Review of the manuscript *Interactive impacts of meteorological and hydrological conditions on the physical and biogeochemical structure of a coastal system*, by Kerimoglu et al, submitted to **Biogeosciences**

Manuscript overview

The manuscript provides a model study into a particular flooding event in Northern Germany in order to determine the driving forces with regard to the marine response (German Bight area) to the event. To this end a slightly altered model is presented and applied under 2012 and 2013 conditions, plus 3 scenarios for 2013 to test the different, expected driving forces (meteorology, riverine input and a particular, 2 months long wind regime). The authors first show the anomalous forcing events, followed by plenty of model validation results and finally the model study into the expected drivers, for which they analyse the abiotic and biotic response of the system. They then conclude that the marine response to the flooding event was determined by both the enhanced riverine input (fresh water, nutrients, inner German Bight) and the anomalous meteorology of 2013 (outer German Bight) interacting with each other to alter the estuarine circulation patterns within the area.

The appendix contains detailed information about changes to the hydrodynamic and biogeochemical model, including applied equations and parameter values. It also contains some more validation results to justify some of the changes made to the model.

Review overview

In all, I'm quite charmed by the paper's objective and presented model study, with the specific aim (rather than a hypothesis) to determine which factors led to the marine reaction to a particular land-based event. The approach is valid and very interesting from a physical point of view. But I miss a good spatial validation of the model (surely the data in Figure 5 can provide that) which would more clearly quantify the problems in simulating the near shore environment. Also, I'm not quite sure why a new model is presented which doesn't include bacteria in a study that aims to understand dissolved oxygen issues in the area. Why not use ECOHAM for that? Or better, a model with benthos included? The authors build on earlier work, and explicitly state that they use a simplified version of ECOHAM from which carbonate and bacterial dynamics have been eliminated (line 484, the geochemical model). They base their new biogeochemical model (does it have a name?) on a previous model by the lead author that included mixothrophs, but these are not in here. So we have a biogeochemical model with just 2 phytoplankton species, 2 zooplankton species, fixed nutrient ratios inside zooplankton (the regulated uptake described in B1), no bacteria and no benthos. Isn't that just a stripped version of ECOHAM? Why not use that model? And if an important feature has been added (e.g. variable N,P ratio within phytoplankton), why not add it to ECOHAM? I would also argue there are more complex models out there better suited for a dynamic, shallow area like the German Bight, particularly for a study involving nutrient concentrations and bottom oxygen conditions. Given the lack of validation with Chla observations (the only station in the area of interest shows a normalized model bias of 1.12) and benthic nutrient concentrations my confidence in the biogeochemical model results is not large. Although the authors are in parts clear about the model limitations, they should add text on 1. Their choice of biogeochemical model, 2. What makes it better suited here than ECOHAM, 3. More Chla validation and 4. The role the sediments play in nutrient dynamics in shallow areas. Or, as an alternative, the authors could limit their analysis to the physical part, which is quite strong in the manuscript and would allow for a better focus of the text: there is enough to analyse there as shown by the authors, and the conclusions would not change.

Recommendation

Major revision

Detailed Comments

L 56-57: One cannot expect that the marine transport of riverine inputs is purely dependent on the inter-annual variability in the river discharges. In any marine area the meteorological conditions (mainly wind and temperature) will play a large part in the transport, as will alongshore currents. Then there are influences like mixing by ships, the presence of off-shore wind farms, and further-afield influences like the Rhine discharge. So I thought this sentence a little odd.

Fig. 2: The diagram is clear until one gets to the appendices, where it is stated that phytoplankton exudates DOM (L463), that zooplankton excrete into the DIM pool (L466) and the unassimilated fraction ingested by zooplankton becomes DOM (L486). None of this is visible in the model diagram, as all functional groups just exude large detritus … ?

L 101: The authors state here that the underwater light conditions are determined by detritus, DOM and a background value representing SPM. But in section B2.2 they state that phytoplankton is also included in the light calculation. Please make this consistent.

L 116: Please provide the website for the atmospheric deposition fields.

L 117: Please state which rivers were included within the Wadden Sea area. Just major ones (Elbe, Weser, Ems, …) or also local Dutch and German rivers like the Accumersiel, Bensersiel, Wangersiel, Miele, etc.? I know from experience that these rivers are also part of the mentioned database, which I think is called the OSPAR ICG-EMO riverine database. So I would assume they were used, but this needs to be stated clearly.

L 124: “a 3600 s time window”, why not say 1 hour time window? In the caption of Figure 4 the authors mention an hourly resolution, not a 3600 s one.

L 134: Again, a website for the ICES data should be provided.

L 139: This section is called Results, but quite a large part of it is model validation results. I would like to see this separate from the forcings analysis (section 3.3 onwards), and would therefore call this section “Model validation” and rename section 3.3 to be section 4 “Results”.

L 144: Naturally the nutrient loads follow the flow peak, but what about concentrations? If we assume heavy rainfall caused more run-off then nitrogen concentrations may stay the same, but phosphorous concentrations (usually from sewage treatment works) may be diluted. So please provide some measure of the changes in concentrations for these rivers.

Fig. 3: The Ems does not show the flood peak found in the Weser and the Elbe, suggesting it was a local event. Nevertheless I would like to see results for the Rhine/Meuse system, which will influence the area of interest here under normal conditions.

L. 146-150: Please provide some information on whether 2012 was in any way an average year or not.

Fig. 4: It seems that 2013 is characterized by mainly eastern winds all the way up to June. So why were only the June-August winds selected for a scenario? Because they do not seem easterly much in that

period. The winter and spring easterlies are now part of the M12 scenario, together with the different temperature record etc.

Fig.5 : Please make this a colour graphs, the gray scales are very hard to distinguish from one another. And why is count on the colour bar at all? I assume this is the number of observations in a given point throughout the year? But why not use three different colours for the three years instead?

Fig. 5: And as said before, I would really like to see a spatial validation graph, which would provide more detail on the nearshore errors in the model. I realise there are quite a large number of figures already in this manuscript, but would suggest some could be put in the appendix, e.g. Figure 6 and Figure 8 (which shows 3 stations which are in the model domain but not in the area of interest, and which therefore do not provide much context for the described work).

L162: Why use Kelvin here when Fig. 4 uses Celsius?

L175: The authors state that the plume was realistically reproduced as the sharp increase in NO3 al Helgoland was captured. But this is not very clear from Fig. 8, rather that 2 observed peaks in DIN are not reproduced by the model and one peak is slightly reproduced. So I'm not convinced that the plume is simulated realistically, just from this figure.

L177: why do the authors have such a high Si value on the western boundary? Is this an artefact of the simulation that generated the boundary conditions?

L181: The model fails to get the spring bloom timing right. I would say: use a different model or just focus on the physics. The Chla comparison for Helgoland is quite bad and this is the only station presented here for validation of Chla in the area of interest. Does ICES have more Chla data in the specific area?

Fig 10: This figure, and also figures 11 and 12 are too small for readers to easily read. I would suggest that the graph itself is made larger in the manuscript but also that the colour bar is changes to one large one on each side (one for S, one for T), so the graph becomes more accessible. These graphs are the essential results presented in the manuscript, so please do them justice.

L223: It is not clear to me why increased stability should have a direct effect on the underwater light penetration, particularly as SPM dynamics are just a background value. Are the authors referring here to limited nutrient exchange and thus less bioshading? They do for the OGB, but in the CGB the flood causes increased stratification and brings in nutrients, resulting in more primary production,. On L246 it is simply stated that increased stratification enhanced the underwater light regime within the CGB. Please explain and provide a reference. Are you referring to increased remineralization within the euphotic zone? I would also like to see some evidence of the underwater light response in the simulations.

L248: Please introduce figure 13 first and explain the DO abbreviations before going into the analysis.

Fig. 6: Can the authors speculate why their biogeochemical model is unable to quantitatively reproduce the observed oxygen minimum? What processes do they think the model misses?

Fig. 15: These again are too small and I cannot see the arrows at all in the difference figures.

L282: Yes, they do but this is rather an open door. Any reader would have expected that from the start, and would have been surprised if this was not the case.

Sec4: Please discuss the lack of bacterial dynamics in the discussion, and the effect this can have on the simulated results.

L312: I would say the model was able to reproduce the *physical* characteristic features of the system quite well.

L316: "The skill of the model … is notable", quite a nice notation as it is meaningless. Notable means it can be noted, it says nothing about it being good or bad.

L320-333: I'm not sure why this is include here, this is not of interest for the general reader I would think. Therefore I would put this in an appendix at most.

L349: I fail to see the prolonged stratification in figure 11. As these are all July averages I don't see a time indication in this figure at all.

L385: I object to the use of the word "satisfactorily" when it comes to the reproduction of the biogeochemical features of the German Bight ecosystem.

Table B5,B6,B8: If parameter values are provided then references on what these are based on should be included as well. Assuming these values have not been published before.

Language

In general I found the manuscript very readable, yet the English used was not always correct or as expected. I found several mistakes regarding single/plural (e.g. L 140, "the discharge rates … peak**s**", L156 "Comparison … **are** shown", L299 "potential sources of error need**s** to be addressed"), omissions of articles (e.g. L 156 *A* "Comparison of", L187 "Despite a tendency to overshoot, *the* range of", L205 *The* "Effect of exchanging", L211 "further to *the* North"), additions of articles in unnecessary places (e.g. L 143 "over **the** central Europe", LL232 "river forcing of **the** 2012 is used", L232 "the plume of **the** DIP") and omission of connecting words (e.g. L157 "are located at shallow sites, *and* therefore provide", L373 "the presence *of* regional differences"). I suggest the authors check their English thoroughly before the next submission. But I love the double negative found on L439: "leading to near-complete elimination of negative values of the total mixing being removed". So the removal has been eliminated?