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Ocean Science Discussions

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

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Dear anonymous Referee,

thank you for your detailed comments and the positive feedback regarding the value of the primary dataset. Thank you also for your suggestions for improving the structure and flow of the manuscript.

We address your concerns about a) the amount of detail (in the methods) and b) the main analysis below, and are positive that these can be handled by a review of the manuscript within a few weeks time. Some of the criticized issues seem to be based on wording, maybe a misunderstanding of what we intend to point out with this dataset and our analysis: In our view the main value of this manuscript lies in the detailed field

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Discussion paper

observations of biogenic (and physical) overturning of sediments under natural conditions. The word overturning now is used in a sense of bed surface change, which certainly is not similar to the amount of material moved inside and at the bed (=bioturbation). The overturning by physical processes can be described with the well known concept of critical and effective shear stress. The observed biogenic overturning on the other hand is a much more complex process, and has so far been understudied under field conditions. We evaluated time-averaged values as a more robust descriptor for typical biogenic overturning of the sediment surface than the instantaneous rates. The narrow range of observed values for different settings shows the precision of the method but also the validity of said average overturning rates as typical values for the entire region. The empirical regressions with the chosen set of physical boundary conditions are provided as possible descriptors for the observed range in average biogenic overturning rates. The high correlation coefficients justify this approach given the small number of observations available at this point. We did not mean to provide a complex model for the interactions between physical boundary conditions and the activity of the benthic fauna.

Knut Krämer

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.

> We agree that the term ‘reworking’ must be used carefully and includes the activity of the benthic fauna in the sediment volume up to their maximum depth of activity. The term was chosen because it is common in the related literature and actually used to describe only surficial changes (e.g., Grant, 1983). We think that ‘surface level changes’ would not grasp the importance of the mechanism for the exchange between sediment and water column (p. 1, l. 3-4; p. 12, l. 19-23). Instead, we would propose the term ‘overturning’ (of surface sediment) as it may help to describe the volumetric changes which transport material across the benthic interface i.e., the sediment surface, which is registered by the method. These terminology problems will be taken up in the discussion. The caveats are already partly discussed (p. 10, l. 12-15), but this will be extended.

2. The current manuscript also does not use the species analysis from the box cores to its full potential.
> Multicorer (MUC) (p. 6, l. 16) cores were used. Due to the small area and volume of sediment covered by this method, it may be unsuitable for a representative description of the benthic fauna. Indeed, no correlations of the observed biogenic overturning rates with bioturbation potential (p. 10, l. 10-13) were found. Given this, we decided to remove all information gained from the core samples and speculations related to individual species or benthic communities and present the observed biogenic overturning rates with an assumption based on earlier studies in the area.

3. The main analysis, described in Section 4.4, is unclear, seemingly constructed from random bits, and the result is demonstrably wrong.
> Obviously we could not get our message through. We answer the individual issues below:

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?

> The regressions were meant to provide a first estimate of biogenic sediment overturning, compared to basic physical values describing the overall setting at the locations rather than touching complex instantaneous interactions. Most of the physical parameters used in the regressions are (more or less) constant over the observed period (d, T, d_{50}). Mainly the flow velocity changes throughout the tide. Thus a point-to-point regression with instantaneous biogenic overturning rates is not considered useful, as the fauna is only active (or can be observed) under sub-threshold conditions for sediment transport. Nevertheless, We agree that as indicator the average flow velocity may be misleading and the maximum flow velocity or shear stress observed may better represent this physical boundary condition. This would also help to overcome the problems with truncation errors noted below.

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?
> The averaging was done for the respective periods of either physical or biogenic activity. The observation cover only part of one tidal cycle due to the limited battery capacity (see Tab. 1). The chosen unit of [mm/d] may be misleading because it suggests that the measured rates were extrapolated to longer periods which is not the case. This will be changed. Not knowing the exact reworking by individual species, we consider an average rate. The rates provided were meant to give an idea of the typical overturning activity for a given station and time. We think that the good correlation of the average rates with the physical boundary conditions justifies this approach.

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).
> We provide a first observation – not a complete model of the relation of physical and biogenic overturning. The individual regressions show possible relations of the

observed biological activity with individual oceanographic conditions. Linear regressions are suggested as a simple first approach to correlate the biogenic activity with the physical parameters. More complex relations would only make sense if we already had a model for the behavior of the benthic fauna with regard to the physical boundary conditions. The function in eq. 12 was not available but constructed by 'summarizing' the individual relations (p. 9, l. 22-23; eq. 11). We will not include the separate equations in the next version to avoid confusion; and just provide the best fit model.

Why is there a mis-match in units (meters, milli-meters, micro-meters)? Consistent units (m, s) should be used throughout.
> The unit [mm/d] is common in describing biogenic reworking rates in the related literature and results in comprehensible values. For the physical parameters, the common units (e.g., d_{50} [μm]) were maintained. From the confusion this generated with regard to the validity period of the measurements (see comment above) and in the regressions we agree that it is better to abandon simplicity for the sake of consistent (SI) units.

Where do eqns 11 and 12 come from?
> Equation 11 was constructed by 'summarizing' the effects of all individual relations (eq. 7-10) in the the factor
$k = \frac{T}{T_{opt}} \cdot u \cdot \frac{d_{50}}{d}$.
Doing so, the combined equation becomes $R_{bio}^2 = const. \cdot k$ or $R_{bio} = const. \cdot \sqrt{k}$. Equation 12 is the result of linear regression between R_{bio} and $\sqrt{k}$.

Why would burrowing organisms respond like this?
> Possible explanations for the behavior of species acting at the sediment surface are given in section 5.2.

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)?
> Eq. 12 was presented without a constant offset, therefore it only contains the gradient (or slope) in the data. A constant value would provide a better match with the obser-

vations. We will change the fit between biogenic overturning and physical parameters according to the following comments.

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.
> Eq. 12 was constructed to include **all** individual relations (and processes). The units are the result of the root function.

A proper phenomenological function with should have fitted constants that are non-dimensional.
> We agree that a dimensionless approach is better to express the relations. A part of the individual physical quantities evaluated (T, u, d_{50}) could be 'summarized' in the non-dimensional particle Reynolds number:
$Re_p = \frac{\rho \cdot u \cdot d_{50}}{\mu}$ (Fig. 1).

What, in the end, is the physical/biological meaning of eqn 11,12?
> Possible explanations for the relation between biogenic and the physical boundary conditions were given in section 5.2. The relations in eq. 11/12 were meant to 'summarize' these effects.

Why use current velocity, and not current shear stress and wave shear stress which were both shown to be important earlier on?
> We chose current velocity as the most simple parameter describing this aspect of the physical forcing. It was the measured parameter. All later calculations of shear stresses would just mimic this.

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.
> We agree that the consistent use of SI units is better for this approach. The regressions will be repeated using multivariate analysis as the parameters are not indepen-

dent (e.g. lower flow velocities and smaller median grain sizes at the deeper stations). We will provide dimensionless equations in a reviewed manuscript.

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?
> This is a presentation of a unique dataset. We provide this data and an interpretation of it, and show how the data relates to physical boundary conditions. We think that this is a common and correct way of research. Providing data will allow others to find better, 'more feasible' answers.

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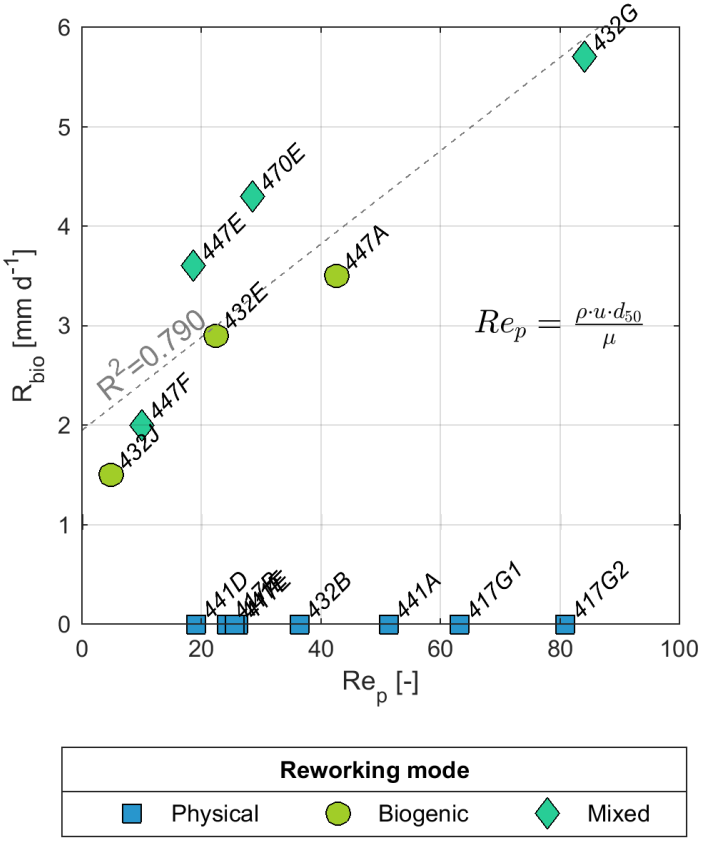

Fig. 1. Correlation of the overturning rates with the particle Reynolds number.