Contributions of Transparent Exopolymer Particles by Specific Phytoplankton Groups in the Cosmonaut Sea, East Antarctic

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General comments

- The main objectives of this study is to quantify the contribution of main phytoplankton groups to the TEP in Cosmonaut Sea, Antarctica. The authors claimed that TEP is mainly contributed from haptophytes at high phytoplankton biomass. However, I found the design of the study, and data analysis are not sufficient to tackle this problem. The reasons are the following:

    1. All the samples of phytoplankton, nutrients, and corresponding physical environmental variables were taken at 0 m. Due to water column mixing, and phytoplankton sinking, the phytoplankton biomass and the TEP they produced will be spread over the whole surface mixed layer, or the euphotic zone. This has important consequences to quantify the phytoplankton biomass, if phytoplankton cells concentrate at certain depth (e.g. subsurface chl a maxima at the bottom of surface mixed layer). TEP production could be depth-dependent, as nutrient and light, which are important drivers of TEP production, changes with depth. Therefore, using data at surface only overly simplifies the physical-biological processes of TEP production in the water column.
    2. The interpretation of TEP production in the lab as theoretical TEP production is problematic. The authors mix the TEP concentration and production in the literature they cited and in this

manuscript frequently. The papers they cited all deal with the relationship between chl a concentration and TEP concentration. These lab culture conditions are not quite relevant to this study, as they are mostly from temperate regions, except for Hong et al. (1997). But the relationship of Hong et al. (1997) should be more relevant, as it was done for Antarctic species. The Chl a - TEP relationship of Hong et al. (1997) are very different from those for temperate species in Passow et al (2002). Unfortunately, the parameters of the relationship of Hong et al. (1997) was not used in the manuscript (Table 2)

3. How the correction factor (a ratio between TEP production in the field to that in the lab) was obtained is murky to me (lines 204-208). As described, it is obtained using MLR. In the MLR, which dependent variable, and independent variables are used? What is the rational for the regression coefficient being used as a correction factor? Even if we believe the correction factor based on the literal meaning, I can't imagine the correction factor could be a constant value that can scale the lab value to field value. The authors listed four assumption of this approach. I argued the assumptions are not valid in specific comments below.

4. In the Results section (3.3) TEP is regressed against diatoms, haptophytes to obtain chl a specific TEP concentration for each phytoplankton group using MLR, and this is further used to calculate the TEP originated from each group based on chl a of each group obtained from pigment analysis. The relationship between chl and TEP are not linear (power relationship) as in multiple references cited in this manuscript. However, MLR treats the relationship between independent and dependent variables as linear. This is biologically not sound.

5. From ecological point of view, the timescale of biomass turnover and TEP turnover are quite different. The timescale for phytoplankton bloom and bust could be short, but the TEP that phytoplankton generated may stay in the water after phytoplankton blooms are over. Therefore, simply regressing biomass of one group of phytoplankton against TEP is not appropriate for interpreting which group of phytoplankton generated TEP. In this study, there is a possibility that diatom bloom just passed when

you started sampling. In figure 2, the distribution pattern of surface silicate are similar to nitrate and phosphate, suggesting nitrate and phosphate were drawn down along with silicate drawdown. This indicates diatom bloom occurred, even though you don't see high diatom biomass. In the area with strong TEP signal, you can also see slightly lower silicate compared with its surrounding areas at the intersection between 65°S and 40°E. This could be my speculations, but it's possible, and can't be resolved by MLR here.

6. The authors claim that phytoplankton of the two main groups are not limited by nutrients. This is a HCLN (High Chlorophyll Low Nutrient) region, and may well be Fe-limited. There have been no mentions of its implication to TEP production. Overall, this paper did not provide any new and original insight to this field.

- **Therefore, I do not recommend for the publication of this paper on BGC.**

Specific comments

- Lines 55-56: The relationships cited are for the biomass and TEP concentration, not production.
- Line 127: the volume of the filtration must be precise. Do you mean"around 100 ml", instead of "approximately"?
- Line 143: "pore size", should be "nominal pore size" for glass fiber filters.
- Line 166: TEP concentration or TEP production? I believe it should be the former.
- Line 170: " The coefficient β, consistently observed to be <1, reflects a density-dependent inhibition effect on TEP production ". Hong (1997) shows beta is 3.63 (Fig. 7). Beta is obviously dependent on phytplankton growth rates, and maybe remineralization relative to production.
- Lines 173-174: What do you mean by "theoretical A and B"?

- Table 2: Chl a concentration in Passow (2002a) reached 291 μg/L, and temperature is around 12 ℃. I would suggest Hong (1997) work conducted in Antarctic may be more relevant to this study. The α and β are 1.01 and 3.63, respectively. However, there is large variability (see Fig. 7 of Hong 1997).

- Lines 188-189: First of all, avoid using the same letter for the correction factor β, and the exponent β. This causes confusing. Secondary, the correction factor represents the ratio between phytoplankton in natural environment to the theoretical value. Even if the "theoretical value" was acceptable, there is no way to guarantee that the correction factor is constant, because many factors influence TEP production in different space and time in the field.

- Lines 193-194: This may not be valid, as there might be a succession from diatom to flagellates, or haptophytes through sampling period. Or diatom bloom just ended before your sampling.

- Lines 196-197: However, TEP production is growth dependent. See Passow (2002). Any environmental variables influencing growth may have an impact on TEP production.

- Line 198: About nutrient limitation, N/P = 16 is not a criteria for nutrient status of phytoplankton, when both nitrate and phosphate are abundantly available. In this area Fe-limitation should not be ignored.

- Lines 202-205: That's probably true, but the proportion of three dominant groups may vary with space and time.

- Lines 209-211: The value of β will have large impact on epsilon.

- Lines 212-213: As the relationships between chl and TEP concentration are non-linear (as in literature you cited), how can you use a linear regression model to find correction for the parameters of non-linear functions?

- Lines 215-216: What is the theoretical TEP value?

- Equation 3: This equation is problematic! Salinity at the surface is lower than deep water does not necessary result exclusively from ice melt. It could be different water masses with different salinity. As

your Fig. 1 indicates, the surface water of different regions are formed by different water masses along with different current. This "dilution" processes might span much larger spatial scale than your area of research.

- Lines 252-260: There is a possibility that TEP is originated from diatom blooms just passed, based on the positions of silicate dradown, and TEP. The center of Haptophytes are not aligned with that of the TEP. Sampling depth of 0 m might have missed the subsurface chl a where higher chl a could be found.
- Line 268: Should be $r^2 = 0.62$?
- Lines 269-270: Percent MV is calculated from salinity. There is no point to calculate the correlation between MV% and salinity.
- Lines 277-279: Why? How are they different from the rest? What are the phytoplankton community structure?
- Lines 287-290: Standing stock is different from production. The correlation between Hapt and TEP does not equal to causality. It could be that diatom bloom proceeded haptophyte, and generated TEP that remained in the water when haptophyte bloom starts without producing TEP, but happened to be sampled.
- Table 4: There is significant correlations between diatom and haptophyte biomass according to Figure 5 above. Therefore, MLR is not valid. VIF is required to show if the regression is inflated by the correlation between independent variables.
- Lines 313-315: This is an HNLC area, with Fe limiting phytoplankton growth!
- Lines 315-322: N/P ratio relative to Redfield ratio shows potential nutrient that limit the phytoplankton biomass. Nutrient limitation is determined by nutritent concentration relative to nutrient uptake kinetics (e.g. half saturation concentration of nutrient uptake) of a species of interest. This area is not limited by nitrate, phosphate, and silicate, but may well be by Fe.

- Lines 326-328: It looks like the biomass of diatoms and haptophytes are similar in the areas with elevated NH_4. The point is not supported by your data here.

- Lines 344-345: "Theoretical TEP production" needs to be defined as in elsewhere.

- Lines 351-353: Where is that coming from? Statistically, the smaller regression coefficient indicates the response of TEP to diatoms is weaker than Haptophytes, when everything else is held constant. The relationship between TEP and Chl is not linear as cited in the table above. But now with MLR you assume they are linear? No wonder you obtain lower β values. Then you insert back the *beta* value here to the exponential functions?

- Lines 357-358: TEP can be remineralized, and phytoplankton may sink. This study can neither support, nor repute the point, based on regression coefficients.

- Lines 363-365: Unfounded speculations! At low temperature, phytoplankton manage to up regulate enzyme production by synthesizing more RNA for some species, high temperature can increase enzyme activity. The temperature here obviously haven't reached suppressingly high temperature yet.

- Lines 368-370: Where is that from? Any temperature-growth response curves support that?

- Lines 371-372: On the contrast, statistically, temperature does not play an important role.

- Lines 379-381: This is not supported by data! In NH_4 elevated areas, the biomass of diatoms and haptophytes are similar.

- Lines 406-408: Hong (1997) show the exponent is > 1 in the Southern Ocean. Other relationship cited here is less relevant to this study, because they are from temperate regions.

- Lines 413-414: Seems circular to me! I am quite confused how the correction coefficient is obtained.

- Lines 435-437: Wrong! abundance-concentration, not abundance-production in those cited work.
- Lines 474-477: This is not a new paradigm, and how each group of dominant species contribute to TEP production is not clear cut yet.