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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

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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

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Discussion paper

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 coauthore 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.

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.

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 parameterisation 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.

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.
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.

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.

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).
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.

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).
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."

3 Results
page 12: You may add here the depth range that is considered ($z_s < z < z_b$).
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).
page 13: The minus sign (- $K_{\nu 2}/\sigma^4$) is confusing.
line 313: do the authors mean... "... (values from 8.64 to 7.78 $\cdot$ 10^{-9} m^{-2} s^{-1})..."?
Equation (12): for non-zero w

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 γ and vice versa). This way the authors may substantiate their

conclusion.
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*.
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).
page 17, lines 416 - 419: "Our results indicate... self-shading negatively influences depth and thickness of the SCML,..."
This is comprehensible.

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?

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} $\longrightarrow$ 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.

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.

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.

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.

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