Biogeosciences Discuss., 6, C2292–C2301, 2009
www.biogeosciences-discuss.net/6/C2292/2009/

Biogeosciences
Discussions

Interactive comment on "Hydrodynamics and light climate structure alongshore phytoplankton blooms in spring" *by* G. Brandt and K. W. Wirtz

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Received and published: 1 October 2009

We thank the referee for clearly pointing to the weaknesses of the manuscript. We are thoroughly revising the entire manuscript with special emphasis on clarity and conciseness.

Please find brief responses to the specific comments below.

1. ***Referee 2:** Abstract*
   L.22: The term "Ecosystem functioning" is mis-/overused in the ms. 'Ecosystem functioning' refers to complex interactions between species in relation to abotic

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parameters in whole ecosystems. It would be better to state something like “for a proper understanding of ecosystem functioning factors such as light and hydrodynamics need to be taken into account...”.
It is right that ecosystem functioning is more than just the phytoplankton spring bloom, but in our opinion the understanding of the coastal ecosystem with all its complex interactions is not possible without the understanding of the spring bloom. However, we take this point and will use the term more precisely in the ms.

2. ***Referee 2:*** *Thus the statement that physical parameters (temperature, transparency, stratification, light) are considered as more important than biological ones (e.g. grazing) is not evinced yet.*
We do not deny the potential role of overwintering zooplankton on the development of the phytoplankton spring bloom. Ultimately, however, the overwintering success of zooplankton is depending on temperature, which is a physical factor. We thus write: " .. the balance between algal production and loss ... is sensitive to a multitude of different factors such as temperature, .., abundance of herbivores, ... ".

3. ***Referee 2:*** *Furthermore, light has been shown to be a major trigger for the initiation of the phytoplankton growth in spring (e.g. Siegel et al. 2002, Sommer Lengfellner 2008) and thus, apart from indirect temperature effects on the initiation of phytoplankton blooms, the role of the light climate should be stressed more specifically in the introduction section.*
We agree on the role of light for the phytoplankton spring bloom and therefore state in the introduction:"light availability ..., which is suggested to be pivotal for phytoplankton bloom control ..".

4. ***Referee 2:*** *P. 4995 L. 11: It is stated that biological forcing is mainly due to turbidity and/or benthic grazing. However, this is not what one would typically*

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expect as major biological trigger mechanisms in the plankton. Indeed, grazing by zooplankton (e.g. copepods, meroplanktonic larvae, microzooplankton) on bloom-forming phytoplankton species is considered as of major importance in coastal as well as oceanic regions. It is not doubted that benthic grazers, e.g. filter feeders, can suppress phytoplankton abundances substantially, however, their impact cannot be regarded as of utmost importance.
Deleted "benthic" in "benthic grazing" to make the statement more general, since the relative contributions of different grazers to the overall grazing impact may strongly vary depending on the individual ecosystem.

5. ***Referee 2:*** *P. 4995 L. 16-20: The section on the role of mixing is unclear. What is meant by stating that "site-specific-mechanisms e.g. freshwater induced stratification, resuspension of benthic diatoms or species composition are of importance" in the context of bloom retardation? How are these factors involved in the retardation of the spring bloom especially with respect to benthic diatoms, as they do not contribute to bloom-formation? The same is true for the following sentence: please rephrase and specify what is meant by "the establishment of general rules for biological responses to various physical forcing".*
We rephrased this section. We tried to make clear that mixing is not always strong enough to prevent bloom formation. Other local ("site-specific") factors may significantly influence bloom dynamics. Iriarte and Purdie (2004), for example, have shown that water transparency is the major trigger for phytoplankton growth in a coastal ecosystem. It is, however, influenced by several factors including tidal currents, precipitation, wind and river run-off. This makes it difficult to find a monocausal explanation for the formation of a phytoplankton bloom. An example for such an invalid simplification of a biological response to a physical forcing in coastal seas would be the determination of a critical surface radiation required for the formation of a bloom.

6. ***Referee 2:*** *P. 4996 L. 1: The sentence about patchiness is out of context. What*

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was its intention?
Deleted the entire sentence, because it does not add relevant information to the ms. Sentence has been shortened and moved to the paragraph on mesoscale patchiness (L. 16)

7. ***Referee 2:*** *P. 4996 L.4: Please comment on how satellite imagery derived chlorophyll abundances data enhanced our understanding on ecosystem function. References should be added like e.g. Platt T. et al. (2003), Nature 423:398.*
   Done. Added references Behrenfeld et al. (2005), Thomas et al. (2003), Platt et al. (2003) and Behrenfeld and Falkowski (1997). Satellite imagery is a unique tool to derive synoptic views of marine ecosystems that are not available by other classical measurements. It particularly enables the assessment of spatial variability of measurements and the significant improvement of large-scale estimates of primary productivity.

8. ***Referee 2:*** *Material and methods*
   P. 4998 L.6: How is chlorophyll measured in the Ferry Box system? Fluorometrically I guess, but it should be stated in the methods section.
   Done. It is, indeed, measured fluorometrically.

9. ***Referee 2:*** *P. 4998 L. 5: Please state why only data from 2004 and 2005 was used. Was data available only for these two years or was the intention to compare data from 2 contrasting years?*
   Both. It was the intention to simulate two years showing distinct phytoplankton dynamics using the same model parametrisation. Besides, the FerryBox route between Cuxhaven and Harwich was suspended in 2005. Prior to 2004, the chlorophyll measurements were not reliable or not available.

10. ***Referee 2:*** *P. 4999 L. 19ff:*
    Where are the estimates for zooplankton grazing and zooplankton assimilation efficiencies derived from?

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Reported values of zooplankton assimilation efficiency span a wide range, depending on the species composition and condition (Edwards & Brindley (1996), Steiner (1990), Convoer (1966)). The value used here lies within this range and, thus, presents a reasonable estimate. It is, however, afflicted with considerable uncertainties given the unknown state of the zooplankton community. Half-saturation constants for different zooplankton species vary over two orders of magnitude (Hansen (1997)). The relatively high value for the half-saturation constant of grazing was not prescribed, but resulted from the calibration process. We will compare this value with the published range of coefficients compiled for different taxa (e.g. Hansen et al. 1997, Hurst & Bunker 2003).

11. ***Referee 2:*** *On P. 5000 L. 19ff. it is stated that zooplankton biomass at the initial position is estimated as a fraction of phytoplankton biomass at a previous time. What does that exactly mean- how was zooplankton biomass estimated in detail?*
    See Eq. A11.

12. ***Referee 2:*** *In addition, the so called “near-by” station Helgoland Roads cannot really serve as a reference site since different conditions are given off the coast when compared to coastal regions in the southern German Bight.*
    As illustrated in Fig. 1, the minimum distance between the closest point and the island Helgoland is less than 20km. The long term station is, thus, the closest available source of consistent zooplankton data. Additionally, the effect of zooplankton lagging behind phytoplankton is a general feature of marine ecosystems (e.g. Lignell et al. (1993) for the Baltic Sea) and not confined to the waters around Helgoland. Even microzooplankton growth is delayed with respect to phytoplankton blooming by 10–20 days (cf. Fig. 3 and 5 in Sommer et al. 2007, Oecologia 150:655)

13. ***Referee 2:*** *P. 5000 L. 21-23: The assumption that zooplankton is lagging behind phytoplankton development holds true for mesozooplankton e.g. copepods.*

When the term 'zooplankton' additionally includes microzooplankton this statement is incorrect since microzooplankters show an instantaneous numerical response in relation to increases in food availability thus showing only a short timelag to phytoplankton growth.
Here, zooplankton also includes microzooplankton and the time-lag may not be as short as suggested by the reviewer, in particular at colder water temperatures (cit. see above). Furthermore, the model accounts for rapid microzooplankton growth as a function of food availability via equation A9.

14. ***Referee 2:*** *Results P. 5003 L. 18 ff: Are you sure that zooplankton had a minor impact on phytoplankton biomass? Apart from sedimentation, grazing by micro- and mesozooplankton is considered as one of the main factors controlling phytoplankton biomass leading to a clear-water phase right after the bloom. I would rather reconsider whether grazing estimates used in the model were appropriate or would need some fine-tuning. This could also be the reason why the simulation showed still increasing phytoplankton biomass while phytoplankton data indicated already the collapse of the bloom (see statement P. 5004 L. 6ff.).*
Especially in the first half of the spring bloom, zooplankton had only a minor impact on phytoplankton biomass in our model. As stated in line 20, however, it causes the collapse of the bloom in 2004 when nutrients are depleted. In 2005, the model underestimates phytoplankton as well as zooplankton biomasses, so that high nutrients concentrations prevent a collapse of the bloom. The major failure of the model in 2005 is, thus, rather the underestimation of the phytoplankton bloom. Assuming higher grazing rates or zooplankton biomasses in both years does not improve the model result. We also re-evaluated existing biomass data for mesozooplankton and found a fair match between those and our estimates (comparison will be included in the MS).

15. ***Referee 2:*** *P. 5004 L. 9: Please specify the thresholds for light levels (e.g. daily light dose) allowing bloom formation. The term 'favourable light levels' is not*

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precise.

There are no static thresholds for bloom formation in the model. Phytoplankton growth in the model is largely controlled by the light climate, which is determined by the PAR at the surface and the turbidity in the water column. Surface PAR at the measurement pile was around $2000\,W\,h\,m^{-2}\,d^{-1}$ in April of both years. It shows, however, a high daily variability. However, the model clearly shows that in 2004 the spring bloom in the study area was triggered by a change of turbidity. According to our parametrisation, a light attenuation coefficient $k_z < 0.35$ allowed for rapid phytoplankton growth. In 2005, a significantly higher k_z prevented local phytoplankton growth.

16. ***Referee 2:*** *P. 5006 L. 10 ff: This section is weak. What do turbidity, the clear water phase and a spring bloom development despite unfavourable light conditions have in common? I don't get what the authors intended to state. Please rephrase the paragraph.*
    Rephrased paragraph.

17. ***Referee 2:*** *P. 5007 L. 10 ff: The section on the 'inflow hypothesis' is quite wordy but not very convincing. Please rephrase.*
    Done.

18. ***Referee 2:*** *P. 5007 L. 24-26: I agree that initial nutrient concentrations are of importance for the initiation of the phytoplankton bloom. The duration of the bloom is, however, strongly affected by remineralisation via the microbial loop since microbial degradation favours a rapid recycling of nutrients thus extending bloom duration. Other nutrient sources than the initial nutrient pools should therefore not be neglected.*
    As clearly stated in the Discussion (p5007 L. 26), we implicitly accounted for additional nutrient inputs (e.g. from pelagic or benthic remineralisation) by using a relatively high Chl:P ratio. Focusing on the spring period and studying more the

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initialization than the degradation of the bloom, we intended to keep the model extremely simple with respect to nutrient cycling.

19. ***Referee 2:*** *P. 5008 Grazing section: The section on grazing and zooplankton abundances is quite speculative and not plausible. It does not hold true that the factor grazing during the phytoplankton spring bloom can be considered as negligible since especially microzooplankters play a significant role as phytoplankton controllers especially during spring bloom formation as they show a rapid numerical response to increasing phytoplankton biomass. The grazing impact is in addition dependant on the overwintering success of zooplankton and accelerated when temperatures in spring are comparably high. These aspects should be included. The whole section on grazing needs a thorough revision.*
The somehow unusual way to initialize zooplankton biomass at one model boundary should not be confounded with lacking zooplankton dynamics in the model. Our conclusions reflect a careful analysis of our model that resolves grazing in a simplified but quantitatively reasonable way (see above). There is no evidence, neither from our model results nor from published data, that differential grazing may have caused the observed alongshore gradient in CHL-*a* in 2005. Stronger grazing, which was assumed during the calibration of the model, does by no means improve the model results. It does, however, impair the underestimation of CHL-*a* levels especially in 2005. As already stated, we will add a short paragraph on the realism of our zooplankton. Based on measurements from Helgoland Roads, CPR (West Frisian transect) and GLOBEC (Renz et al. 2008) for April and May 2005 we will show that simulated zooplankton biomasses at the end of the bloom are comaparable or even higher than these reported values.

20. ***Referee 2:*** *P. 5010 23ff: The impact of different light requirements on specific algal groups is stressed in this paragraph. Indeed, different light climates can result in dominance shifts during bloom formation. Typically, however, temperate marine regions are characterized by an early spring diatom bloom occurring in*

March/ April followed by a bloom of Phaeocystis thereafter. Another possible explanation for a shift from a diatom to a Phaeocystis bloom would be that during warm winter-spring conditions a strong grazing on diatoms suppresses diatom bloom formation thus leading to a bloom of inedible phytoplankton species (like Phaeocystis, see Irigoien et al. 2005 for details).

We fully agree on the importance of phytoplankton species composition for predicting the growth response to variable light conditions. In our discussion we dedicated an entire section (6.5) on algal community structure, especially on the shifts between diatoms and Phaeocystis. However, our model does not intend to assess the interaction of different algal groups. We are convinced that for this purpose a better tailored model approach (e.g. the Phaeocystis-resolving model of Lancelot et al.) is more indicative. The major aim of this study is to elucidate the dominant factors for the spring bloom phenomenon with emphasis on alongshore gradients. Different light requirements or grazing pressures in the model are able to significantly change the absolute CHL-*a* levels. The distinctive alongshore gradient in 2005, however, is very robust against those parameter variations and only reproducible with realistic hydrodynamics and the inflow of waters with higher CHL-*a* . We identified the lack of detail regarding the different algal groups as a potential source of error (cf. Ch. 6.5) that may have prevented a better reproduction of the measurements, but from our model results there is no evidence that this lack of detail may have caused the observed alongshore gradient in 2005 or the significantly different spring bloom dynamics in both years. From our perspective, it is the strength of the model that it is able to qualitatively explain the spring bloom dynamics of both years without changes of the parametrisation, indicating the importance of the physical forcing for pattern formation. With this conclusion we do not exclude the possibility that a more sophisticated representation of the biology could potentially improve the model result.

On behalf of all authors

Gunnar Brandt

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