Review of 'Steady-state solutions for subsurface chlorophyll maximum in stratified water columns with a bell-shape vertical profile of chlorophyll', by Gong et al.

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.

1. 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.
2. 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 therefor 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).
3. The treatment of grazing loss, is, in the least, an over simplification (yes, Fennel and Boss, 2003, used a similar one). Grazing loss depend 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 non-linear, 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.

4. 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).

5. 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 approximation is required before we can assume they apply in the field.

Minor comments:
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. 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 N^2, 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.

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).

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

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.

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

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.
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.

p. 9521
l. 10: I think you mean ‘requiring a positive solution’ rather than ‘According to the property of the logarithm function’.
l. 13: This condition is identical with Eq. 4a of Fennel and Boss when vertical sinking is constant as function of depth.

p. 9523.
l. 19: ‘surprised’ should be replaced with ‘surprising’.
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.

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

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