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Interactive comment on "Relations of physical and biogenic reworking of sandy sediments in the southeastern North Sea" *by* Knut Krämer et al.

Anonymous Referee #1

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Review of 'Relations of physical and biogenic reworking of sandy sediments in the southeastern North Sea' by Krämer et al.

Summary.

The manuscript describes a set of observations from a number of sites in the German Bight, including sediment characteristics, current velocities and sea-bed micro topography. Successive scans of the sea-bed micro topography are subtracted to reveal changes in bed level, which are interpreted in terms of physical and biological reworking of the sea bed. Linear relationships are fitted through the biological reworking data as a function of environmental characteristics.

General comments.

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Discussion paper

The topic of the manuscript is interesting and relevant. The manuscript reads well, and I found very few grammatical or typographical errors. The figures are clear and easy to read. The primary data set is interesting and relevant, and worth publishing. However, the structure can be improved (Data and Methods contains elements of Results and vice versa). Also, lots of detail is missing, to the level that it would be next to impossible to replicate the work/results. Moreover, I have fundamental concerns regarding the analysis and interpretation. I will explain all of this below. Overall, I think this takes the manuscript beyond major revisions (which would come with a deadline that's too tight to do the additional work required to bring it to a level that is publishable). As the basic data are sound and interesting, and the authors write well, I would suggest rejection, and encourage the authors to re-think the analysis, re-write the manuscript, and re-submit when they are ready.

Analysis - main concerns.

1. The authors use changes in surface level as a measure for reworking. For migrating bed forms, if done at a high enough temporal frequency, this gives a good estimate of the volume of bed material that is moved. However, this does not hold for biogenic reworking by burrowing organisms. These may move a whole column of sediment down to their maximum burrowing depth, with only minimal changes in sea-bed elevation. Hence, at best, the results presented for biogenic reworking represent a lower boundary for the range of potential true values. As a result, the terminology (up to the title!) is misleading, and 'surface level change' (or something equivalent) should be used instead of 'reworking'. Also, these caveats should be stated clearly.

2. The current manuscript also does not use the species analysis from the box cores to its full potential.

3. The main analysis, described in Section 4.4, is unclear, seemingly constructed from random bits, and the result is demonstrably wrong. Why use 'time-averaged values for the varying quantities' when the reworking rates are instantaneous values (eqn 5) -

or are they? If the reworking rates are also time-averaged, why was this done before regression? Surely regression can also be carried out (and better) on instantaneous data? How was the time averaging done - in the same way for all the quantities? Over an exact tidal cycle so there are no truncation errors? Or different for the biogenic reworking which is only active part of the time? Why have separate linear regressions if a function (eqn 11, 12) is available (I now guess that it may be a step in constructing 11 and 12, but this is not clear from the text - and this doesn't make it a correct approach)? Why linear - are the processes expected to be linear? Apparently not (eqn 11, 12). Why is there a mis-match in units (meters, milli-meters, micro-meters)? Consistent units (m, s) should be used throughout. Where do eqns 11 and 12 come from? Why would burrowing organisms respond like this? How can eqns 11,12 be correct (or an un-biased phenomenological relationship) if all but one of the data points are above the functional curve (Fig 12)? Another argument why it can't be correct is that the 'constant' 3.524 has units (mˆ0.5 sˆ-0.5 - ignoring the mess of m vs mm, sec vs day for simplicity), and hence contains part of the processes. A proper phenomenological function with should have fitted constants that are non-dimensional. What, in the end, is the physical/biological meaning of eqn 11,12? Why use current velocity, and not current shear stress and wave shear stress which were both shown to be important earlier on? This should all be re-done, using a uniform set of units, starting from relationships that make biological/physical sense, and using dimension analysis to plug the gaps, and using multi-variate regression if/where appropriate. I am not sure if this kind of approach is realistic and feasible, also given the relatively few data points that the authors have. One option they could consider is to abandon this approach and do something more feasible with the data?

Structure.

1. To improve the flow, Section 3.3 should come after Section 3.6.

2. Figures 2 and 3 are presented in Data and Methods, but they are Results.

3. There needs to be a section before the current 4.1 that describes the results of the core samples in terms of species: numbers and biomass. In 2.4, these species need to be described in terms of size and bioturbation characteristics (type, depth and activity), incl. references. These data need to be included in the discussion/interpretation, including in the new analysis (4.4).

4. Section 4.4 is largely Theory/Methods - split the derivation of the methodology to a section in 3 and keep the implementation/numbers/results in 4.

5. Figure 10 is not referenced nor discussed.

Detail.

Title: needs changing to reflect definition of 'reworking' - see above.

Results: past tense should be used throughout to describe the results.

p. 2, l. 21 patterns of

p. 3, l. 12. patterns of ... with water depth

p. 3, l. 16-17. Requires a reference. Tidal currents are not necessarily small near amphidromic points (quite the reverse!)

p. 3, l. 18. regularly observed for

p. 3, l. 20. semi-monthly time scale

p. 3, l. 20. adding: to what?

p. 3, l. 20,22. 'subcritical' vs 'sub-threshold': please use uniform terminology throughout. I have reservations about 'sub/supercritical' as these terms have a different meaning for fluids/gasses in thermal physics.

p. 3, l. 24-29. This paragraph requires references. Also please discuss presence of megaripples and sand waves.

p. 3, l. 25. migrate over a distance of the order

p. 4, l. 1. There are five macrofauna communities in the southeastern North Sea.

p. 4, l. 1-6. It would help the non-biologist if common names are also given. Moreover, are these communities (as said) or species? If communities, surely they consist of multiple species - please describe. Please also include here a description of the species found and analysed in the box cores, including their burrowing characteristics (type, depth, activity, timing of activity, etc.).

p. 4, l. 9-14. Please include a diagram of the lander configuration with instruments.

p. 4, l. 14. ...12 hours after which the lander was recovered. (?)

p. 4, l. 15. 10-minute averaged: why?

p. 4. Refs to Soulsby. Please remove page numbers - this does not conform with the journal standard. Also they are apparently not consistent between prints (they are different in my copy).

p. 4, l. 17. Only data with...: how frequently did this occur, i.e. how much data was discarded?

p. 4, l. 18-19. Waves from model. This surprises me. Can this not be derived from the ADCP data?

p. 4, l. 25-28. Soulsby used field data to fit their model, so the influence of maximum waves is already included in eqs 1 and 2. Remove this, and remove it from all the calculations.

p. 5, l. 3. laser diffractometer. Please provide resolution and accuracy.

p. 5, l. 5. tau_crit, d50. Please provide equations. How was d50 derived from the size distributions?

p. 5, l. 7. roughly. How roughly? Why not exactly? Why not more often? How does

this affect the calculations (eqn 4,5)?

p. 5, l. 9. sledge. This suggests something pulled along the sea bed, which I don't think is the case? Is there a better word for this? Carriage? See also remark about adding a diagram of the lander.

p. 5, eqn 4 and 5. It would be better to use central differences in time rather than upwind? The current formulation results in an R at t(i-1/2). How are the other data treated on this time frame?

p. 5, l. 23. and the temporal pattern

p. 5, l. 28. in a characteristically regular pattern

p. 5, l. 29. manifests itself in

p. 6, l. 3. was not identified. (?)

p. 6, l. 7. Figure 5 is reference before figure 4.

p. 6, l. 6-7. I don't recognise what's described in the text in what's plotted in Fig 5.

p. 6, eqn 6. Needs a reference.

p. 6, par. 3.6. Is the sampled area big enough, i.e. does it contain enough ripples, to get unbiased estimates of ripple height?

p. 6, l. 17. species: which?

p. 6, l. 18 bioturbation potentials and reworking modes were determined. How? Also these are not in Results - please add.

p. 6, l. 19. descriptions of burrowing structures and dimensions: reference and provide these descriptions, how was the comparison done?

p. 6, l. 20. bioturbators responsible for the observed changes in bed level. (?)

p. 6, l. 22. critical shear stress: requires equation and reference.

p. 6, l. 23. Fig 4-8. This is Results.

p. 6, l. 24. why separate, not just combined?

p. 7, l. 5. evidence of

p. 7, l. 6/7. deployments (Table 1).

p. 7, l. 12. station: where: on the top or in the trough of a dune?

p. 7, l. 15. reworking rate (41.7 mm d-1)

p. 7, l. 17. The characteristic spatial pattern of biogenic erosion ...20 mm and a depth of ***

p. 7, l. 19. identified: how?

p. 7, l. 23. remove former

p. 8, l. 2. did not contribute to the actual

p. 8, l. 4 characteristic biogenic erosion

p. 8, l. 7 suggest(ed): Why? Which other members were also involved?

p. 8, l. 11. Figge, 1981. Why rely on 40-year old data for this? This can well have changed! You've done particle size analyses - use your own data!

p. 8, l. 9. settled: how much? Also: this suggests a higher mud content than 5-10%?

p. 8, l. 16. cannot: why not?

p. 8, Section 4.2. This section needs to be improved and expanded, using specific information from the core samples.

p. 8, l. 23-24. refer to fig4b in this sentence.

p. 8, l. 27-28. Why is this here? Why is it relevant? It is not used further on. If it is relevant, please give the equations (these are thick books), justify in the Introduction

and discuss in the discussion. Also, I don't understand why this is the only station with a significant reduction in ripple height, as the rates of bed-level change are similar as for other stations (Fig 5, 7).

p. 9, eqn 7-10, 12. Please provide error estimates of the regression coefficients.

p. 9, l. 19. T<10. How do you know it's not T<7?

p. 9, l. 20. Flow velocities: it's crucial to know how these were averaged.

p. 9, l. 25. Topt=25. Why? (this comes later, but that bit should be moved to Methods)

p. 10, l. 10-12. This statement is not backed up with data.

p. 10, l. 15. pollutants contained in

p. 10, l. 17-18. No, this gives 4 values for R_bio that will generally not be the same.

p. 11, l. 10-15. Or both?

p. 11, l. 27-29. Quite a statement to make based on 1 sample! Suggest to remove this line of reasoning from the manuscript.

p. 12, l. 3, fig 6: does not display ripple height.

p. 12, l. 3. different: than what?

Figures.

Figure 3: the colour scale saturates - please improve the plots such that maximum and minimum values are shown properly. What are the white bars (missing data?)? How are these dealt with in the calculation of R?

Figure 4-8: delta_z/delta_t: these have a sign, please represent. Currently the figs suggest that the bedforms keep growing.

Figure 9-12: The data (R_bio, d, d50, T, u) are said to be averages. Please provide error bars for all of these calculated from the scatter of the data that compose these

averages.

Figure 5-8: why not plot ripple height as in Fig 4?

Figure 9: I'm confused, as on p.6, l. 1-2 it was stated that mixed was not determined, yet here it is...

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