Response to Reviewer comments: EMPOWER-1.0 : an Efficient Model of Planktonic ecOsystems WrittEn in R: Anderson, Gentleman and Yool (GMDD 8, 53-140, 2015)

I. Overarching issues

(1) EMPOWER is an ecosystem model testbed, not the NPZD model embedded within it (which is used for illustrative purposes).

Referee #1: I feel a comparative description of some 'competing' marine ecosystem models (e.g. Blackford et al., 2004; Le Quere et al., 2005) would strengthen the argument for using less-complicated models, such as the simple NPZD model implemented here.

Referee #1: The models mentioned above (Blackford et al., 2004; Le Quere et al., 2005) are cited in the discussion but are not compared to EMPOWER in terms of their research applications or skill in reproducing observed data, which would provide further justification for less complex models such as EMPOWER.

Referee #2: This manuscripts explains the technical details of a simple NPZD model that runs in a two-layer vertical setup. The authors claim that using simple ecosystem models such the one described in the manuscript …

Reply: The important point to note is that EMPOWER is not an ecosystem model in its own right but, rather, a modelling framework, using slab physics, for testing and evaluating ecosystem models and their associated formulation and parameterisation. The NPZD model we use is for illustrative purposes although, nevertheless by using this ecosystem model we do make the case that useful science can be done with simple models. Inevitably this means, to some extent, climbing into the ongoing debate about model complexity but this is secondary to the main focus of the ms which is that modellers need to comprehensively test their models, comparing different formulations and parameterisations, with EMPOWER being provided as an ideal tool for this purpose. The text has been improved to make this clear:

(i) When stating the objectives of our work at the end of the Introduction we have added the following text to clarify matters: "Here, we demonstrate the use of EMPOWER-1.0 in combination with a simple representative nutrient-phytoplankton-zooplankton-detritus (NPZD) model. It should be noted, however, that EMPOWER-1.0 can be used to test and examine the performance of simple and complex models alike. Our choice of a simple ecosystem model is motivated by the fact that simple models are conceptually straightforward as well as being easy to set up and analyse.".

(ii) We previously started the Discussion talking about simple vs complex models and this was inappropriate in that, as stated above, the complexity issue is not the primary focus. We have now moved an amended version of this paragraph towards the end of the Discussion (see below) and provided a new opening paragraph: "Marine ecosystem modelling is somewhat of a black art in deciding what to include in terms of state variables, which formulations to apply for key processes such as photosynthesis, grazing and mortality, and in finding suitable parameter values. The proliferation of complexity in models has only served to increase the plethora of formulations and parameterisations available to choose from. Successful model construction and implementation inevitably requires the testing of model performance, involving the study of sensitivity to different functional forms and/or parameter values. Complex ecosystem models have come to the fore in recent years that, for example, include any number of plankton functional types, multiple nutrients, dissolved organic matter and bacteria, etc. (e.g., Blackford et al.,2004; Moore et al., 2004; Le Quéré et al., 2005). Simulations are often carried out within computationally demanding 3-D general circulation models (GCMs) and, of course, the realism in ocean physics thus gained is to be welcomed. The caveat is, however, that improvements in prediction can only be achieved if the biological processes of interest can be adequately parameterised (Anderson, 2005). The key is, as described above, to undertake extensive analysis of ecosystem model performance and we propose that the use of a simple slab physical framework is ideal in this regard. …".

The topic of model complexity remains relevant to the work and we have rewritten the opening paragraph of the Discussion and moved it to later on in the text: "EMPOWER-1.0 is provided as a testbed which is suitable for examining the performance of any chosen marine ecosystem model, simple or complex. We chose to demonstrate its use by incorporating a simple NPZD ecosystem model. Simple marine ecosystem models are, however, all too often brushed aside in marine science today. While our objective here is not to delve deeply into to ongoing debate about complexity in models (e.g., Fulton et al., 2004; Anderson, 2005; Friedrichs et al., 2007; Ward et al., 2010), we would nevertheless like to comment on the worth of simple models. When it comes to the representation of the marine ecosystem, complex models have come to the fore that have, for example, any number of plankton functional types, multiple nutrients, dissolved organic matter and bacteria, etc. (e.g., Blackford et al.,2004; Moore et al., 2004; Le Quere et al., 2005). There is a similar trend with ocean physics toward large, computationally demanding models. Many publications in recent years have involved the use of 3D models (e.g., Le Quéré et al., 2005; Wiggert et al., 2006; Follows et al., 2007; Hashioka et al., 2013; Yool et al., 2013b; Vallina et al., 2014), although 1D models are also well represented (e.g., Vallina et al., 2008; Kearney et al., 2012; Ward et al., 2013). Of course, the improved realism that is gained by using complex models is in general to be welcomed, with the caveat that improvements in prediction can only be achieved if the processes of interest can be adequately parameterised (Anderson, 2005). That is a big caveat and one made harder to achieve because it is often difficult and/or time consuming to thoroughly test the formulations and parameterisations involved. Simple NPZD-type models have a useful in this regard. Albeit with tuning (but the complex models are tuned also), our NPZD model was successfully used to describe the seasonal cycles of phytoplankton and nutrients at four contrasting sites in the world ocean. It was readily used to test different parameterisations for photosynthesis and mortality. At least in terms of basic bulk properties, simple models produce realistic predictions and are easily to thoroughly investigate and assess. The whole issue of model complexity ought in any case to be question dependent (Anderson, 2010), e.g. simple models may be useful to address questions on biogeochemical cycles whereas more complex models may be necessary to answer more ecologically relevant questions such as the effect of biodiversity on ecosystem function. The use of the EMPOWER testbed allows the user to investigate and determine whether a particular ecosystem model is sufficiently complex, or indeed too complex, to address the question of interest."

(2) Year selection for model comparison

Referee #1 point 20: p. 81, line 3–4: The way in which 2006 is a characteristic year is not explained.

Referee #1 point 41: Figures 11, 12 and 13: Data shown are for 2008 or 2009 – the choice of these years (rather than 2006) is not explained in the text.

Referee #2 point 10: Page 80 - Line 29: "Averaging data across years ... to compare the model to data" – I do not agree with this. If the model is using climatological forcing, the data should be climatological as well. Just show average monthly outputs for the model to smooth out the bloom as well as it happens with the data. Or otherwise run the model using the MLD forcing from 1998 to 2013 and then average the model outputs to construct a climatology. The data are not measured daily anyways; usually sampling is once or twice per month.

Referee #2 point 11: Page 81 - Line 04: "in this case 2006" – Why 2006 and not any other year? This is an arbitrary choice. One can then select the year or years that best fit the model output. I don't think this is a robust comparison.

Referee #2 point 14: Page 83 - Line 12: Figure 11 uses year "2008" – Why the authors now select 2008 and not 2006? These choices look too arbitrary to me.

Reply: We agree with the referees that our choice of years was arbitrary. This was done purely to select a representative year that characterised the location well, but without introducing problems caused by bloom timing that would affect a simple average across years. After taking statistical advice, we now select years objectively as follows (quoting from the revised text): “A characteristic year was therefore chosen for each station by firstly converting the data [all years] to log(chlorophyll), then calculating mean log(chlorophyll) for each year and finally selecting the median year (an odd number of years is required, so we used 1998 to 2012). The resulting year selections were 2002, 1998, 2007 and 2006 for stations BIOTRANS, India, Papa and KERFIX respectively.” A new Figure 9 is provided which shows data for the time series for each station overlaid (1998-2013), with the selected years highlighted (see Appendix).

As noted above, averaging data across years, as suggested by reviewer #2, might in some way be objective but would be wholly unconvincing as the characteristic features of the seasonal cycle, such as the spring phytoplankton bloom in the North Atlantic, would be “ironed out”. This is clarified in the text: “Regarding chlorophyll, data are SeaWiFS 8-day averages (O’Reilly et al., 1998), for which we had access to years 1998 to 2013. Averaging data across years to provide a climatological seasonal cycle of chlorophyll is not meaningful as key features, such as the spring phytoplankton bloom, are smoothed out because the bloom timing is variable between years.”

Looking at the selected years (Figure 9), it is clear that BIOTRANS shows a cleaner (less noisy) seasonal cycle compared to India and we therefor chose to switch to station BIOTRANS as the primary focus for our parameter tuning exercise. The sensitivity analyses (photosynthesis calculation; mortality terms) are pertinent to all stations and we have expanded the results for all four stations. The switch to BIOTRANS, as well as the focus on sensitivity for all stations, means that we have redone all the model results (see Appendix).

II. Referee #1

General Comments

Referee: Anderson et al. provide a detailed description of the two-layer slab model ‘EMPOWER’. They also describe their parameter fitting methodology at four stations, as well as a structural sensitivity analysis, which assessed the calculation of daily depth integrated photosynthesis and the mortality terms used. In addition to providing a methodological framework for model testing that can be recreated by the modelling community, it is interesting that they find their model has a greater degree of sensitivity to the attenuation of light in the water column than the choice of P-I curve used in terms of calculating daily depth integrated photosynthesis.

Reply: We wish to thank the referee for his/her positive comments about the ms. As noted in Overarching Issue (1), EMPOWER is not a model but rather a modelling framework, using slab physics, for testing and evaluating ecosystem models and their associated formulation and parameterisation.

Referee: The introduction is well written and well informed; however, I feel a comparative description of some ‘competing’ marine ecosystem models (e.g. Blackford et al., 2004; Le Quere et al., 2005) would strengthen the argument for using less-complicated models, such as the simple NPZD model implemented here. Elizabeth Fulton has published several papers regarding marine ecosystem model complexity (Fulton et al., 2003a; Fulton et al., 2003b; Fulton et al., 2004), which may contribute to the discussion about ecosystem model complexity here and in the final discussion.

Reply: Reiterating the points made in Overarching Issue (1), EMPOWER is not itself an ecosystem model and, as such, there are no competing ecosystem models and no comparison to be made in this regard. It is the physical setup which is necessarily simple in EMPOWER and we already justify this with “Despite the simplicity of the two-layer slab physics, these models are sufficiently well formulated to permit realistic and insightful simulations of marine ecosystems …”. This justification is then elaborated in the following section (2. Slab models), highlighting the utility of the slab approach from early pioneering studies until the modern day. We now cite Fulton et al. (2003a,b) and Fulton et al. (2004).

The issue of model complexity does crop up and, indeed, we believe that by using an NPZD model within the EMPOWER framework, we show that there remains a place for simple models in contemporary marine science. Nevertheless, model complexity is not the main focus of the ms (the need for modellers to thoroughly test formulations and parameterisations in their model, and the provision of EMPOWER for this) and have toned down our discussion of complexity issues, in particular removing this from the start of the Discussion (see reply to Overarching Issue (1)).

Model complexity has different aspects and one is that there is a distinction between model complexity in terms of structure and complexity in terms of functional forms. This issue is raised in the Fulton et al. papers indicated by the referee. The EMPOWER testbed is ideal for testing and evaluating the use of different functional forms for processes such as photosynthesis, grazing, mortality, etc. We better emphasise this point in the revised text, e.g. our new opening paragraph for the Discussion (see Overarching Issues (2), point (ii)).

Referee: The models mentioned above (Blackford et al., 2004; Le Quere et al., 2005) are cited in the discussion but are not compared to EMPOWER in terms of their research applications or skill in reproducing observed data, which would provide further justification for less complex models such as EMPOWER. Similarly, comparison to low complexity global models such as that of Tyrrell (1999) – which has been used for educational purposes and research (e.g. Chuck et al., 2005) – would add completeness to the discussion.

Reply: As noted previously, EMPOWER is a model testbed, not an ecosystem model and, as such, there is no comparison to be made between EMPOWER and models such as Blackford et al. (2004) and Le Quere et al. (2005). Our objective was most definitely not to compare simple and complex ecosystem models to say which fare better, nor to necessarily promote simple ecosystem models at the expense of simple ones. Rather, it was to promote and provide a testbed, based on simple physics, that allows testing of ecosystem models or, indeed, intercomparison of performance between different models. EMPOWER is well-suited for undertaking an intercomparision of, for example, our NPZD model and ERSEM (Blackford et al., 2004), but this would be a major exercise in itself and is well beyond the scope of our study.

We agree that it would be useful to mention box models and have added the following paragraph to the Discussion section: "Bearing in mind Steele's two-layer sea, the first slab model of its kind (section 2), it is worth noting that simple ocean box models are akin to slab models in terms of physical structure, but represent spatial areas (e.g., ocean basins or the global ocean) rather than point locations in the ocean. A mixed layer or euphotic zone is positioned above a deep ocean layer, with mixing between the two but usually without a seasonally changing mixed layer depth. Tyrrell (1999), for example, used a global ocean box model to study the relative influences of nitrogen and phosphorus on oceanic primary production. Box models were likewise used by Chuck et al. (2005) to study the ocean response to atmospheric carbon emissions over the 21st century. Slab models, including EMPOWER, effectively convert to simple box models if the seasonality of mixed layer depth is switched off. Without a seasonally varying MLD, box models have limited capacity to capture seasonal plankton dynamics because of the role played by MLD in mediating the light and nutrient environment experienced by phytoplankton. Our results demonstrate sensitivity to accurate representation of the submarine light field."

Referee: Model skill in reproducing observed chlorophyll and nitrate concentrations is not quantified and, although the description of 'fit' is detailed, it would certainly facilitate comparison of parameter sets and model setups. Lewis and Allen (2009) and Lewis et al. (2006) are examples of quantifying model skill that come to mind. Although the majority of the paper is well referenced, there are a number of points throughout that would benefit from additional citations (for details see my specific comments below). The results section also has numerous qualitative statements that require quantification (again see my specific comments below).

Reply: Quantitative skill assessment is an important part of ecosystem modelling, but is tangential to the central aim here, namely the provision of EMPOWER as an ecosystem model testbed. We undertake an illustrative use of an NPZD model in EMPOWER and compare it to data. Other models will involve other data sets, each with its own unique requirements in terms of assessing model-data misfit. In the case of our assessment, visual inspection is easily sufficient (e.g. one not need quantitative measures of skill to see that the fit in Figure 11 (new numbering; fitted BIOTRANS model) is better than that in Figure 10 (unfitted BIOTRANS model)) . The manuscript is already lengthy and providing a quantitative skill assessment, such as the Nash Sutcliffe method and/or multivariate statistics (e.g., Lewis and Allen, 2009; Allen and Somerfield, 2009: J. Mar. Syst. 76, 83-94) would unnecessarily increase length and the description therein would not be necessarily applicable to other uses of EMPOWER. In response to the reviewer's comment, we have updated the text to summarise our approach: "It is not our objective here to provide thorough quantitative assessment of different model simulations in terms of objective quantification of model-data misfit but, rather, to demonstrate the utility of EMPOWER as a testbed for model evaluation. Different ecosystem models and associated data sets will necessarily require different skill metrics and so a lengthy description and use of quantitative metrics is not appropriate here. Very often anyway, as is the case here, visual inspection of model-data misfit is sufficient to determine the best options for model formulation/parameterisation. If quantitative methods are required, these are readily accessed from the literature (e.g., Lewis and Allen; 2009; Lewis et al., 2006)."

Specific/Technical Comments

Referee: 1) p. 55, lines 1–9 and p. 56, lines 11–15: No example studies are cited to support the statements made and direct further reading for those interested.

Reply: We have added suitable references to back up three of the statements made in these lines:

(i) "Ecosystem models are ubiquitous in marine science today, used to study a range of compelling topics including ocean biogeochemistry and its response to changing climate, end-to-end links from physics to fish and associated trophic cascades, the impact of pollution on the formation of harmful algal blooms, etc". References added: Steele (2012; Prog. Oceanogr. 102, 67), Gilbert et al. (2014; Global Change Biol. 20, 3845), Holt et al. (2014; Prog. Oceanogr. 129, 285), Kwiatkowski et al. (2014; Biogeosciences 11, 7291)

(ii) "Anderson et al. (2014), for example, commented on the "enormous" diversity seen in chosen formulations … and asked whether reliable simulations can be expected given this diversity. This question applies not just to modelling DOM, but also to most processes and components considered in modern marine ecosystem modelling." References added: Fulton et al. (2003; Ecol. Modell. 169, 157), Anderson et al (2010, 2013; both already in list of references)

Referee: 2) p. 56, line 27: Are the models referred to reviewed by Gentleman (2002)?

Reply: Yes, Gentleman's article is a review. The title of her paper is: "A chronology of plankton dynamics in silico: how computer models have been used to study marine ecosystems". To strengthen our sentence yet further, we have now also cited Anderson and Gentleman (2012).

Referee: 3) p. 59, line 1: It would be helpful to know the location of George Bank.

Reply: Sentence amended to "…who constructed a model of seasonal phytoplankton dynamics for Georges Bank, a raised plateau off the coast of New England, northeast U.S.A. (Riley, 1946), …".

Referee: 4) p. 63, line 17: Pluralise station, i.e. "...(stations Papa in the north: : :".

Reply: Amendment made, as indicated.

Referee: 5) p. 63, line 21 and forward: There are several versions of the World Ocean Atlas, it would be helpful to make the version used clearer (i.e. WOA 2009).

Reply: The version was clear from the citation in the reference list but, nevertheless, we have added the version (2009) to the main text, as requested.

Referee: 6) p. 64, lines 19–20: An explanation of why you focused on station India would be helpful.

Reply: In fact, we have now switched focus to station BIOTRANS: see Overarching Issues (2). We have added the following text to justify this focus: "This station is chosen as our primary focus, inspired by the North Atlantic Bloom Experiment in 1989 as part of JGOFS (the Joint Global Ocean Flux Study; e.g., Ducklow and Harris, 1993; Lochte et al., 1993). It exhibits the characteristic spring blooming of phytoplankton of temperate latitudes, followed by relatively oligotrophic conditions over summer, and has been the subject of previous work using slab models (Fasham and Evans, 1995)."

Referee: 7) p. 66, line 8: kPAR is not defined.

Reply: The text now reads: "Light is assumed to vary with depth according to Beer's law (I = I0 exp(-k_{PAR}z)), where k_{PAR} is the attenuation coefficient, …".

Referee: 8) p. 66, line 20: I would find an example plot illustrating changes in surface irradiance throughout the day (both sinusoidal and triangular patterns) helpful.

Reply: New Figure (Figure 6) produced, as requested.

Referee: 9) p. 67, line 17: Explicitly stating the coefficients in question would simplify reading, i.e. ": : :polynomial coefficients (b0,i – b5,i) are listed in Table 2."

Reply: Text amended to: "The values of the polynomial coefficients ($b_{0,i}$ – $b_{5,i}$) are listed in Table 2."

Referee: 10) p. 68, lines 2–5: This sentence is repeated from p.66, lines 19–21.

Reply: We have removed the latter sentence from the text.

Referee: 11) p. 69, lines 21–24: Symbols φ and ϕ seem to be used interchangeably.

Reply: Problem fixed, opting solely for ϕ. Part of this problem was due to editorial work and we will check the proofs carefully to ensure there are not further problems in this regard.

Referee: 12) p. 70, line 13: Word order should presumably be "Regarding phytoplankton non-grazing mortality: : :".

Reply: Text amended to "Regarding phytoplankton non-grazing mortality …".

Referee: 13) p. 71, line 8: It would be helpful to direct the reader to the equations in which each term is used, as you have done for GGE (Eq. 13).

Reply: The problem is that terms for faecal pellet production ($1-\beta$) and excretion ($\beta(1-k_{NZ})$) appear not in the zooplankton equation, but in equations for detritus (Eq. 15) and DIN (Eq. 14) which have not been introduced yet. It would be awkward to refer to these equations ahead of their presentation in the text, so we have made no alterations here.

Referee: 14) p. 71, lines 1–8 and p. 72, lines 10–23: Perhaps referring to Table 3 somewhere here would help the reader follow the variables being defined.

Reply: Have amended the text to "Splitting into these various parameters (Table 3) …." for the first instance but made no alteration for the latter as there is little reference to parameter values there.

Referee: 15) p. 75, lines 1–2: This sentence is repeated from p. 73, lines 13–14.

Reply: The first instance of this repetition has been removed from the text.

Referee: 16) p. 75, line 5–15: Please state the equation numbers corresponding to the functions.

Reply: Text amended as requested: "The key function call is FNget_flux which contains the ecosystem model specification (section 3.2). The rate of change is calculated for each term in the differential equations and allocated to a 2-D array (flux no., state variable no.) which is then passed back to the core (permanent) code for processing. Other functions are: FNdaylcalc (calculates length of day; Eq. A7), FNnoonparcalc (noon irradiance, PAR; Eq. A5), FNLIcalcNum (undertakes numerical (over time) calculation of daily depth-integrated photosynthesis), FNLIcalcEP85 (calculates L_I using the equations of Evans and Parslow, 1985; Appendix C1), FNaphy (calculates chlorophyll absorption, effectively parameter α, in the water column after Anderson, 1993; Eq. C14) and FNLIcalcA93 (calculates L_I using the equations of Anderson, 1993; Appendix C2)."

Note that we had erroneously missed out the equation for day length and this has now been added to the text as a new equation in Appendix A, Eq. (A7).

Referee: 17) p. 75, line 18: I would find it helpful to have the state variables explicitly listed here.

Reply: The text has been amended to: "Initial values for state variables (N, P, Z, D)."

Referee: 18) p. 76, line 14–16: Perhaps the output files listed should be added to Figure 7.

Reply: The output files are already listed on this Figure: "Write to output files: out_statevars.txt, out_aux.txt, out_fluxes.txt".

Referee: 19) p. 80, line 21: Possible typo of 'that' instead of 'than'.

Reply: Typo amended.

Referee: 20) p. 81, line 3–4: The way in which 2006 is a characteristic year is not explained.

Reply: See response to Overarching Issue (2). Year selection is now done on an objective basis.

Referee: 21) p. 81, lines 6–10 and p. 82, line 23: Comparative statements are made in terms of model fit but these are not quantified. For example, how much 'too high' was predicted chlorophyll in spring and summer?

Reply: This referee comment is followed by a number of similar ones below, asking for better quantitative description. Given that we have redone the results with a focus on station BIOTRANS, it is not easy to respond on a point-by-point basis. Rather, here are a number of examples where we have updated the text in response to the referee's criticism:

(i) Figure 10 (simulation of BIOTRANS with initial-guess parameters): "The peak of the spring bloom is more than double that observed and post-bloom chlorophyll is also consistently elevated (by approx 0.2 mg m-3) relative to observations (Fig. 10)".

(ii) Figure 13 (simulation of station India using BIOTRANS parameters): "In fact, the predicted spring bloom is rather high, approximately double the maximum in the observations for year 1998 (Fig. 1113a), although not outwith what is seen in the multi-year data (Fig. 9)."

(iii) Figure 15 (simulation of station KERFIX using station BIOTRANS/Papa parameters): "A similar exercise was carried out for station KERFIX. Using the same parameter set as for station Papa, predicted chlorophyll was too high (by approximately 0.05 mg m-3) during the austral summer (Fig.

15). ... Predicted nitrate is somewhat too low (by about 4 mmol m-3) if the BIOTRANS parameters are used but is markedly improved with the adjusted values for parameters $V_P^{\max}(0)$ and Imax."

(iv) Figure 16 (comparision with results using exponential P-I curve, station BIOTRANS): "Results changed little with respect to the baseline simulation, the only noticeable difference being the magnitude of the spring bloom which was about 0.2 mg m-3 greater when using the exponential P-I curve."

(v) Figure 17 (comparison with results using triangular irradiance assumption): "A larger spring bloom (approx. 0.5 mg m-3) is seen when using the triangular assumption. Irradiance is underestimated relative to the sinusoidal pattern ...".

(vi) Figure 21 (model simulations with phytoplankton mortality terms removed): "In contrast to the representation of linear mortality, many models do not include a non-linear phytoplankton mortality term but it seemed to perform well here. When it was removed, the predicted phytoplankton spring bloom was rather too high (more than double that observed)."

(vii) Figure 22 (model simulations with zooplankton mortality terms removed): "Removal of quadratic mortality resulted in significantly lower phytoplankton levels decreasing by as much as 50% which is unsurprising since more zooplankton means more grazing. Perhaps less obvious is the result that removal of quadratic closure resulted in similarly large changes in predicted post-bloom nitrate levels ...".

Referee: 22) p. 82, lines 24–25: Why is low overwinter chlorophyll is a common feature in slab models?

Reply: This is an interesting question and a detailed analysis is beyond the scope of our article. The answer probably lies in the phytoplankton mortality terms and we already address this issue in section 4.4: "The model is relatively insensitive to the phytoplankton mortality terms although setting m_P=0 (i.e., removal of the linear term) promoted net phytoplankton growth over winter, increasing coupling to zooplankton grazers and giving rise to smaller phytoplankton blooms in spring (Fig. 21). Predicted seasonality in NO_3 drawdown was barely affected by phytoplankton mortality parameters. Removal of the linear term improved the model fit for chlorophyll over winter for stations Biotrans and India. It seems hard to justify that loss rates should go to near zero at low population densities (the consequence of using a quadratic term only) because all organisms have metabolic requirements. Nearly all marine ecosystem models do, therefore, include a linear term for density-independent phytoplankton mortality and, for our baseline simulation (section 4.2), we chose to keep this term on a purely conceptual basis."

Referee: 23) p. 83, lines 23–27 and p. 84, line 2 and 18: Again comparative statements are made but not quantified.

Reply: See reply to Referee point 21) above.

Referee: 24) p. 86, line 12: 'A93' does not seem to have been defined in the text.

Reply: Amended to: "…when using the method of Anderson (1993), …".

Referee: 25) p. 87, line 8–14: Please cite examples models and studies supporting the statements.

Reply: It is difficult to find references that categorically say that 1-D and 3-D models have difficulty dealing with this issue (overwintering phytoplankton) and so we have removed the reference to 1-D and 3-D models: "The slab model has difficulty dealing with this issue …".

Referee: 26) p. 87, line 14: Please quantify "too high".

Reply: reply to Referee point 21) above.

Referee: 27) p. 89, lines 11–13: Please cite example of some of the pioneering work by Riley, Steele and Fasham.

Reply: We now cite Anderson and Gentleman (2012) in this regard, which is a detailed analysis of Riley's methods, set in context of contemporary oceanography.

Referee: 28) p. 91, lines 12 and 14: It would be helpful to include the equation number for Beer's Law and the piecewise Beer's Law.

Reply: The text in question now refers to Eqs. 9 and 10, as requested.

Referee: 29) p.92, line 21: Please clarify the magnitude of nitrate drawdown that the following is being compared to: ": : :nitrate drawdown was slightly greater (0.5 mmol Nm-3) with the MEDUSA parameterisation."

Reply: The results have changes with our new focus on station BIOTRANS rather than station India. The new associated text is: "Results (not shown) were almost identical to the baseline simulation for station BIOTRANS (Fig. 11), with the exception that the peak of the spring phytoplankton bloom using the MEDUSA light parameterisation was only 0.7 mg chl m^{-3}, 0.2 mg m^{-3} less than that in the standard run."

Referee: 30) p. 93, lines 7–8: Please cite examples.

Reply: References added to this text: Anderson and Williams (1998), Oschlies and Schartau (2005), Salihoglu et al. (2008), Llebot et al. (2010).

Referee: 31) p. 94, lines 6–8: Please provide supporting citation(s).

Reply: Text altered to: "There is no consensus on best practice, despite the fact that different approaches to partitioning of zooplankton losses between detritus, nutrient and DOM differs markedly between models and can have a significant effect on modelled ecosystem function (Anderson et al., 2013)." Note that this citation compares different models in this regard and so there is no need to additionally cite the examples of individual models.

Referee: 32) p. 95, line 17: On what basis do you recommend the equation for shortwave irradiance?

Reply: We recommend it because it is state-of-the-art. Given that we might be asked to explain that also, we have replaced the word "recommend" with "use" in this sentence.

Referee: 33) p. 103, line 10 and p. 104, line 7: Should it be 'ASCII' rather than 'ASC II'?

Reply: Alterations made, as indicated.

Referee: 34) Table 1: Should the legend read: "Characteristics of published slab models?"

Reply: We believe the previous text was sufficient, but have nevertheless altered it to the above.

Referee: 35) Table 2: Referring the reader to Eq. 10 would be helpful.

Reply: The caption to this table now reads: "Table 2. Coefficients for use in Anderson (1993) calculation of light attenuation (Eq. 10)"

Referee: 36) Figures 2 and 4: Would it be possible to combine these figures and give a more detailed description in the legend?

Reply: We suggest that it is inadvisable to combine these Figures. Figure 2 is specifically about the layer structure of slab models and is based on Steele (1974). It sits in the section on the introduction to slab models (section 2). The focus is not on specifics of the ecosystem but, rather, the physics. In contrast, Fig. 4 is specific to the description of our NPZD ecosystem model and so is presented in section 3.2 (Ecosystem model description).

Referee: 37) Figure 3: Use of 'BIOTRANS' and 'NABE' is inconsistent.

Reply: Figure corrected.

Referee: 38) Figure 6: Units are not given on the colour bars.

Reply: The contoured properties are I_P, I_D and I_{tot}, as identified above each panel, and with units identified (d^{-1}). There is no need to repeat the units on the colour bars.

Referee: 39) Figure 7: Is there an over arching 'main' module or subroutine that contains the sections of code shown in this flow diagram? There is also repetition in the 'Functions' section – is this intended?

Reply: We are not sure what the reviewer is asking here. There is a section of core code, identified as such ("permanent code"). A peculiar aspect of R is that the functions are listed in the code prior to the core code. We see no need to alter Figure 7.

Referee: 40) Figures 8 and 9: Could these be combined in the same way as for stations Papa and KERFIX (Figures 12 and 13)?

Reply: In principle, Figures 8 and 9 (now renumbered as Figs 10 and 11) could be combined but we believe their impact is more effective if they are left separate. Fig. 8 is first introduced on p. 81 line 6 and there is then a large amount of description of the model calibration until Fig. 9 is introduced (p. 82, line 22). Amalgamating the two Figures would potentially confuse the reader by presenting the reader with the fitted model prior to the description of the calibration process.

Referee: 41) Figures 11, 12 and 13: Data shown are for 2008 or 2009 – the choice of these years (rather than 2006) is not explained in the text.

Reply: See reply to overarching Issues (2). We now use an objective means of selecting years.

Referee: 42) All figures displaying observational data do not cite its source.

Reply: The source of the observational data is stated in the text: "The model is compared to seasonal data for chlorophyll and nitrate within the mixed layer, for each station. Nitrate data are climatological, from World Ocean Atlas 2009 (Garcia et al., 2010), as is the model forcing in terms of mixed layer dephs and irradiance. Regarding chlorophyll, data are SeaWiFS 8-day averages (O'Reilly et al., 1998), for which we had access to years 1998 to 2013."

Referee: 43) Figure 17: 'A93' and 'EP85' are not defined.

Reply: A93 has now been expanded to Anderson (1993). EP85 is no longer relevant to this Figure.

III. Referee #2

General comments:

Referee: This manuscripts explains the technical details of a simple NPZD model that runs in a two-layer vertical setup. The authors claim that using simple ecosystem models such the one described in the manuscript are still useful because one can run them very fast and then be able of evaluating how changes in equation formulation or parametrization affect the ecosystem dynamics. I can see the point of this argument and I somehow agree with it, although with some reservations. Personally I think that the dichotomy over "simple vs. complex" models is overstated and should not be a matter of too much debate: in my view models are (or should be) "question-dependent" – simple models are okay to answers some questions such as biogeochemical cycles while more complex models are required to answer more ecologically relevant questions such as the effect of biodiversity on ecosystem functioning.

Reply: See Overarching Issues (1).This ms is not about model complexity or arguing in favour of simple NPZD-type models. It is about providing a plankton modelling testbed with simple physics, which can be used to test ecosystem models, simple and complex alike. We chose to use a simple NPZD model because of ease of presentation and transparency of results.

We wholeheartedly agree with the referee's comment that the dichotomy over simple vs complex models is overstated and should be question-dependent. The following text has been added to the end of the paragraph in the Discussion about model complexity: "...The whole issue of model complexity ought in any case to be question dependent, e.g. simple models may be useful to address questions on biogeochemical cycles whereas more complex models may be necessary to answer more ecologically relevant questions such as the effect of biodiversity on ecosystem function. The use of the EMPOWER testbed allows the user to investigate and determine whether a particular ecosystem model is sufficiently complex, or indeed too complex, to address the question of interest."

Referee: For those interested on community- or ecosystem-level properties (total phytoplankton or zooplankton dynamics, carbon or nitrogen cycle, etc.) using NPZD is good enough and probably better than using models that resolve phytoplankton or zooplankton diversity. NPZD have been around at least 25 years (Fasham 1990) and have proven useful to understand many aspects of ecosystem dynamics. Having said that I am not sure that simply coding another NPZD model deserves a publication in a journal such as GMD because I can't really see how this is going to move the field forward. Besides that, I find the article quite technical and therefore slightly boring. I did not find any relevant error or mistake in this work, but neither any major advance or originality. The manuscript can be seen as a very well written technical report. I leave the editor with the decision about if this work is within the scope of the GMD journal.

Reply: To reiterate, EMPOWER is an ecosystem model testbed, not an NPZD model: see Overarching Issue (1). The rationale of GMD is that it provides for complete and comprehensive model description (quoting from the journal website): "Model description papers are comprehensive descriptions of numerical models … should be detailed, complete, rigorous, and accessible to a wide community of geoscientists. In addition to complete models, this type of paper may also describe model components and modules, *as well as frameworks and utility tools used to build practical* *modelling systems*, such as coupling frameworks or other software toolboxes with a geoscientific application" (our emphasis).

As noted previously, our ms is not particularly concerned with the specific NPZD model used, but instead uses this as a straightforward "default" with which to illustrate its actual focus: the EMPOWER testbed. That said, through investigating the sensitivity of the modelled plankton system to key processes and parameterisations, such as light attenuation and mortality, the ms does add significant scientific content. For instance, the finding that model results are very similar whether using simple (MEDUSA's two waveband submodel) or complex (Anderson, 1993; based on the full spectral model of Morel) light schemes is of wider value to plankton modelling. Furthermore, a key point of the ms is the demonstration the utility of EMPOWER in making these kind of comparisons and to thereby encourage, and provide a tool, for modellers to do so.

Minor comments:

Referee: 1) Page 54: The abstract should say at some point that the model is a simple NPZD configuration. It's not clear now until one starts reading the main text.

Reply: We agree and the relevant sentence in the abstract has been amended to: "In order to demonstrate the utility of EMPOWER-1.0, we implemented a simple nutrient-phytoplankton-zooplankton-detritus (NPZD) ecosystem model and carried out …".

Referee: 2) Page 55 - Line 11: The code is "transparent" – What the authors mean by this? The simplicity of the code? No code is transparent and its simplicity is subjective anyways.

Reply: (note that the text in question is page 54, line 11) Our use of transparent is consistent with the definition in the Concise Oxford Dictionary: "evident, obvious, easily understood". Our code is neat and tidy and well structured in terms of layout and readability. We disagree with the reviewer that no code is transparent (i.e. "easily understood"). For sure, there are many opaque codes out there, but not ours. We see no reason to alter the manuscript text with respect to use of the word "transparent".

Referee: 3) Page 56 - Line 05: "They require expertise and time to set up". I don't find much difference between 0D and 1D models in terms of difficulty (3D are another story).

Reply: Simple slab ecosystem models can be set up and run with minimum expertise in a matter of hours. I (TRA) use them for teaching (my course is "Ecological Modelling") and students, with no experience at all, get to grips with them quickly. With due respect to the referee, the same cannot be said for 1-D models which require much greater expertise to set up, run and analyse. Of course, 3-D models are another big step, as indicated by the referee. Maybe in future someone can present a 1-D modelling testbed for publication and (crucially) for download in in GMD, and we encourage this. 1-D testbeds have, for example, been used successfully by Marjorie Friedrichs (e.g. Friedrichs et al., 2006: Deep-Sea Res. II 53, 576-600; J. Geophys. Res. 112, C08001) but these have not been presented in GMD nor made available for generic use by the scientific community via free download.

Referee: 4) Page 56 - Line 28: "Of course" – I think this statement is unnecessary.

Reply: "Of course" removed from the text.

Referee: 5) Page 57 - Line 03: "we submit" – I think this statement is unnecessary.

Reply: We think it is appropriate to keep "we submit" in the text. The fact that the great pioneers experimented extensively with their models is not an obvious point of fact and by using the words "we submit" we are making a case with the reader that s/he should be made aware of this rather important, yet rarely acknowledged, aspect of scientific progress.

Referee: 6) Page 66 - Line 05: "density" – Do you mean plankton concentration? The density of water?

Reply: (note that the text in question is page 65, line 5) We meant phytoplankton concentration and have inserted "phytoplankton" before the word "density" in the sentence in question to avoid ambiguity.

Referee: 7) Page 68 - Line 15: "kpar = f(bj,Cj)" – is not this parametrization too complex for such a simple model?

Reply: (note that the text in question is page 67, line 15) No, this parameterisation is not too simple. All models, be they simple NPZD or complex, benefit from accurate parameterisation of the submarine light field. Use of the Anderson (1993) piecewise Beer's law (Eq. 10) gives rise to major improvement in the predicted light field with depth and concomitant predictions for photosynthesis and ecosystem dynamics (Fig. 16). For scientific use, we therefore strongly recommend the use of Eq. 10 (the piecewise Beer's law) rather than Eq. 9 (simple Beer's Law).

Referee: 8) Page 70 - Line 10: "Eqs(11) (12)" – I might be missing something but these equations appear to me as exactly the original Fasham parametrization.

Reply: Eqs. 11 and 12 are not the same as those used by Fasham because the prey preferences are treated differently. FDM used a relative scaling for prey preferences (FDM's eqns A1 and A2), such that preference for a particular prey item is equal to the relative proportion that prey type contributes to the overall perceived food. This is in contrast to our preference for a particular prey item, which is equal to a scaling of the density of that prey. This seemingly subtle difference is what causes our grazing to be classified as passive switching vs. FDM's active switching. Clarification is provided in Gentleman et al. (2003), as cited in the text. Additionally, we specifically relate our equations to Holling Type 3, which is familiar to most people.

Referee: 9) Page 78 - Line 09: "The NPZD model we have presented is a new one" – I honestly do not think that this NPZD can be called "new" at all. The code is new, the model is not.

Reply: The equations used for processes such as light attenuation in the water column, photosynthesis, grazing and mortality have, on a case by case basis, been used in previously published models and in this sense there is nothing new. As a unified whole, however, the model is most certainly new, incorporating what we consider to be the latest state-of-the-art representations of the processes in question. If the model were already "on the shelf", as implied by the reviewer, we would be able to cite it and give minimal description. But this is not the case. Given the apparent antagonism to the word "new", we have amended the sentence in question to: "The ecosystem model we have presented uses the NPZD structure in combination with up-to-date formulations for key processes such as photosynthesis, grazing and mortality. As such, it has not been previously published and so there is no readily available complete set of parameter values to draw upon."

Referee: 10) Page 80 - Line 29: "Averaging data across years ... to compare the model to data" – I do not agree with this. If the model is using climatological forcing, the data should be climatological as well. Just show average monthly outputs for the model to smooth out the bloom as well as it happens with the data. Or otherwise run the model using the MLD forcing from 1998 to 2013 and then average the model outputs to construct a climatology. The data are not measured daily anyways; usually sampling is once or twice per month.

Reply: See reply to Overarching Comment (2).

Referee: 11) Page 81 - Line 04: "in this case 2006" – Why 2006 and not any other year? This is an arbitrary choice. One can then select the year or years that best fit the model output. I don't think this is a robust comparison

Reply: See reply to Overarching Comment (2).

Referee: 12) Page 81 - Line 24: "varied +/- 10%" – Why such a small change? Sensitivity analysis usually perform +/- 30% or 50% change in parameter values.

Reply: The use of normalised sensitivity analysis (Eq. 16) means that sensitivity is quantified as the change in a chosen variable relative to the change in the parameter. E.g., if changing a parameter by 10% causes a 10% change in the variable of interest, the S(p) score is 1.0. So the absolute change in the parameter is not so important and, indeed, this metric is usually best applied using relatively small changes in the parameter (minimising non-linear effects). For another example of the use of the S(p) metric see Anderson (1994: J. Exp. Mar. Biol. Ecol. 184, 183-199) and in that instance parameters were also varied +/- 10%.

Referee: 13) Page 83 - Line 02: "There is also a hint that ... 2006, this not particularly surprising" – This is not a valid argument (see my previous comments about climatologies)

Reply: This text has been removed.

Referee: 14) Page 83 - Line 12: Figure 11 uses year "2008" – Why the authors now select 2008 and not 2006? These choices look too arbitrary to me.

Reply: See reply to Overarching Comment (2). We now select years on an objective basis.

Referee: 15) Page 83 - Line 18: "grazer controlled phytoplankton in iron limited ecosystems" – The current consensus is that phytoplankton in HNLC is more controlled by iron limitation than by grazers and I personally agree with it.

Reply: There is certainly general agreement that iron limits phytoplankton growth rate but that does not mean the system (e.g. phytoplankton biomass) is controlled by iron to the exclusion of other factors. We are of the belief that the statement in quotes above remains entirely valid as a hypothesis today, as stated by Price et al. (1994). The situation is summarised well by Mongin et al. (2006: Deep Sea Res II 53, 601-619): "Results suggest that primary production in HNLC systems is controlled by some combination of the light/mixing regime, grazing pressure and Fe limitation, as evidenced most clearly in the equatorial Pacific (e.g., Coale et al., 1996; Landry et al., 1997) and Southern Ocean (e.g., Boyd et al., 2000; Price et al., 1994)." More recently, Kidston et al. (2013; as cited in ms) wrote: "Although results [of iron enrichment studies] support the importance of iron in regulating primary productivity, they do not imply that iron is the ultimate control (Fennel et al., 2003). Recent studies show that the factors controlling phytoplankton biomass in the Southern Ocean are still open to debate. … Banse (1996) studied the effects of underwater irradiance, iron and grazing on SAZ chlorophyll and found that zooplankton grazing was controlling the phytoplankton populations."

Referee: 16) Page 83 - Line 25: "Vmax acting as a proxy for iron limitation" – This is way to crude. If the model does not resolve iron cycle it should not be compared against HNLC regions.

Reply: The art of modelling does not necessarily require the explicit representation of every aspect of the real system and it is entirely reasonable to vary appropriate parameters in the model to act as proxy for iron limitation. In similar fashion to our study, previous NPZD models of HNLC systems have not explicitly modelled iron as a separate state variable, e.g. the pioneering work of Frost (1987) through to recent work by Kidston et al. (2013).

We accept, however, that our previous justification of the parameters we changes was inadequate. We now cite a key reference (Alderkamp et al., 2012: J. Phycol. 8, 45-59), decreasing both $V_P^{\max}(0)$ and α at the two HNLC stations: " Low growth rate of phytoplankton may be expected relative to the North Atlantic because of iron limitation. Parameters $V_P^{\max}(0)$ and α may typically decrease by 50% relative to iron-replete conditions (Alderkamp et al., 2012). For stations Papa and KERFIX, we therefore assigned $V_P^{\max}(0)$ = 1.25 g C (g Chl)$^{-1}$ h^{-1} and α = 0.075 g C (g Chl)$^{-1}$ h^{-1} (W m^{-2})$^{-1}$ [half the iron-replete values used for the North Atlantic stations]."

Referee: 17) Page 84 - Line 20: "It is perhaps unsurprising ... curves are similar" – Why then bother doing a sensitivity analysis?

Reply: The operative word is "perhaps" because models often do produce surprises. It is only by doing sensitivity analysis that one finds out, for sure, what models are sensitive to and what they are not, and where therefore to focus effort in parameterisation.

Referee: 18) Page 86 - Line 09: "The sensitivity shown here is at least as great as that for the choice of P - I curve" – Which you say was quite low right?

Reply: (note that the text in question is page 85, line 9) Correct, but that is not the point. The point is that there has been lots of work on P-I curves and the selection thereof for models. Yet other aspects of the photosynthesis calculation, such as whether to assume a triangular or sinusoidal pattern of irradiance over the day, have received little attention despite the fact that model results are at least as sensitive.

Referee: 19) Page 87 - Line 12: "Many models do not include a non-linear phytoplankton mortality" – Using a squared mortality term amounts to imposing a carrying capacity.

Reply: We are not sure what the referee is asking here. Yes, using a quadratic mortality term effectively imposes carrying capacity. But this does not alter the fact that many marine ecosystem models do not include a non-linear phytoplankton mortality term.

Appendix: Figures (many of which have changed due to the results being redone for station BIOTRANS).

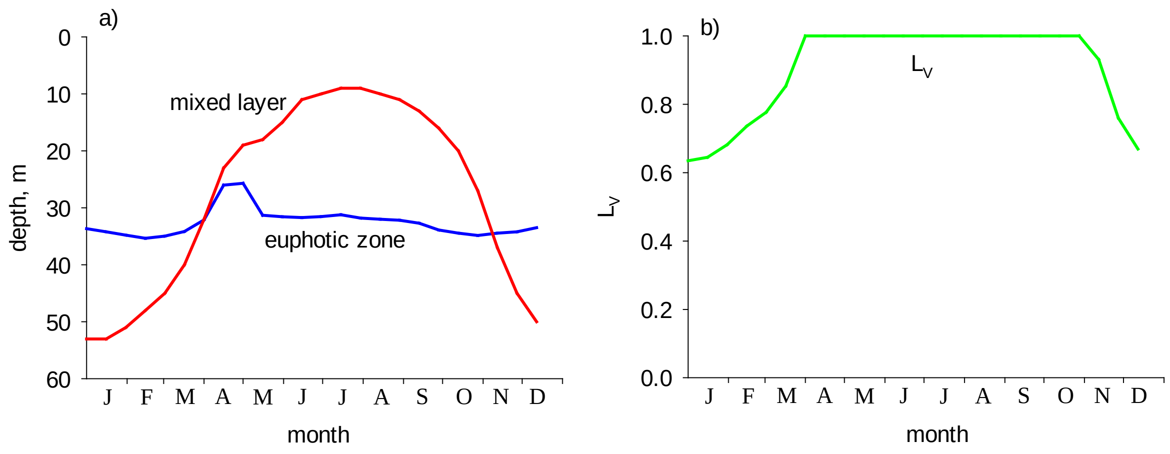


Figure 1 Forcing used by Riley (1946) in his model of George's Bank: a) Depths of euphotic zone and mixed layer; b) Diminution in photosynthesis due to light limitation (L_V).

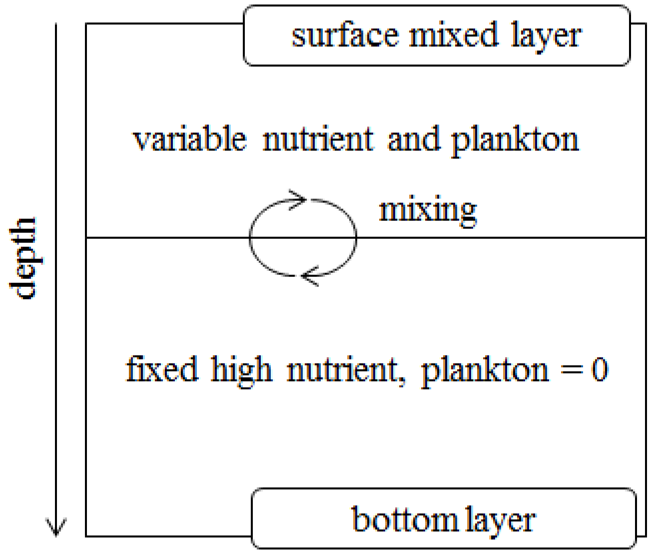


Figure 2. Two layer slab physics framework (adapted from Steele, 1974).

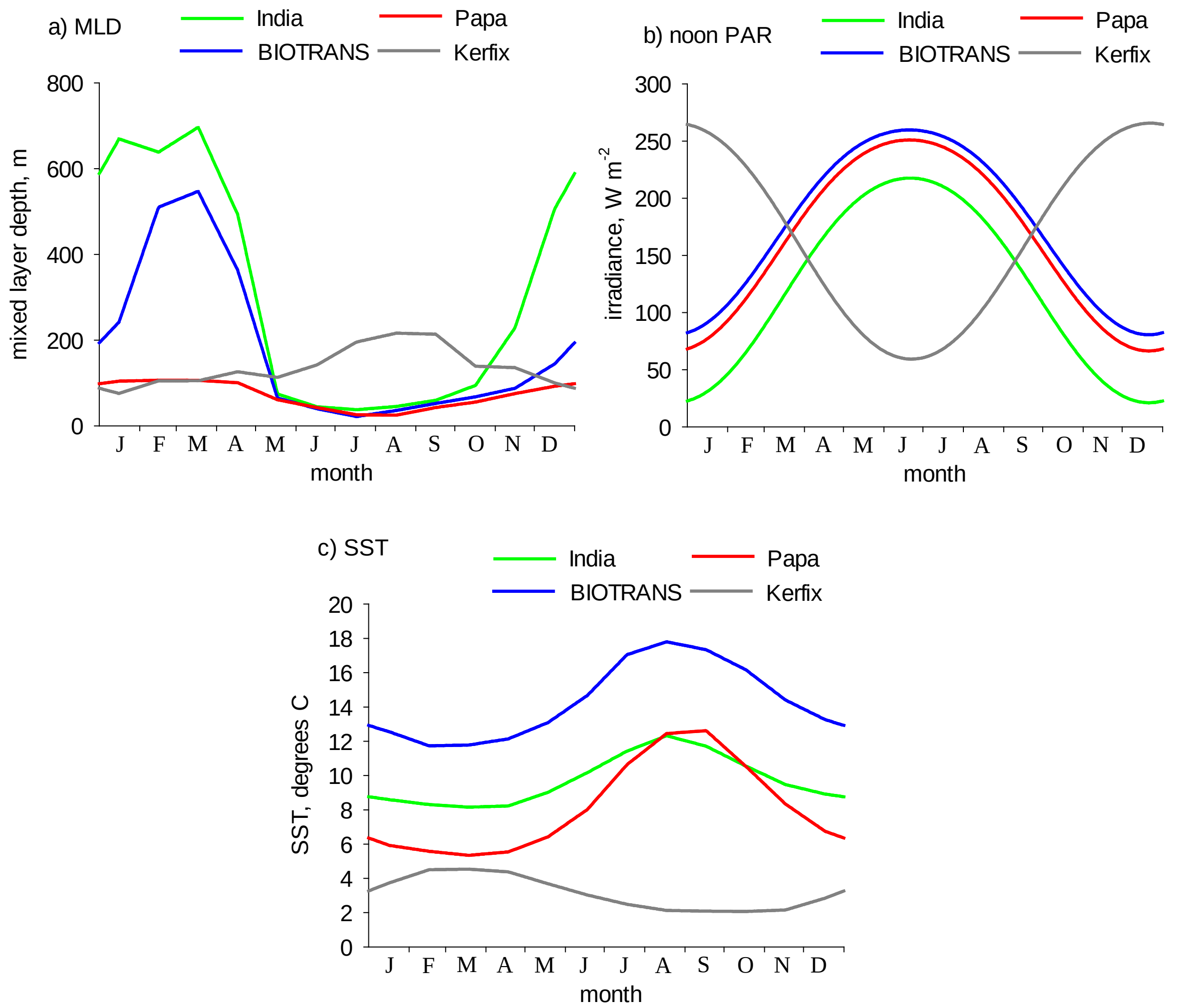


Figure 3. Model forcing for stations India (60ºN 20ºW), BIOTRANS (47ºN 20ºW), Papa (50ºN 145ºW) and Kerfix (50º 40'S 68º 25'E): a) mixed layer depth (m), b) noon irradiance (W m^{-2}), c) sea surface temperature (ºC).

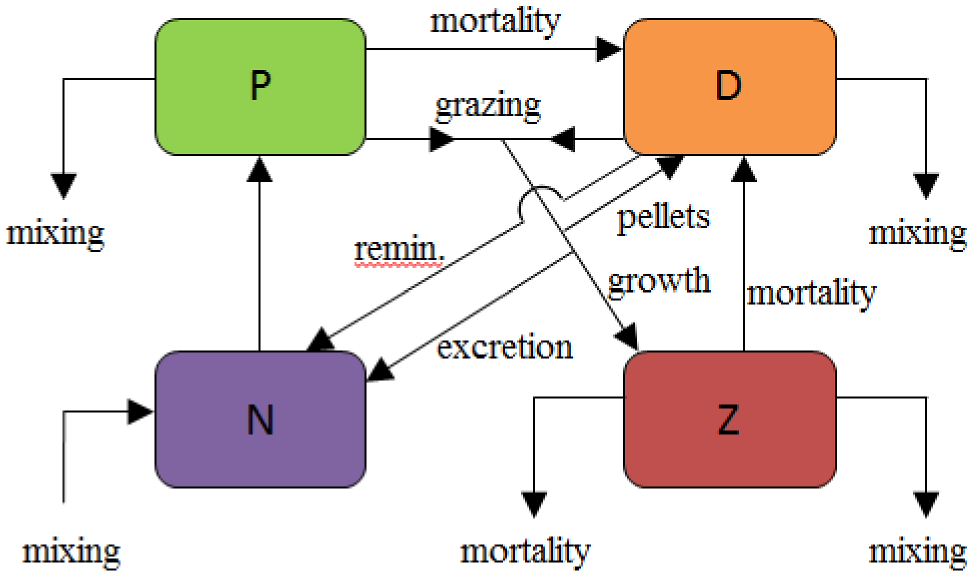


Figure 4. Structure of the NPZD model.

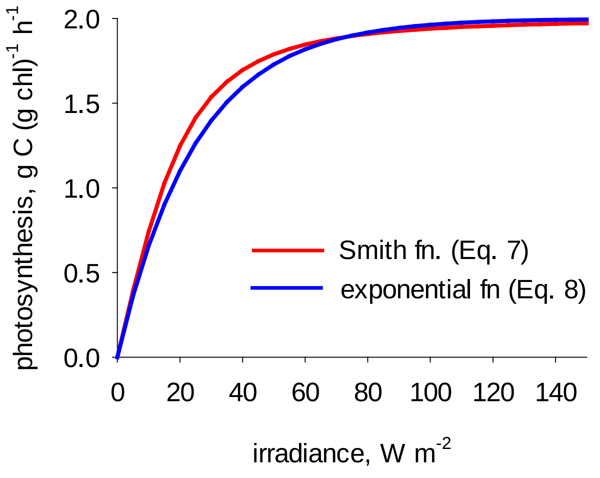


Figure 5. Photosynthesis-irradiance curves with parameter settings: V_P^{max} = 2.0 g C (g chl)$^{-1}$ h^{-1} and α = 0.08 g C (g chl)$^{-1}$ h^{-1} (W m^{-2})$^{-1}$.

Figure 6. Figure 6. Triangular versus sinusoidal patterns of diel irradiance (example day length of 12 h).

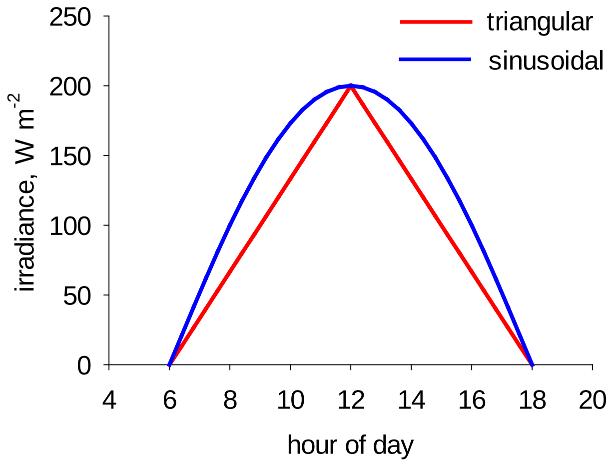


Figure 7. Contours of the zooplankton specific ingestion rates (I_P, I_D) versus densities of the two prey types (P = phytoplankton and D = detritus) as characterised by the sigmoidal grazing response (Eqs. 11, 12) using parameters I_{max} = 1 d^{-1}, k_Z = 0.52 mmol N m^{-3}, ϕ_P = 0.67 and ϕ_D = 0.33. Upper two panels illustrate assumed interference effect of one prey type over another, e.g. for a given P, increasing D reduces I_P. The lower panel illustrates assumed optimal feeding (i.e. total ingestion, I_{tot}, always increases with increase in P or D) and the benefit of generalism (i.e. increase in I_{tot} due to consumption of P and D vs. just P).

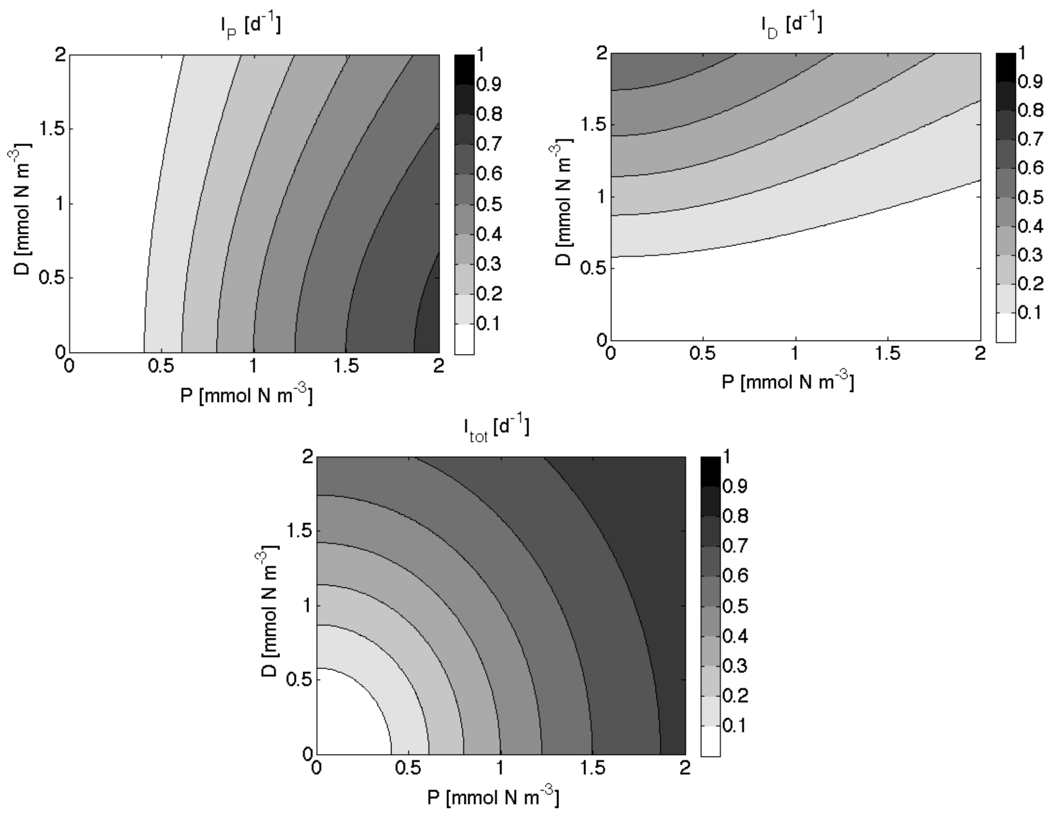
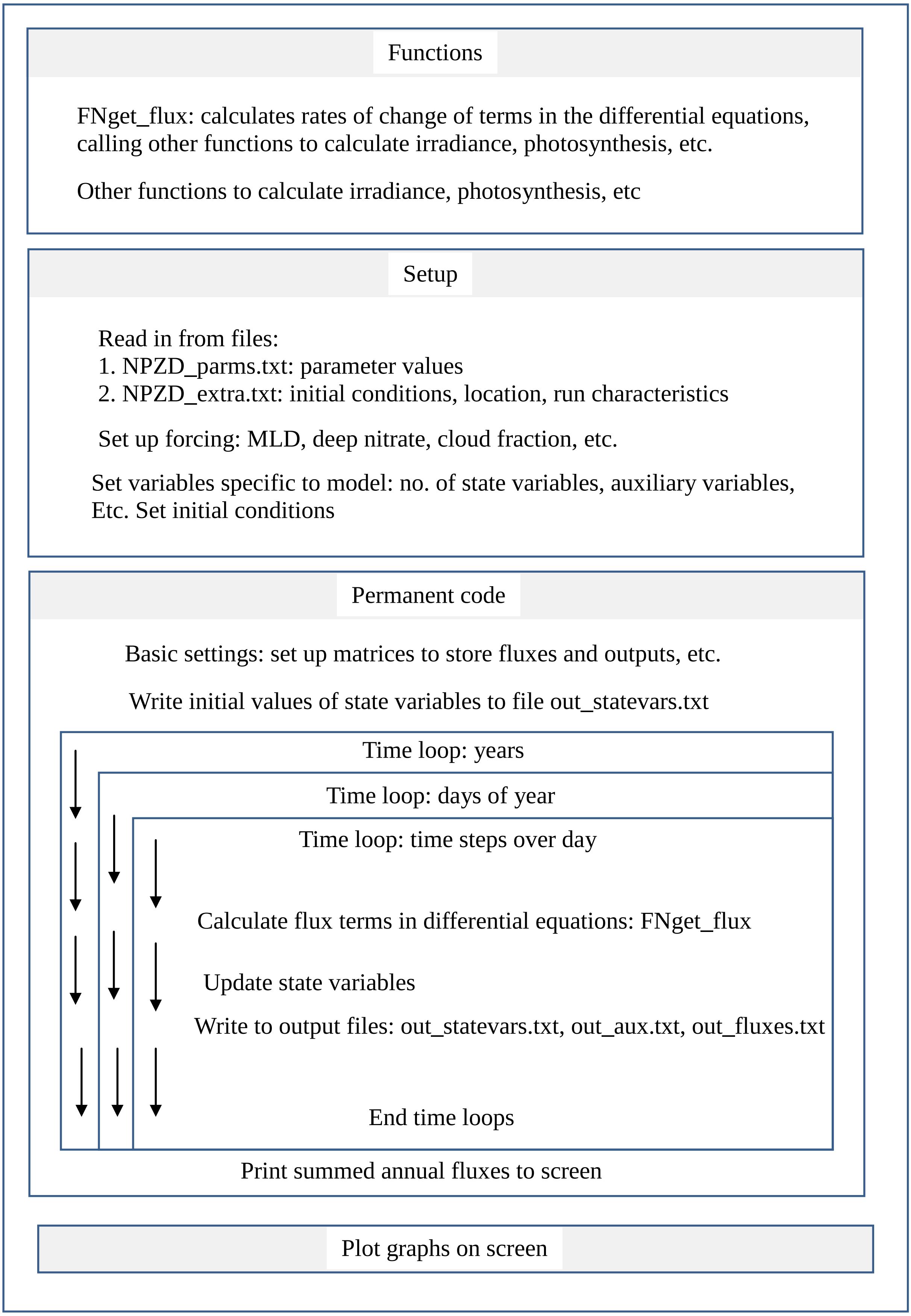


Figure 8. Structure of the model code



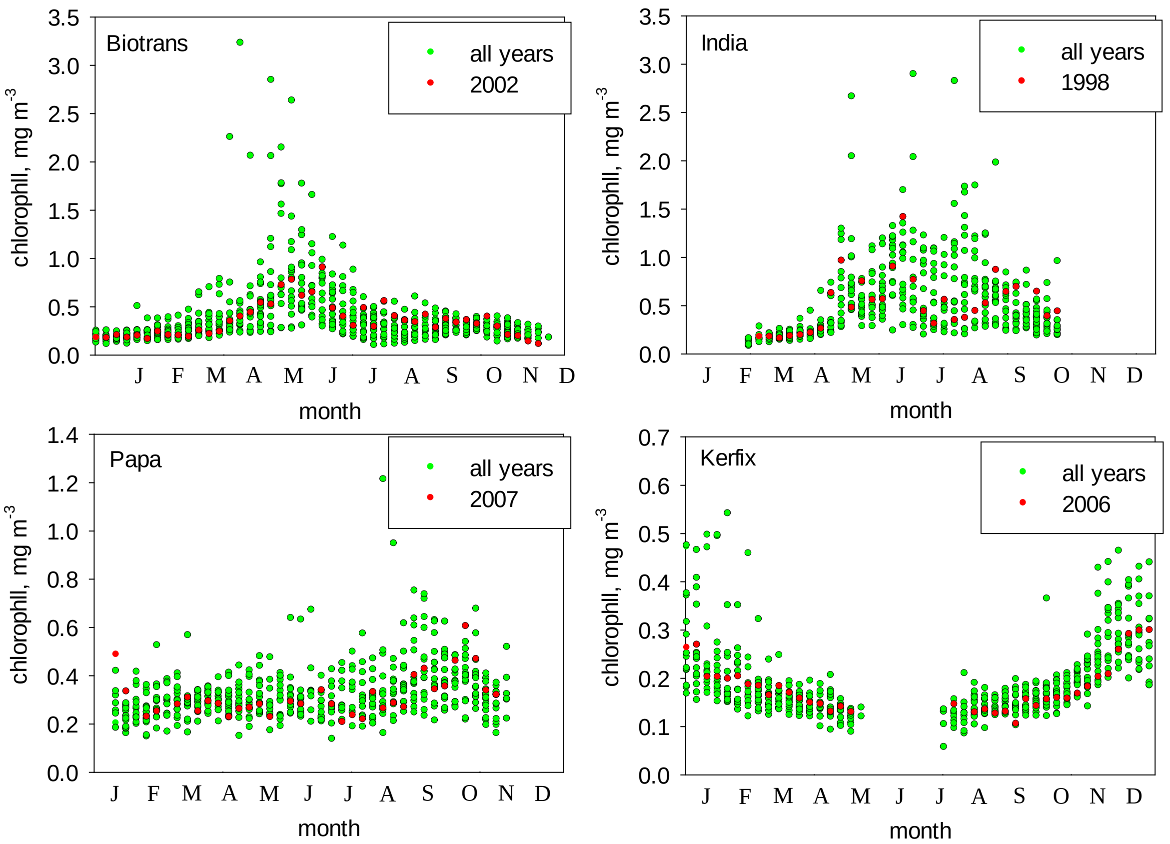


Figure 9. SeaWiFS chlorophyll data for each of the four stations, years 1998 to 2013 overlaid, with selected median year (see text) highlighted.

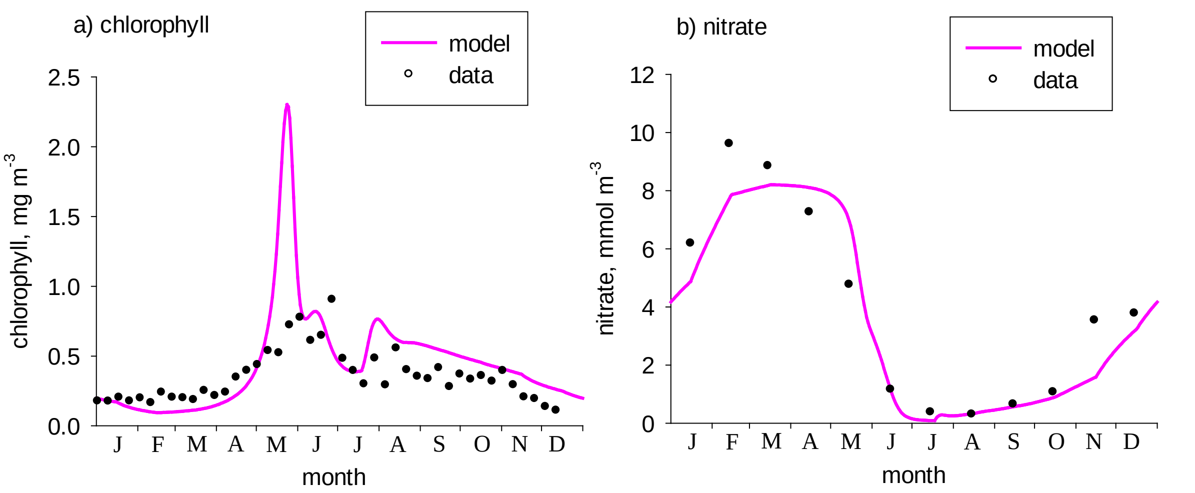


Figure 10. Simulation for station Biotrans using first-guess parameters compared to data (year 2002) for a) chlorophyll and b) nitrate.

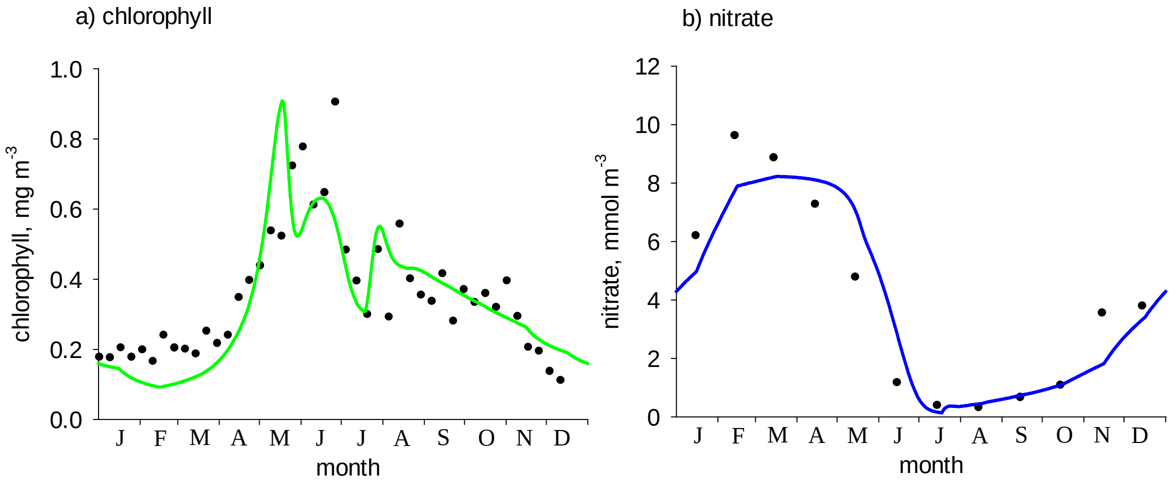


Figure 11. Simulation for station Biotrans after parameter tuning (see text): a) chlorophyll, b) nitrate.

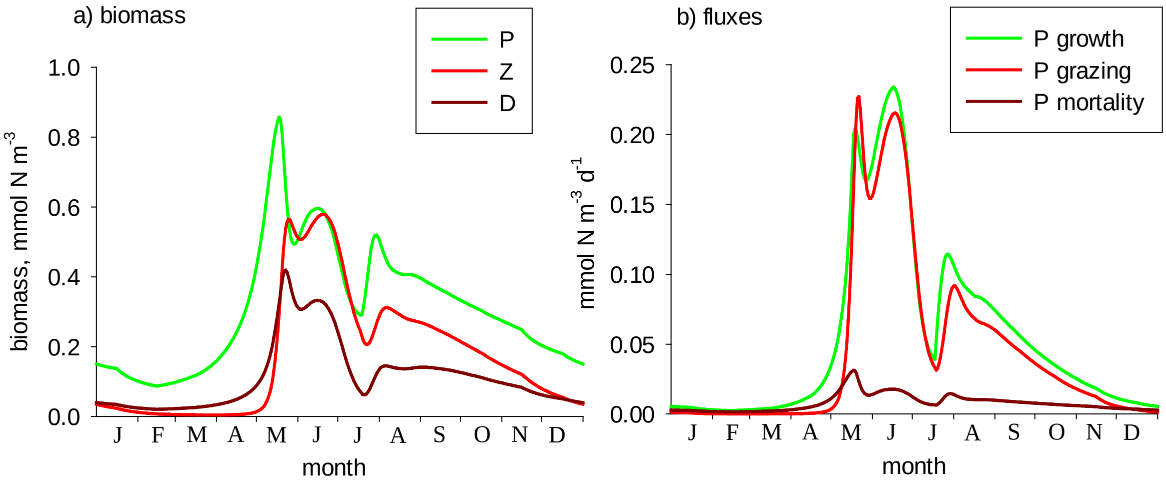


Figure 12. Predicted state variables and fluxes for the station Biotrans simulation: a) P, Z and D and b) phytoplankton growth, grazing and non-grazing mortality.

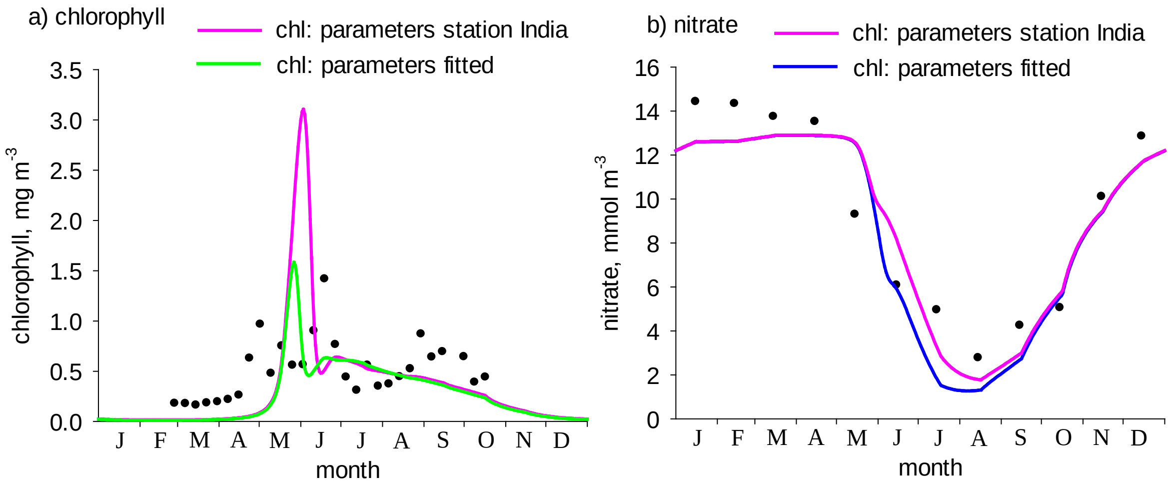


Figure 13. Simulation for station India: a) chlorophyll, b) nitrate. Data are for year 1998.

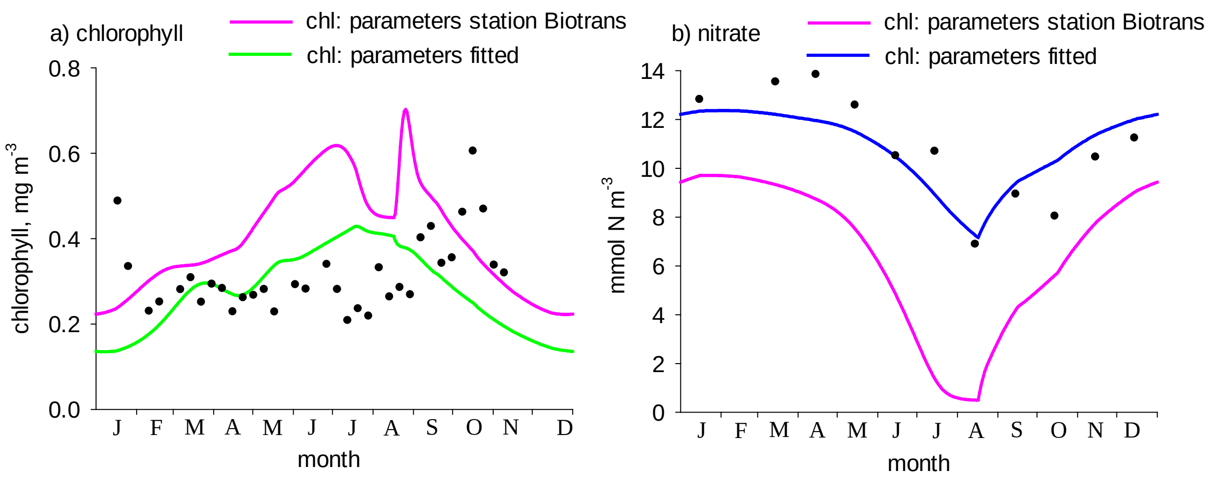


Figure 14. Simulations for station Papa before and after parameter tuning: a) chlorophyll, b) nitrate. Data are for year 2007.

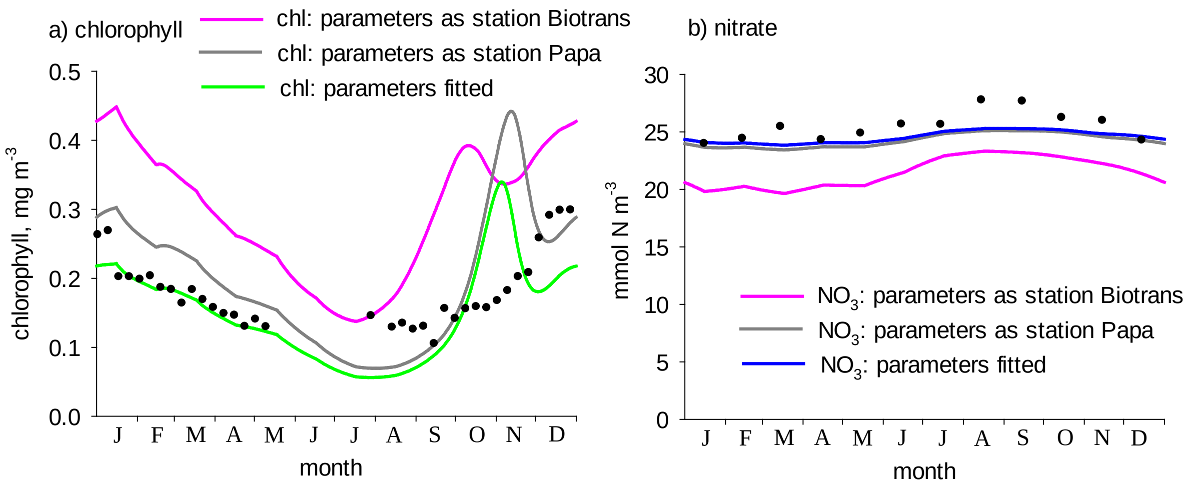


Figure 15. Simulations for station Kerfix before and after parameter tuning (see text for details): a) chlorophyll, b) nitrate. Data are for year 2006.

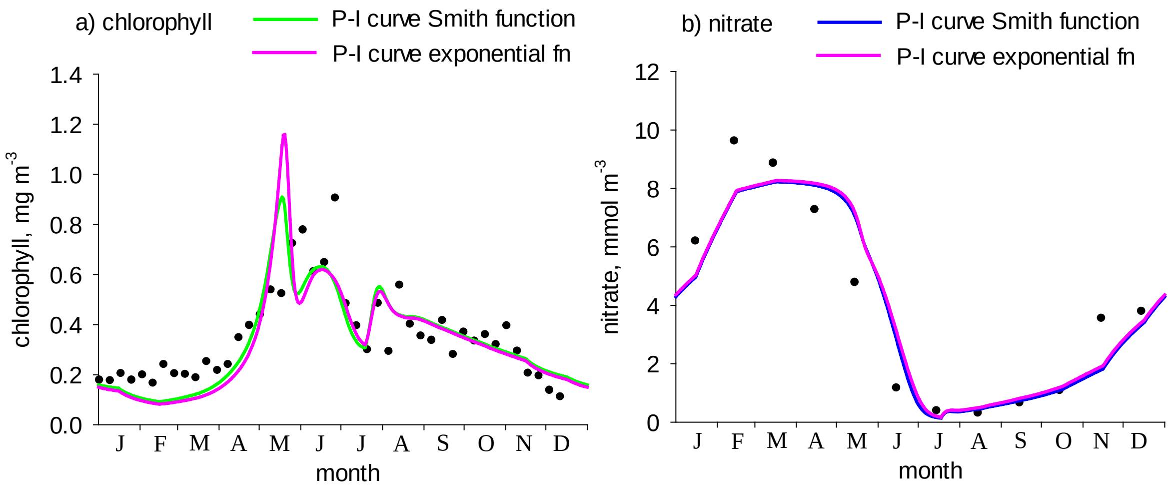


Figure 16. Simulations for station Biotrans showing sensitivity to choice of P-I curve: a) Smith function (standard run) and b) exponential function.

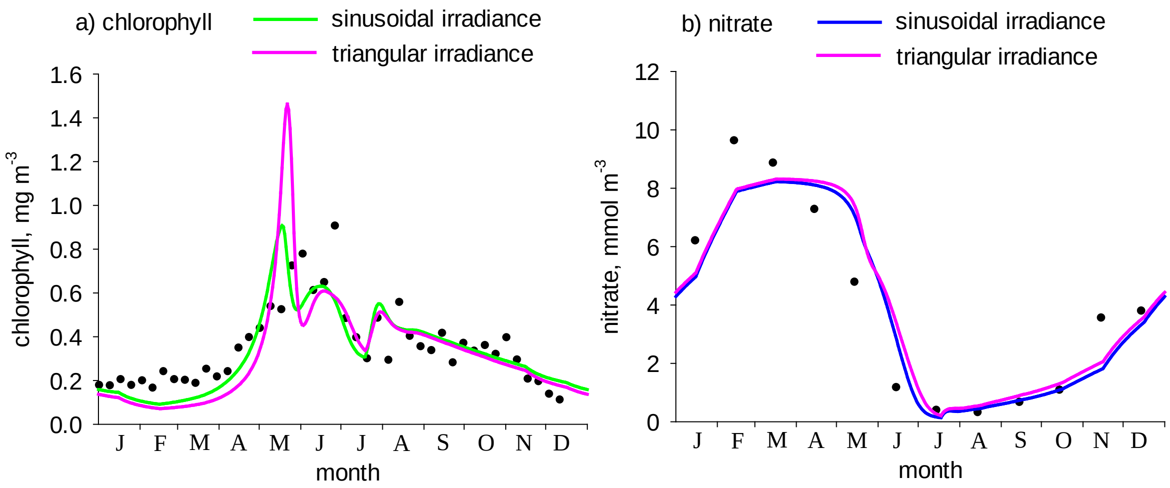
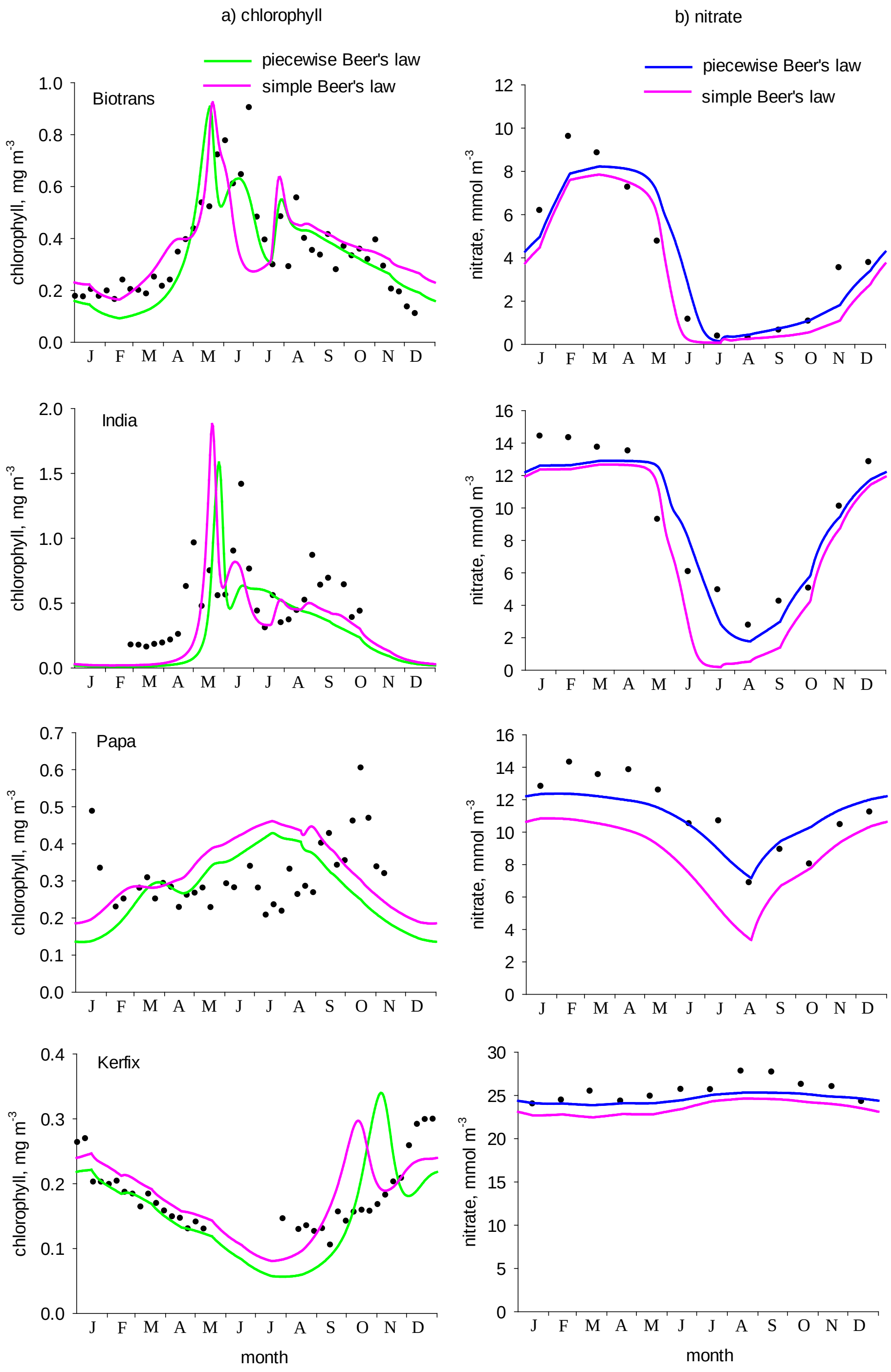


Figure 17. Simulations for station Biotrans showing sensitivity to choice of diel variation in irradiance: a) sinusoidal (standard run) and b) triangular.


a) chlorophyll
b) nitrate
piecewise Beer's law
simple Beer's law
Biotrans
India
Papa
Kerfix
chlorophyll, mg m-3
nitrate, mmol m-3
J F M A M J J A S O N D
month

1 Fig. 18

Figure 18. Model simulations for all four stations showing sensitivity to choice of method for calculating light attenuation in the water column: a) piecewise Beer's Law (Eq. 10) and b) simple Beer's law (Eq. 9).

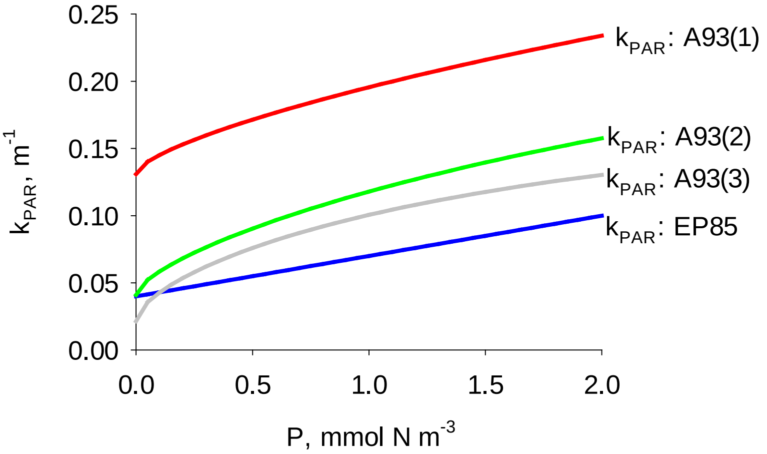


Figure 19. Light attenuation as predicted by Evans and Parslow (1985) and for the three layers (0-5, 5-23, >23m; 1,2,3 respectively) in Anderson (1993), as a function of phytoplankton concentration.

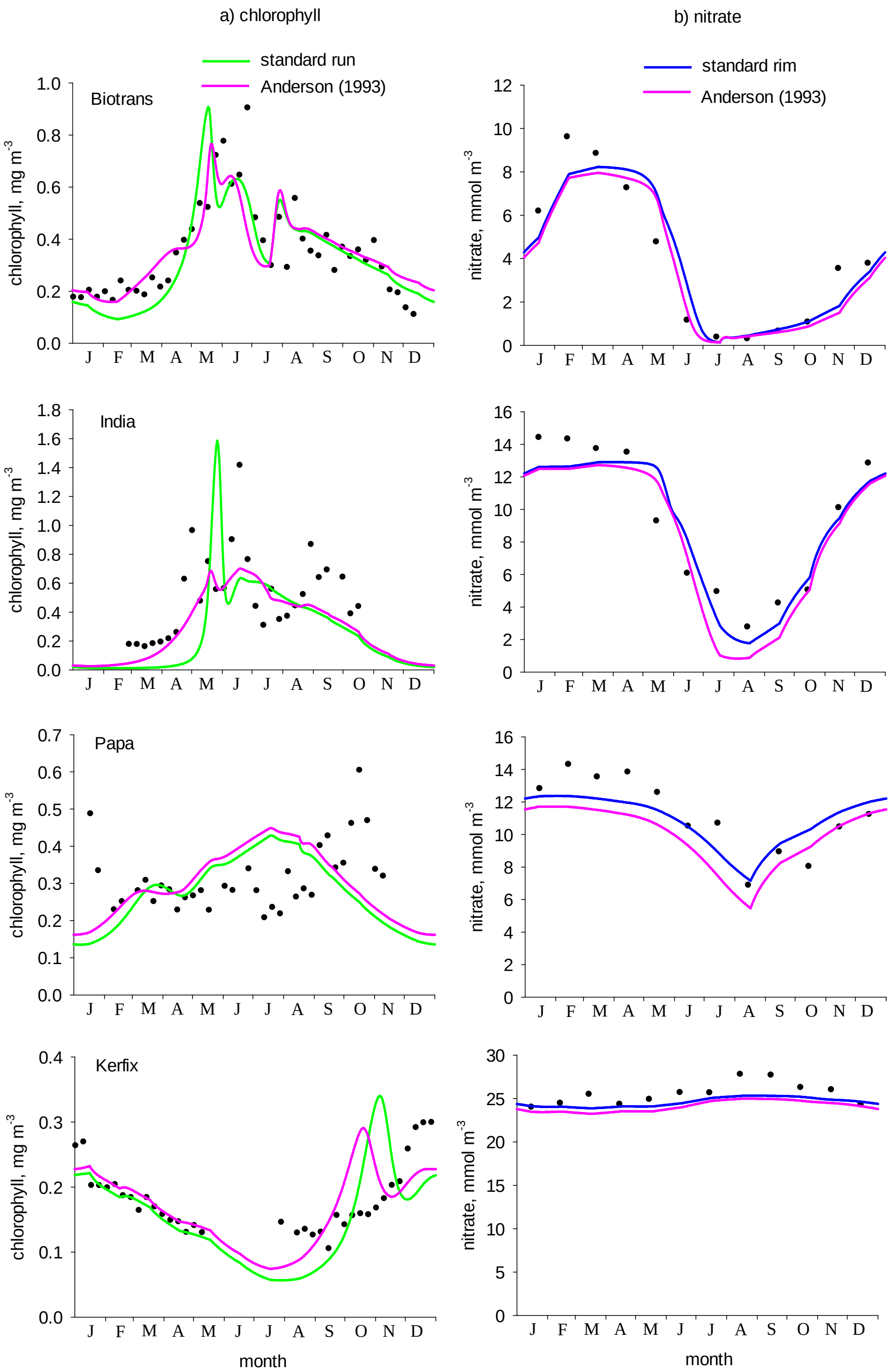
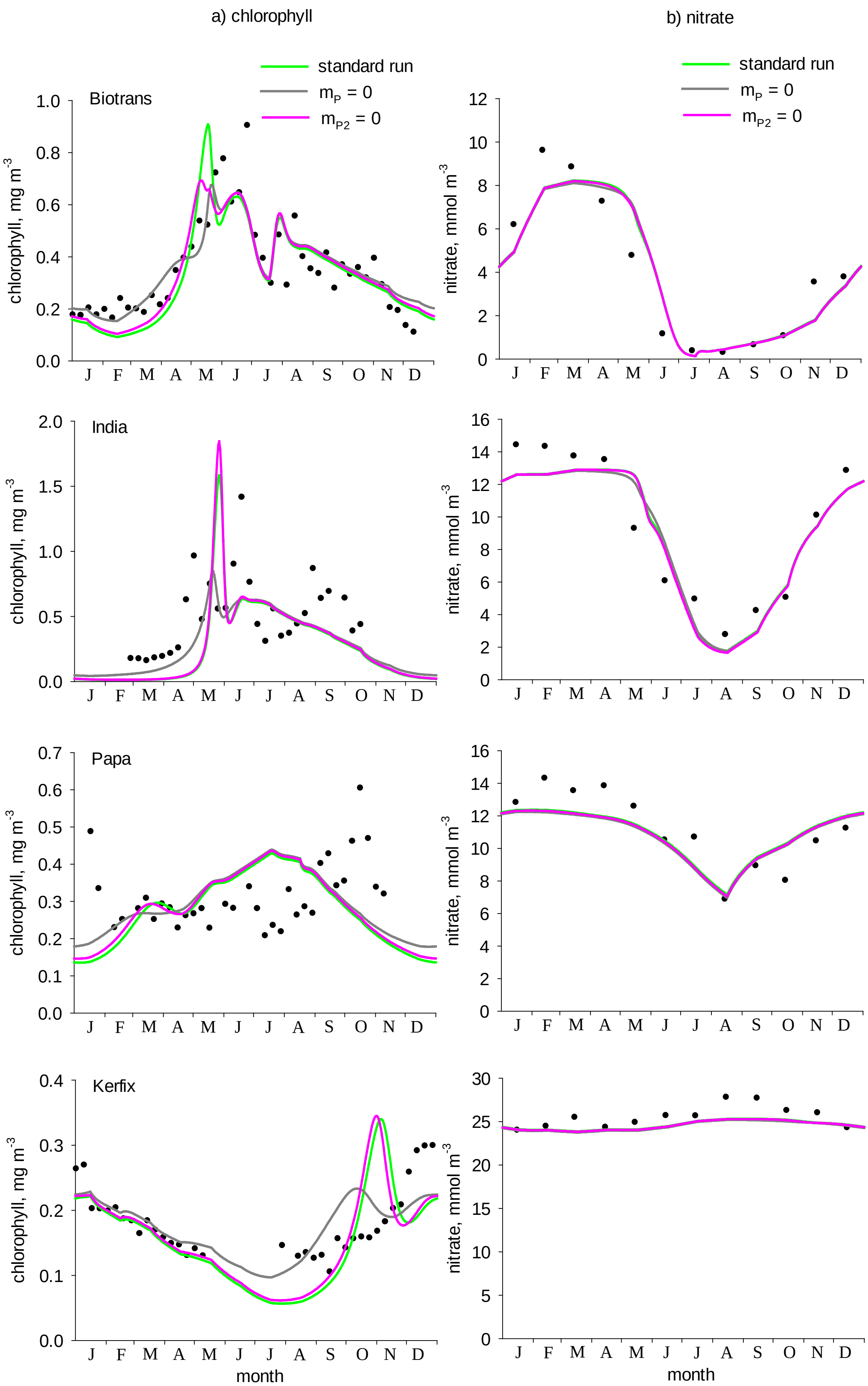

a) chlorophyll
b) nitrate
standard run
Anderson (1993)
standard rim
Anderson (1993)
Biotrans
India
Papa
Kerfix
chlorophyll, mg m-3
nitrate, mmol m-3
J F M A M J J A S O N D
month
month

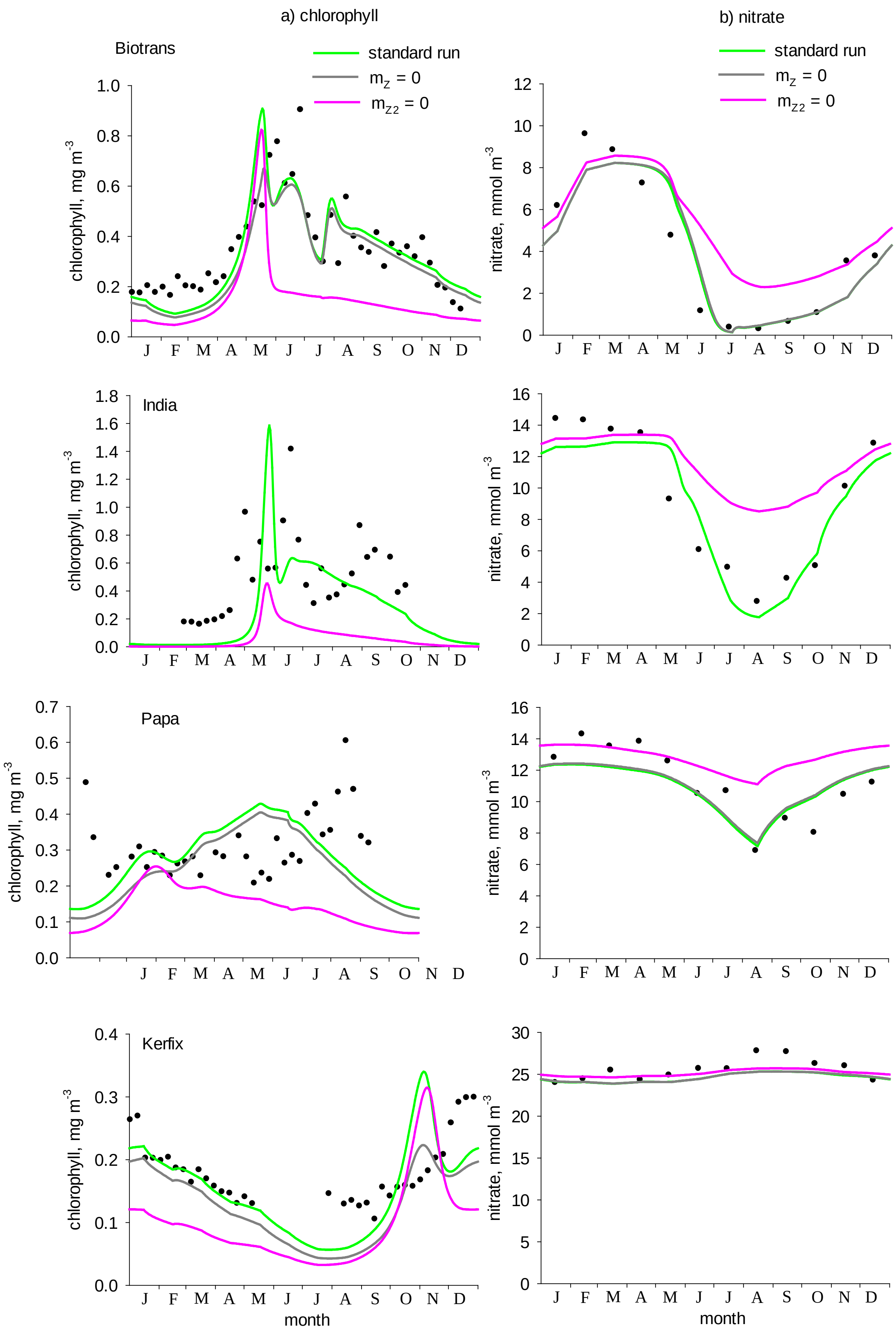
Fig. 20

Figure 20. Simulations for all four stations comparing methods for calculating daily depth-integrated photosynthesis, standard run (numeric integration) and the algorithm of Anderson (1993) which is an empirical approximation of a full spectral model: a) chlorophyll and b) nitrate.


a) chlorophyll
b) nitrate
standard run
$m_P = 0$
$m_{P2} = 0$
Biotrans
India
Papa
Kerfix
chlorophyll, mg m^{-3}
nitrate, mmol m^{-3}
J F M A M J J A S O N D
month

1 Fig. 21
2

Figure 21. Simulations all four stations showing model sensitivity to phytoplankton mortality. Parameters m_P (linear mortality) and m_{P2} (quadratic moratlity) were set to zero in turn. a) chlorophyll, b) nitrate.


a) chlorophyll
b) nitrate
Biotrans
standard run
$m_z = 0$
$m_{z2} = 0$
India
Papa
Kerfix
chlorophyll, mg m-3
nitrate, mmol m-3
J F M A M J J A S O N D
month

1 Fig. 22

Figure 22. Simulations for all four stations showing model sensitivity for zooplankton mortality. Parameters m_Z (linear mortality) and m_{Z2} (quadratic moratlity) were set to zero in turn. a) chlorophyll, b) nitrate.