Biogeosciences Discuss., 11, C5841–C5848, 2014
www.biogeosciences-discuss.net/11/C5841/2014/

Interactive comment on "Steady-state solutions for subsurface chlorophyll maximum in stratified water columns with a bell-shape vertical profile of chlorophyll" *by* X. Gong et al.

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Received and published: 11 October 2014

Reply to Reviewer: Emmanuel Boss, University of Maine

This paper deals with an important problem, the distribution of phytoplankton in the upper ocean. A kinematic distribution is assumed which is forced on a dynamical set of equation so that parameters associated with the profile could be inferred.

I find the paper of interest, including novel results. The paper is, in general, clearly written, however it is ridden with English mistakes. I urge the writers to consult with an English native speaker before submitting a final version.

I am in favor of publishing this paper, but I have some major comments that I feel, if addressed, can clearly improve this paper's utility.

Response: We are very grateful for the suggestions and comments. The revised manuscript will be edited using the service provided by Elsevier WebShop English language editing. Please see the revision.

Comments— The kinematic solution assumed (a Gaussian, eq. 7) is not an exact solution of the dynamical equations (1-2) used (even at steady state). At best, it is an approximation. This needs to be clearly spelled out. For example, you should substitute the solution(s) you get into the ODE (1-2) and see how well the terms balance each other (or how small the residuals are relative to the sizes of each terms). Best to do it after appropriate non-dimensionalization of the equations.

Response: Agree. The Gaussian function of vertical Chl a profile is, at best, an approximate solution for Eqs. (1-2), and we will spell out this point in the newly added Section 4.2, please see the revision. After nominating the values of model parameter (please see Table 2) and substituting the solutions we get into ODE (1-2), we find that at depth of SCML the dominant balance is between growth of phytoplankton and vertical eddy diffusion.

Comments— The assumption should be clearly spelled out, including their limitation. As a start, the continuous profile of phytoplankton assumed is clearly not consistent with a piecewise eddy-diffusion coefficient. The surface concentration of chlorophyll is nowhere zero in the ocean, and if diffusion in the ML is indeed sufficient to homogenize it (as assumed) the phytoplankton function could not have a continuous derivative across the boundary between the two diffusivities (as assumed – the only case where it may work is if the vertical derivative of P is identically zero at the transition between the diffusivities). It is assumed that the maxima is significantly deeper than the base of the ML – it is therefore not surprising kv,1 plays no role in the solution, and in fact you will obtain the same kinematic solution if you simply used a single constant eddy diffusion coefficient for the whole water column. You neglect photo-acclimation and assume Chl_a=phytoplankton – this is a significant simplification as it is well known that phytoplankton increase inter-cellular pigment concentration when light level decrease (e.g. Fennel and Boss, 2003).

Response: We will spell out the assumption and limitation by adding a new Section 4.2 in the revision. Please see the revised version. We agree that K_v_1 plays no role on SCM, and the corresponding results will be deleted in the revision.

Comments— The treatment of grazing loss, is, in the least, an over simplification (yes, Fennel and Boss, 2003, used a similar one). Grazing loss depends strongly on concentration (it is an encounter based process) and, given that zooplankton can move, or, in the least, grow faster where more food is available, are unlikely to have a constant concentration distribution (which is assumed for a constant epsilon). I realize that accounting for it will cause the equations to become nonlinear, and probably non-solvable, but mentioning this limitation is needed. This assumption is in the heart of the Sverdrup's critical depth model, which we now understand to have significant limitations.

Response: Agree. We will spell out the assumption and limitation by adding a new Section 4.2 in the revision. Please see the paper.

Comments— You claim (e.g. Appendix B) that nutrient limitation is required to get a SCM. In Fennel and Boss, 2003 we found, we similar equation, that we do get it with saturating Nutrient (in this case vertical velocity is required).

Response: Many thanks to Dr. Boss for noticing this question. Just as Fennel and Boss (2003), in nutrient-saturated case, to get a SCM the vertical velocity is required, because the primary importance of sinking in the formation of a SCM lies in its nutrient-depleting effect on the surface layer (Hodges and Rudnick, 2004). Ryabov et al. (2010) simulated the formation of a SCM by starting with an initial nutrient rich system. They first observed a rapid formation of a transient phytoplankton maximum close to the

surface. This phytoplankton profile is, however, not stable. With the depletion of the nutrient in the surface layer the production layer, i.e., the layer where the growth rate is larger than the loss rate of phytoplankton, shifts downwards, until the system reaches a stable DCM configuration. Mellard et al. (2011) analytically derived that in equilibrium nutrient limitation in the surface mixed layer is required to get a SCM. Thus, the sentence in the revised Appendix B will be modified as 'Note that we have known that the stable SCML occurs only when the growth of phytoplankton within the surface mixed layer is nutrient-limited (Mellard et al., 2011; Ryabov et al., 2010), . . .'.

Comments— We are still far from the days when we can use remote sensing to get a phytoplankton profile (unless using empirical parameterization such as in the works of Uitz or Westberry). To start, you could suggest field experiments (e.g. grazing, primary productivity, and measurements of turbulence) that could test if your results are consistent with reality (rather than assume that your model captures reality). Models are always approximations. Testing these approximations is required before we can assume they apply in the field.

Response: In the revision, we point out the requirement of field experiments for testing whether our results are consistent with reality. In addition, encouraging by Reviewer 3 and Jaime Pitarch we have tried to apply our theoretical results to three time-series stations in different regions, i.e., the South East Asia Time-series Station (SEATS) in the South China Sea, the Hawaii Ocean Time-series (HOT) station, and the Bermuda Atlantic Time-Series Study (BATS) site, please see the revision.

Minor comments:

p. 9515, l. 11: these equation do not include the 'fundamental physical and biological processes', as best they are judicious simplification created to make this extremely complex problem tractable. Upper ocean dynamics exhibit many processes (turbulence, internal waves, storms, slant-wise and vertical convection) which are not captured in the model (a constant eddy diffusion coefficient, basically assuming a constant

Nˆ2, e.g. Gargett, 1984). Similarly the biological representation is extremely limited (some may call it simplistic); the microbial loop is represented by \alpha, all the loss process, but sinking, are assumed to be linearly proportional to phytoplankton concentration. Constant sinking velocity. Temperature plays no role. Spelling it out does not diminish from your results but makes sure that the reader does take it with a grain of salt.

Response: In the revision, we will delete this sentence 'fundamental physical and biological processes', and will spell out the assumptions in the newly added Section 4.2.

p. 9516

Eq. 5. This is another approximation (e.g. Morel, 1988, JGR). Even in a layer of constant optical properties, k_d varies with depth, particularly near the surface (due to sun angle and equilibration between loses to absorption and redistribution of light by scattering). You assume in your model that k_d is not a function of P hence you neglect 'self-shading' (another assumption).

Response: We will point out the approximation and the assumption in the revision.

Eq. 6 – you do not require continuous flux between your two layers, which you should (and which will not be consistent with your profile).

Response: Thank you for this suggestion. To consist with the Gaussian profile, a gradual transition from one area to another written in terms of a generalized Fermi function (Ryabov et al., 2010) will be added, that is, K_v (z)=K_v2+(K_v1-K_v2)/(1+eˆ(((z-z_s))/l)), where parameter l characterizes the width of the transient layer. In our study, we assumed this transient layer is infinitely thin.

Eq. 7 – suggestion: why not use a single amplitude (A, or P_max=h/sigma \sqrt(2 pi))? It will simplify the reading of the manuscript. In the least change h (often used to denote layer depth) with int_P or something else which will make the reading of the paper easier.

Response: We will adopt a single amplitude, i.e., P_max=h/sigma \sqrt(2 pi) in the revision.

p. 9518, Eq. 10: checking units I find them inconsistent between the left and right side of the equation and hence this equation is wrong.

Response: Sorry for the typo. The factor of the first term on the right hand side should be $-$Kv2/σ4, instead of $-$Kv2/σ2.

p. 9520, l. 27: 'the popular compensation depth' is only sound within the assumption of its model. Since, like you, Sverdrup assumed a constant epsilon, it is not surprising you find similar results. This does not validate your or Sverdrup approach wrt to ocean ecology. In particular the treatment of grazing (a constant epsilon throughout the water column which is independent of phytoplankton concentration) is lacking in your (and Sverdrup's) approach. See Behrenfeld and Boss, 2014, for a review of this issue.

Response: According to the review paper on this issue (Behrenfeld and Boss, 2014), we will rewrite this paragraph, please see the revision.

p. 9521, l. 13: This condition is identical with Eq. 4a of Fennel and Boss when vertical sinking is constant as function of depth.

Response: Thank you for this suggestion. This will be added in the revision.

p. 9523, l. 21-22: It is by design (having the SCM be much deeper than the surface ML) that k_v1 has no influence on sigma. No surprise there.

Response: We will delete this sentence in the revision.

p. 9525, l. 3: nothing 'dramatical' with logarithmic functions. They increase much slower than exponential or power-law functions.

Response: We will delete this word in the revision.

p. 9526, Eq. 26 is identical to that of Fennel and Boss, 2003, for constant settling velocity.

Response: We will add this sentence in the revision.

p. 9514

l. 3: environment -> environmental.

l. 15, 18: ‘the infinite assumption’ – not clear. I think you refer to the ‘delta-function’ layer.

l. 25: ‘etc’ does not belong there. Remove it.

p. 9518

l. 7: ‘where is the balance…’ should read ‘where there is a balance…’.

l. 8: delete ‘carefully’ and add and ‘s’ to ‘reflect’.

l. 11: not clear what you mean.

l. 20: should be ‘steady’

p. 9520

l. 5: replace ‘obviously’ with ‘identically’.

l. 24: no ‘etc’, and since you called epsilon the loss-rate and described it above, there is no need to, again, describe the processes causing loss.

p. 9521

l. 10: I think you mean ‘requiring a positive solution’ rather than ‘According to the property of the logarithm function’.

Response: Many thanks for your detailed corrections. The grammatical errors have been corrected in the revision; meanwhile this paper will be edited using the service provided by Elsevier WebShop English language editing. Please see the revision.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C5841/2014/bgd-11-C5841-2014-supplement.pdf

Interactive comment on Biogeosciences Discuss., 11, 9511, 2014.