Dear Editor,

Please, find below my comments on the manuscript (ms) by Le Fouest et al. “Modeling the impact of riverine DON removal by marine bacterioplankton on primary production in the Arctic Ocean” intended to be published in Biogeosciences Discussions.

General comments

The ms addresses a current topic, as Arctic Ocean (AO) is an important ecosystem globally, for example as a CO_2 sink of anthropogenic emissions and by producing significant commercial fish stocks. AO is receiving relatively large amounts of riverine dissolved organic nutrient load, and temperature increase due global climate change has been projected to increase this load, and hence affect also the above key system features.

The ms deals mainly with modeling nutrient flows in the pelagic AO ecosystem with focus on the importance of riverine dissolved organic nitrogen (RDON) in controlling phytoplankton primary production (PP) vs. bacterial production (BP) (autotrophy vs. heterotrophy) balance in a scenario of melting sea ice (1998-2011). A coupled 3D hydrodynamical model and biogeochemical model is applied to understand the role of labile RDON in AO system functioning and to project system changes under global change. The widely applied MIT model is used for 3D hydrodynamical modeling, and I assume it is adequately implemented (I am not an expert in this and have no further comments here). The structure of the planktonic nutrient flow network seems adequate, and in my opinion the ms represents a necessary first step to reach its goals. However, though the food web interactions and their formulations and parameterisations seem mostly realistic to me, I have comments and criticism on some aspects that are given below.

Specific comments

1. In the model it is assumed that bacteria prefer (take up faster) DON compared to ammonia (eqns A19-A22; P 16974, L 10-12). This may indeed be the case with small nucleotides and amino acids, as is shown in the article referred in the ms to justify the above assumption (Kirchman et al. 1989). However, these compounds form a small part of released DON (dDON; Fig. 1), and their share must be minimal in the bioavailable riverine RDON pool, being processed by bacteria during transport from drainage area. Ammonia is readily taken up, but bulk RDON compounds need to be broken down exoenzymatically before transport into cells (accordingly, the authors define bioavailable RDON as the fraction of total DON degradable within one month; P 16965, L 4-7)! Finally, why can’t bacteria take up nitrate, like they in reality do (P16973, L 14)?
2. The nutrient uptake efficiency or affinity (α) of osmotrophs for nutrients can be given as α = maximum nutrient uptake rate / half-saturation constant of uptake. In this study (Table 2) α for bacterial ammonia uptake is 1/0.1 m^3/(mmol-N*d), being smaller than α for small phytoplankton (SP, = 1.4/0.1 m^3/(mmol-N*d)); and even large algae (LP) show similar α as bacteria (= 1.4/0.5 m^3/(mmol-N*d)). This contradicts with the theoretical and empirical results that smaller cells are more efficient in taking up nutrients than large ones, showing a quadratic penalty with respect to size (radius, though adjustments like diatom cell vacuoles devoid of nutrients can diminish this penalty; e.g. Fenchel 1987; Ecology – potentials and limitations, Oldendorf-Luhe; Thingstad & Rassoulzadegan 1999, Prog. Oceanogr. 44: 271–286; Lignell et al. 2013, Limnol. Oceanogr. 58: 301–313). Thus, more than an order of magnitude smaller α would seem more appropriate for <5 µm SP compared to bacteria.
3. The above 2 issues rise the question, have the authors tested a stand-alone version of their nutrient flow model (Fig. 1)? That is, has the model been verified with appropriate time-course data from enclosed or semi-enclosed systems (e.g. plankton nutrient treatment responses in mesocosms), where uncertainties arising from hydrodynamics are minor.
4. Something seems to be missing from eqn A20, and judging from text (P 16974) it should read S = $(NH4, 0.6DONI)_{\mathbf{min}}$ (bold ‘**min**’ added, right or?). Authors should carefully check the equations, as also eqn A23 seems incomplete (below); this also concerns parameter Table 2 (e.g. LP sinking rate unit should probably read md^{-1} instead of m^{-1})!.
5. Due to above problems (issues 1 and 2) evaluation of model functioning is not straightforward. i) The model seems to function so that phytoplankton grow on ammonia (and nitrate) and bacteria grow mostly on DONI (RDON+dDON, ammonia uptake appearing to be redundant, especially

since it's further constrained with DONI availability; denominator in eqn A22). ii) Bacteria cannot become N-limited in substrate (dissolved organic matter, DOM) uptake. iii) Labile dissolved organic carbon flow is not explicitly included, but bacteria fulfill their C needs along with DON uptake (DON pool is estimated from DOC with fixed C:N ratio of 40), and maximal bacterial ammonia uptake is constrained by DONI availability (eqns A19-A22).
In summary, with the temporal (annual) and spatial (AO) scales applied, and with the order of decade residence time of AO water body (P16958, L 21-22) it seems that the model may be able to reproduce reasonably well annual average PP and BP values as long as bacteria are C-limited. This is also because most of the N incorporated into bacterial biomass is subsequently recycled in the planktonic grazing processes.

6. Related to point 5, can the authors come up with any empirical data (or reference) on C vs. N limitation of bacteria in AO?
7. Basically two model runs (with and without RDON inputs) are reported in the ms, resulting in point estimates of annual PP and BP averages of the deterministic model (P 16960, L 12-18; Fig. 5). It is unfortunate, that no uncertainty analyses are included in in model examinations (cf. comments above), also hampering trend evaluations! The ms deals with the impact of RDON inputs on AO system, and the authors report percentage labile RDON range of 8-24% (of total RDON) as annual averages in loads of different rivers (P 16965, mean of 15% has been applied). Thus, the labile RDON range would be a natural candidate for initiating model sensitivity analysis. Not the least, because the authors seem to feel that way themselves (P 16965, L 13-15)!
8. Finally some small questions and comments:
   i. Why is nutrient/food limited growth formulated differently with different functional plankton groups, instead of using consistently one Monod/Michaelis-Menten/Holling type expression? The latter alternative would improve model transparency and evaluation of parameter set used.
   ii. Why does large zooplankton not release dDON via sloppy feeding in model like small zooplankton – and like experimental studies suggest?
   iii. The dPON sedimentation loss term includes a strong quadratic penalty for increasing dPON concentrations, mimicking aggreagate formation and subsequent fast sedimentation (eqn A23 should probably read sed_dpon**d**PON, with bold '**d'** added, or?). The authors need to give a reference or show data to justify this formulation!
   iv. Protozooplankton (SZ) shows maximum grazing rate of 1 d^{-1} and growth efficiency of 30% (Table 2), which translates to maximum SZ growth rate of 0.3 d^{-1}. This value sounds low to me – do the authors have empirical proof for it?
   v. The ms would benefit from a linguistic check.

To conclude, despite its weaknesses this ms is in my opinion a worthy first step towards evaluating the effects of labile RDON inputs on AO biogeochemistry, including recent development of PP vs. BP balance. However, one can be question the value or potential of the present model in projecting AO system responses to temperature increase and permafrost thaw due to global change, if this leads to increased riverine inputs of humic compounds with high C:N ratio. To forecast AO ecosystem responses to these scenarios, mechanistically more sound models, allowing for flexible stoichiometry and N-limitation of bacterial substrate uptake are probably needed.