After the first round of reviewing by PCI ecology of the experimental project "*Experimental test for local adaptation of the rosy apple aphid (*Dysaphis plantaginea*) during its recent rapid colonization on its cultivated apple host (*Malus domestica*) in Europe*.", authors have answered accordingly and adequately the main questions that I was asking and issues that I pointed. Also authors modified figures and added figures and text for clarification of the different approaches and methodologies.

More description on the fitness value acquisition was provided: two different absolute fitness values will be used as i/ colony growth rate, ii/ the aphid sizes for the three categories (larvae, winged &apterous) and will be run separately in the models as different fitness dependent variables. Also fitness value measurements were described more precisely and seem adequate to me as the local variability will be accounted for and standardized with referent susceptible Golden Delicious genotype behaviors.

The tree selection was mainly done on the two geographic/genotypic groups corresponding to Belgium (same as west France) vs Spain. No wild Eastern French genotypes were included (Cornille et al 2015). However more genotyping data are being analyzed for each tree genotype and will be integrated in the result. Additionally, genomes of the 28 genotypes are to be sequenced and will be available for all the tree genotypes and R genes will be scanned. Possibly presence of R genes could be further described and accounted for in the model.

Tree ecophysiological traits will not be evaluated anymore as they supposedly do not bring more support and valuable information to the authors. I believe this is not completely true (and may be of very good use for other types of studies more on the trees adaptation) but regarding the size and ambition of the experiment and the work to be done, it makes sense to decrease the volume and number of modalities.

As for the possible systemic response of trees resulting from the multiple infection of the eight parasite genotypes, authors propose to add a leaf effect into the model to account partly for this variability. However I do not understand this claim: "each aphid genotype will be randomly infested on each leaf of each tree, at least eight times, we will therefore control for this systemic infestation". How is it possible to control for systemic infestation: if a tree genotype shows a particular resistance toward one aphid genotype amongst eight wherever they are inoculated, it might also increase the overall resistance to the other seven.

In conclusion, overall I believe that the author propose a sufficiently robust study to be recommended on PCI ecology. So I recommend this Study Information on PCI ecology and I am looking forward to having a look at the results.