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Interactive comment on "Phototrophic pigment diversity and picophytoplankton abundance in permafrost thaw lakes" *by* A. Przytulska et al.

Anonymous Referee #2

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The manuscript by Przytulska et al. studied the phototrophic communities in permafrost thaw lakes of subarctic Quebec, mainly through specific pigments analysis, flow cytometry and molecular methods. It is suggested that the diverse phototrophic groups and abundant picophytoplankton in those special ecosystems could potentially contribute to higher trophic levels and lessen the release of GHGs. While the sampling design is sound and the results are interesting, I have some comments and suggestions on improving the quality of the manuscript.

1) There's a general lack of information on methodological description. For example, what analysis system, scanning atlas and quantification calculation is used for the HPLC analysis? What is the relationship between phytoplankton groups and specific pigments? To what extend the CHEMTAX is applied or not at all? There's no clar-

ification on the terms of “photoprotective, photosynthetic, and accessory pigments”. Unclear what sampling dates and layers (surface and/or bottom) were at each location, and this makes it hard to follow the results. No information on specific samples used for each analysis, e.g. What samples are used to run the correlation analysis between picocyanobacteria and temperature? Are the bottom waters included as well? Please at least include the information of P value and observation numbers for each statistical analysis.

2) Another issue is the inconsistency and complexity of samples and methods chosen for different statistical analysis. Could this be a potential cause for the “insignificant” results/relationship of variables? For instance, it is not fully convincing that no grouping of pigment characteristics were found among sites, especially knowing the significant environment heterogeneity between thaw lakes and SRB reference. What about the distribution of picophytoplankton? Also, is it common that the variation of environmental parameters and pigments composition between lakes of the same type so big (see the thaw lakes on marine clays for example)? I suggest to also re-analyse the molecular data exclusive of heterotrophic eukaryotes such as ciliates and fungi. Amplification biases should be addressed in more details.

3) I suggest the author to strength the discussions, in a more direct manner detailing the similarities and differences of phototropic community found between thaw lakes and reference lakes, and their contributions to the microbial community compared to heterotrophs. As written, it is currently difficult to recognize the key information of the results and evaluate the ecological significance phototrohic plankton have in the heterotrophic thaw lakes (e.g. in terms of lessen the emission of GHGs). It would be interesting to count and calculate the abundance and biomass ratios between heterotrophs and autotrophs in the thaw lakes, or even compare the ratio of picocyanobacteria to heterotrophic bacteria.

Specific comments: P. 123, L.6: Should be “. . ., while picoeukaryotes were inversely correlated with conductivity.” P. 125, L.10: Please add the information of sampling

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time and depths of each lake in Table 2. P. 130, L. 5-20: Please also mention the temperature differences among the lakes. P. 131, L. 16: Please clarify the sampling year described in the manuscript and the Table title. P. 131, L. 16-25: I found it very hard to follow the pigment results present in Table 2 and Figure 4, especially when there're 10 different pigments from 17 sampling sites at 4 different environments. I would suggest the authors to, 1) Unify the legends/terms for pigments in Table 2 and Fig. 4, and be consistent using them in the results and discussion section; 2) If the special purpose of Table 2 is to compare the different contribution of photosynthetic and photoprotective pigments, please add a few columns in Table 2 to calculate the total percentage of each at different stations. P. 133, L.11-13: This result seems too speculative. Also, it should be Figure S1. P. 135, L. 20: Inconsistent information on the prevalence of diatom (see L. 21-22 of P. 140). Please clarify. P.136, L. 5-6: Please add a reference here. P. 136, L. 7-8: "The concentrations of β, β-carotene, were conspicuously high in the NAS lakes." This was only found during summer season of year 2012? P. 136, L. 26-29: How is this related to the occurrence of zeaxanthin? In any case, this information is useful but maybe fits somewhere else better? P. 137, L. 27-30: The fraction/contribution of picoplankton to total phytoplankton community (especially in lake KWK and NAS), in terms of either pigments or biomass, should be also discussed. P. 139, L. 10-14: Did the authors have a closer look at the dominating dinoflagellate species? P. 139, L. 24: Please add a reference here.

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