

In this manuscript, the authors sought to understand the relationship between nutrient availability and investment in the growth-defense trade-off. The authors used clones of rubber trees to understand the within species variation of patterns in growth and defense trade-offs. The authors found limited support for any patterns of growth vs defense investment correlating with environmental factors. This manuscript is of great value to the field and would be of interest to many readers of PeerJ. I enjoyed reading the introduction and felt the article was promising. However, as I continued, there are several issues in the methods and the statistical interpretations that make it hard for me to evaluate the discussion portion of this paper. I suggest the author's make major revisions to the statistical analyses and reassess their findings before resubmitting.

- 1) The authors do a great job at explaining plant defense theories. However, a major claim is that allocation to growth vs defense may differentiate among conspecifics. While this is true, the authors have ignored much of the work done on allocation vs acquisition of resources (van Noordwijk and De Jong 1986, De Jong and van Noordwijk 1992). Individuals may also show variation in growth-defense trade-offs due to variation in nutrient acquisition as well as allocation. Granted, this is difficult to tease apart, the authors should try to explain this as a possible cofactor of their findings.
- 2) Many of these references are decades old. While much of the work on plant defense theory is from the 1980's, there is an abundance of literature supporting many of these hypotheses. The authors should include examples and references to newer supportive work throughout the manuscript.
- 3) I do not fully understand the significance of the "clones". Clones would be genetically identical, and anticipated to respond in similar ways. I am either not understanding the design with clones, or the authors are making conclusions that would not be valid with the genetically identical relationships. Clones would work nicely to pose repetition at sites and make variability easier to understand across sites. However, the authors' purpose was to understand variation within a species, and this can not be concluded from cloned individuals with the same validity as we are missing the overall within species diversity.
- 4) The statistical analyses are not well explained and the authors' explanation of statistical results appears to be incorrect. What do the authors mean by "regressed"? What type of regression? With so many parameters, I am guessing that they used a multiple regression and did a stepwise comparison of models to ensure they included only variables that were important. Did the authors check for any collinearity among the variables? Furthermore, phylogenetic relationships are a major factor in this study and important to support hypotheses. The authors need to run a phylogenetic PCA to get a more valuable interpretation of the data in a phylogenetic context. They observed phylogenetic signal, but did so using Pagel's λ . There are better parameters (especially with a relatively small sample size). I suggest the authors use tools from the following papers to better assess phylogenetic implications (Revell et al. 2008, Munkemüller et al. 2012, Perkovich and Ward 2021). The interpretation of the r^2 values is incorrect. The authors state in line 250 that there are positive correlations. However, the reported r^2 values are very close to 0. Authors should consult Hemphill 2003.

There are a few minor issues that I have noted in the manuscript and below:

Introduction

Much of the introduction is missing newer references that support the plant defense hypotheses explained. The authors do a great job explaining most of the hypotheses, but should include the original papers along with newer publications that support the hypothesis.

Line 48: Replace the word “limiting” to “limited”.

Lines 51-53 should include some newer references along with the original publications.

Line 53: this should cite Herms and Mattson 1992 (full reference in pdf revisions).

Lines 52-55: The growth-defense trade-off is not very well explained. I suggest rewording to better explain the idea.

Line 62: change “were” to “are”

Line 92: insert the word “that” between “defense” and “differs”

Line 107: delete the additional space

Line 109: I do not agree that growth is straightforward. Plants can allocate resources into many different tissues and growth can be measured in many different ways including height, stem diameter, growth rate, and growth in specific structures such as foliage and roots which would all be measured in various ways.

Materials and Methods

Line 159: Place the values outside of the parentheses and put the "N:P:K" inside the parentheses.

Line 183: Define “LMA”.

References

De Jong, G., & Van Noordwijk, A. J. (1992). Acquisition and allocation of resources: genetic (co) variances, selection, and life histories. *The American Naturalist*, 139(4), 749-770.

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Münkemüller, T., Lavergne, S., Bzeznik, B., Dray, S., Jombart, T., Schiffrers, K., & Thuiller, W. (2012). How to measure and test phylogenetic signal. *Methods in Ecology and Evolution*, 3(4), 743-756.

Perkovich, C., & Ward, D. (2021). Herbivore-induced defenses are not under phylogenetic constraints in the genus *Quercus* (oak): Phylogenetic patterns of growth, defense, and storage. *Ecology and Evolution*, 11(10), 5187-5203.

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Van Noordwijk, A. J., & De Jong, G. (1986). Acquisition and allocation of resources: their influence on variation in life history tactics. *The American Naturalist*, 128(1), 137-142.

