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Interactive comment on “Plankton ecosystem functioning and nitrogen fluxes in the most oligotrophic waters of the Beaufort Sea, Arctic Ocean: a modeling study” by V. Le Fouest et al.

Anonymous Referee #1

Received and published: 19 December 2012

The manuscript by LeFouest et al is devoted to the modelling of the AO ecosystem in an oligotrophic regime. The approach is designed to explain a comprehensive (albeit very limited in time) set of biogeochemical measurements. The paper presents a very impressive effort in combining modelling and observations, especially important when taking into account how limited observations in AO are. However in spite of being very impressed with the effort, I have some serious concerns about the approach and cannot recommend this paper for publications until the authors address them (including some additional model runs).

Most critical points:

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1. A steady state approach is used, however no consideration is given as to whether the steady state is meaningful. It is achieved (if the axes on Figure 4 are correct) after 2-3 years of integration. Such a long time scale in a system with a strong annual signal means that the system never has a chance to reach or even approach it, if forced by the variable seasonal forcing! You initialise from observed distributions, so to make the approach valid you have to achieve near-equilibrium as soon as possible (ideally on a time scale of a month) with a minimum deviation from the initial conditions.

2. You can substantially shorten equilibration timescale if you sort out your LP-LZ-NO3 behaviour in the upper part of the water column. It appears that your large zooplankton grazing term does not work well at very low concentrations and need either adjustment of the parameters or even change of the functional form. I suggest that additional experiments to achieve steady non-zero level at the surface are needed.

3. You are discussing *very* low concentrations near the surface. Are they above detection limits? I would like to see detection limits and error bars for all measured variables.

4. You assume that the steady state can be achieved in the system ignoring horizontal advection. Arctic is a very “advective” system. Are you sure that your resulting ammonium concentration (triple of what was observed) is not an artefact of missing advection? You probably cannot constrain its effect, but at least some discussion on possible consequence of omitting advection should be presented.

Relatively minor points:

L3. What are your evidence of “greater stratification” in the AO? This is a controversial topic. Retreat of the ice can promote both types of factors (increasing and decreasing stratification). Please substantiate.

L25. This is a very strong statement made on a basis of a single modelling example in the Bering Sea. A lot of effort has been made recently on modelling of AO production

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(see independent papers of the following authors: Jin, Zhand, Deal, Popova, Wassmann, Slagstad, Dupont). Such a criticism (even if correct) must be substantiated.

Figure 3. You are missing arrow between DON and NH₄ (especially in a view of its importance!)

Figure 4. Please show days instead of hours.

All Figures are too small.

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