***Interactive comment on* "Analytical solution of nitracline with the evolution of subsurface chlorophyll maximum in stratified water columns" *by* Xiang Gong et al.**

Anonymous Referee #2

1 General comments

The study of Xiang Gong and coauthors is concerned with the existence and characteristics of a nitracline in the presence of subsurface chlorophyll maxima (SCM). The authors derive analytical solutions that describe possible steady state results of a one-dimensional vertical model of nutrients (dissolved inorganic nitrogen) and phytoplankton biomass. Analytical steady state solutions are nicely derived for stratified conditions, with some weak mixing below a shallow upper mixed layer. A piecewise function is introduced as an approximation of the vertical distribution of phytoplankton biomass. This elegant approach was described and applied in an earlier study by X. Gong, J. Shi, H. W. Gao, and X. H. Yao, published 2015 in Biogeosciences, 12, 905-919. The authors take various different perspectives on the steady state solution. One of their main conclusions is that nitrate consumption by the phytoplankton has to be replenished by an upward flux of nitrate, which is interpreted as the major contribution to new primary production.

It is still fascinating to realise how much can be learned from analytical solutions of a model. X. Gong and his coauthors derive and explore steady state model solutions, elucidating interrelations between the characteristics of the nitracline and SCM. The stepwise derivation of particular solutions is generally good, but some readers may eventually lose track of all initial/original model assumptions. While reading about half of their study it became increasingly difficult to understand the actual meaning of the derived solutions, albeit mathematical steps were reproducible in most cases. For example, after the introduction of the depth of maximum growth (z_0), many statements are made and conclusions are drawn that may lead readers astray. The authors tend to interpret their analytical solutions to be indicative for true conditions. But the solutions only reflect steady state conditions of model results. Furthermore, the authors give the impression that their analytical solutions are straightforward and can be used to make inference about nitracline features, once z_m, h, and σ have been derived from observed profiles of chlorophyll a (Chl a) concentration. To do so would be inappropriate, which should be explicitly stated in the study. It is a conceptual problem that has to be reasonably addressed by the authors. Some major revision of the manuscript is therefore needed before the study can be recommended for publication in Biogeosciences.

Response: Many thanks for the helpful suggestions and comments. We will try best to revise and make physical meanings more obvious with those derived solutions. We will also tone down the statements and conclusions to avoid any misleading. For example, we will move the statements drawn from Equation (17) (line 361-364) to the Discussion. The challenge and uncertainty will be included when we present those implications. Please see the revision.

The analytical solutions presented are, apart from Equation (18) (see specific comments), correct. However, the author's should stress that the analytical solutions are valid only for estimates of z_m, h, and σ that are consistent with the model's numerical steady state solution. The numerical steady state solution in turn depends on the forcing, boundary conditions and on the combination of parameter values. The approximations of z_m, h, and σ are entirely conditioned by the model results and thus also depend on the combination of model parameter values. To combine the analytical steady state solutions with observed z_m, h, and σ (as derived from vertical profiles of chlorophyll a concentration) is only meaningful after model calibration (identifying a model solution that is in some agreement with the observed z_m, h, and σ). A calibration requires the numerical model to be run in the first place. In other words, the equations, e.g. for the depth of the nitracline (z_n), are valid only for z_m, h, and σ that remain dynamically consistent with the imposed model. Otherwise, the derived equations are not applicable.

Response: Agree. We will state that the analytical steady state solutions of nitracline are applicable only for estimates of z_m, h, and σ that are consistent with the model's numerical steady state solution. The challenge and uncertainty will be included when we present those implications.

Another concern is, although already addressed/discussed by the authors, the neglect of photoacclimation dynamics. The process of photoacclimation is essential for those systems (with stratified conditions) the authors focus on, and such a model approach would be better suited to make inference about the basic interrelations between a nitracline and a SCM. A possibility would be to include some additional parameterization that could yield variable γ, which can be derived from e.g. Cloern et al. (1995, L&O, 40(7), 1313-1321). When resorting to a parameterisation of Cloern et al. (e.g. their Eq. 15), some care has to be taken only with respect to the temporal integral of daily irradiance that is averaged over the upper mixed layer in their study. A certainly more realistic model would be one with equations that explicitly resolve variations of the Chl a-to-carbon and nitrogen-to-carbon ratio of the algae. An interesting aspect would be to see whether the "symmetric", piecewise Gaussian function would still be useful to approximate profiles of simulated Chl a, even if still *applicable to fit phytoplankton nitrogen biomass. The authors only discuss possible shifts in depth (location) of the SCM. They do not consider skewed profiles of Chl a, with a sharp SCM, as can be seen in many Chl a observational profiles.*

Response: In the revision, we will parameterize Chl: C using Eq. 15 of *Cloern et al.* (1995). Then let R= Chl: C, the nitrogen content of phytoplankton γ will be written as γ= 1/(6.625*12*R), corresponding to a C:N ratio of 6.625 and a carbon atomic mass of 12. The detailed results will be added in a new Section 4.2 to illustrate how and to which extent photoacclimation influence the relationships between a nitracline and a SCM. In addition, the simulated Chl a will be fitted by the piecewise Gaussian function. The limitation, i.e., the skewed profiles of Chl a with a sharp SCM was not considered, will be added in Section 4.5.

2 Specific comments

Abstract

lines 26-27: "..., we derive analytical solutions for the system of phytoplankton and nutrient."

The authors derive analytical solutions of a specified model. The model is well suited to explain basic dependencies between a nitracline and a deep chlorophyll a maximum.

Response: Agree. This sentence will be rewritten, i.e., we derive analytical solutions of a specified nutrient-phytoplankton model. The model is well suited to explain basic dependencies between a nitracline and a SCML.

lines 31-34: "The inverse proportional relationship..., suggesting that the light level at the nitracline can be used as an indicator for integrated new primary production."

It is not clear whether the model approach is appropriate to clearly distinguish between regenerated and new production. The dynamical model equations only resolve some instant remineralisation, with a direct mass flux from the phytoplankton back to the nutrient pool.

Response: Agree. We will modify this sentence and the related results in the text accordingly.

1 Introduction

The introduction is nice. It is well written and informative.

line 112: "... was used to fit vertical chlorophyll profiles."

Here the authors should clarify that the Gaussian function is used as a fit to the steady state solution of the model.

Response: Agree. We will spell out that the Gaussian function is used as a fit to the steady state solution of the model. In reality, the Gaussian function is also applicable for many profiles of Chl a in stratified waters, especially open ocean.

2 Definition and models

pages 5 - 9: The model is nicely described and sufficient details are provided. I would suggest to introduce λ not here but where it is needed (on page 18).

Response: Agree. We will move the introduction of λ to page 18.

page 9, lines 235 - 237: "We use the biologically reasonable parameter values given in Table 1 to represent the system at station SEATS..."
Thus, a specific (calibrated) model solution is considered as an example.

Response: Agree. We will consider how to incorporate this in the revision.

pages 10 - 11: **Definition of the nitracline**

The text is well written. The concept described in the final paragraph (lines 270 - 280) is clear. However, it is still confusing because simulated as well as observed profiles of N yield $\frac{d^2N}{dz^2} \approx 0$ (or $\frac{dN}{dz} \approx$ constant) over some distinct depth range, e.g. as depicted in Fig. (2).

Response: Many thanks for noticing this issue. The depth of nitracline in our study was defined as the location of maximum nitrate gradient in the euphotic zone, which can be expressed by $\frac{d^2\mathrm{N}}{dz^2} = 0$ and $\frac{d^3\mathrm{N}}{dz^3} < 0$, not implying $\frac{dN}{dz}$ = constant. The equality $\frac{d^2\mathrm{N}}{dz^2} = 0$ means $\frac{\mathrm{dN}}{dz} \approx$ constant only when $\frac{d^2\mathrm{N}}{dz^2} \equiv 0$ in the domain. Thus, to determine the depth of nitracline from simulated as well as observed profiles, we have to plot the profile of nitrate gradient and find the maximum nitrate gradient ($\frac{dN}{dz}|_{\max}$). This will be clarified in the revision.

The described balance between uptake and recycling only works for this particular kind of model approach. The authors may add "According to our model approach (Eq. 2) the depth where $\frac{d^2N}{dz^2}=0$ represents a balance between the growth rate and the phytoplankton loss rate."

Response: Agree. This will be added in the revision.

3 Results

page 12: You may add here the depth range that is considered ($z_s < z < z_b$).

Response: Agree. The depth range $z_s < z < z_b$ will be added in Eq. (8).

line 112: "... the fitted function of chlorophyll..."
Suggestion: "... the fitted, depth dependend function of chlorophyll ($\gamma P(z)$). This reminds the reader that P actually includes an exponential in Eq. (8).

Response: Agree. This sentence will be revised as "... the fitted, depth dependent function of chlorophyll (P(z))...".

page 13: The minus sign ($- K_{v2}/\sigma^4$) is confusing.

Response: We will delete the minus sigh in the revision.

*line 313: do the authors mean... "... (values from 8.64 to 7.78*10^{-9} m^{-2} s^{-1})..."?*

Response: Because K_{v2} is 1-9*10^{-9} m^2 s^{-1}, σ is from several meters to tens of meters, thus the ratio of K_{v2} to σ^4 is from 8.64*10^{-9} to 7.78 m^{-2} s^{-1}. This will be clarified in the revision.

Equation (12): for non-zero w.

Response: We will add the condition of non-zero w for Eq. (12) in the revision.

Depth of the nitracline

page 15, lines 353 - 364: This derivation only works when Blackman's law of limiting factors (light and nutrient limitation) is applied. Hence, it is a particular model assumption. The maximum rate discussed here first of all represents a net primary production term. Only in the context of this particular model version it is also interpreted as new primary production. The sentence "It follows that the light level at the nitracline is an indicator of integrated NPP in the water column." is a strong statement. This finding strongly depends on the underlying model equations. It would be good to see different steady state solutions of the model while varying values of ε and α (e.g. increasing ε while decreasing α and vice versa). This way the authors may substantiate their conclusion.

Response: We will spell out that the derivation only works when Blackman's law of limiting factors (light and nutrient limitation) is applied in the revision. We will also examine the simulated results by varying values of ε *and* α, please see the revision.

Equation (18): The inclusion of γ in the last term is incorrect. The parameter γ can be removed. This is because K_c is normalised to nitrogen biomass and not to Chl a.

Response: Sorry for the typo, we have removed "γ".

page 16, lines 377 - 380: "Equation (18) also indicates that both a higher recycling rate (α) of dead phytoplankton and a larger loss rate (ε) lead to a shallower nitracline, while the enhanced maximum growth rate of the phytoplankton (μ_m) moves

the nitracline depth down."
It would be good to see this conclusion consolidated by some model results. This way the authors can also demonstrate the predictive power of applying Eq. (18). The parameters could be varied just as discussed by the authors and it would be interesting to see how well an updated z_n (based on the model runs with the parameter values varied) matches the predicted z_n of Eq. (18) (based on the previous model results, e.g. of P).

Response: Thank you for this suggestion. We will run the N-P model to examine how well the modelled z_n matches the predicted value and the results will be added in the revision.

page 17, lines 416 - 419: "Our results indicate... self-shading negatively influences depth and thickness of the SCML,..."

This is comprehensible.

Response: We will think about and revise it accordingly.

4 Discussion

In presence of surface nutrient input

page 22, lines 527 - 535: This is certainly the case for the model assumption of an instant remineralisation of organic matter that originates directly from the phytoplankton.
Must this (the need to include a surface nutrient source) also be expected for a model approach where dissolved organic matter (DOM) and detritus are explicitly resolved?

Response: After examining the N-P-D model given by Beckmann and Hense (2007), we found that the surface nutrient input is not necessary for a model approach where dissolved organic matter (DOM) and detritus are explicitly resolved. In the revision, we will spell out this assumption.

Vertical profiles of nitrate gradients

page 24, line 605: In Fig. (4) the profile of $\frac{dN}{dz}\cdot 20$ does not correspond with the shown profile of N. The N profile clearly indicates a constant $\frac{dN}{dz}$ (of approximately 0.38 mmol N $m^{-4}\rightarrow$ 7.6 mmol N m^{-4} $=\frac{dN}{dz}\cdot 20$) in the depth range of 50 -70 m. The shown $\frac{dN}{dz}\cdot 20$ does not reveal this feature. The authors need to clarify this.

Response: Thank your comments. We will check what exactly happens and clarify the issue in the revision.

Limitation and application

page 24, lines 610- 636: As important as the model assumptions for the sinking and remineralisation of particulate organic matter is photoacclimation. The authors should consider to include one or two figures with profiles of Chl a concentrations with typical but different shapes of the SCM.

Response: In the revision, we will spell out the limitation of photoacclimation in this Section and revise the manuscript accordingly.

page 26, lines 648 - 657: I used the parameter values of Table (1) and the values for z_m, h, and σ from Table (2) to calculate the corresponding z_n and $\frac{dN}{dz}$ (Eqs. 11 and 14). I obtain z_n=70 m and $\frac{dN}{dz}$ = 0.025 mmol N m^{-4}. In Table the solutions are z_n=79 m and $\frac{dN}{dz}$ = 0.24 mmol N m^{-4}. I cross-checked my equations and all values and have not found any explanation for this discrepancy. I thought that all values presented are consistent with the imposed model dynamics and thus valid for any of the analytical steady state solutions presented.

Response: Sorry for the typo. We found that the value of γ should be 1/1.59, not 1.59. We will recalculate the values. Please see the revision.

Summary

pages 26 - 27: The authors may here stress that the important findings are conditioned by the model equations imposed. The interpretation of NPP is not straightforward and becomes particularly difficult to specify under steady state conditions of a weakly mixed water column. The authors construct NPP from the model equations that rely in Blackman's law of limiting factor for the growth rate. I suggest to the authors to refine their statements, clarifying their findings are based on the assumption that a prominent instant recycling process exists.

Response: Thank you for the helpful suggestions. We will rewrite the summary by stressing the assumptions of our model results, especially the statements about NPP.