

Interactive comment on “Modeling the impact of riverine DON removal by marine bacterioplankton on primary production in the Arctic Ocean” by V. Le Fouest et al.

Anonymous Referee #2

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Review of Le Fouest et al.: Modeling the impact of riverine DON removal by marine bacterioplankton on primary production in the Arctic Ocean.

This paper deals with the warming of the Arctic and changes to the dissolved organic matter input originating from the rivers around the Arctic basin. They have applied a pan Arctic ocean-sea model coupled with a biogeochemical model to investigate the effect of predicted changes on primary and bacterial production.

The background of the study is the prediction of larger input of DOM from rivers due to the melting of the permafrost. This DOM will be a major input of nutrients and carbon to the system and will increase bacterial production. Projected increase in surface

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temperature will also affect the bacterial productivity.

The physical models used in this study have been published and used in several studies and thus the methodological framework appears robust. On top of this the authors have coupled a biogeochemical mode representing the planktonic ecosystem. Primary producers were divided into two groups depending on size. This is a bit simplistic as different groups of phytoplankton can have big impact on several key parameters independent of size; but perhaps this covers the major groups of relevance for this region. Zooplankton groups are also simplified, but probably fulfil the need in this model. Having a fixed Redfield stoichiometry does not, however, represent a realistic scenario, and this is a weakness in the model. The authors acknowledge this weakness and are right that including flexible stoichiometry is not a trivial aspect.

In order to test the effect of bacterial degradation of DON, the mode was run with and without RDON removal by bacteria, and the results were compared with measurements of primary production and bacterial production in the area. The model with DON utilization of bacteria produced the best results compared with measurements. The model indicates that DON increases bacterial production which in turn increases primary production during the summer months due to increased recycling of NH_4 , which is taken up by the phytoplankton.

In most places it seems the RIV run of model does not even meet measured BP, and I was missing some more discussion on why that is. Is there an underestimate of DON available or something else not accounted for in the model?

All in all the paper presents a step forward in modelling different scenarios for the Arctic Ocean and understanding the dynamics of the DON pool is certainly a key aspect, and in this respect the paper is both timely and justified.

Minor comments The text is generally well written but with some unnecessary long and difficult sentences. For example this sentence from the Abstract: “In this study, in order to elucidate on the processes regulating the production of phytoplankton (PP)

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and bacterioplankton (BP) and 5 their interactions, we employ a biogeochemical model coupled to a pan-Arctic ocean-sea ice model (MITgcm) to explicitly simulate and quantify the contribution of usable dissolved organic nitrogen (DON) drained by the major circum-Arctic rivers on PP and BP in a scenario of melting sea ice (1998–2011).”

In my opinion, breaking up these types of sentences into two sentences would increase the readability, but this is a matter of personal preference.

P16962 L12: “The PP increase it tightly linked..” should ‘it’ be changed to ‘is’?

Interactive comment on Biogeosciences Discuss., 11, 16953, 2014.