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Review of PhD thesis of Alena Vítová

The thesis submitted by Alena Vítová deals with dispersal, recruitment and community dynamics in grasslands. Therefore, the candidate has chosen different experimental settings to alter species pools in different ways or inhibit/foster pathways of colonization after gap creation (soil disturbance). The thesis contains a well written general introduction (chapter 1), where Mrs. Vítová introduces the background of her studies. She also refers to general ecological concepts of species pools and dispersal. Furthermore, she describes the need of experimental approaches to answer open questions of community assembly in relation to dispersal and establishment. After this introduction, the thesis contains four papers of which three are already published in international peer-reviewed journals. Thus, most of the thesis has already be externally evaluated, in case of chapter 4 and 5 by highly recognized ecological journals, ensuring scientifically reliability of the work. Nevertheless, I also assessed these chapters/papers separately and will comment on each of them in what follows.

Chapter 2 (paper 1 published in Plant Ecology):
This chapter deals with a sowing and transplanting experiment, which approaches scientifically interesting ideas of how community assembly works, i.e. if and how species enter an established community. Although this is a rather small study, results are relevant and extend the knowledge of plant species ability to establish in a mature plant community. A specific value of this study is that it covers five years, which is a rather long time period for most ecological studies. Thus, authors will have done substantial field work to gather data for the presented analyses. The only information I miss is where seeds were collected and which effects the provenance of the seeds could have had. Furthermore, the role of the moss and litter layer, which was removed in half of the sowing plots, could be discussed in more detail. However, as journal articles are very restricted in length, there is always something which has to be dropped to keep word limits. This is rather indication of the relevance of the experiment, which revealed quite many interesting results.

Chapter 3 (paper 2 as manuscript):
This study consists of two sowing experiments, again based on extensive fieldwork. The research question could be formulated and mentioned a bit more direct to guide the reader and they might also help structuring the text accordingly (using sub headings), although the overall aim of the study is evident. The authors tested if species are able to germinate and establish and thus they related the success of the specific species performance to their community affinity and habitat preferences, asking for the relevance and strength of the abiotic and biotic filters. As the results are quite complex – with lots of different factors with two experiments– it

might be helpful to give some more results in tables (e.g. the F-test) or in figures (e.g. relationships with Beal index or Ellenberg IV) instead only in the text. However, this is not a critique in terms of content, just a suggestion for presentation. This study gives very important and quite drastic results, as the number of species which established on the long-term is very small (in non-residents), with nearly none of these species reproducing during the runtime of the experiment. The discussion in appropriate and well written, although one could also think of discussing the role of the biotic soil feedback, which might be quite different for non-resident compared to resident species. And again, I miss information on where seeds were collected.

Chapter 4 (paper 3 published in Functional Ecology):
This chapter deals with an extremely interesting and topical study, which tries to disentangle the generative and vegetative propagation. This study is based on a very nicely designed and replicated experiment, although conducted in just one grassland. The whole chapter is very well written, clearly structured and focused on ecologically very relevant hypotheses. It is again based on enormous field work and presents data from three years. This study significantly advances our understanding of gap colonization and characterizes the (temporally variable) contribution of vegetation and generative propagation. It also gives results for four different functional groups. Furthermore, it one of the first times separates the role of seed rain from that of the seed bank. This study is scientifically absolutely valuable and a very good example of a clever way of experimental hypothesis testing.

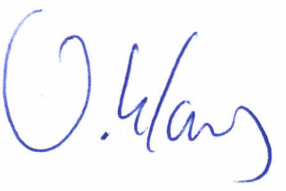
Chapter 5 (paper 4 published in Journal of Vegetation Science):
This study is based on the same experiment as the previous chapter. Here, the focus is set to the spatial arrangement of recruits, especially of clonal spread versus seed rain (and seed bank) recruits. It also assessed underlying environmental factors within gaps. The manuscript is well written and data has been appropriately analysed. Results allow unique and important insight in recruitment, especially concerning spatial patterns of seedlings from seed rain and seed bank. In conjunction with the previous chapter, very relevant mechanisms during gap colonization from both vegetative but also generative sources have been characterized. As these studies are based on work within one grassland only, a next step could be to tests and extent results to a broader range of grasslands and other habitats.

General discussion
Alena Vítová sums up her results in a general discussion, stressing the relevance of dispersal, propagule pressure and establishment as from the seedling status. Although this chapter is well written, it lacks a bit a perspective for further research and just shortly mentions ways to practically make use of the findings from this thesis.

In conclusion, Alena Vítová has submitted a very comprehensive thesis which is based on extensive fieldwork and thorough analyses. She generally showed the necessary technical and scientific skills to hold a PhD. Based on all this, I strongly recommend the PhD committee to allow the candidate admission to the formal PhD defense.

Sincerely yours,

Dr. Valentin H. Klaus



Review of the thesis by Alena Vítová: Plant dispersal and establishment. Important factor s affecting species composition of meadow communities.

The thesis examines grassland ecology, focussing particularly on the question which species coexist in a grassland, the role of disturbance in this coexistence and how they are filtered from some local species pool. It is based on four papers, three of which have already been published in well-established ecological journals.

Strong points of the thesis

Choice of the subject. Although there is abundant literature on species pool, its estimation and effects, there are very few experimental tests of its effects and experimental identifications which species belong to it. The current thesis makes an explicit (namely in the paper 2, partly also in the paper 1) assessment of the species pool and compares the effects found with the indirect (nonexperimental) methods of species pool estimation.

Further, paper 3 and 4 are based on the same experiment assessing the role of vegetative spreading and seed spreading for the community assembly. Although its design looks trivial, it tests effects that are widely assumed to exist in the ecological literature, but there are very few reliable tests of them. The paper 3 performs a direct comparison of both effects for a large set of species.This makes it a really important contribution to our understanding of functioning of emadow communities.

Good experimental design and good analysis of the data. All experiments are properly designed and well described, and the analysis of the data is good (with the proviso of a few questions below). One may say that a thesis submitted at Ceslke Budejovice is likely to have all these qualities, but it is not a general standard and should be appreciated as such.

Weak points of the thesis

In general, I see no noticeable flaw in the thesis; there are only a few (essentially minor) difficulties in the scope of the papers and in their framing. First, although the thesis has a general introduction and a conclusion section, the first two and the second two papers remain a bit separate from each other, each of them with a separate questions and contexts. Also the paper four is a bit aside from the rest in its stress on spatial patterns.

Further, as the role of disturbance for species regeneration is one of the main lines of thought in the thesis, a reader will necessarily be interested in knowing how frequent such distubances are in the studied field system and what is/can be their true role in shaping the community composition. Estimating disturbance rates is notoriously difficult, and we possess only very limited data on it, but the more so such data would be valuable. As effects of disturbance on community richness are now hotly debated again (see e.g. Fox 2013, Trends in ecology and evolution), one may wish to see implications of the current findings.

Questions to be discussed

(to be discussed at the defence)

(*) the author discusses at length (p. 24) both fixed-factor and random-factor anova (and performs both analyses), but does not tell us which one she favours and why, and how to interpret results if both types differ in their findings (as they do in the key effect of residence). This seems to be leaving the decision with the reader and can lead to very ramified structure of a paper if done more than once.

(*) assessment of the total number of seeds per one established plant critically depends on the plant lifespan (p. 31). Do you have assessment of how long these plants can live and how they differ in this parameter?

(*) I like the finding of the first paper that two of the three resident species failed to establish, what the authors interpret as an indication that such tests are not necessarily the best proof of habitat limitation. What approach/data would you suggest if one were searching for a more definitive demonstration?

(*) I am not sure whether I fully understand the motivation of the second paper: is it conceived primarily as methodological study (how to assess species pool, incl. comparison of different indices) or as a study assessing the role of disturbance in establishment?

(*) In the paper two, it would help to know which species invaded the target community from the edges. These species may represent a nonrandom sample of the species present and hence their effect on the establishing plants may not completely represent the effect of the community. Do you think this may be a problem?

(*) I am not very happy about the species list used for the sowing experiment in the third paper. Namely, there are a number of annual species, which cannot be assumed to persist in a grassland community, and their life cycle is very different. Running an experiment to confirm that they cannot survive is hence a bit trivial, and one does not need a Beals index to say that. This opens a broader question how were the species selected? Was there any filtering on the growth form? If not, why there are no woody species there? A random sample of 60 species from the Czech flora, even unweighted by abundance, is very likely to contain some woody species.

(*) The values of the Beals index depend on the overall frequency of species in the source database (it is the probability of finding a species in the plot given its species composition, and as such it is bound to depend on the target species frequency in the database). Was there any correction on that? If yes, what do you aim with this correction (and how it was done)? If not, why the overall frequency should determine likelihood of being in the particular species pool?

(*) The author says that Ellenberg values are based on species realized niches (e.g. p. 56). I know that this is a common statement, but is it really true? How would you define realized niche of a species?

(*) In the paper three, I find strange that dicots were put into one group. This confounds phylogenetic variation with functional variation, but still may have a good reason which I am eager to know. What do you base the expected behaviour of dicots on? (BTW, in the discussion, non-homogeneity of this group is often invoked as an explanation.)

Minor comments and questions

(not to be necessarily discussed at the defence)

p. 21: assessment of the stand properties: I wonder to what degree these values involve a circular argument. In particular, definitions of soil nutrients and substrate stability use vegetation parameters which is making such circular argument likely. These parameters could be used for a passive projection over an indirect ordination, but not as an explanatory variable.

p. 21: how were the three non-resident species selected? Success of the experiment depends on similarity of their niches to the resident species (as the paper 2 shows), and the paper provides little clue to this.

p. 27: table legends do not say what is the response variable

p. 22: *Campanula* has such small seeds that wind can be safely assumed to the dispersing agent

p. 44: the motivation of the two experiments should be better explained (I understand that having two experiments may simply be a contingency, but still these two experiments answer slightly different questions which may help here.)

p. 44: "densities were slightly increased": how much?

p. 45: why a generalized linear model was not used instead of a log transform?

p. 45: "two releves in the target community": more detail is needed here

p. 45: calculation of CWM: have you also tried different weighting of species abundance?

p. 62: where the seeds came from?

p. 51, Fig. 5: I really like the way how the data are presented here. Why the change of r with time was not tested for statistical significance?

p. 73: all recruits were marked also in the control plots?

p. 72: gamma radiation treatment: do you have evidence that fungi and soil biota were left intact?

p. 106, last para: L(r) is always greater than zero. L(r) – r has the property mentioned in the 8th line from the bottom. (It is correct in the figures.)

p. 116: how large was the variation the red:far red ratio? Does it correspond to values that can be physiologically active?

p. 116: could negative correlation (regular pattern) at the fine scale be explained by germination inhibition?

Conclusion

In summary, the thesis convincingly demonstrates ability of the candidate to perform good research in plant ecology. It is based on a solid amount of field and analytical work, it shows a good understanding of the subject and the ability to present results in the form of scientific papers. I am fully convinced that the candidate deserves a PhD. title and wish the candidate success with her further work.

I am happy to recommend the thesis for defence.

Tomáš Herben

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