Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-276-RC1, 2017

Biogeosciences Discussions
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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

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Received and published: 25 August 2017

The study of Shelford and Suttle investigate the re-mineralization of nitrogen from bacterial lysates and its transfer to primary producers in laboratory and field experiments. Their data indicated that the viral shunt is an important pathway in nitrogen recycling. Subsequently, the authors showed the whole picture of nitrogen regeneration and transformation process from bacterial lysate to primary producers.

Main concerns: The Introduction is too simple. The authors should provide more background information such as the relationship between primary producers (e.g. Syn) and heterotrophic bacteria with and without viruses as a linkage. In addition, why use Vibiro

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in the experiment? Any previous study indicated there is possible relationship between Vibiro and phytoplankton? Furthermore, Why N is important to phytoplankton? I think such information will help readers of Biogeosciences. It is interesting that in the field experiments, picophytoplankton such as Syn did not grew up with lysate. Please discuss this.

Minor comments: P2 L19: Please use “bacterioplankton” instead of “bacteria” to avoid possible confusion between biological and oceanographic definition. P2 L4: “which evidence suggests can support”? P2 L8: from “cells” into seawater. P4 L3: Please mention that Syn is not axenic here. P4 L6-8: “nutrient limitation“ does not mean N limiting. P5 L3-5: I am not sure whether the authors removed the 15N in medium (that is, not used by Vibrio) when they prepare the lysate for field experiment. P5 L14: The ultrafiltrate was stored for one year and bacteria grew. How to demonstrate these bacterial population is similar to in situ one? How about their nutrient (e.g. C and N) limitation condition, which may affect their utilization of lysate? In addition, if bacteria grew, I think viruses also grew. P9 L13-19: Usually people wash the bacterial cells on GFF filters to avoid possible effects of attached POM. P11 L11: Please clarify the demonstration of “N-limited Synechococcus strain cells”. According to the Methods, DC2 strains were grew in modified artificial seawater with bicine and NH4Cl until the end of exponential growth. Any data of DON or ammonium in the culture? In addition, near the end of exponential growth, the dead Synechococcus cells would release inorganic and organic nitrogen as well. P11 L17-18: It seems bacterial abundance in the DC2 group was higher than bacterial assemblages at the beginning (Fig. 2C)? Please consider and discuss the possible impacts of Synechococcus-associated bacteria on the results. P12 L2 and Table 2: Maybe it is better to provide total amount (u mol), not concentration (uM), of N released and N addition to experimental incubations. P12 L13-15: Why in the filed study viral-lysate addition had no apparent stimulation of picocyanobacteria in abundance? P12 L21: It is always confusing when they said “bacterial growth by addition of bacterial lysate”. Maybe change to “Vibiro lysate”. P17 L10-11: Confused here. Natural bacterial community structure, phytoplankton composition and

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N concentration in the lysate, which results in the incomplete or complete uptake? Table 2: Vibrio sp. Strain PWH3a abundance pre-virus in SB and JP were 7.89×10ˆ10? Ten times lower than in SI and FRP? Fig. 1: I suggest the authors change DC2 to Syn. Fig. 2: The difference of NH4 and bacterial abundance at Day 0 should be discussed. Fig. 3. No title for X axis. Fig. 5. Please give a title for this figure.

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