Review of the Ph.D. thesis by Martin Kopecký

Vegetation changes in Czech lowland forests over the past decades

The thesis comprises four papers, the first examining the validity of the sampling method used in the other papers, two papers describing vegetation changes in two lowland forests in South Moravia, and one paper re-using the data from one of these forests to address a specific topic of impact of an alien plant species. In addition, the introductory chapter makes the links between these four papers and puts them into broader context.

In general, the thesis is very well written and really pleasant to read. The texts are concise, clear, focusing on important points, and own results are always interpreted on the background of relevant literature. A strong aspect of the thesis is skilful data analysis including illustrative graphs that help readers understand the meaning of the results.

The thesis is topical, given the recent paradigm shift in conservation of European lowland forests that used to be managed by humans for millennia. Studies like these are extremely important to provide scientific basis for conservation management planning.

Though in general I am excited about the results and quality of the thesis, I have a few comments and questions:

Paper 1. Vegetation resurvey is robust to plot location uncertainty

1. This paper gives us a good message: analyses from repeated sampling of historical forest vegetation plots are reliable even though the position of the old plots is known with some uncertainty. Although I believe that the main conclusion of this paper is correct, I feel that the study is slightly unconvincing because of the inappropriate sampling design, in which the exactly and approximately relocated plots came from different sites. For example, several approximately relocated plots, but no exactly relocated plots, were from the SW part of the study area. If vegetation change was consistently small in this part of the area, these plots might influence the general pattern in the subset of approximately relocated plots, which would then indicate no larger change than in the subset of exactly relocated plots. To make the analysis convincing, I would recommend sampling one approximately and one exactly relocated plot at each site where exact relocation was possible, and analyse only those sites where both types of new plots were available.

2. Sampling for this study was done only after publication of papers 2 and 3, both based on data sets from approximately relocated plots. What would you do if this study indicated that approximate relocation causes significant pseudo-changes in the examined vegetation characteristics?

Paper 2. Half a century of succession in a temperate oakwood

This paper is based on second repeated sampling of Horák's plots from Milovice Wood, which Jiří Danihelka and myself resampled in 1992. In our paper from 1993 (actually very poor paper written by autodidactic novice ecologists), we interpreted the observed vegetation

changes purely as a result of ungulate pressure, reflecting the fact that at that time the effect of coppice abandonment on biodiversity was hardly recognized. After the paradigm change a few years later, you interpreted the same changes mainly as the result of coppice abandonment, with ungulate impact being a factor of secondary importance.

3. I am afraid that the study site of Milovice Wood is not a good model for testing the effect of coppice abandonment on diversity, because of the strong effect of another factor than succession, which is the impact of ungulates. In your study, you made conclusions based on a data set including 9 plots from outside the game preserves and 37 from inside, and mostly interpreted the results for the whole dataset (albeit the change in species composition was significantly different between the inside and the outside). Moreover, Ellenberg indicator values for light seemed to be uncorrelated with the temporal change in herb-layer species composition. Therefore, I would argue that the interpretation that coppice abandonment is the main cause of the observed change is a speculation not supported by the data, but I will be happy if you convince me at the defence that I am wrong.

4. It is believed that historical forest grazing significantly contributed to high herb-layer diversity of lowland forests. If so, why do you think that in Milovice Wood herb-layer diversity declines under grazing pressure? Is the current deer impact so different from the historical impact of domestic livestock? Can it be due to recent atmospheric nitrogen deposition that current grazing leads to increase in nutrient-demanding and ruderal species, while historical grazing supported light-demanding woodland herbs?

Paper 3. Non-random extinctions dominate plant community changes in abandoned coppices

Interestingly, I realized I reviewed an early version of this paper in the thesis of Martin's supervisor 10 year ago. The current version is considerably improved and in my opinion it is the best paper of the thesis, though the other papers are also good. The newly developed method of Temporal Nestedness Analysis with the original R script is especially noteworthy (I expect it will be often used in similar studies in the future).

5. I just wonder why the title of this paper is so non-appealing. Two year ago I noticed that the paper was published, but I was not attracted to read it, because I felt that the finding that extinctions are non-random just repeats the well-known general fact. Do you know any system in which species extinctions are random? If the title was more specific such as "Non-random extinctions of light-demanding species ...", I would definitely read the paper already two years ago just after it was published.

6. Why did you shift from using Morisita-Horn (abundance-based) to Simpson (presence-absence based) similarity index between papers 2 and 3?

7. Why do you think species with persistent seed bank got extinct more often than those with non-persistent seed bank?

Paper 4. Long-term effects of alien herb invasion on forest plant communities

This is a very useful example of how repeated sampling of historical vegetation plots can be used to identify the impact of alien species. This study is much more convincing than many

others in invasion ecology that are based either on short-term experiments or comparisons of nearby invaded and non-invaded sites.

8. I think that the statement that it is a "common belief" that *Impatiens parviflora* has negative impact on diversity is too strong. Though some people may think so, I don't think it is a common belief.

9. When calculating characteristics of invaded and non-invaded plots, e.g. evenness, did you retain or exclude *Impatiens parviflora*?

10. Why didn't you use Zelený-Schaffers test when comparing Ellenberg values of invaded vs. non-invaded plots? [By the way, blue and orange colours for invaded and non-invaded plots are reversed between the main paper and Appendix S1: don't forget to change it before the paper is published.]

11. In Conclusions, it is misleading to say that there is growing evidence that not all invasive species have negative effect. Invasion ecologists never asserted that all invasives are harmful, though maybe some journalists or militant conservationists may have made such statements. See "The Tens Rule" in classical papers by Mark Williamson from the early times of invasion ecology in the late 1980s/early 1990s: "1 in 10 of those imported species appear in the wild, 1 in 10 of those introduced become established, and 1 in 10 of those established become a pest". Certainly the recent paper by Thomas & Palmer (2015) doesn't provide any solid evidence, because they used a dataset which contained almost no harmful alien species and over-interpreted their results claiming that alien species in general do not negatively affect diversity. Indeed they do, though it is since long well known to invasion ecologists that this concerns only a relatively small subset of aliens.

A general question:

12. How many of the vegetation plots used in the thesis were sampled by its author?

Summary

In spite of a few concerns expressed above, I evaluate the Ph.D. thesis of Martin Kopecký as an excellent piece of scientific work. Its author demonstrated his ability to identify topical scientific questions, analyse the data following the most up-to-date standards, put his results into the context of current literature, and write readable papers that are accepted in high-level international ecological journals. Based on this evaluation, I fully recommend this thesis for defence.

Brno, 5 September 2015 Milan Chytrý