Ocean Sci. Discuss., 6, C713–C729, 2009
www.ocean-sci-discuss.net/6/C713/2009/

Ocean Science
Discussions

Interactive comment on "Modelling approach to the assessment of biogenic fluxes at a selected Ross Sea site, Antarctica" *by* M. Vichi et al.

M. Vichi et al.

vichi@bo.ingv.it

Received and published: 27 October 2009

We thank Referee #1 for the thoughtful comments. We have now revised the manuscript taking into account all the comments.

1 General comments

We have itemized below the major criticisms raised by the reviewer

1. t*he objectives outlined in the introductory have not been achieved.*
   We believe that the objectives outlined in the Introduction, i.e.:

1) quantification of the organic matter production in the euphotic layer and comparison with available observations; 2) quantification of heterotrophic consumption processes in the mesopelagic layer; 3) estimation of model-derived sinking rates at given depths within the aphotic zone; 4) comparison with sediment traps data,
have all been achieved. We agree that this model was not able to succeed in the final aim we indicated, i.e. “to reproduce the (observed) vertical fluxes of autochtonous organic matter as forced by the dominant water column physics” . We changed the word “reproduce” in the introduction with “simulate” and changed the sentence at line 59 of the revised manuscript, in which we removed the emphasis on the particulate fluxes, since about half of the work is about the assessment of the trasformation of organic carbon in the euphotic and mesopelagic layers:
“Particularly, this work will focus on the assessment of the production/consumption rates of organic material down to the mesopelagic layer and how they determine the vertical fluxes of organic particles.”
We have also completely rewritten the conclusion in the revised manuscript to better explain the major outcomes of the work.

2. *the biological model applied resolves many processes that are not relevant for addressing the major questions of the study, whereas it is actually limited in resolving important processes (particle aggregation, differential settlement).*
This is true, and we nevertheless think this is a positive feature. This is indeed one of the fundamental assumptions of the BFM model, meant to describe the global ocean biogeochemistry. Redundant mechanisms are therefore necessary to accomplish the different conditions in contrasting environments. We are aware that some important processes are still missing and this kind of study was instrumental to evaluate the model capabilities to describe important low-latitude conditions as the one found in the Ross Sea in the context of global ocean biogeochemical modelling. We have added this consideration in the revised conclu-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

sion. On the other hand, differential settlement was taken into account by using two different kind of particulate material, although not done through a dynamical aggregation but considering a different origin. See also answer at Sec. 2.2 of this document.

3. *no conclusions can be drawn from the modelling results.*
   This is a rather strong comment and it means we failed to highlight the major conclusions in the final part. The conclusions have now been rewritten according to the points discussed in Sec. 4. Our guess is that we focused too much on the model failures without pinpointing the lessons learnt and the lines of investigations suggested by model results. If a model is "wrong", it means that it cannot explain certain processes on the basis of its assumptions. The major assumptions of the BFM model are derived from generic considerations on the functioning of lower trophic levels, and not specifically tailored for low-latitude ecosystems. If the model fails to reproduce the observed fluxes than we can conclude that a constant size-class differential settlement is not adequate and other processes have to be considered. We agree with the author that aggregation may be one of the candidates (cfr. Sec. 2.2 below). We have now put this issue more clear in the revised conclusions. As stated in the Introduction, the aim of the paper was not to "fit the data" but to "use the data" as constraints in a deterministic process study. We believe the word "assessment" in the title was given a different interpretation. We meant to use the model to evaluate the nature and quality of the fluxes. This does not necessarily mean that the model as it stands is the best to reproduce the biogenicfluxes in low-latitude ecosystems. To better clarify this issue, we have changed the title in the revised manuscript to "Modelling approach to the study of biogenic fluxes at a selected Ross Sea site, Antarctica".

4. *relevant information, like integrated or averaged mass exchange rates between all model compartments, are not depicted and therefore a reader will not be able to comprehend the predominant model dynamics.*

We agree with the Referee that this kind of information is interesting and the first author already applied this methodology in another work in a shallow coastal sea (Vichi et al., 1998). The theoretical value of this kind of studies is relevant, although there are no sufficient measures to estimate the relative flow of carbon among the various constituents, and therefore this information cannot be constrained. However we do not think that this is mandatory to comprehend the predominant model dynamics in this work. The predominant model dynamics is autotrophic during the bloom period, and we clarify this in the revised manuscript with the addition of Fig. 2 and the comments related to Net Community Production provided in point 4 of Sec. 2 (this document). We have also shown that, through a combination of POC availability and sinking velocity, it is possible to reproduce the bulk of microbial respiration in the mesopelagic layer. This does not imply that our solution is unique, and further studies are needed to estimate the role of heterotrophy in modulating the biogenic fluxes. However, our model suggests (and we wrote this in the revised conclusion) that the mismatch in production phase during the melting of the sea-ice season may be sufficient to generate the time gap between model results and trap observations, thus emphasizing the role of the bloom start.

2 Answers to specific comments

2.1 Model assessment

1. *Growth rates according to the formula given in Smith et al. (2000) were originally determined from POC measurements. Smith et al. (2000) discuss that their rate approximations may seriously underestimate actual growth rates. In effect, it would be possible to infer growth rate estimates based upon POC (not only from diatom carbon biomass) with the model proposed here and compare these*

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

results with estimates of Smith et al. (2000).
We thank the reviewer for this comment. We indeed compared the simulated growth rate with the method suggested by Smith et al. to demonstrate what was pointed by the authors. We used diatom carbon concentration since most of the production in the model is due to diatoms. In Fig. 1 we show the same estimate computed with diatom carbon and total POC as suggested by the reviewer, in order to be more in line with the experimental methodology of Smith et al. In the case of the model, the difference is small during the bloom period (POC estimates being generally lower in the initial phase). We added some specific sentences at lines 234 and 243 of the revised manuscript to clarify the issue.

2. *The paragraph on respiration is unclear and from Figure 8 one cannot distinguish between observed rates and model results.*
We rewrote the paragraph to make it more clear. This is an important piece of the paper as it demonstrates that the current model setup is able to reproduce the observed carbon degradation rates in the mesopelagic layer. There are no observational values in the figure, only model results to be compared with data in the text. We have also rewritten the figure caption that generated the misunderstanding.

3. *For a model assessment, the authors need to provide information (or become explicit in their assumptions) about the range of uncertainties of the export flux data (e.g. when separating organic carbon from other material collected in the trap). Langone et al. (2000) report deviations between 210Pb flux estimates and the trap samples. In their study they discussed that only if 50% of 210Pb was scavenged then 210Pb flux estimates would match flux into the traps. Other studies suggest that the efficiency in collecting particulate matter in cone-shaped sedimentation traps can be problematic. This issue remains unresolved in the manuscript.*
We thank the reviewer for this comment. We have now discussed this issue in

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Sec. 4 as follows (lines 434-451 of the revised manuscript):
The process study on changing the detritus parameter values presented in Sec. 3.6 suggests that by varying the dimension and quality of the detrital particles we can obtain several patterns of vertical fluxes that have been actually observed in the Ross Sea sediment traps (DiTullio et al., 2000; Langone et al., 2000; Collier et al., 2000; Gardner et al., 2000; Langone et al., 2003; Asper and Smith, 2003). Several of these studies also recognize that the efficiency in collecting particulate matter in cone-shaped sedimentation traps can be problematic. For instance, Langone et al. (2000) reported deviations between ^{210}Pb flux estimates and the trap samples that they attributed to the low efficiency of both ^{210}Pb scavenging and sediment trap collection. Usbeck et al. (2003) reported significant underestimations of fluxes by shallow sediment traps, which collected on average at approximately 50% efficiency. A similar conclusion was reached by Yu et al. (2001) and Fleisher and Anderson (2003) based on geochemical tracers. In the Ross Sea, a comparison of all available data of export estimates based on dissolved inorganic N drawdown or ^{234}Th deficits and sediment traps gave even higher discrepancies (Catalano et al., 2009). Therefore, the trap data presented in Fig. 9 are affected by an unknown uncertainty that may be estimated as at least a factor of 2. This implies that also the magnitude of some of the simulated combinations shown in Fig. 11 are likely to be comprised in the range of the observations.

4. *A serious model assessment at this particular site in the Ross Sea must not only consider local data. Other data should be regarded; from nearby sites that show similar characteristics in terms of plankton composition and productivity (those areas in the Ross Sea where diatoms dominate together with heterotrophic dinoflagellates). More data are available for assessing the model performance, mainly from those studies that were extensively cited in the manuscript. Thus, cross-validation would become feasible, for instance, if the model were applied to a location further south (or within other similar diatom/heterotrophic dinoflagel-*

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

lates domains in the Ross Sea). For example, a more critical model comparison with data from Region III of Sweeney et al. (2000) would be substantial.

We conducted our model assessment using data from a region larger than the site B area. We used data from the PNRA cruises collected in the western open-ocean region and shelf break. The model is ideally located at site B, especially because site B is characterized by low productivity and diatom-dominated, which are the features that a generic model used for the global ocean is expected to capture (Vichi et al., 2007). This further comment has been added in the revised introduction. We are aware that other data are available and indeed we used primary production observations from other studies as detailed in the manuscript. However at site B we had concurrent measurements of microbial respiration in the mesopelagic layer. These kind of data are not available at other stations apart from the AESOPS study that is however in the polynia-dominated area.

We thank the reviewer for suggesting to consider the data presented in Sweeney et al. (2000). We computed the Net Community Production in the first 100 m as the difference between phytoplankton production in the model and the respiration of the entire community (including all heterotrophs depicted in Fig. 2 of the manuscript). The model results for the period 1996-97 are shown in Fig. 2 (this document and new Fig. 8 of the revised manuscript).

The cumulative NCP over the bloom period is the same order of magnitude as estimated by Sweeney et al. (2000) in Region III, which comprises the site B area (2.6±1.0 mol C m^{-2}, their Table 3). The amount of particulate organic carbon produced in the first 100 m is comparable with available estimates. However, as discussed by Sweeney et al. (2000), their values may be understimated because of the uncertainty related to the quantification of CO_2 exchanges at the air-sea interface. The model indicates a large variability of NCP during the bloom period. The method used by Sweeney et al. (2000) is based on the average total CO_2 deficit between summer and spring cruises, therefore it may not consider events of successive production peaks and ingassing. This implies that model results

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

are on the lower side, which may be due to the lack of net primary production at the starting of spring conditions.

As for the application of the model at another site, this is actually being done through three-dimensional global ocean studies. As stated in the revised introduction and conclusion sections, this study was also useful to test the genericity of the BFM model formulations in the open ocean Antarctic region.

2.2 Model configuration

The authors stress that they added a fast sinking detritus compartment that is solely sustained by fecal pellets production of mesozooplankton. A slowly sinking detritus compartment is primarily fuelled from organic matter through means of cell lysis and sloppy feeding of microzooplankton. Basic model assumption is that fast sinking detritus is ultimately linked to the abundance of diatoms and to mesozooplankton grazing. Yet, such dominant pathway, where fast sinking fecal pellets from mesozooplankton are exclusively responsible for the export flux, has not been documented for the diatom/heterotrophic dinoflagellate dominated areas in the Ross Sea. Rather, it is suggested that particle aggregation is the mechanism responsible for the rapid export flux of organic matter in the respective diatom domains (Asper and Smith, 2003), which is consistent with the interpretation of microzooplankton (ciliates and heterotrophic dinoflagellates) being the major consumers of the diatoms (for example Caron et al., 2000). Biomass of microzooplankton could be related to diatom biomass and POC in the model and could be, for example, compared with measurements of Dennett et al. (2001).

We are grateful to the Referee for this comment and for the clear explanation. We indeed concluded that the characterization of detritus, the choice of the sinking velocity and the degradation rates are crucial for the timing and magnitude of the vertical fluxes. Aggregation is one of the processes that regulates these conditions, as it was suggested in some areas of the Ross Sea. However, the information on particle aggregation discussed in Asper and Smith (2003) are derived from stations in Region I and II (Sweeney et al., 2000) and not from Region III where Site B is located. Therefore, we argue that the hypotheses of particle transfer through dense particulate may still be viable in this low-productive area.

Furthermore, the parameterization of aggregation requires an even higher number of unknown parameters that would limit the interpretation of the results. Thanks to our process study, we can argue that different combinations of sinking velocity and availability of particulate detritus are not sufficient to explain the timing of observed deposition fluxes in the sediment trap. Our discussion was thus mostly on the relationship between the peak of the bloom period and the deposition flux and not on the processes of formation of this particulate. The fact that in certain cases (for instance by combining slow deposition from the previous year and fresh deposition) it was possible to obtain a higher flux in the bottom trap than at the surface may indeed suggest that aggregation can also play an important role during the late summer-winter period.

2.3 Sensitivity analysis

1. *The study includes a model sensitivity analysis where sinking speed of detritus and remineralisation rates are varied. Choosing these two parameters is critical, as they are not truly independent. Increasing the sinking speed while enhancing remineralisation can yield very similar model flux from organic matter back to inorganic components. The non-linear response seen in the model results must be attributed to the onset of a detectable sinking flux which in turn affects the quality of the detritus exported.*

   Considering all the Referee's comments, we recognize that the choice of the section's title is not completely pertinent with the kind of investigation we carried out. A sensitivity analysis is actually meant to find the best set of parameters that fits a given set of observations. On the one hand we applied a sensitivity analysis

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

method (i.e. by assuming independence of parameters as correctly pointed out by the referee) but on the other hand we used it for hypotheses testing and process understanding. We have therefore changed the title to “Process studies” which better represents the kind of analysis we conducted.
The enhancement of the degradation rate analysed in Sec. 2.6 does not mean that detritus is remineralised back to inorganic nutrients at the same rate. As we explained in p. 1490, line 14 of the manuscript, and according to the model equations in (Vichi et al., 2007) that we have now reported in Sec. 2.2 (line 150 of the revised manuscript), the increase of the degradation rate (from 0.01 in exp. D4 to 0.5 in D5) does not increase the remineralisation of 50 times, but only the availability of substrate to bacteria. The specific remineralisation rate in the productive surface layer changes of about 10% on average and of 50% in the period after the peak (in February/March, shown in Fig. 3 of this document for phosphate), and the amount of this flux is still very small with respect to the bulk of nutrients found in this region (Fig. 5 of the manuscript). Therfore, the quality of the produced detritus is only slightly modified by this non-linear effect and the major result of this change occur in the mesopelagic layer, where the bacterial respiration has been investigated in more detail and compared with available data (Sec. 3.4).

2. *The timing of biomass accumulation (which specifies the flux of phytoplankton biomass to detritus) within the upper layers depends on how light-limited growth relates to iron availability. Light-iron co-limitation is parameterised in the model. Assumptions made for the parameterisation will largely determine the timing and extent of the diatom bloom. In the proposed model this co-limitation is expressed by two non-dimensional regulation functions that are multiplied. Given this multiplicative regulation of phytoplankton growth, small deviations between light-limited growth and iron limitation are likely to have a strong impact on model results. This parameterisation introduces a critical model sensitivity. It is thus meaningful to do variations of those parameters that determine the model's co-*

limitation of primary production while varying sinking speed of detritus.
We do not completely agree with the Referee comment on the criticality of these parameters in the context of the presented work. It is true that light-iron co-limitation, as a major determinant of the initial slope of the P-E curve response in phytoplankton, largely affects the initial biomass production. Indeed, modifying the parameters of the P-E curve changes the timing and the duration of the bloom and we have used values that lead to a fast response after the melting of sea ice. This can be appreciated by considering the spring increase of chl:C ratio and specific growth rates shown in Fig. 7 of the manuscript. However, our model results show that, using these parameters, the biomass flux lags the observations of about 2 months, implying that the biomass accumulation starts very early in the season. Any other modification of the parameter formulation and values is likely to further enhance this lag. This is why we suggest that the delay in the model is linked to light availability, which is in turn related to the sea ice conditions. Part of the comment above has been included in the Discussion section at line 368 of the revised manuscript.

3. *Also, the authors have to provide a quantitative measure of data-model deviations for the sensitivity analysis, going beyond a simple visual inspection of modelled export flux and the sediment trap data.*
As indicated in the introduction (lines 8 at page 1480), the aim of this work was not to fit the data. We have explained in point 1 above that what we mistakenly defined sensitivity analysis was not used to derive the best set of parameters but rather to explore the range of solutions as a function of the model assumptions. Indeed, sensitivity analysis is an inadequate term to refer to this exercise and we changed the title of the section. The Referee is right by saying that objective assessment is crucial for model evaluation, as the leading author has demonstrated extensively in Vichi and Masina (2009, now accepted in Biogeosciences). However, the number of available data is still too limited to undertake a thorough and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

statistically significant objective assessment as the one done at the global scale.

3 Manuscript assessment

The authors need to depict the integrated mass exchange between model compartments, for carbon, nitrogen and phosphorous respectively
See the answer to point 4 in Sec. 1 above.

All the suggested references have been added to the revised manuscript.

Figure captions

Figure 1 - Simulated specific growth rate of surface diatoms (Fig. 7b in the manuscript). Blue: computed as photosynthesis - respiration - excretion; red: estimated from diatom carbon concentration; magenta: estimated from total particulate organic carbon.

Figure 2 - Simulated daily Net Community Production (NCP) in the first 100 m and integrated over the the bloom period.

Figure 3 - (a) Percent difference in phosphate remineralisation between experiment D4 (degradation rate of fast-sinking detritus = 0.01 d^{-1} and D5. (b) Phosphate remineralisation rates from bacteria from which the difference shown in (a) was derived. Simulation results are integrated over the first 100 m.

References

Asper, V. L., Smith, W. O., 2003. Abundance, distribution and sinking rates of aggregates in the Ross Sea, antarctica. Deep-Sea Res. Pt. II 50, 131–150.

Catalano, G., Budillon, G., Ferla, R. L., Povero, P., Ravaioli, M., Saggiomo, V., Accornero, A., Azzaro, M., Carrada, G., Giglio, F., Langone, L., Mangoni, O., Misic, C., Modigh, M., 2009. The ross sea. In: Liu, K.-K., Atkinson, L., Quinones, R., Talaue-McManus, L. (Eds.), Carbon and Nutrient Fluxes in Continental Margins: A Global Synthesis. Springer-Verlag, New York, pp. 303–318.

Collier, R., Dymond, J., Honjo, S., Manganini, S., Francois, R., Dunbar, R., 2000. The vertical flux of biogenic and lithogenic material in the Ross Sea: moored sediment trap observations 1996-1998. Deep Sea Research II 47, 3491:3520.

DiTullio, G. R., Grebmeier, J. M., Arrigo, K. R., Lizotte, M. P., Robinson, D. H., Leventer, A., Barry, J. P., VanWoert, M. L., Dunbar, R. B., 2000. Rapid and early export of Phaeocystis antarctica blooms in the Ross Sea, Antractica. Nature 404, 595:598.

Fleisher, M., Anderson, R., 2003. Assessing the collection efficiency of ross sea sediment traps using 230th and 231pa. Deep-Sea Res. Pt. II 50, 693–712.

Gardner, W. D., Richardson, M. J., Smith, W. O., 2000. Seasonal patterns of water column particulate organic carbon and fluxes in the Ross Sea, Antarctica. Deep Sea Research II 47, 3423:3449.

Langone, L., Dunbar, R. B., Mucciarone, D. A., Ravaioli, M., Meloni, R., Nittrouerand, C. A., 2003. Rapid sinking of biogenic material during the late austral summer in the Ross Sea, Antarctica. In: DiTullio, G., Dunbar, R. (Eds.), Biogeochemistry of the Ross Sea. Vol. 78 of Antarctic Research Series Monograph. American Geophysical Union, pp. 221–234.

Langone, L., Frignani, M., Ravaioli, M., Bianchi, C., 2000. Particle fluxes and biogeochemical processes in an area influenced by seasonal retreat of the ice margin (northwestern Ross Sea Atarctica). Journal of Marine Systems 27, 221–234.

Sweeney, C., Hansell, D. A., Carlson, C. A., Codispoti, L. A., Gordon, L. I., Marra, J., Millero, F. J., Smith, W. O., Takahashi, T., 2000. Biogeochemical regimes, net community production and carbon export in the Ross Sea, Antarctica. Deep Sea Research II 47, 3369:3394.

Usbeck, R., Schlitzer, R., Fischer, G., Wefer, G., 2003. Particle fluxes in the ocean: Comparison of sediment trap data with results from inverse modeling. J. Mar. Sys. 39 (3-4), 167–183.

Vichi, M., Masina, S., 2009. Skill assessment of the PELAGOS global ocean biogeochemistry model over the period 1980–2000. Biogeosciences Discuss. 6, 3511–3562. Accepted in Biogeosciences

Vichi, M., Pinardi, N., Masina, S., 2007. A generalized model of pelagic biogeochemistry for the global ocean ecosystem. Part I: theory. J. Mar. Sys. 64, 89–109.

Vichi, M., Zavatarelli, M., Pinardi, N., 1998. Seasonal modulation of microbial-mediated carbon fluxes in the Northern Adriatic Sea. Fisheries Oceanography 7 (3/4), 182–190.

Yu, E., Francois, R., Bacon, M., Honjo, S., Fleer, A., Manganini, S., van der Loeff, M., Ittekot, V.,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

2001. Trapping efficiency of bottom-tethered sediment traps estimated from the intercepted fluxes of 230th and 231pa. Deep-Sea Res. Pt. I 48, 865–889.

Interactive comment on Ocean Sci. Discuss., 6, 1477, 2009.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

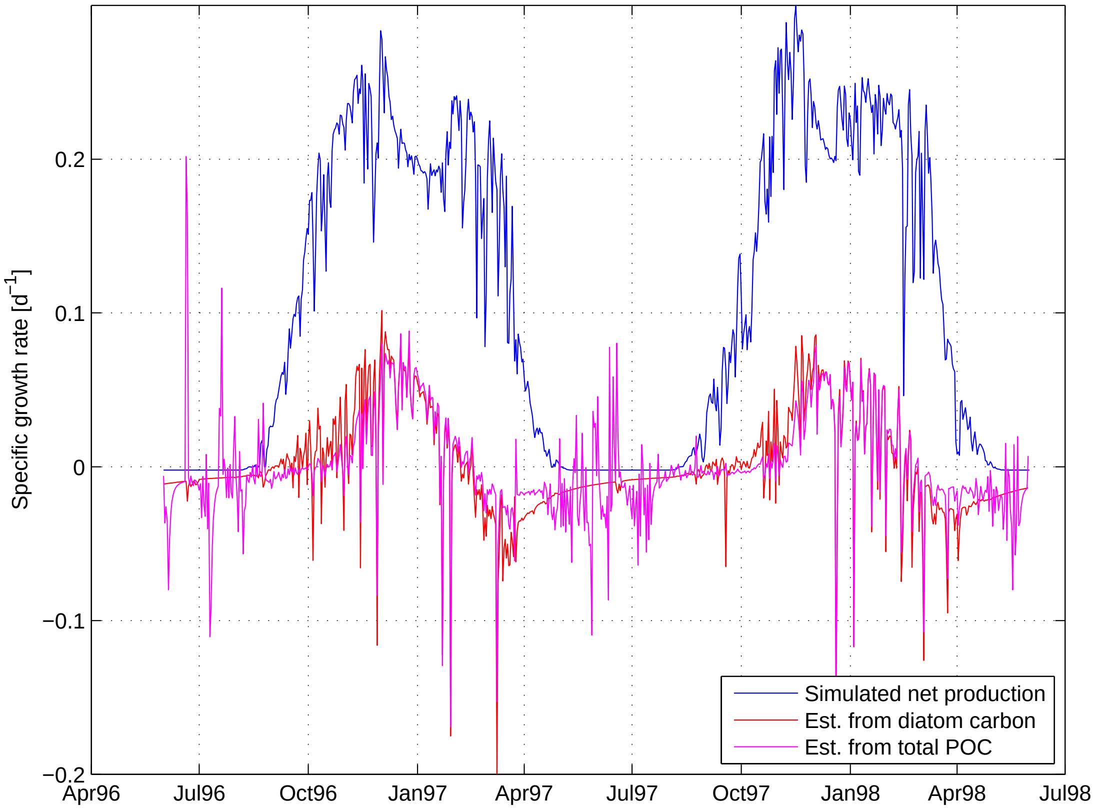
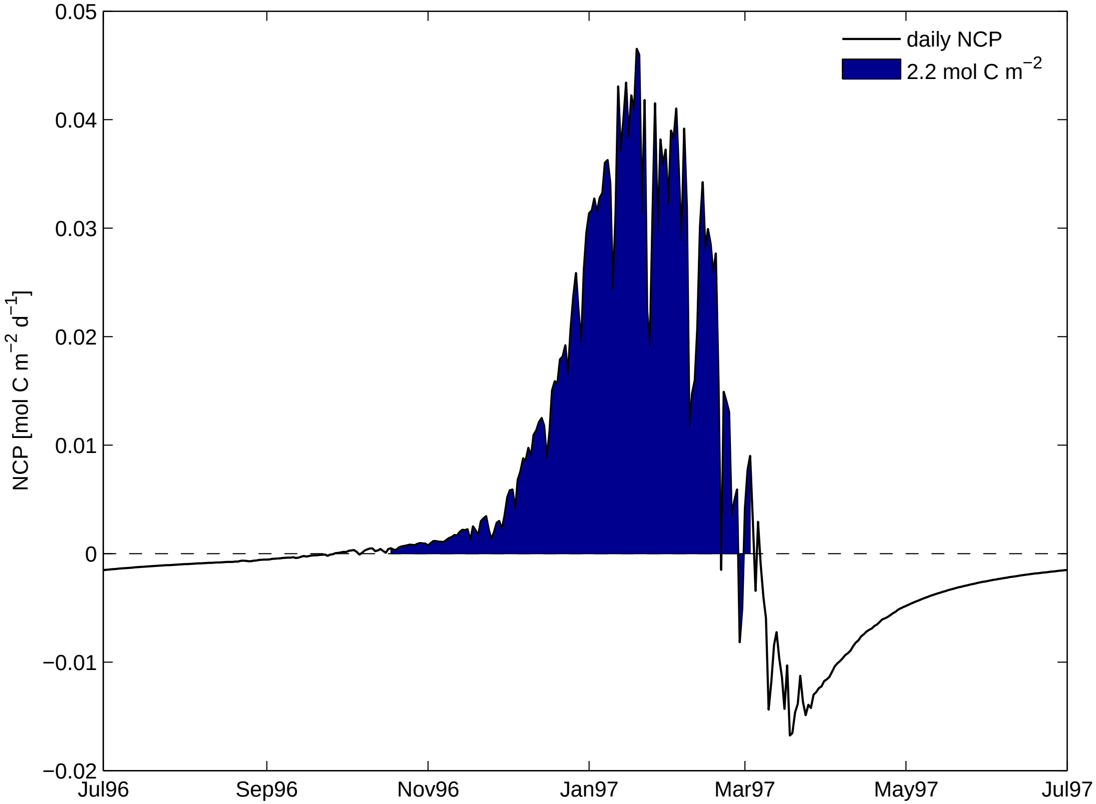

Specific growth rate [d^{-1}]
0.2
0.1
0
−0.1
−0.2
Apr96
Jul96
Oct96
Jan97
Apr97
Jul97
Oct97
Jan98
Apr98
Jul98
Simulated net production
Est. from diatom carbon
Est. from total POC

Fig. 1.



0.05
0.04
0.03
0.02
0.01
0
−0.01
−0.02
NCP [mol C m^{-2} d^{-1}]
Jul96
Sep96
Nov96
Jan97
Mar97
May97
Jul97
daily NCP
2.2 mol C m^{-2}

Fig. 2.

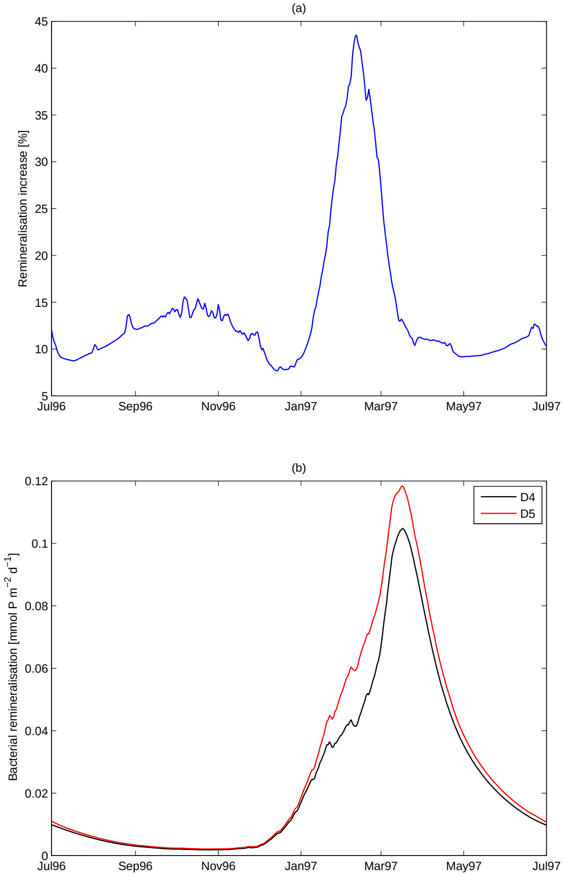


Fig. 3.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper