

## Review of the Ph.D. thesis

### Pavla Doubková: The role of arbuscular mycorrhizal symbiosis in plant performance under the specific edaphic conditions of serpentine soils

Referee: RNDr. Milan Baláž, Ph.D.

The submitted thesis focuses on the functioning of arbuscular mycorrhiza in the unfavorable environment in serpentine-based soils, using the field scabious (*Knautia arvensis*) as a model plants. It clearly states the initial aims and reasonable working hypotheses, which are then addressed in a series of experiments and in the field trials. These experiments are in the thesis presented as six manuscripts, five of them already published in the journals with high impact (*Plant & Soil*, *Soil Biology & Biochemistry*, *Mycorrhiza*), one is still under preparation.

My overall impression from the thesis is great. First of all, five already published articles is the unusually high number of published work for the Ph.D. thesis. The whole work is written in very good English (if I can conclude, I am not a native speaker, of course). The first four chapters in the thesis, i.e. Introduction, Aims, Overview of experiments and Synthesis and summary of results) give the reader very comprehensive information about the topic studied. The information in the manuscripts builds the complex knowledge related exactly to what was stated as the thesis topic and aims.

Due to the apparent qualities of the presented Ph.D. thesis it is quite difficult to state here some reasonable questions. In the following paragraphs are thus mostly the comments for which I do not expect necessarily the applicant's response, there is only one question (printed in bold) for which I would like to hear the answer.

1. In my view, much more emphasis should be paid to the fact that on your experimental sites the serpentine soils were often much higher in mineral macronutrients (N, P) compared to their non-serpentine counterparts (see e.g. Table S1, p. 161). This fact, which itself is rather the exception than the rule, may have far reaching consequences for arbuscular mycorrhiza formation and functioning, but surprisingly it is not mentioned in

the Summary. Instead, the opposite information is presented there (“deficiencies of essential macronutrients are often in serpentine soils”). The results presented in the Summary would be viewed from a quite different point if the reader would know the real nutritional status of soils in your experiments.

2. The term “a model plant species” was in some parts of the thesis confusing for me. When citing the other studies, it would be better and more informative to type the scientific name of organism used instead of “the “model plant species” (for example, for Schlechter and Bruns 2008, 2013 it was *Collinsia sparsiflora*, not *K. arvensis* as I initially thought).
3. I would be very cautious when stating the causal relationship in the situation when the data are only indicative and are based on the correlations only. E.g., the first sentence in the second paragraph on the page 32 is wrong – your study provides no evidence that the soil pH shapes both the composition and species richness of the AMF communities. Considering the direct impact of pH on the important soil characteristics such as nutrient availability, other soil microorganisms communities etc., direct mechanisms of pH on AMF may be under debate.
4. The same comment is valid also for the statement that the richness of AMF communities was also affected by the identity of neighboring plant species (p. 33, the last paragraph).
5. As regards the MS6, if I would be its reviewer, I would expect that the authors will change the basal idea of the article, i.e. that they are comparing the effects of stochastic and deterministic processes on AMF colonization, species richness and community composition. The conclusion that the latter factors dominate (l. 397-399) is incorrect simply because only one (!) factor contributing to the stochastic processes was studied – and nothing can be said about the population if there is only one replicate!
6. **And finally the question: I was surprised that “the long-term process to obtain pure AMF isolates from the non-serpentine sites failed” (p. 20), and the isolate of a different geographical and edaphic origin was used in the experiments. Have you any explanation for that?**

Summarizing my review, I declare that the overall quality of the thesis is excellent and the author clearly demonstrated her ability to conduct solid research and to defend it to the other scientists and produce very nice series of publications. Therefore, I recommend this thesis to be accepted for the award of Ph.D.

In Brno, November 20, 2014

RNDr. Milan Baláž, Ph.D.