

Review of Yang et al. for PCI

Paris, 21 December 2017

Dear Sylvain,

Both reviewers and I found that your study addresses an interesting topic, and that the data and analysis you present are generally sound. However, the reviewers point to a few statistical inconsistencies and that your conclusions do not follow from the observations and should be tempered. I agree with the reviewers and think that your paper would be improved if you revise it in light of their suggestions.

I single out the major concerns that were raised. First, you could try to control for phylogenetic relatedness and geographic distribution. It appears that taking the data from this study together with that of the accompanying paper (Petrone-Mendoza et al.) is possible. Although such model would be unbalanced you might have some power to dismiss these factors; and even if the results are negative or ambiguous, they would strengthen the discussion. A second related concern is that both the appropriate trait scaling and testing for an interaction between rosette surface and treatment are missing. Including them would also facilitate the interpretation of results. In general, it is highly pertinent to clearly state possible life-history differences between species that would lead to a correlation between mating system and competitive ability independently of ploidy. You need to better explain why positive results in only one out of the three fitness traits measured is sufficient to support your conclusions. Lastly, I would argue that a causal connection deleterious recessives and mating system cannot be established with the comparative data you have. It would be interesting, for example, to perform mutagenesis and check for differential inbreeding depression.

With my best wishes,

Henrique