identical forcing conditions. Instead we offer this hybrid model as a refined version of the Coco et al. (2007a) model.*** *The hybrid model has additional advantages beyond being more tightly coupled to observational data, most notably in favorable comparison to previous observational and analytical work."*

Additionally we have added a line stating that this paper is not a comparison between the two models, but instead a refinement of the previous model that shows new behaviors.

Line 7; page 536

"*...We then outline the sorted bedform model and the modifications to incorporate the new data driven components. Finally we show the results of the new hybrid model (i.e., the appearance of two pattern modes) and discuss advantages and disadvantages of this data driven approach.* ***This paper does not attempt to quantitatively compare the new hybrid model against older modeling efforts: instead we offer this new model as a refinement to the previous model that is additionally able to capture new dynamics."***

Section 6 – i. The author's claim that the new model needs more energetic conditions to move sediment in the new model than the Nielson (1986) model is obvious as Nielson used smaller sediment sizes than the current model. ii. As well as advantages, you should

mention disadvantage of this approach: (a) the formula is not physically based and (b) does not perform well at smaller sediment concentrations

i) **Nielsen (1986) used 21 measurements of C_0 from various experiments, with grain sizes ranging from 0.14 mm - 0.55 mm. The dataset used in our study ranges from to 0.15- 0.75 mm, similar to the Nielsen (1986) data. Even at the smallest grain sizes of our study (0.15 mm) we are within the range of the applicability of the Nielsen (1986) data (i.e., The Nielsen predictor should be valid). As the reviewer suggested, the added table will make this comparison more clear. We have added a line showing that the overprediction by Nielsen (1986) is found in other studies.**

ii) **Thank you for pointing this out, we have amended the text to include these caveats.**

Line 19-27, p 548

"The C_0 predictor developed in this study is an equilibrium predictor therefore the role of time
variance of $C_0$is not addressed (e.g., Vincent and Hanes, 2002). However, the data was collected in burst mode, a technique that involves time averaging. Burst measurements may reduce the effect of some time dependent processes (e.g., advected clouds of sediment, wave groups, etc.). ***The GP predictor is constructed solely with regard to the measurement data and is not based on 'first principles'.*** *Using the independent testing data, the new GP predictor has a lower NRMSE and higher correlation coefficient than the Nielsen (1986) and Lee et al. (2004) predictors,* ***however the GP predictor does not perform well at low concentrations (Figure 6)****. Notably, more energetic conditions are required to move sediment using the GP predictor as compared to the Nielsen (1986) prediction scheme previously used in the sorted bedform model.* ***This result is similar to previous work that suggests the Nielsen (1986) predictor may overestimate reference concentration (Bolaños et al., 2012; Thorne et al., 2002)"***

Section 7 – i. I am not sure about the claim that the new model out-performs Lee et al. (2004) model. NRMSE of new model is only marginally lower than Lee et al (2004) model, even though the correlation coefficient is higher. However, it should be noted that the range of validity of Lee at el. (2004) model is significantly larger than the new model (Figure 6). ii. To consolidate the claim that the new model is able to generate novel behaviour in the sorted bedform model where sorted bedform morphology changes when the size of the coarse fraction is modified, you should do the similar experiment using Coco et al. (2007a) model.

i) **The reviewer is correct that the GP predictor performs only marginally better than Lee using the NRMSE. However since the GP performs better using both NRMSE and correlation coefficient, we feel that our statement about better performance is substantiated: we clarify this position in the conclusion (see below). We have added a line (above) stating that the GP does not perform well at**

low concentrations in the discussion section, and we add a line in the conclusion stating this as well.

Line 12-15, p. 552

"A new predictor for near bed reference concentration developed using genetic programming performs better than previous empirical parameterizations ***when evaluated with two error metrics. However the GP predictor shows poor performance at low concentrations.*** *"*

i) This is an excellent point. We have now added a line after testing with the older (Coco et al. 2007) model. As we mention above in Section 5 comments, we cannot (in good faith) quantitively compare the Coco et al. (2007a) and Hybrid models.

line 4-8, Page 547

"When coarse sediment diameter is larger than 0.008 m, bedforms are strikingly different: bedforms develop faster, wavelengths and height increase significantly, coarse sediment is only present in the trough of the bedform (not along the updrift flank) and bedforms migrate upstream (Fig. 9). ***This behavior is not observed in the Coco et al. (2007a) model.*** *"*

Again we thank the reviewer for providing us with many good ideas and giving us the opportunity to clarify our position.