

**Response to Tomas Van Oyen (R2)**

**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 Dr. Tomas Van Oyen for taking the time to carefully read our submission and provide an extensive, very helpful review.**

**We now address each comment below:**

Major Comments

It appears from the manuscript that the authors claim that the new sorted bedform model outperforms the sorted bedform 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.

**Comments from both reviewers highlight the need for us to clarify the scope of our manuscript, specifically the comparison between the Coco et al. 2007a model and the presented hybrid model. We do not intend for this paper to be a direct comparison between the two models, but instead a refinement of the previous model that shows new behaviors. The new hybrid model has several substantial differences to the Coco et al. (2007a) model. We now clarify this position at several places in the paper**

**Line 7; page 536**

*“...We then outline the sorted bedform model and the modifications to incorporate the new data driven components. **This new model is meant as an update to the Coco et al. (2007a) model. The new predictors in the ‘hybrid’ model are more accurate and better performing than the formulations used in the Coco et al. (2007a) model. Finally, we present a novel experiment with the new ‘hybrid’ model to show autogenic behaviors that were not present in the Coco et al (2007a) 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.**”*

**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 Thielert (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 Thielert, 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 of quantitative differences between the mathematical descriptions 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  $C_o$  predictor from the Nielsen (1986) formula (which over-predicts sediment transport; Figure 6) to the new GP derived  $C_o$  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.”*

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.

**We have added several sentences to address this comment:**

**P547 L 21-25**

*“As bedforms migrate the position of the sorted bedform trough changes. Fine sediment under the bedform trough, once too deep to experience fluid- sediment interactions, is excavated and suspended. Winnowing of fine sediment and coarsening locally in the bedform trough, repeated as the bedforms migrate, results in the development of a horizontal layer of buried coarse sediment, a “sorting lag”.*

***In all results presented here bedforms migrate and bedform wavelength continues to grow through the model run and wavelength does not saturate. This perpetual coarsening of wavelength under conditions of unidirectional currents is identical to the behavior of the Coco et al. (2007b) and Murray and Thielert (2004) model under unidirectional current forcing. (In the previous results, wavelength coarsening also occurs under the more realistic conditions of an asymmetrically reversing current, although coarsening is more gradual than under a unidirectional current.)”***

**P549-550 L 0-5**

*“When coarse grains are smaller (essentially identical to increasing wave conditions in terms of increasing coarse sediment mobility) bedforms conform to Mode 2 expectations with smaller features, slower migration rates, and coarse sediment along the updrift flank of bedforms. When coarse grains are larger (essentially identical to decreasing wave conditions in terms of decreasing coarse sediment mobility) bedforms show characteristics of Mode 1 features with larger, bedforms, faster migration rates, and coarse sediment in the bedform trough. **Bedform wavelength continues to grow in all model results shown here as a result of unidirectional current. However, results in this contribution show that, for any given instant in model time, modeled sorted bedform patterns display relatively homogenous wavelength and height (similar to Coco et al., 2007a and Murray and Thieler 2004). Observational work shows sorted bedform fields have a well defined pattern scale (i.e., a similar height and wavelength throughout the entire bedform field; see the compilation of observed bedform features in Coco et al 2007b for more details). It remains unknown whether the well defined pattern scale of observed sorted bedforms reflects a saturated (steady state) wavelength or the uniformity of bedform wavelength and height at a given moment of pattern evolution.**”*

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.

**The Mode 1 bedform behavior in Coco et al (2007b) is the result of strong path dependence (systematic changes in wave height during a single model experiment as a way to represent the passing of a storm). In the Coco et al (2007) model, this specific forcing scenario is able to suspend a significant portion of the fine material leaving a predominantly coarse bed during the storm event. After the high wave/storm event passes, the fine sediment settles from the water column in locations where the bed is slightly less coarse. This is similar to the behavior in the Hybrid model except the suspension of fine sediment and the coarsening of the bed is entirely autogenic using steady wave conditions. Using steady wave conditions, Coco et al., (2007b) was unable to create the characteristics we show in this model.**

**We now add several lines saying this:**

**In the Abstract we highlight the autogenic nature of these two pattern configurations; Line 9; page 536**

*“...Finally, we present a novel experiment with the new ‘hybrid’ model to show autogenic behaviors that were not present in the Coco et al (2007a) model (i.e., the appearance of two pattern modes) and discuss advantages and disadvantages of this data driven approach...”*

P535 L 28- P536 L2

*“..However **under steady wave conditions the finite amplitude models by Murray and***

*Thieler (2004), and Coco et al. (2007a) did not reveal the presence of two distinct pattern modes. Instead modeling showed the presence of coarse domains solely on the downdrift flank of bedforms. Coco et al. (2007b) did show the potential for coarse domains to occur in the trough of bedforms, but this configuration was highly path dependent (i.e. the result of a high wave event that is both preceded and followed by smaller waves)”*

**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 autogenic in the hybrid sorted bedform model. This pattern configuration is not observed under steady wave climates in the Coco et al. (2007a) model and only appears as the result of specific changes in forcing (Coco et al. 2007b)”***

**And finally in the conclusion: P552. L18-20**

*“..However, the new hybrid model is able to generate novel **autogenic** behavior in the sorted bedform model: sorted bedform morphology changes when the size of the coarse fraction is modified...”*

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 were 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?

**We state in the manuscript that the mobility of coarse sediment is decreased (but it is not zero). We have rewritten the sentence in the hopes of adding more clarity**

**P547 L 7-9**

*“Bedforms migrate rapidly upcurrent as a result of the decreased mobility of coarse sediment: **coarse material is mobile but is not transported significantly up the flank of the bedform and instead remains predominantly in the trough. This is a result of low coarse sediment mobility relative to the downslope transport term in (4)”***

**The critical value of shear stress for motion is still present in the model and additionally, after receiving this review, we manually verified that there was no violation of the incipient motion criteria.**

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.

**As we have mentioned in the comments above, we cannot in quantitatively compare the Coco et al. (2007) model and the Hybrid model presented here because of the substantial differences in the sediment transport formulations (which means that the same forcing conditions can produce qualitatively different behaviors in the two versions of the model, including the presence of sorting in one and the absence in the other). We can provide qualitative descriptions, which we have done in the paper. We have also clarified the scope of the paper, addressed in the comments above.**

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.

**We have rewritten several sections of the paper to clarify our argument.**

*P535 L 24- P536 L10*

*“Sorted bedforms show several configurations which we divide into two distinct end-member patterns typified by the location of the coarse domain, either in the trough of the bedform or on the flanks of the bedforms (appearance on both the updrift and/or downdrift are possible; e.g., Goff et al., 2005; Ferrini and Flood, 2005). We note that within an individual sorted bedform field the pattern configuration can change (Ferrini and Flood, 2005). Previous work with the finite amplitude models by Murray and Thieler (2004) and Coco et al. (2007a) showed the presence of coarse domains solely on the downdrift flank of bedforms. Coco et al. (2007b) did show the potential for coarse domains to occur in the trough of bedforms, but this configuration was highly path dependent (i.e. the result of a high wave event that is preceding and followed by smaller waves). Van Oyen et al. (2010; 2011), through linear stability analysis, showed the presence of two pattern modes in the initial infinitesimal-amplitude instability that correspond to these two distinct configurations. However Van Oyen et al. (2010, 2011) showed that each pattern mode is the result of separate feedback mechanisms, where coarse domains present in troughs occurred as the result of a flow-bathymetry feedback while coarse domains present on bedform flanks is the result of the previously described sorting feedback (referred to as the ‘roughness’*

*feedback by Van Oyen et al., 2010; 2011).*

*With the goal of presenting a new hybrid model we first describe the development of the near bed suspended sediment reference concentration predictor from the large dataset of Green and coworkers (Green, 1996, 1999; Green and Black, 1999; Vincent and Green, 1999; Green and MacDonald, 2001; Green et al., 2004; Trembanis et al., 2004). 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 **configurations solely from a sorting feedback**) and discuss advantages and disadvantages of this data driven approach.”*

P549 –P550

*“Observational work has previously **detected several** distinct varieties of sorted bedforms, which we group into two end member categories where coarse sediment **appears either in the trough and bedforms where coarse sediment is located on the flank (both updrift and/or downdrift flank is possible;** Goff et al., 2005; Ferrini and Flood, 2005). Van Oyen et al. (2010, 2011) found that these two pattern **configurations** appear in linear stability analysis. Mode 1 bedforms, where coarse domains are located in the bedform trough, have a faster growth rate when waves and currents are weaker and result in bedforms with longer wavelength, larger amplitude, and faster migration rates. Mode 2 bedforms, where coarse grains appear along the updrift **and downdrift** flank of the bedform, have a faster growth rate when waves and currents are stronger and results in bedforms with smaller wavelengths, smaller heights, and slower migration rates. Yet results from linear stability analysis are applicable only at the scale of an infinitesimal perturbation.*

*Results from the finite amplitude hybrid model also show that coarse domains can occur either on the updrift flank of the sorted bedform or collocated with the bedform trough, matching **some aspects of** previous observational work. However instead of relying on two **separate** feedback mechanisms, the hybrid model is **able to reproduce these two pattern configurations solely via the sorting mechanism**. The presence of two distinct pattern modes occurs while current and wave conditions remain unchanged but coarse grain size is varied. When coarse grains are smaller (essentially identical to increasing wave conditions in terms of increasing coarse sediment mobility) bedforms conform to the Mode 2 **features of Van Oyen et al. (2010; 2011)** with smaller features, slower migration rates, and coarse sediment along the updrift flank of bedforms. When coarse grains are larger (essentially identical to decreasing wave conditions in terms of decreasing coarse sediment mobility) bedforms show characteristics that resemble the Mode 1 **features of Van Oyen et al. (2010; 2011)** with larger, bedforms, faster migration rates, and coarse sediment in the bedform trough.*

*Several features of Mode 1 bedforms **in the hybrid model** warrant additional attention. Linear stability analysis (Van Oyen et al., 2010, 2011) suggests infinitesimal Mode 1 bedforms should migrate in the current direction. **The large scale Mode 1 bedforms formed in the finite amplitude hybrid model show upcurrent migration, which has not previously been observed in field examples of sorted bedforms.** Furthermore, Mode 1 bedforms develop in the linear stability analysis as a result of a bathymetric-flow feedback (Van Oyen et al., 2010, 2011). The finite amplitude hybrid*

*model presented here does not parameterize hydrodynamics at small enough scales to permit the development of bedforms as a result of a flow-bathymetry feedback. In contrast to the linear stability analysis, Mode 1 bedforms in the hybrid model develop as result of the sorting feedback operating at finite amplitude. Future work with more detailed hydrodynamic parameterizations could shed light on the interplay between flow-bathymetry interactions and the sorting feedback in the Mode 1 regime at finite amplitudes. **However, these results do suggest that the finite amplitude hybrid model is able to capture the dynamics observed in the field. The presence of two distinct pattern modes in the hybrid model is a direct result of incorporating new data driven parameterizations of the sediment transport process. In this contribution we explore only one specific mechanism that results in Mode 1 sorted bedforms, increasing the diameter of the coarse grain size fraction. There are likely other mechanism by which Mode 1 bedforms may develop instead of Mode 2 bedforms, notably by increasing water depth, decreasing wave forcing, or decreasing current velocity.***

P552 L20-22

*“This model behavior more closely resembles field observations showing sorted bedform coarse domains that occur in multiple positions along the bedform (however downdrift coarse domains still do not appear in this model)”*

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.

**We have added an additional line to the manuscript to state that upcurrent migration of bedforms has not been observed:**

**p.550 Line 4-7**

*“Several features of Mode 1 bedforms warrant additional attention. Linear stability analysis (Van Oyen et al., 2010, 2011) suggests infinitesimal Mode 1 bedforms should migrate in the current direction, at odds with the finite amplitude hybrid model. **Upcurrent migration has not previously been observed in field examples of sorted bedforms”***

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?

**This is an excellent question and we address this issue in an additional paragraph in the discussions section.**

**Line 9; page 549**

*“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.*

*Results shown in this contribution use two new prediction schemes based on GP (i.e., ripple morphology and reference concentration). We believe the new ripple prediction scheme of Goldstein et al. (2013) is an improvement over the previous method used in the Coco et al. (2007a) model, however ripples in this model only significantly impact the vertical sediment diffusivity ( $E_v$ ) and the roughness height ( $z_0$ ). The reference concentration, since it sets the magnitude of suspended sediment, is more strongly related to the new behaviors in the model and as a result we focus our analysis on the reference concentration.*

*Observational work...”*

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.

**Oehler et al. (2012) developed a boosted regression tree and artificial neural network model for reference concentration with the exact same dataset as this study. However the splitting procedure for training/validation/testing data was entirely different (they did not use a selection routine). As a result the independent testing data used to evaluate the GP derived predictor in this paper is significantly different (as well as much larger) than that used in the work by Oehler et al. (2012). This difference means that we are unable to directly compare the predictors from the Oehler et al (2012) study with the GP predictor developed in this work. This issue of ‘transferability’ and comparison between ML/data driven predictors developed in different papers is one we are actively working on: reviewing the ML literature and developing a set of ‘best practices’.**

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, Gutierrez et al. (2005) suggest a significant correlation between the near bed shear stress (controlling the sediment

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

**Thank you for bringing this caveat to our attention. We now add a new line to the text.**

**P548 L19-20**

*“The  $C_0$  predictor does not explicitly account for nearbed currents that may be important mechanisms for enhancing suspension in sorted bedform fields (e.g., Gutierrez et al., 2005). 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).”*

**Associated new Reference to add to the manuscript:**

*Gutierrez, B. T., G. Voulgaris, and E. R. Thieler (2005), Exploring the persistence of sorted bedforms on the inner-shelf of Wrightsville Beach, North Carolina, Cont. Shelf Res., 25, 65–90.*

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.

**This is an excellent comment, and work is underway to map out the phase diagram for different modal behaviors in the new Hybrid model. We believe this represents a significant study of its own. A line has been added stating that there may be other routes to Mode 1 behavior.**

**P550 Line 15-19**

*“However, these results do suggest that the finite amplitude hybrid model is able to capture the dynamics observed in the field and suggested by the analysis of infinitesimal features through linear stability analysis. The presence of two distinct pattern modes in the hybrid model is a direct result of incorporating new data driven parameterizations of the sediment transport process. **In this contribution we explore only one specific mechanism that results in Mode 1 sorted bedforms, increasing the diameter of the coarse grain size fraction. There are likely other mechanism by which Mode 1 bedforms may develop instead of Mode 2 bedforms, notably by increasing water depth, decreasing wave forcing, or decreasing current velocity.**”*

**Specific comments:**

*p. 532*

- i) 1. 8: “This newly ... predictors” → “We demonstrate that this newly ...

predictors”

**We have clarified this line:**

**P532 L8**

*“We demonstrate that this newly developed parameterization performs better than existing empirical predictors.”*

ii) 1. 16: “However, ... modeling.” A bit a strange sentence. Please rephrase

**We have clarified/simplified this line:**

**P532 L16**

*“Results suggest that the new hybrid model is able to capture dynamics previously absent from the model, specifically, the two observed pattern modes of sorted bedforms. **Lastly we discuss the challenge of integrating data driven components into morphodynamic models and the future of ‘hybrid’ modeling.**”*

p. 533

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

**We have clarified this line:**

**P533 L3**

*“...The inaccuracy of individual predictors has significant consequences in nonlinear morphodynamic models because **errors accumulate** as inaccuracy is (1) propagated through the nonlinear pieces of the model....”*

p. 535

– 1. 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  $\frac{1}{4}$  can hardly be described as related to only a slight bathymetric relief. Please adjust the manuscript suitably taking into account these observations.

**We have refined this sentence to reflect our intended meaning:**

**P535 L7**

*“Spatially extensive (km scale) patches of segregated coarse and fine grained sediment (Fig. 1) with only slight bathymetric relief (cm–m scale) **relative to bedform pattern wavelength (10 m–km)** are present...”*

i) 1. 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.

**The references to Van Oyen et al (2010; 2011) were added to this line because both studies show that Sorted bedforms can develop because of a sorting feedback. We will remove the Van Oyen et al references from this line.**

- ii) 1. 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.

**We have addressed this comment above in the ‘Major Comments B’ section**

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.

**This comment is extremely helpful because a major point of our manuscript was in fact to show that both pattern configurations can appear as a result of solely considering a sorting (or ‘roughness’) feedback (this appears in P550 L11-12). We hope that by addressing ‘Major Comment B’ above that we have clarified our position.**

*p. 536*

- iii) 1. 18: The use of the variable  $d_0$  for the wave orbital diameter at the bed is a bit confusing.

**We are unsure how to address this comment and would like Dr. Van Oyen elaborate.**

*p. 537*

- 1. 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?

**This line has been rewritten to avoid confusion:**

**P537 L5**

*“In addition to the multiple settings and significant amount of data, this dataset is ideal for application in the sorted bedform model because **three of the six experiments in the composite dataset** are derived from a sorted bedform field (Green et al., 2004; Trembanis et al., 2004).”*

- 1.8 - ... : 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.

**We have added a citation to our previous work:**

**P537 L21-25**

*“However, in the genetic programming literature we could find no proven “best practice” for selection of the data subsets or an optimal percentage of training, validation, and testing data (Kuschu, 2002; Panait and Luke, 2003; Gagné et al., 2006), and therefore use a technique that was used successfully in a previous study (Goldstein et al., 2013).”*

*p. 539*

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

**Thank you for pointing this out, and we will add the missing summation sign in the LaTeX version of the MS.**

*p. 542*

- 1.9: 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.

**As we state above the incipient motion criteria is not violated in these model iterations. We add a line stating that the predictor does not perform well at low concentrations.**

**Line 19-27, p 548**

*“...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 shows poor performance at low concentrations (Figure 6).*

*The poor performance may be the result of nonlinearities in sediment transport that are not captured by the prediction scheme, a noise in the experimental signal at low concentrations, or other as yet unknown reasons. ”*

**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.** ”*

*p. 543*

- 1 15: Equation 2: the flux of suspended sediment needs to be integrated from some level to the free surface, please specify in the equation.

**We have added an additional line to address this comment:  
P543 L21**

*“...efficiency factor (set to 0.035). **The integration of suspended sediment flux begins at the height where reference concentration is defined. The second...**”*

**We define this height later in the text on p. 545 L1.**

*“The reference concentration is applied at the height of the ripple crest, as in Coco et al. (2007a).”*

*p. 544*

- 1. 19: Please provide some indication of the impact of assuming  $U_{sig} = U_w$
- Equation 9:  $C_0 \rightarrow C_{0,s}$

**We add a line to address this issue.**

**P548 L6-9**

*“The GP reference concentration predictor relies on  $U_{sig}$ , while the sorted bedform model uses  $U_w$ . In the hybrid model we assume  $U_{sig} = U_w$  where  $U_w$  is calculated from linear wave theory. **We force the sorted bedform model with monochromatic waves so this assumption does not impact the model results shown.** Additionally we direct the reader to other methods available to estimate  $U_{sig}$  from surface wave parameters (e.g., Wiberg and Sherwood, 2008).”*

*p. 561:*

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

**In the Acknowledgements we thank Terry Hume at NIWA, NZ for providing Figure 1.**

*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?

**We have added to the caption (though it should be noted that this figure was made for this paper, and is not a ‘copy and paste’ reproduction from Oehler et al. (2012)):**

**P562 Figure 2 caption:**

*“...Tmean). A similar figure appears in Oehler et al. (2012).”*

*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.)

**The ‘fuzziness’ of this picture is actually horizontal (cell to cell; or pixel to pixel) changes in cell coarseness, which are not smooth but discrete. This might account for the apparent fuzziness of the picture.**

**Again we thank the Dr. Tomas Van Oyen for carefully reading and reviewing our manuscript and we hope that these responses, clarifications, and corrections make our manuscript more clear.**