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Discussions

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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Anonymous Referee #3

Dear referee,
thank you very much for your valuable and constructive comments on our manuscript! Pointing out some underrepresented discussion points, such as about potential further explanations for spatial heterogeneity of the Wadden Sea eutrophication levels, clearly helped to improve our manuscript. Your comments are set in italic and respective answers are set normal

General comments

The authors analyse an extensive data-set of vertical SPM concentration profiles in the German Bight. They analyse this data-set to reveal the overall cross-shore spatial structure in settling velocities of (aggregates of) particulate matter. They find a maximum in estimated settling velocities in a 15-20m depth zone. The authors put forward two alternative possible explanations for this maximum. First, this could be the zone with optimal balance between turbulence controlled floc-formation and break-up, resulting in the largest flocs and hence the largest settling velocities. Second, they hypothesize that the composition of SPM could vary along a cross-shore gradient. They demonstrate that a functional relation linking settling velocity to primary particle size, floc size, fractal dimension of flocs and relative amount of organic matter in SPM can also show a maximum when all explanatory variables are allowed to vary linearly along a cross-shore gradient. They discuss these observations in terms of the biogeochemical cycling of the German Wadden Sea and hypothesize that along shore differences in the spatial variability of settling velocities could explain regional differences in nutrient cycling and eutrophication status. Although speculative, the discussion of potential implications is interesting. To interpret the observations the authors make use of existing hydrodynamic model simulations, i.c. to estimate the vertical eddy diffusivity and energy dissipation rate. As such they are able to present a 2D data set of SPM concentrations and settling velocities as a 1D relation with energy dissipation rate. This is a nice example of the combined use of in-situ observations and model simulations to retrieve new and insightful information from a system. My major comment is regards the discussion section, which I find short and incomplete. On a first reading it is not clear that the authors are discussing two alternative explanations for the observed maximal settling velocity. This could be made more clear. Neither of both possible explanations of a maximal settling velocity is properly discussed on how realistic it is. No attempt is made to compare the epsilon values at which the a maximum occurs to other research on floc formation and break-up. Is the reported epsilon range of maximal settling ve-

locities indeed those that lead to maximal floc size, and under which circumstances? Similar for the results of the conceptual model. Is there any additional support that the different explanatory variables increase linearly over the same distance across the shore? Only the end-members are shortly introduced in relation to other work. It is unclear whether the location of the maximum in the conceptual model can indeed be in the same zone as the observed maximum settling velocity, and under which assumptions. Consequently, it should be discussed which of both potential explanations the authors find the most important, or whether both are deemed equally important. Finally, the authors make no attempt to discuss other explanation for the spatial variability in eutrophication status of the Wadden Sea and why the proposed mechanism here is the most plausible, as seems to be implied by their manuscript. Partially because of the incomplete discussion, I find the conclusions often speculative, e.g.

We agree that we could have been more explicit about shear rates and related sinking velocities. We therefore further expanded the first paragraph of the discussion by giving more references on shear rates and related maximum sinking velocities.

We now clearly state that our conceptual model is a first order and linear approach to tackle the problem. In the discussion, we now give a brief statement that the location of the transition zone is potentially subject to change when different and non-linear parameter changes are assumed which implies that seasonal seasonal effects can likely happen. The latter requires further investigations.

We tried to make it more clear now that the formation of a sinking velocity maximum is predominantly driven by the apparent shear rate. In addition / on top of that, algae with their excretions potentially modulate the region at which shear rate the maximum sinking velocity occurs. So biological effects likely contribute to the occurrence and extension of the transition zone. Our view is that algae have adapted and rather make use of the general circulation pattern than act against them which complicates the disentangling of the roles of biological and hydrodynamic effects.

We now introduced a whole paragraph about potential further explanations for the heterogeneity of the Wadden Sea eutrophication status. We further tried to make clear that the formation of the transition zone is one among other potential processes leading to the observed differences in the eutrophication status.

P11 L21-22 "Algae with their seasonality seem to be strongly involved". There is no evidence in the underlying paper supporting this statement. On the contrary, the authors demonstrate that also turbulence controlled flocculation and break-up can explain the existence of a maximal settling velocity. In that case, algae are not involved in the establishment of the coastal transition zone. Overall, I think the authors will be able to address most of the comments with a few additional paragraphs and rewriting the existing text. Therefore I recommend Minor Revisions for this paper.

We acknowledge the reviewers comment and tried to improve the manuscript in this respect in several places throughout the text. We believe to now better describe the way, how phytoplankton potentially affect the location and establishment of the transition zone in interaction with the given turbulence regime.

Technical points

The draft needs many textual and technical improvements, and different parts of the methods need further clarification among which: p4 L25 eq 1 Clarify: z-axis pointing downward

Clarified: "... fluxes in the positively downward pointing vertical direction z ..."

p4 L28-30. The text motivates why k is taken constant over time (i.e. because settling time scales are much longer than the tidal time scale.). This does not motivate why k can be taken constant over depth, which is the essential assumption to arrive at solution

(2). This should be motivated separately. In fact, equation (1) only makes sense as a tidally averaged balance in which k_v is already taken constant in time. Otherwise dC/dt would not vanish, and horizontal advection would not be zero.

We now state: "... vertically averaged k_v of a profile. This implies the underlying assumption of a rather homogeneous turbulence intensity which we account for in the below described further data processing.", and thus clearly state the underlying assumption. We refer to this assumption again in the motivation for the splitting approach.

P4 L30 I think the authors are wrong here: the exponential solution of equation (1) assumes zero net fluxes across the vertical boundaries. Otherwise source terms would show up in the solution.

We stated that we assumed steady state conditions. Thus, the sink and source terms balance each other and are in combination zero. Therefore, source terms can be neglected. See also comment below.

P4 L30 rephrase: "Assuming zero fluxes across the vertical boundaries and a vertical diffusivity that is constant over depth, equation (1) can be integrated over a water column with arbitrary height H_p. This results in an exponential concentration profile. The unknown integration constant can be expressed as a function of the (unknown) average concentration of the profile as: "

We introduced a subsentence 'allow fluxes across the profiles borders, which cancel out under steady state, we can derive an analytical model' to make clear, why no source and sink terms appear.

P4 L30: motivate why you express the exponential profile as a function of the unknown average concentration over the profile. You could as well express it as a function of the concentration at a specified depth (e.g. at the top or bottom of the concentration)

Given the availability of an analytical solution, both methods are equivalent and the depth-averaged observed SPMC is a good estimate for the analytically used mean value.

P4 L31 C_m is not defined

We apologize and now defined it. We added C_m after 'analytical model'.

P4 L31 Be precise in your notation: in this formula $< . >$ denotes a depth average over concentration profile, while later in the text, you state that $< . >$ means an ensemble average.

We now clearly formulate that it is the vertically averaged k_v value and deleted the word 'ensemble' since it seemed to introduce ambiguity.

P5 L2-4 It would be helpful to specify the turbulence model that was used in the hydrodynamic simulations and how vertical turbulent diffusivity was estimated from it.

We added the sentence: "Vertical mixing is parametrized by means of of a two-equation $k-\epsilon$ turbulence model coupled to an algebraic second-moment closure (Canuto et al. 2001). The implementation of the turbulence module is done via the General Ocean Turbulence Model (GOTM; Umlauf & Burchard 2005)"

P5 L2 required → requires

Done.

P5 L 8 remove comma between "both" and and "observed"

Done.

P5 L13-15 How do you motivate the choice for the limits of density gradient standard deviation. You state that they are "somewhat arbitrary", which implicates that they are also "somewhat motivated". Please do so.

We now write: "Both limits for $\mathtt{std}(\Delta\widetilde{\sigma_T})$ and $\Delta\delta_z\sigma_T$ were applied to select for similar vertically structured observed and modeled density profiles. The chosen values, however, were somewhat arbitrary and therefore considered in a Monte-Carlo-type simulation (see below)"

P5 L14 See above: here $< . >$ denotes averaging over ensembles. Be precise.

We deleted 'ensemble' for ambiguity reasons and clarified the meaning for σ_T' by introducing: '(here specifically the vertical mean)'

P5 L14 Clarify: What do you mean by ensemble average, is it the average over an epsilon-bin?

No, in this context, it is not a bin-wise averaging. We hope that we clarified it by being more explanatory (see our comments above).

P5 L17-27. I have difficulty to believe this is a correct way to split the vertical SPM profile in subsections. To my best knowledge, density stratification in the German bight is related to water temperature. SPM concentrations and their vertical gradients in the German bight seem too low to me to induce a significant impact on densities and induced turbulence dampening. Large density gradients are in itself the cause of reduced turbulence, whatever the cause of the density gradient. This indeed causes a strongly reduced turbulent diffusivity in the deeper waters compared to the surface waters. As I understand it, this is the principal reason why profiles need to be split in 2 sections on which 2 exponential profiles with constant k_v are fitted, one representative for "deep" waters and the other for "surface" waters . Therefore, the authors should

motivate why they not just split the profiles at the level where maximal density gradient exists, and what the difference would be with their method.

We have introduced a new paragraph to better motivate the chosen procedure. We generally agree that strong density gradients are a good indicator to split the profiles. However, a recent publication by Franks (2014) suggests that density gradients are a rather inadequate proxy for turbulence intensity. We believe that strong vertical SPMC gradients probably indicate vertical turbulence intensity changes. In short, by introducing a splitting dependency on co-occurrence on density and SPMC gradients we thus believe to better acknowledge the turbulence intensity that potentially affect sinking velocity.

P5 L23. Here again: what kind of averages are denoted with $< . >$. It seems each single profile was considered, thus $< . >$ would mean averaging over depth.

Yes, it is the depth average. We thus introduced '. . . , where $\langle X \rangle$ again denotes the vertical average.'

P5 L34 It took me a while to find what F means. Perhaps this can be clarified here again

Done.

P7 L8. Rephrase: "The sensitivity of w_s to changes in each parameter was assessed by varying each parameter separately while keeping the other parameters at their typical values for coastal waters, I.e...."

Rephrased to: "The sensitivity of w_s to changes in each parameter was assessed by varying each parameter separately while keeping the other parameter parameters at their typical values for coastal waters, i.e. for an organic-rich aggregate with an assumed diameter of $D = 100\,\mu$m, and $\omega = 0.5$, $D_\mathsf{p} = 4\,\mu$m, and $d_f = 2$."

P7 L11 "Vertically and temporally averaged model-derived ..." Over which period was the temporal averaging

We added the sentence: "The temporal averaging was carried out for the times of the cruises while accounting for the length of the tidal cycle"

P7 L26 & Fig5 I find the tilted figure a bit difficult to read, why not put it straight up?

We wanted to emphasize the 3D spatial context, in which the 1D conceptual model is set. To improve readability, we now clearly state what the red and blue lines stand for, which was admittedly not the case before (previously only expressed via the ylabel colors).

P11 L4 Change "rises" to "raises"

Done.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-667/bg-2015-667-AC2-supplement.pdf

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