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Interactive comment on "Virus mediated transfer of nitrogen from heterotrophic bacteria to phytoplankton" *by* Emma J. Shelford and Curtis A. Suttle

Anonymous Referee #3

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This study describes a set of experiments that were designed to examine phytoplankton utilization of N from viral lysate of heterotrophic bacteria in laboratory and in natural seawater. The results showed that N in viral lysates can be used by primary producers. The approach and the results of the experiments are interesting and informative. The statement by authors is correct: there is a real lack of data showing the transfer of nutrients due to viral lysis of other organism. This study makes a nice start to fill in the gap. I would like to recommend the publication of it, but I have some comments that need to be addressed.

For the laboratory culture experiments, there are 6 treatments:

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1. DC2 + Bac + Lysate 2. DC2 + Bac 3. DC2 + lysate 4. Bac + lysate 5. Bac only 6. DC2 only

In these treatments, the total volume 200 ml containing 10 ml DC2, 100 ml bac, 10 ml lysate + N,P free artificial SW in various combinations depending on treatments.

1. The initial DON and no DON were not measured in all the treatments.

For the laboratory study, this lack of DON measurement might not change the relative effects between treatments, but this changes the estimate of N uptake and subsequent estimate of N flux by viruses.

For the field study, no DON data in the ultrafiltrate from natural seawater might have affected the estimate of N uptake based on the N15 uptake from the added lysate. Thus, the application of the N15 uptake to the N utlization in the water column is weak.

The authors need to address this issue and give consideration of their estimate of N flux via viral shut.

2. Dissolved organic phosphorus was not measured. How could DOP affect the results?

3. The lysate contains viruses. What effect is on the treatment (Bac+lysate)?

4. Lysate only treatment may be needed as a control to if it is contaminated with bacteria and its released N contributed to growth of DC2+lysate. Without this treatment, the explanation of growth of DC2 in DC2+lys is weakened. Can this serve as evidence that DC2 is capable of using dissolved organic matter?

5. Giving the scope of this study, section 4.3 is mostly speculative, particularly in these estimates of global pictures.

Specific comments: Page 5 line 13 and 14 “The ultrafiltrate was stored in the dark at 4°C for a year until used in experiments” give a reason why this does not affect the results if it is contaminated with bacteria.

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Page 12 line 16 and 17 “In FRP, the SW+lysate treatment decreased to less than 0.04 μM before climbing again to 0.25 μM, correlated with a spike in bacterial abundance.” The sentence needs revision

Page 18 4.3 section, some estimates on the yearly basis are based daily bacterial loss rates of 10-20%. This is not right, the converstion of bacterial loss rates by viruses should be converted to yearly loss rate.

Page 27 Table 2, please explain how you get the multiplicity of infection (MOI) for the experiments of laboratory, SI and FRP, SB and JP. Page 4 line 4 “by adding 5 M bicine. . ..” or 5 mM? Page 26 line 4 “phosphate (PO42-), ...” should be PO43-,

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