

Interactive comment on “Evidence for a maximum of sinking velocities of suspended particulate matter in a coastal transition zone” by Joeran Maerz et al.

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Reply to anonymous referee #2

Dear referee,

thank you very much for your really valuable and constructive comments on our manuscript! In the following, a detailed listing of answers to your general and specific comments is given. Your comment is set in italic and respective answers are set normal.

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General comment:

“The manuscript aims to present new evidence to understand SPM dynamics in coastal seas, especially the mechanism that sustains the net-transport of SPM towards coastal systems, such as the Wadden Sea. The authors have used an approach that indirectly calculates the settling velocity in a cross-shore direction by using measured SPMC profiles and modelled turbulence data. The approach is original and the results are convincing. However, I am not in favor to call the obtained results an ‘evidence’, as the methods is based on a lot of assumptions that are not providing ‘evidence based data’. Some of the assumptions used are further speculative and thus not supporting evidence. The latter would, e.g. be the case if settling velocity would have been measured directly. Nevertheless, even without direct evidence, the manuscript remains valuable and will surely inspire other scientists to look for similar mechanism in other coastal regions or to adapt existing monitoring programs to collect more evidence based data to prove the hypothesis. My major comment is therefore to not argue that you have found evidence to accept the hypothesis. Change the title accordingly and weaken the conclusions. As I have also some specific comments (see below) that are going further than minor changes I recommend to accept the manuscript only after major revisions.”

Changed the title to “Maximum sinking velocities of suspended particulate matter in a coastal transition zone”

We weakened the conclusions by replacing ‘evidence’ with ‘strong indication’ and also weakened the retention capability by including ‘processes would have the potential to retain fine sediments and nutrients in coastal areas’. To make the importance of turbulence features more clear in the evolution of the transition zone, we additionally included a semi-clause on the relevance of the energy dissipation rate intensity.

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Specific comments:

“SPM concentration or SPMC: try to be consistent. Sinking velocity or settling velocity: idem”

We are now using SPMC and sinking velocity throughout the whole text.

“p2 127-28“ Cohesive sediments and POM can undergo aggregation and fragmentation processes that change transport properties and thus their sinking velocity”. Change the sentence into: flocculation changes the settling velocity and thus the transport properties and not the opposite way.

Order changed.

In previous sentence you make the difference between cohesive and non-cohesive minerals, thus between clays and the other minerals such as quartz or carbonates. Do you have evidence that only the cohesive minerals are involved in flocculation? What about very fine quartz or carbonates that can due to electric charges or the presence of specific organic molecules (EPS) be involved in flocculation?”

We changed the sentence to: “Fine-grained minerals of sizes typically up to $8\mu\text{m}$ (Chang et al. 2006) and POM can undergo aggregation and fragmentation processes that change sinking velocity and thus transport properties.”

“p2 137-39: “In shallow waters near the coast, where turbulence and thus resuspension are high, SPM concentration is usually enhanced and dominated by mineral particles with high densities”. Better: The SPM concentration consist of flocs that are composed mainly of mineral particles. p2 139-40: “By contrast, in deeper off-shore regions, SPM concentration is comparably low and consists to a higher extent of POM with low

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densities.” Better: ... SPM concentration is lower and the flocs are looser and more organic.”

Changed accordingly.

“p2 151: what do you mean by ‘restructuring processes’?”

We now give a reference to Becker et al 2009, who investigated restructuring (e.g. resulting in compaction – less fractal flocs) process under shear stress theoretically.

“p3 167: add a reference for the ‘typical power law relation’”

We included the reference: 'Dyer 1989'.

“p3 194: an average depth of 80 m is not what I would call ‘shallow’. Further, the water depth in the German Bight seems to be much less than 80 m from Fig 1.”

We changed the sentences to: “The surveyed German Bight (Fig. 1) is located in the south-east of the North Sea and features a typical depth of about 30 m to maximal 50 m. The North Sea is a shallow shelf sea with an average depth of 80 m . . .”. Compared to typical definitions of continental shelf with about 200 m water depth and for the Antarctic continental shelf of even about 400 m, an average depth of the North Sea with 80m is shallow.

“p4 199: The Rhine is not the only source of SPM and nutrient in the southern North Sea: see the general circulation pattern that brings Channel water into the North Sea and all the subsequent sources. What about the East Anglia plume that extends towards the German Bight?”

We agree that the East Anglia plume occasionally extend towards the German Bight. We now state this in the description of the study area. However, during the cruises no

SPMC increase was found in the outer region of the surveyed study area which could be attributed to the East Anglia plume. We checked the scanfish data again in this respect. For surface waters, this is also visible in Fig. 7, upper panel. Additionally, most of our observations stem from regions that are usually not affected by the East Anglia plume.

“p4 I110-113: I don’t understand the hypothesis of ‘line-of-no-return”

We added the sentence: “The line-of-no-return is conceptually described as the imaginary line beyond which particles escape coastal trapping mechanisms such as e. g. density gradient-driven undercurrents (Postma 1984).” to give more explanation.

“p4 I119: The cruises are all in spring/summer period. Would the hypothesis of maximum settling velocity also be valid in winter? Do you have winter data?”

We added the sentence: “No winter measurements were carried out.” Hence, the validity of the hypothesized/found zone of maximum sinking velocity has still to be explored for winter. We think that we made it clear in the conclusions that more investigation on seasonal patterns are required.

“p4 I123: sampling rate of 11 s-1: better 11 Hz”

Changed.

“p4 I124: conductivity or specific conductance. I am not sure that the term ‘specific conductivity’ is used.”

Changed to: “specific conductance”

“p4 I126: Is a Seapoint turbidity meter appropriate to be used in the high turbidity coastal areas where you say that SPM concentration is > a few 100mg/l.”

Typically, SPMC of much lower values were measured. As shortly described, obvious faults – such as clipping due to too high turbidity values would have been removed during post-processing of the data. In addition, the Seapoint turbidity meter we are using has a range up to 500 FTU, a value that was never reached during our surveys.

“p5 I147: “If we assume that the sinking time scale is larger than the tidal period”. How valid is this assumption? The time scale of a tidal cycle is about 12.5 h (or if you consider ebb/flood: about 6h), and of the sinking time scale about 3-6h ($w=1$ mm/s in 10m water depth: 3h, $w_s=0.5$ mm/s in 10 m water depth: 6h). This seems to me quite similar. What are the consequences of this assumption on the results?”

We assume that the effects are of second order. Would we relax the assumption on the sinking time scale, we had to assume a parabolic viscosity profile. This would result in an SPM profile with an power law shape $a * SPMC^b$ instead of the exponential profile. However, the introduction of a parabolic viscosity profile would introduce a new unknown constant, the reference concentration in the bottom boundary layer. Despite this theoretical considerations, we still believe that the differences are small. Since we do a local fit of the SPM profile, the the difference between a power law fit and an exponential fit are smaller than the errors introduced by the parameter fitting. Moreover, the introduction of the reference concentration for the power law fit would introduce a further source of uncertainty and an additional free fitting parameter. This free parameter would be difficult to bound, since the measurements did not resolved the bottom boundary layer.

Concluding, although we know that the assumption of an exponential concentration profile provides an upper bound on the sinking velocity, it is a good trade of between accuracy and over-fitting of free parameters.

“p5 I150: C_m is not defined. Is this the depth dependent SPMC?”

Yes, it is the analytical model description of the theoretically expected vertical SPMC

distribution. We added C_m after 'analytical model'.

“p5 1160ff: How big is the difference between model results and observations. It would help a lot to better understand the procedure if you would show examples of the fitting, interpolations etc.”

We now provide an exemplary figure, where the different steps of the data processing with respect to model-observations comparison, profile splitting and analytical model fitting are shown. Additionally, we added a sentence in the text to point to the figure.

“p6 1174 On what is this criterion ($< 0.0005 \text{ kg m}^{-3} \text{ m}^{-1}$) based see line 168 where it is 0.015.”

We noted in the description of the Monte-Carlo-type simulation at the end of Sec. 2.3 that threshold values were selected upon visual inspection criteria. To have direct correspondence, we now added: “Since the critical value was set by visual inspection, it was also considered in the Monte-Carlo-type simulation described below.”

“p6 1175-176: “The co-occurrence of strong gradients in σ_T and SPMC can indicate dampening of turbulent mixing and potential particle properties’ changes.” Is it always the case that strong gradients in sigma T are co-occurring with those in SPMC? What if only one of the two parameters has strong gradients?”

A vertical gradient of SPMC is what we expect to see, while a strong gradient in σ_T can, but must not necessarily, imply potential particle property changes. We added the sentence “While we generally expected vertical gradients in SPMC, strong vertical gradients in SPMC potentially reflect weak mixing and are thus probably an additional indicator for changes in turbulence intensities.” and included an expanded explanation for our splitting approach. See also the text and the comment to reviewer #3.

“p6 line 189: On what is the cost function value of 0.05 based?”

We added “, chosen by visual inspection,”.

“p6 line 190: the variable F is not defined.”

It is/was defined in Sec. 2.2 I.127 of the reviewed version of the manuscript. It is fluorescence. We now nevertheless introduce it here again and clearly state: “... SPMC, and fluorescence to SPMC ratio ($F/SPMC$) ...”

“Why is it necessary to bin the data. If I understand well the methods than the data have already been interpolated on 5 cm resolution.”

For every profile, for which one w_s value was found, variables SPMC, etc. were vertically averaged. Afterward, those value and the according w_s were binned with respect to epsilon, which relates to turbulent shear as an important driver in flocculation processes. We rephrased the sentence to: “The variables SPMC, and fluorescence to SPMC ratio ($F/SPMC$) were vertically averaged for the respective (sub-) profiles. Afterward, variables were binned with respect to modeled ϵ ...” We hope, the procedure becomes more clear now.

“p7 l 209: “be described operationally as fractal dimension-like”: what do you mean by ‘operationally’, skip ‘-like’”

Both, “operationally” and “-like” were written to state that fractal dimension is here not meant in a strict mathematical way – so that there exists a cutoff, especially for very small particles reaching the limit of the primary particle diameter size. Since this is a detail that does not concern our results and the conceptual model, we simplified the sentence and skipped both, “operationally” and “-like”.

“p8 l246-249: I don’t agree on the potential significance depicted by $F:SPMC$ ratio. Fig 3b shows in fact that F is increasing towards the high turbulent areas (towards the

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coast thus). As SPMC is however stronger increasing than F the ratio is dropping. The highest alga concentrations are generally found in the nutrient rich, high turbid coastal areas.”

We agree that the total amount of biomass tend to increase towards the coast. However, the relative weight/weight ratio of POM/SPM typically decreases towards the coast (so high loss on ignition in the deeper regions compared to low loss on ignition in the shallow, turbid regions – this is ubiquitously observed and also conceptually introduced in our introduction). Since on first approximation, fluorescence F is proportional to POM, it should be eligible to use F/SPM ratio as proxy for loss on ignition. To clarify our assumption, we rephrased and added a sentence: “Under the assumption that fluorescence can be applied as a proxy for POM, the potential significance of algae and their products for particle composition is depicted by the mean ratio between measured fluorescence and SPMC. High F/SPMC ratios likely indicate rather organic-rich particles, while low F/SPMC ratios potentially indicate rather mineral particle-dominated flocs. ”

“Fig 4 is not in the right order: you first refer to Fig 5.”

Changed.

“p8 l 260: you assume in Fig 5 that settling velocity is varying linearly and not vice versa. How valid is this assumption?”

We do not assume that sinking velocity varies linearly, we only assume linear changes of parameters that enter the calculation of the sinking velocity according to Eq. 8. Since for parameters ω , ρ_p , and ρ_o , sinking velocity has a linear dependency (and if $d_f = 2$, for D_p and D as well), it is obvious that w_s also varies linearly. When considering all linearly changing parameters for the calculation of sinking velocity w_s , then w_s shows a maximum. Anyways, there was the word 'results' too much, we removed it.

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“p8 1262: ‘sediment particles’: do you mean ‘mineral particles’?”

Changed from ‘sediment’ to ‘mineral’.

“p9 1277: ‘former studies’: which one?”

We give now a reference to Dyer 1989.

“p9 278-280: “The correlation is, however, rather poor . . . and can be explained by . . . ” and of course of turbulence. SPMC and turbulence determine the settling velocity. ”

We now explicitly mention turbulence, while we do not include the added sentence since it appears similarly a few lines later at the end of the Sec. 4.1.

“p10 1308-309: “This suggest that the region can be considered as a transition zone, hindering mineral particles to escape further off-shore.” What about the effect of deeper water depth that result in a dilution of the SPM? Or the fact that the transition zone is further off-shore and thus tidal currents and tidal-current ellipses changes? Is what you have observed (gradient in SPMC) not also related to these processes? Maybe that this is not relevant for the study, but I am intrigued by features like the East Anglia plume that extend far into the North Sea (up to the German Bight), and that are not restricted to certain turbulence regimes or ‘transition zone’. Do you see the East Anglia plume in your data: higher SPMC further off shore?”

We thank the reviewer for this aspect and now acknowledge the dilution effect and potential effect of parallel to the shore line occurring currents by introducing the following sentence in the discussion: “Generally, dilution of SPMC occurs due to cross-shore wise increasing water depth and locally occurring currents parallel to the coast potentially confine horizontal SPMC distribution to the near-coast region (Staneva et al. 2009).”

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We re-checked all campaigns and never observed a pronounced increase in the outer German Bight in the region of the so-called 'Entenschnabel'. The East Anglian Plume is a transient feature at the outer parts of the German Bight and was, in terms of SPMC, not observed in our study. The latter can, for surface waters, also be inferred from the MERIS image (Fig.7).

We added the sentence: "To simplify, consider the course from the coast to open waters. Once formed, relatively dense, fast sinking flocs, whose properties are adapted to the transition zones' turbulence level, would easily settle out of the water column once transported offshore. That is because turbulence becomes too weak to break those aggregates apart and to retain them in suspension. Thus, only loose, organic-rich particles are kept suspended in the water column while mineral-rich particles tend to settle to the near-bottom waters."

"p11 | 363-364: "Hence, the ability for nutrient retention is diminished and would lead to generally lower nutrient concentrations in similarly affected Wadden Sea regions." You have supposed similar parameters in whole the Wadden Sea. Is this correct in view of the different behavior of the Sylt-Romo basin?"

Maybe we misunderstood the comment, but that is exactly what we discuss: the Sylt-Rømø-Bight likely features a lower turbulence levels than other tidal inlets, also visible in Fig. 2, which is reflected in the mapped w_s and thus the retention capability for nutrients is diminished. Thus the concept of the transition zone can help to understand the lower nutrient concentrations in the Sylt-Rømø Bight compared to the East Frisian islands since the transition zone is not as pronounced along Sylt-Rømø as along the East Frisian islands. We rephrased a sentence to: "Hence, the ability for nutrient retention is diminished and would contribute to the lower nutrient concentrations compared to other Wadden Sea regions", which hopefully clarifies our view.

"p11 | 367: I don't understand what the physical basis is to link gradients in SPMC to

the spatial distribution of w_s .

We added the sentence: “Flocs with high w_s , that are likely mineral-dominated, rapidly sink out of the water column and thus lead to strong cross-coastal diminishing of SPMC.” and believe that the aforementioned description on comment “*p10 1308-309*” additionally help to understand the relevance of w_s for cross-shore SPMC gradients.

“Figure 2: What is GETM?”

Caption changed to: “Map of hydrodynamic model-calculated time- and depth-averaged energy dissipation rate epsilon for the times of the cruises.” (GETM is the general estuarine transport model that was used in the study of Gräwe et al. 2015)

“Figure 4: is not really convincing as it is a log-log plot with only few very low SPMC values. Figure 7: Meris image: use a more appropriate scale (e.g. starting at 1 mg/l instead of 0.1 mg/l).”

We now chose a different binning so that more bins are in the low SPMC range. The scale for Fig. 7 has changed accordingly

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2015-667/bg-2015-667-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2015-667, 2016.

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