Reviewer #1

The authors have addressed my concerns and the manuscript has generally improved. Concerning my point regarding the description of the Holling type III equation, I have now understood what the authors mean. I have also realized that my comment was possible misleading.

Thank you for your comments on our manuscript.

In order to make the statement even clearer, I would suggest to merge line 127 (where the Holling type III equation is first described mathematically) with line 188: “the Holling type III functional response (formula) can be approximated to g0pop2 at low prey concentration and asymptotically approaches a constant value for high p..”

We have edited the sentence as suggested.

Reviewer #3

In this new version, the authors have improved the article and properly addressed most questions and suggestions. However, I still have some comments on the manuscript.

Thank you for your comments and careful reviews.

General comments:

1) I agree that considering a linear response of phytoplankton growth to light is a valid simplification considering that the focus of the paper is on the wintertime, when Photosynthetic Active Radiation (PAR) levels are low. However, I think that this assumption should be discussed in more detail and treated more carefully through the paper:

We have revised our treatment of light in this version of the manuscript. We now use a saturating response function. This change has some impact on the optimal parameters, particularly the mortality parameters. We use a saturation value of I_0 = 40 as in Bouman et al. 2018 and an annual cycle of light as reported in Westberry et al. 2016.

We would also like to note that there was an error in the parameterization of the annual cycle of light that diminished the importance of light in the previous version. We have now corrected this, but the impact on the results is minimal.

a) In L200, it is stated that the assumption has minimal impact on the wintertime period. Until when could this impact be minimal? Until when the PAR experienced by phytoplankton (taking into account MLD and surface PAR) is low enough to assume a linear response in winter-spring? I think this should be better elaborated and justified, combining some numbers with information from the literature about P-I curves (for example, P-I curves for the North Atlantic during winter-spring).

Using the parameters discussed above, the P-I curve is fairly linear, however we have now chosen to include a non-linear functional response because a photoadaptation parameter is implicit in a linear model.

b) Also, although the model captures the annual cycle with this assumption, I still think that a saturating response of phytoplankton growth to light might slows down growth during the spring bloom, contributing to the recoupling between phytoplankton and zooplankton populations. This could improve the match between model predictions and observations (Fig. 3). Although I understand that not including this response simplifies the model parametrization, I still think that it should be discussed in more depth.

We have added additional discussion of the response to light to the third discussion paragraph. "The magnitude and timing of the spring bloom and interactions between zooplankton and phytoplankton populations in the springtime may be affected by factors not considered here, such as a non-linearities in phytoplankton photophysiology."

2) The choice of some numbers and assumptions should be better justified and/or supported with references. For example, in L112, a reference should be added in relation to the choice of the MLD definition (or a paper that uses a similar criterion). Also, in L227, I miss a reference for the choice of the N pool value of 30 mg N m-3.

We have added a reference to Kara et al. 2000 for the mixed layer depth definition. Mignot et al. 2018 verified that the criteria works well for this region (second paragraph of the section "Data sources and processing").
The value of 30 mg N m^{-3} was chosen based on examination of BGC-Argo float profiles in the region. We have included this information in the revised manuscript.

Specific comments:

L5: I think this sentence should be better written. For instance, a function decreases or increases depending on the direction we are moving on the x-axis. Also, it is a bit confusing the part "in phytoplankton concentration at low concentrations". Maybe you could simplify the sentence, something like "...(or more generally non-linear) at low phytoplankton concentrations…"
We have rewritten this sentence. It now reads "However, certain mathematical formulations of grazing as a function of phytoplankton concentration that are quadratic at low concentrations (or more generally decrease faster than linearly as phytoplankton concentration decreases) can reproduce the fall to spring transition in phytoplankton, including wintertime biomass accumulation."

L24: Maybe introduce the sentence that starts with "Phytoplankton" with an "Also, /Additionally, " to clarify that this is a complementary hypothesis and mechanism.
We have added "also" to this sentence.

L46: Please replace "are modeled" by "can be modeled".
We have made this edit.

L75: Similar problem as in L200. What is low irradiance in numbers?
The theoretical argument does not depend on specific parameter values and it would be a distraction to quote specific values.

L160: This is the title, but the paragraph starts with the type II functional response. If you mean that "Grazing linear at low phytoplankton concentrations", please add this last part. Grazing was also linear in the previous section (2.1). Could you please add something to the title to make clear the difference with the previous section?
We have changed this title to "Grazing linear at low phytoplankton concentration".

L180: Similar to the previous case. Thus, if you mean that is quadratic in winter when phytoplankton concentrations are low, please specify.
We have changed this title to "Grazing quadratic at low phytoplankton concentration."

L181: The term stronger is not clear for me. I'd just indicate that is non-linear. Also, please indicate here that this happens at low phytoplankton concentrations. You indicate this later, but should be mentioned from the beginning.
We have rephrased this sentence to address both points and it now reads "The situation is different if we prescribe a phytoplankton grazing function that decreases more rapidly than linearly as p *decreases."*

L188: This is actually just one scenario of the "disturbance-recovery hypothesis", the one described in Behrenfeld (2010), where this hypothesis was initially called the "dilution-recoupling hypothesis". Later, in Behrenfeld et al. (2013), the hypothesis was extended to any environmental process that disturbs the balance between phytoplankton growth and losses and got its current name. Thus, maybe you just can simplify it to "consistent with the DRH".
We have simplified this sentence as suggested.

L232: I'd erase "much".
We have made this edit

L245: "observational timeseries", to make it clear. Also, although then it is mentioned the days of the year when this peak occurs, please include also here the days of the year that more or less delimit the peak and a reference to Fig. 3.
We have edited this sentence by adding the word "observational" and mentioning the days of the year when the peak occurs.

L259-260: Maybe I am missing something here but, why starting the sentence with a "Despite this"? Is it not sensible to think that if there is larger winter phytoplankton concentration using the type III grazing function (compared to type II), this grazing rate is lower?
We changed "Despite" to "However" which does not imply an inconsistency between the high phytoplankton concentration and the low grazing rate.

L285-288: How do you define the bloom onset? When net phytoplankton biomass accumulation is positive or when phytoplankton biomass is above a particular threshold?
We are defining bloom onset based on the net population growth rate. We have revised this sentence to read "The deficiencies of the bulk model are evident at the spring bloom onset (period of rapid net population growth), which is slightly delayed in the model relative to the observations, occurring once the mixed layer has shoaled rather than during mixed layer shoaling."

L309: Maybe not the best reference as this paper focuses on the "dilution-recoupling hypothesis", which describes just one scenario of the "disturbance-recovery hypothesis". Please, use Behrenfeld et al. (2013), Behrenfeld and Boss (2014) or Behrenfeld and Boss (2018). This might also be applied to the Introduction section of the paper.
We have changed this reference to Behrenfeld and Boss 2018. We have left the reference to Behrenfeld 2010 in the introduction because that section focuses on the dilution-recoupling hypothesis.

Figure 2: Color scales for grazing/growth are different among panels. If making the same scale for all panels is not appropriate, at least please indicate that the scales are different in the caption. In the first line of the caption, replace "(e.g" by "; e.g." or ", e.g.". Also, include spaces in the units when necessary (e.g., mg C m-3). At the end of the caption, change it by "depends also on phytoplankton concentration in the case of a Holling type II".
We have added the sentence "Note the different color scales in each panel." We have replaced the parentheses with a comma and included spaces in the units.
We have not edited the last sentence of the caption because the sentence is accurate as it is written.

Figure 3: Add in the caption "dashed thin black line".
We have made this edit

Technical corrections:
We appreciate the reviewer's careful editing. We have made these editorial corrections.

L212: Add a comma after "Mignot et al. (2018)".

L230-231: Add spaces around equal signs.

L231: Add a comma after "However".

L233: Add space before "If".

L312: Add a comma after "Furthermore".

L360: Add space after "deepen".

Bibliography:

Behrenfeld, M. J. 2010. Abandoning Sverdrup's Critical Depth Hypothesis on phytoplankton blooms. Ecology 91: 977-989.

Behrenfeld, M. J., and E. S. Boss. 2014. Resurrecting the ecological underpinnings of ocean plankton blooms. Annual review of marine science 6: 167-194.

Behrenfeld, M. J., and E. S. Boss. 2018. Student's tutorial on bloom hypotheses in the context of phytoplankton annual cycles. Glob. Change Biol. 24: 55-77.

Behrenfeld, M. J., S. C. Doney, I. Lima, E. S. Boss, and D. A. Siegel. 2013. Annual cycles of ecological disturbance and recovery underlying the subarctic Atlantic spring plankton bloom. Global biogeochemical cycles 27: 526-540.