Comments and Suggestions for the Author:
In the manuscript, the authors isolated a lytic vibrio phage that targets *V. parahaemolyticus* whose its genome was sequenced and annotated. The phage GHSM17 was capable of suppressing some *V. parahaemolyticus* cell growth *in vitro* and was tolerant to a relatively wide range of harsh environment, thus it might be a potential biocontrol for the bacteria. The authors also bioinformatically analyzed the phage genome which the results classified it in genus Maculvirus. Their finding scientifically sounds and most results match their claims. The work is quite interesting and should be of interest to the phage audience.

Major comments and questions:

- Since the authors claimed that the phage GHSM17 is lytic phage, do authors have any experiments showing that the phage does not enter lysogenic cycle? Even though the phage genome does not contain any lysogenic-related genes, it might be possible that some of them might not be predicted during the annotation. I would suggest the author to perform some wet experiments to support their claim.
- It seems to me that the re-growth of SC123 occurs after 4 hours; however, the cell number as measured by OD does not much increase after that. The authors then claimed that SC123 was still suppressed by the phage. I would suggest that the author should try doing the experiment at the longer period to see if the cell would re-grow or even measure the number of viable cells at the end. This will help illustrating whether the OD that was measured are from the cell debris or live cells and it will make your claim much stronger. (It would be also nice to explain why the authors included SC123 into the experiment.)
- In the genome comparison, KF2 showed the highest ANI to the phage GHSM17. The author should indicate the number of their identity. This might be informative to show if the phage GHSM17 is a novel phage (For your information, based on ICTV taxonomy, genome identity more than 95% are classified as the same species).
- The author should be aware of indicating the number of host range since this number might be very varied as the hosts are used. I would suggest the author to use text saying something “relatively broad host range” instead of using number.

Minor comments

- Do the authors have “n” number for the measurement made from the TEM image? Please indicate “n” number and add standard deviation.
- Line 62: Italicized the scientific name
- Line 92: change “terile” to “sterile”
- Please double check the writing in “Material and Methods”
- Figure 1A: Please add scale bar and add magnified image of the plaque
- Figure 2B: the one step growth curve seems strange to me. Could you explain why the graph does not appear sigmoidal as it should when the phages are released from the host cell?
- Figure 3C: Please add standard deviation
- Line 284: please add a comma
- No references in text to supplemental figure S2
- Line 294-295 is not explained in the figure.
- Line 311: it would be nice to illustrate the 7 clusters in the figure.