I think most of the comments I raised previously were considered and I appreciate that several points have been nuanced.

Still I think I would have few main concerns that remains:

First, while reading the manuscript it is not clear what material belongs to the main text and what material belongs to supplementary. Consequently, the statistical analysis still appears quite redundant especially for fecundity and mortality measures. I also suggest to include a visual for the workflow including the different experiments, the number and the origin of the individual considered and the test used to analyze them in order to help to keep track with the different analysis done.

Second, relying on experimental infection the authors demonstrate that not all the exposed Daphnia develop expected infected phenotype (the so-called *white phenotype*). Considering that for several phenotype traits those exposed Daphnia exhibit intermediate value compared to healthy and infected Daphnia, could one consider that exposed Daphnia are simply infected Daphnia that do not display all the symptoms of infection with the same intensity and thus don't display the white phenotype? If so, how would that impact conclusions drawn on natural infection on which the infection status was evaluated on the basis of the white phenotype only? Still on this point, it is several times mentioned that the high virulence of the virus could explain the low prevalence in the investigated ponds. If some daphnia could potentially be infected without having the white phenotype, it is possible that the prevalence is not as low as expected. This point reaches a previous point made by the recommended Editor.

L. 178 and 180, it is written that experimental infections were conducted on 23 and 44 juveniles each time obtained from 11 distinct mothers. I am assuming that for practical reasons those juveniles result from clonal reproduction and thus I was wondering if the fact that some of the juveniles could have the exact same genotype caused problems in terms of independency of the data and if so how this was considered in the analysis.

<u>Minor comments</u>

L. 137 consider adding the duration of the survival experiments.

L. 465: Please explicit here the difference between the two ponds it is referred to.

L. 535-53: Slower daphnia being less parasitized because they encounter the parasite less is appealing, but this does not align with the observation that healthy daphnia are faster than exposed Daphnia.

L. 545: a lack of cost to resistance associated with *Pasteuria* resistant might be linked to the fact that a great part of the resistance to this bacteria is constitutive, which makes the comparison may be not as relevant depending of the basis of DIV-1 resistance.

L471. It is stated that Daphnia predators were found only in La Villette pond. Could the fact that the interactions the predators were already in this pond have influenced some of the trends on Daphnia phenotypic differences? For instance, predators could have selectively predated bigger infected individuals and hence this phenotype could be less represented in the pond.

Table1: Were individuals from both ponds pooled in the analysis on the size? Ideally the pond effect should be included in the model. If it is the case, then each pond should be named instead of the line called "both" in the table.

Supplementary Tables: Consider adding caption or reframing the table so there is one table for each dependent variable, the explicative variables in lines and the model statistics in column (df, statistic, p-value).