Review of doctoral thesis – M. Weiser (2015): Plant body as a behavioural platform – an ecologist's insight

This is certainly one of the best theses I have seen in my life. Martin Weiser is obviously able to pick up interesting and important questions for his research, has a good command of the current knowledge about the issues he has chosen, suggests appropriate experimental setups, and engages his students and colleagues in the subsequent performance and evaluation of the experiments. Martin also has a good command of English (as far as a non-native speaker as myself may judge) to produce accurate description of his thoughts, leading often to brilliant discussion of the results, so much remote from the usual boring statements like "Joe Public achieved similar results, while John Doe has found something else".

The dark side of Martin's highly intellectual presentation and discussion of results is that his prose is sometimes too terse and demanding to digest, at least for us – readers with not so big brain. While reading the thesis, I had this problem particularly with the introductory part, to which I had to devote disproportionally large part of the time I spent with the whole thesis. Luckily this introduction is not too voluminous and the flying pig picture provided much needed refreshment for my poor soul. The only thing I have missed in the introductory part was an attempt to connect the results of the four studies more than by a brief reference to the importance of plant modularity. At least the first two studies both deal with plastic phenotypic responses in above- and below-ground body parts, using quite a similar set of species, so a comparison of the plasticity and its extent for the species studied in both projects would be welcome. This is, in fact, my first question to Martin: **Even if such a comparison was not yet done on the collected data, what outcome you expect? Is there any trade-off between the extents of aboveground and belowground plasticity or should they be positively correlated?**

My other questions and comments are more down-to-earth, focusing on the chosen methods and the interpretation of results and I hope they might help Martin and his co-authors to further develop the manuscripts. I list the questions and comments in a sequence corresponding to the ordering of papers in the thesis, but those that I would like to see addressed by Martin at his defence (if time permits) are emphasized with bold typeface.

pg. 24: In the study of plant plasticity in response to light quantity and quality, you have filtered the pool of plant species of the Czech flora by requiring that the chosen species are perennial hemicryptophytes of mesic, unshaded habitats. To what extent this filter suppresses the variation in functional traits that you have used as predictors of the plasticity in aboveground plant parts? What disadvantages have you seen in not doing such filtering (or parts of it)?

pg. 26, 2nd par: Did you calibrate your measure of *TotalArea* with the actual leaf area? If so, do they relate in a linear manner?

pg. 27, 2nd par: Were the measured parameters of plant bodies log-transformed in the analyses? Why or why not?

pg. 27, last sentence of 2nd par: What does your description actually mean? Have you replaced the treatment factor levels with an average R/FR and PAR for the particular treatment combination or were the block- (tent-)specific measurements used?

pg. 27, 3rd par: I would like to be mistaken, but I fear you have picked up wrong method here. If I understand your description correctly, you have performed variation partitioning between two predictors (R/FR and PAR) and you have deduced presence of interaction between their effects from a non-zero overlap in the explained variance between the two predictors. If so, this is not correct. Variation partitioning is not an appropriate method to test for interaction. Overlap between two predictors evidences <u>just</u> the correlation between the predictors, not the interaction between their effect upon the response variable(s). In a classical two-way ANOVA (or in its multivariate counterpart) with fully balanced design the overlap would be always zero, yet there might still be a very important and significant interaction term. Any overlap found by variation partitioning in your analyses is therefore just an artifact of possibly unbalanced design (if any plants died) and/or of the R^2 adjustment and/or the consequence of the way the factor levels were replaced with the R/FR and PAR readings.

pg. 27, last three lines: Unfortunately, Mantel test is not a very good method (it has low power) for the type of data you have - see Legendre & Fortin paper (2010) in Molecular Ecology Resources 10: 831-844. In addition, your test can be further biased due to the inclusion of traits not much varying among selected plant species (see my earlier comment about filtering of species during their selection).

pg. 28, 2nd par: The interpretation of the components of species response to treatments and, in fact, their sensibility depends much on the choices made in the redundancy analyses: were the response variables (plant parameters) standardized or just centered? Did you log-transform them?

pg. 28, 3rd par (last of the Methods section): “For each ordered set of scores,” … “end of the score set.” I have difficulties to decipher what you talk about here. This description would deserve more elaboration, perhaps a graph with a scheme. Or maybe I am just stupid.

pg. 30, last par: In the light of all your discussion about the magnitude of reaction norms due to changes in PAR versus R/FR ratio, I was wondering about your thoughts on how relevant the magnitude of norms might be to an evolutionary interpretation of phenotypic plasticity? I would think that more than the extent and type of effort made by a plant, the gains such plant achieves by its effort are more relevant, particularly when you compare various aspects of plant body response. Do you have any relevant data coming from your experiment that you could use for addressing such question or - if not - can you suggest how you would quantify the actual advantage gained by having say a large reaction norm for particular body size parameter?

pg. 44, end of 2nd par: I wonder why you have treated - in your second study - the absence of data on lateral spread for non-clonal species as missing values? Would not be better to estimate the typical radius of plant body size for such species or even to specify the later spread as a zero value? Due to your decision, you might have missed some of the most important functional trait constraints on the root plasticity.

pg. 45, 3rd par: While I fully support your decision to log-transform the ratio of root placement values, I doubt the transformation effect can be described as “removing any effect of plant size”. Could you elaborate on this statement? This point seems important as you put much faith in the Discussion into the removal of total size effects in your study. Log-transformation makes the allometric relations in your data linear (and makes the relation additive, rather than multiplicative), but the effect of size must be still removed using the total size as covariate.

pg. 47, 1st par of Discussion: Given the treatment with lower nutrient contrast did not work so well, can you comment on how realistic are the high contrast values in the habitats your plant species naturally occur? I do not believe that following producer’ recommendation on fertilizer rate is a relevant threshold here - plants in agriculture and gardening realms are being notoriously overdosed with nutrients.

pg. 62, 2nd par: were the response variables (root system parameters) log-transformed? Why or why not?

pg. 62, end of 2nd par: I am worried you hit here the same problem as I have outlined for the first study: overlaps in explained variation among predictors do not represent what is called an interaction by statisticians. But on the other hand, I understand you have defined pair-wise interaction terms between seed mass, nutrient level and time explicitly in the model, so I am confused. How these interaction terms were used in the variation partitioning?

I have no comments concerning the last paper, which was already published. I feel uneasy about such simple models that omit much of the field-relevant complexities (here for example the aboveground competition) that might, in fact, reverse the conclusions of these models. But such an scepticism is likely just my intellectual failure.

In the end, I would like to reiterate my appreciation of the high quality of the submitted thesis of Martin Weiser. It proves without any doubts his ability to perform excellent and independent research. I wish him many happy years spent with the plants and their wonderful bodies.

P. Šmilauer

Petr Šmilauer 15 August 2015

Slavče u Českých Budějovic