Answers to Referee 1

We thank Ingrid Holzwarth for her dedicated effort to read and comment on our paper. In the following we will give our point-by-point answers to the individual comments or paragraphs of her review. Reviewer's comments are written in italics, while authors' answers are in kept in plain font.

General comments My overall impression is that the manuscript is a draft with a collection of several interesting topics (importance of a 3D model to capture estuarine biogeochemistry, nitrogen cycling, hydro-dynamic transport times, grazing, influence of river discharge, small-scale hydrodynamic pat-terns influencing the system-wide biogeochemistry) but no consistent story line and no clearly expressed conclusion in the end. My general advice is to focus on one aspect from the collection of interesting aspects above and present it in a clear and concise way.

We are glad that the reviewer discovers several interesting aspects to our work. We agree that the paper needs further streamlining and focusing on the most relevant aspects. At the same time we would like to remind here that a complex system such as the coupled physics and biogeochemistry of the Elbe estuary require an integrate description. An excess of fractioning the integrate system into sub-systems and aspects increases the danger of overlooking important interconnections. For example we could not explain a spatial shift in a nutrient pattern without accounting for advection/diffusion of salinity and freshwater discharge, nor could we do so without accounting for organic inputs at the weir or diatom growth in the shallow limnic part. Our work demonstrates that the specific spatio-temporal organization of the nitrogen turnover in Elbe estuary depends on the physical and biogeochemical processes around the bifurcation of the river near Hamburg and specifically in the shielded from the tidal currents harbor basins and side channels. Following the reviewer's advice, in the revised manuscript we will focus on how the specific dynamics in the Hamburg port area affect the nitrogen cycling in Elbe estuary.

The manuscript presents a work very specifically done for the Elbe Estuary without discussing the global situation or the situation in other similar, or different, estuarine systems. Almost no literature apart from specific Elbe Estuary literature is included

We agree that the linking between our present work and its relevance for other estuarine systems needs to be improved. We will provide a better representation of studies on other estuaries as well as a statement on the global relevance of our study in the revised version of the manuscript.

Though I agree on the necessity to use a 3D model for analyzing estuarine biogeochemistry, this point is not worked out well: (1) The argumentation for the necessity of a 3D model fails.

A 3D model is necessary in case axial gradients, lateral exchange processes and stratification of physical and biogeochemical properties interact and control one another. The coupled physics and biology in the main channel of Elbe river could and have been analyzed in a simplified manner using 1D or 2D representations of the river geometry. In the area of the Hamburg port however, stratification, lateral gradients and exchange feed back onto the longitudinal dynamics (as seen for example in both observations and model from the estuarine ammonium maximum, a sharp peak located about Elbe-km 625, Fig. S-2). That is why a 3D model is necessary to increase the realism of the simulation beyond what has been achieved in the past modelling. This knowledge might also be useful for estuarine management. We find that the benefits put forth by the unstructured 3D coupled physical-biogeochemical modelling in this particular case are of general scientific interest and deserve to be made public.

(2) The authors make little use of the additional information they get from their 3D model compared to information from measurements or a 1D/2D model.

While aspects of the advantage of the highly resolved model have been partially addressed by our analysis (e.g. along-section profiles in Fig. 11) we agree that additional information retrieved specifically form a unstructured 3D model will improve our manuscript. Following the referee's advice we will add time-averaged maps of the state variables most relevant to the heterotrophic decay around the channel bifurcation and port area to the manuscript. Such maps clearly reveal the hot-spots of remineralisation and hypoxia in the small channels and basins.

Apart from the issues above, the model (physical and biogeochemical) is not sufficiently validated for the argumentation later on. While I see nitrogen and nitrogen species well-validated, all other quantities are not. In any case, the physical model need better validation, and – given the poor agreement shown for salinity – probably some more effort in setting up a better physical model is required.

The Elbe set-up is a derivative of an already published set-up (Stanev et al. 2019) and it is also hydrodynamically coupled to the latter (Page 4 Line 114-116). Therefore, the hydrodynamical model of the herein presented coupled physical-biogeochemical model has actually been validated for the Elbe region and underwent the peer review process. However, the period of integration has been extended and the simulation has been compared to new observations available, i.e. the ship-borne measurements. We admit that simulation of salinity for certain periods in 2012 does not match expectations. There can be several reasons such as for example non-optimal boundary forcing or overestimated horizontal diffusivity. Both, operational model development and control of physical diffusion are developing topics of the modelling community. We agree that additional validation is helpful and will increase credibility of the model. We will provide additional validation for salinity, temperature and entangled with the nitrogen turnover biogeochemical properties using the available stationary observations. We will deliver appropriate tables and diagrams to provide a decent overview about the physical and biogeochemical model performance for the period of model integration.

Specific comments Title It is not clear what the authors mean with "joint perspective". The manuscript presents a model-ling study with the use of observational data for model steering and validation, which is the usual way to do.

We agree that in particular the second part of the paper is essentially a modelling study. On the other hand we use observations for complementary analysis of nitrogen turnover processes (Figs. 8, 9). As a compromise we propose to change the manuscript title as "Nitrogen cycling in the Elbe estuary: Observations and numerical modelling".

Abstract Line 7-8: observations are only used for steering & validation here.

We agree that we use observations for boundary forcing and validation, however complementary analysis of estuarine nitrogen cycling is based on observations. For example on page 10 Line 307-309 we have written “In autumn, observations revealed concave bending at low salinities, indicating a nitrate sink in this area (Fig. 8c) or gradually increasing nitrate concentrations in the river Elbe (Fig. 4a).”

Line 9: “a coupled physical-biogeochemical 3D unstructured model, applied for the first time to the estuarine environment”. It is not clear, what the authors mean: if the sentence relates to the specific model they use (Schism – ECOSMO) it might be true but is nothing new and remarkable in the abstract. However, the expression could also be misleading: I'm not sure if the authors mean that it was the first time ever that a coupled model has been applied to an estuary. In this case, the authors are referred to Cerco et al. (1993), Wool et al. (2003), Wild-Allen & An-drewartha (2016), Lajaunie-Salla et al. (2017) – to name only some.

We agree that our formulation is not optimal. Our statement intended to stress two aspects: 1) we present the first unstructured coupled 3D modelling using realistic bathymetry of Elbe estuary, 2) for a first time the ECOSMO biogeochemical model is applied in the estuarine environment. We thank the reviewer for recommending above references. The cited studies are certainly relevant for our study as they employ 3D coupled physical-biogeochemical model in the estuarine context. In the context of discussing the novelty of our modelling effort, we would like to point out that non of these modelling studies used unstructured mesh to resolve small scale features and curved narrow channels. However we find useful information in particularly in the more recently published of the above cites studies and we might integrate their findings into our reasoning.

Line 13 – 21: - Apart from the validation for nitrogen, model validation for all other physical and biogeo-chemical quantities is not sufficient to support these findings. - What is the new& important finding and what are its consequences?

The new and important aspect of our work resides in the use of an unstructured mesh in the context of 3D coupled physical-biological modelling of an estuarine ecosystem using realistic topography. Such a tool enables the integration of coupled dynamics in narrow, curved channels and harbor basins of Elbe estuary. Our modelling study reveals the hot spots of heterotrophic decay of the Elbe estuary ecosystem. To our knowledge, the past modelling studies have not elaborated on this aspect of the estuarine ecosystem dynamics. Following the reviewer's advice, we will better clarify the novelty of our work and its consequences for the understanding of the ecosystem in the revised manucript. Depending on the availability of observational data, we will also deliver additional validation of the relevant for our reasoning variables.

Page 2 Line 31: The reference Jickells et al. 2014 is not appropriate for the statement.

We will look for an appropriate reference to or change the statement. A possible reference would be Weilbeer (2014), who mentions ongoing maintenance dredging which does however not imply morphological changes in the future.

Line 38-39: If oxygen depletion is mentioned later on, it should be mentioned that depletion was most severe during this period.

We will add this information to the manuscript.

Line 61-62: Sentence unclear: in x direction (main flow direction), all of the above mentioned models describe the gradient between the weir and the German Bight.

We will reformulate this phrase and point out that herein also the feedback of vertical and lateral gradients and exchange processes on to the along-channel gradient is taken into account.

Entire last paragraph: the models mentioned before all focus on oxygen, the authors focus on nitrogen (or meso-zooplankton?). The authors should consider that this might be a completely different story.

It is correct that the cited studies focus on oxygen. In our model, nitrogen and oxygen concentrations are dynamically linked through production and degradation processes. We will clarify the interdependency of nitrogen and oxygen in the revised manuscript. We will also add a statement about the specific approach implemented in ECOSMO in comparison with the previous studies on the Elbe system.

Page 3 Line 70-74: limiting to one or two questions would probably help to give a more concise and focused article.

We agree that the paper needs substantial streamlining. As said above, we see the novelty of this work in addressing degradation processes in the port area. We will re-organize the manuscript such that this main aspect is better exposed.

Line 75-81: the stations of WGMN Hamburg (Blankenese, Seemannshöft, Bunthaus) should also be taken into account. They provide high-quality data in an area where later on important conclusions are drawn from model results. And they give a differentiation of phytoplankton between diatoms, green algae and blue-green algae.

Some of this data are indeed available for the given period. Blankenese data is currently not available according to the online tool. In particular the chlorophyll-a data available for stations Bunthaus and Seemannshoeft- that is upstream and downstream from the major part of the port area- could be useful for validation and reasoning. Following the reviewer's advice, we will make use of these data.

Line 80: Use boundary "values". At this point I miss the information about the location of the model boundaries.

The landward model boundary is at Geesthacht (Fig. 1). The stationary measurements at the tidal weir at Geesthacht (Elbe-km 585.9) were used to force the model at its landward boundary. We have specified this on page 5 line 154-156 of the manuscript. We will remove the statement about model boundaries at line 80 to avoid confusion at this point of the manuscript.

Line 85: it should be mentioned that measurements are taken approx. 1m below the water surface. No vertical (and no lateral) gradients available.

We will add this information to the manuscript.

Line 92-95: references missing

We will add references.

Page 4 Line 96-97: unclear: were climatologic averages used? Daily values? Is the same method used for all nutrient species?

We used daily valus of river runoff (cf. line 91). Nutrient observations were sparser in time. We generated a daily time series of observed nutrients using cubic spline interpolation. We used the same method for all nutrient species. We will clarify that in the text.

Line108: What is GCOAST? Is it important information?

The "Geesthacht COAstal model SysTem" (GCOAST) refers to a high-resolution modelling tool that couples ocean, wave and atmosphere dynamics. It is currently under development at Helmholtz-Zentrum Geesthacht. This could be relevant information because the modelling of estuarine ecosystem dynamics presented in this study is part of a greater effort to develop a modelling tool for research into the entire land-ocean transition zone.

Page 5 Line 128: What exactly are the state variables? What is their functional relation? The authors say that organic matter decomposition was very important (which is true), so was the ECOSMO2 setting changed from the references given before? If it is not changed, this information should be clearly given here. In case it is changed (also in case certain parameters are changed), the model functions and their parameter values have to be described in more detail.

The biogeochemical model we use, ECOSMO2, is in detail described in Daewel and Schrum, 2013. The state variables and processes remained unchanged. The parametrization remained largely unchanged except we allow denitrification to happen at oxygen concentrations below 120 mmol m^{-3}(line 135-136 in manuscript). The interaction between the water column and the sedimentary layer remained unchanged as well, it has however been implemented into the FABM framework. We will add an illustration of the FABM module and a table of the used in this study parameters and coefficients to the revised manuscript

Line129: Assumes Redfield ratio in phytoplankton, I guess?

Yes right, as was written in line 130 of the manuscript.

Line 133: What kind of feedback is meant here?

We will remove this phrase from the manuscript.

Line 134: Unclear: first, the authors say that the sediment is a mere storage. Then they say, mineralization and nitrogen release was possible.

Yes, it is right that the sediment is a mere storage, i.e. a reservoir of nutrients at the bottom. This reservoir gets filled by sedimentation. It can be depleted by remineralisation and resuspension of the nutrient. We will provide a better understandable description in the revised manuscript.

Line 140-141: Holzwarth and Wirtz 2018 say: turbidity is so high that depth plays a secondary role. What is the value for the background attenuation coefficient used here? In case it is the same value used for the North Sea it will certainly be too low. See comment above: biogeochemical model description is missing.

We will correct this statement. The background attenuation coefficient was 0.05 $1m^{-1}$. We will add a table with the coefficients used to the revised manuscript

Line 156: Which station exactly? What is the temporal resolution of the observational data?

We refer above line 91-92 to daily discharge measured at Neu Darchau. Will be clarified in the revised manuscript.

Line 159: Is the conversion factor in line with the conversion used in other Elbe studies (some of them were already cited)? If not, why different values were used? Entire paragraph with Geesthacht boundary values: please provide a plot containing the time series of the used boundary values.

Hillebrand et al. (2018) give estimates of the Chl(a)-to-carbon ratio in Elbe river ranging from 18.2 µg Chl-a/mg C during winter months up to 32,3 µg Chl-a/mg C during summer months. Herein cited studies used values between ~27 µg Chl-a/mg C and ~ 50 µg Chl-a/mg C (Schöl et al., 2014; Holzwarth and Wirtz, 2018). Given that in our model C:N ratio equals 106:16 we effectively used a conversion value of ~ 20 µg Chl-a/mg C. We used this value following the applied by Zhou et al. (2017) 1,58 g Chl-a/ mol N. Assuming Redfield ratio of C:N this value appears to be in the range of Chl-a/mg C proposed by Hillebrand et al. (2018) for Elbe river.

Following the reviewer's advice, we will provide time series of the used boundary values at the tidal weir in the revised version.

Page 6 Line 164: Please clarify: 2012 was used as spin-up period?

Yes, 2012 was ran as spin-up and then repeated as actual herein presented simulation. We will clarify this in the revised manuscript.

Line 171: Why can the authors assume, based on M2 tide, that water levels deviate less than 10 %? Besides: - I consider 10 % as far too poor, given a water depth more than 15 m in large parts of the estuary. This would imply a substantial error in water volume. - More information on tidal analysis for observational and modeled data (mean high and low water, tidal range, slack water times) would be helpful to assess the quality of the physical model, essentially including an analysis of current velocities.

We agree that based on assessment of model performance regarding the M2-tide only it is unlikely to assure that errors are less than 10%. Model performance is actually better (see also Stanev et al., 2019). We will add a quantitative skill assessment for the period of model integration (2012-2013) for available along Elbe estuary gauge data in the revised version.

Line172: “ventilation” might be misleading in this context (could be understood as “reaeration”)

By ventilation we mean forcing and stirring of the estuary by tides. We agree it could be misunderstand in terms of reaeration. We will reformulate this phrase.

Line 174-175: The stationary monitoring data of the water and shipping administration, which are distributed along the salinity gradient, from Hamburg to Cuxhaven should be used for validation. Using these data, a time series comparison for the entire simulation period, including basic statistics, is possible. This would give a much better impression on the quality of model results than comparing 4 single transects. Same for temperature.

We agree that stationary measurements of salinity and temperature should be taken into account. We will provide corresponding plots and basic statistics in the revised version of the manuscript.

Line 176: The deviation in salinity is quite high. Given the really high overestimation in the October 2012 profile (up to 15 psu, if I understand it correctly) I strongly recommend to include the stationary measurements, see above. Either the ship-based observational data are bad, or the physical model needs improvement.

To better address this comment we will provide comparison of model vs. observations using stationary data in the revised version of the manuscript.

Line 181: I assume the authors refer to model results (please clarify, also in the figure text)? Given the poor agreement between observational data and model results I recommend not to show this figure until better agreement is proven.

Yes, here we refer to seasonal variation of simulated salinity. We might change Fig. 3a for a stationary time series showing seasonal variability of salinity in both model and observations.

Page 7 Entire paragraph on dispersion and tracer: I recommend proving good performance of salt transport (see above) before calculating a dispersion coefficient based on salinity. More: the relevance of the entire section is not clear to me. I recommend leaving it out completely and better spending more effort in the validation of the physical model. Besides, mixing plots for model data are unnecessary from my point of view: having a model, you can extract from your model results the production rates for all your state variables in your entire model domain. Then you clearly see (in numbers!) whether you have a biogeochemical activity or not. You can even see where it comes from. Maybe the authors can make better use of this information which is so far not used in their study.

The model-derived dispersion coefficient illustrates the seasonal modulation of along-channel dispersion. Even if it is overestimated during the period of low discharge, the seasonal trend remains correctly addressed. However, as we plan focus the study on the lower limnic part the salinity-derived dispersion coefficients will not be very relevant for our reasoning. We will follow the reviewer’s advice and remove this graph. Regarding the plots of the nitrogen-salinity relationship, please see our answer below to “Page 10 …”. We will consider showing production rates for the focal area.

Page 8 Line 232: where shown?

The model performance regarding simulation of inorganic nitrogen species is decribed in detail from Page 8 Line 247 until Page 9 Line 269. There we refer to Figs. 4, 5, 8 and 9 illustrating the comparison model vs. observations.

Page 9 Line 248-249: earlier you say that denitrification is included in the model.

Yes, it is included in the model. The rates are mentioned in Sec. 5.1 (Page 14 Lines 451-456) with illustration given in Fig. 13d.

Line 271 ff: Regarding the rather poor agreement of model salinity and temperature, the oxygen values should not be validated using saturation. Dissolved oxygen concentration should be used instead to avoid any offset due to incorrect salinity and water temperature.

We would prefer to use the saturation, to see the over- or under-saturation and compare the saisonal data. However, we will provide a comparison model vs. observations in units mmol $O2\ m^{-3}$ for the supplementary material.

Line 279: What means high accuracy? This comment relates to the entire validation section, both physical and biogeochemical: analysis of the comparison between observational data and model results seems to be done by plotting and "visual" judgement on the goodness of fit.

We will provide further skill assessment of model performance both physical and biogeochemical in the revised version of the manuscript.

Page 10 I don't see an additional benefit for model validation from comparing mixing curves and I there-fore recommend leaving out this section.

This nitrogen-salinity relationships show very nicely the source-sink behavior of the treated herein nutrient. The issue of conservative or non-conservative mixing is central to the Elbe estuary literature which is why we would prefer to include respective plots when presenting a modified modelling approach. In addition these figures illustrate the good overall agreement between model and observations when looking at the estuary in isohaline coordinates. However, we might think of reducing the number of plots to one per nitrogen.

Page 11 The entire chapter 5 has a structure which makes it hard to follow a central theme.

We agree that our system description needs better structuring. We will separate results and discussion section in order to increase readability of the manuscript.

Line 326: Announcing to explain the "full spacio-temporal dynamics of the estuarine ecosystem" is a very big promise and I strongly recommend being more moderate.

We will change "full" for something else like "better spatially resolved" (in comparison with earlier works).

Line 348-359: First, the model used in this study has not been validated for grazing or grazers. Second, the reference given (Schöl et al. 2014) is weak because the conclusion presented therein only bases on model results. Therefore, findings regarding grazers have to be validated, also to sustain the conclusions drawn from it later on. Observational data (counts of organisms, for ex-ample) might exist at the WGMN Hamburg or at BfG for the harbor area. Moreover, the model has also not been validated for phytoplankton. High-quality phytoplankton data are available, especially for the area that shows high model diatom concentration in Figure 10 (a).

We agree that this validation is missing. However, observational evidence of grazers in the area of the port of Hamburg (main channel) has been published (Schoel et al. 2008, in German). The same publication shows that chlorophyll- a and grazer concentrations are anti-correlated in the along-channel direction, very similar to what we have presented as results of our modelling study. That is why we assume that grazing of phytoplankton in Elbe estuary is a valid approach. We will add the above publication to our reference list. We will also follow the reviewer's advice and provide validation of the model against the available chlorophyll-a measurements in the area of the Elbe river bifurcation.

Page 12 Line365-367: The authors don't prove this statement.

We reformulate this statement: Our results confirm a change of biogeochemical regime where the change in channel geometry occurs (i.e. we do not contradict previous works), however we demonstrate that changing depth is not the only possible reason but the horizontal geometry plays an important role with harbor basin opening a spatial niche for increased respiration, sedimentation, hypoxia and remineralisation.

Line 367ff: The relation to the text in the first section of this paragraph isn't clear.

We agree that this statement is misplaced here. We will either remove it or amend it to the conclusions.

Line 376 and following two sections: When focusing to such a small region compared to the entire model domain, the authors should provide better validation for this specific area. A system-wide M2-comparison, which also shows its highest deviations in the focus region (roughly km 620 in figure 2(a)), is not sufficient. In addition: lacking validation for phytoplankton and grazers, see above.

The dynamically important aspect is the gradient in kinetic energy between the tidal channels and the narrow and shallow basins. This gradient is an obvious consequence of the local geometry. Following the reviewer's advice we will provide better validation of the local water levels. This data is available.

Page 13 Line 415 – Page 15 Line 476 This section can be shorted substantially because it contains many well-known and well-described processes and relations (contained in the references of the paper). The three – zone - description in the last section (Line 470 – 476) has first been described by Schroeder (1997). New aspects worked out by the authors are not clear.

We will shorten this section and will make better use of references.

Line 458: where is fig 12e?

We change Fig. 12e as Fig. 12a, which shows the nitrate concentration at the station inside the harbor basin.

Page 15 Line 478 – Page 16 Line 512 What is new &relevant in this section?

This section describes the effect of inter-annual variability of the forcing onto the estuarine ecosystem taking into account the increased in comparison with earlier studies resolution of the complex channel geometry and associated increased spatial resolution of the coupled physical-biogeochemical processes. The ecosystem response to river discharge is very relevant both to scientific discussions as well as management and policy makers. We see both novelty and relevance given.

Page 16 Chapter Conclusions Line 514 –522: Everything that is mentioned here has been shown already several times, for the Elbe and for other estuaries. A 3D model that is able to simulate all these processes is also not new but more or less common (see the comment on the abstract).

In the revised manuscript we will better expose he benefits of the unstructured modelling and its relevance in the coupled physical-biogeochemical context. The revealed by our work hot spots of heterotrophic decay in the Elbe estuary can be a relevant aspect for estuaries worldwide, both in natural configurations or due to port construction. Using 1D, 2D or even structured curvilinear 3D models these processes could not be simulated in a generic way (i.e. without local parametrization of the physics and biogeochemistry) revealing the same systemic features. We will add corresponding statements to the revised manuscript.

Line 523 – 528: Validation missing

The validation is given for the main channel (Figs. 8,9 and S-1, S-2). In fact our simulation reproduces the nitrogen species distribution and magnitude with good accuracy which supports the predicted by the model organization of heterotrophic decay in the port area.

Line 530 – 540: This section is more a description. What is the important conclusion out of it?

This sections wraps up the results of the study. The important conclusion is that most of the time, i.e. given average or near-average river runoff, the system reveals a persistent compartmentation. However, the hindcast demonstrates that during the June 2013 flood event the usual and known compartmentation collapses being restored in the aftermath of the flood event.

Line 542 – 543: This has already been shown for different estuarine systems worldwide.

We will provide information on related studies in the revised manuscript. We will also better expose the relevance of using an unstructured 3D model and the importance of resolving narrow channels and complex channel geometry in estuarine ecosystem modelling.

Line 543: From my point of view the importance of the harbor is the most interesting aspect that remains. However, I miss: 1. Better validation, 2. more focus, 3. analyzing hydrodynamic aspects as well (e.g. larger water volume in the harbor compared to section upstream), 4. comparison to other similar systems (like Guadalquivir-Sevilla, Loire, Humber or other estuaries containing large ports).

We are grateful for this comment. We agree that resolving the harbor in this modelling study represents the key novelty. We will streamline the manuscript to better present this key aspect of the work. We will also provide better validation, covering the entire period of model integration and assess model performance using statistical measures. We will consider deepened analysis of the hydrodynamical aspects concentrating on the focal area of the study (channel bifurcation/harbor). We agree that…

Figures There are many figures. Some figures (like 8, 9 or especially 12) are not easy to understand. Some panels (especially Figure 13) are very small. Check for Figure 2(b) to (e) if the description of x and y axis and the conclusion that in the model salinity intrudes too far into the estuary is consistent. There is no white dashed line visible in Figure 10.

We will reduce complexity of figures 8,9 and 12, increase panel size were needed and add white dashed line to panels in Fig. 10. We agree that the number of figures hould be reduced. Therefore we might change part of the visual comparison between model and observations in Figs. 4,5,6,7 as tables giving ranges and basic statistics.

Technical correction There is a plenty of typing errors, careless mistakes and wrong word order, some obviously due to copy& paste. I'm not a native English speaker, and therefore I'm not in the position to give suggestions for language corrections. However, I have the impression that language, including grammar and use of uniform tenses, needs substantial improvement.

We will consider correction by a native speaker of the manuscript text prior to submitting the revised version of the manuscript in order to improve the language.

Additional references:

Hillebrand, G., Hardenbicker, P., Fischer, H., Otto, W., & Vollmer, S. (2018). Dynamics of total suspended matter and phytoplankton loads in the river Elbe. *Journal of soils and sediments*, *18*(10), 3104-3113.

Schöl, A., Blohm, W., Becker, A. , Fischer, H. (2008). Untersuchungen zum Rückgang hoher Algenbiomassen im limnischen Abschnitt der Tideelbe. https://www.bafg.de/DE/08_Ref/U2/01_mikrobiologie/algen_tideelbe.pdf?__blob=publicationFile

Weilbeer, H. (2014). Sediment transport and sediment management in the Elbe estuary. *Die Küste, 81 Modelling*, (81), 409-426.