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

Anonymous Referee #2

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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

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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.

Specific comments

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

p2 l27-28 “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. 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?

p2 l37-39: “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 l39-40: “By contrast, in deeper off-shore regions, SPM concentration is comparably low and consists to a higher extent of POM with low densities.” Better: . . . SPM concentration is lower and the flocs are looser and more organic.

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p2 l51: what do you mean by ‘restructuring processes’?

p3 l67: add a reference for the ‘typical power law relation’

p3 l94: 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.

p4 l99: 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?

p4 l110-113: I don’t understand the hypothesis of ‘line-of no-return’

p4 l119: 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?

p4 l123: sampling rate of 11 s-1: better 11 Hz

p4 l124: conductivity or specific conductance. I am not sure that the term ‘specific conductivity’ is used.

p4 l126: 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.

p5 l147: “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, ws=0.5 mm/s in 10 m water depth: 6h). This seems to me quiet similar. What are the consequences of this assumption on the results?

p5 l150: Cm is not defined. Is this the depth dependent SPMC?

p5 l160ff: 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.

p6 l174 On what is this criterion (< 0.0005 kg m-3 m-1) based see line 168 where it is 0.015.

p6 l175-176: “The co-occurrence of strong gradients in _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?

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

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

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

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

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

p8 l262: ‘sediment particles’: do you mean ‘mineral particles’?

p9 l277: ‘former studies’: which one?

p9 278-280: “The correlation is, however, rather poor . . . and can be explained by . . ..”

and af course of turbulence. SPMC and turbulence determine the settling velocity.

p10 l308-309: “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?

p11 l 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?

p11 l367: I don’t understand what the physical basis is to link gradients in SPMC to the spatial distribution of ws.

Figure 2: What is GETM? 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 in staed of 0.1 mg/l).

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