Review of PhD. thesis
SPONTANEOUS VEGETATION SUCCESSION IN MINED PEATLANDS
by Petra Konvalinková

This thesis consists of five papers (3 in English and 2 in Czech). I think that fundamental is the first one named "Spontaneous vegetation succession in mined peatlands: a multi site study", which will be published in Preslia. The second one manuscript named "Environmental factors determining spontaneous recovery of industrially mined peat bogs: a multi site analysis from Central Europe" is very similar but focusing only on industrially milled peatlands. The rest ones more or less only repeat the findings made largely in the first study. Petra maximized the production of papers based on only one data set and I hope that creation of new datasets and new papers based on them will follow in the future.

All the papers with the exception of one manuscript were reviewed and so I have only few questions and suggestions:

Chapter II
Spontaneous vegetation succession in mined peatlands: a multi site study

Methods are not detailed enough to tell us, how the woody species data were collected. I suppose that Petra uses only one composite cover value for each woody species (see Fig. 2 and Appendix 1). Not taking into account if the species occur in herb, shrub or tree layers. I'm interested if Petra tried also analysis with all woody species excluded. I know from my own experience, that excluding of woody species could erase most of differences between natural undisturbed vegetation and late successional stages especially on some block-cut sites.

Chapter III
Environmental factors determining spontaneous recovery of industrially mined peat bogs: a multi site analysis from Central Europe

I haven't seen any 6-15 years old successional stages on milled peatlands with *Ledum palustre* or *Oxycoccus palustris* as stated in Fig. 3. And that is why I think, they are not very common. If we look into Appendix 1 in Chapter II, we also cannot find these species in releves originated in milled sites. This shows they are absent there or occur with frequency lower than 1%. Am I right?

Chapter IV
Restoration of Central European mining sites: a summary of a multi site analysis

I cannot find any numbers in italics indicating approximate estimates as mentioned in caption of Table 1.

Chapter V
Mined peatlands

I'm quite sure that borůvka černá (bilberry) is not the same plant as *Vaccinium vitis-idaea* (cowberry). They differ at least in colour of fruits. I recommend avoiding whole zoological description of locality Soumarský most. The only sentence: "There are few species of water birds on water bodies" doesn't fit well into scientific paper.

I was very disappointed due to lack of any experiments in all presented studies. These could for example show how crucial is introduction of proper *Sphagnum* species into rewetted sites during restoration and how it can accelerate succession towards peat-forming plant communities. *Sphagnum* introduction (practised for example on Soumarský most) is very efficient and relatively cheap restoration measure and I recommend emphasizing it in all possible occasions.

Final decision

Submitted PhD. thesis contains new and valuable findings especially because it covers majority of peatlands heavily disturbed by mining in Czech Republic and in case of successful defence I recommend it for acceptance.

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REVIEW OF PHD-THESIS *Spontaneous Vegetation Succession in mined Peatlands* OF MS. PETRA KONVALINKOVA

In general I must say I am pleased with this study. The candidate has carried out a serious piece of research and, consequently, we now know more about the biodiversity of developing mined peatlands. Moreover, she has expressed interesting ideas how to restore degraded forms. I consider the quality and quantity of the work as more than sufficient to endorse the candidate the title of doctor of sciences. That is to say, after a succesfull defence of course.

Having said this, I do have some general points which I would like to discuss more in detail with the candidate.

1. *The space-for-time approach.* The candidate has used a so called space-for-time approach combined with a large-scale spatial analysis. The advantage of such an approach is of course that developments over much larger time-scales can be analysed than would have been otherwise possible within the limited time frame of a PhD study. However, the approach has also certain serious disadvantages. The candidate herself mentions an obvious one when she compares the effects of 2 different peat cutting techniques on the subsequent vegetation development. Unfortunately the techniques have been used in different time periods implying that the effects of time and technique cannot be entangled. Although the conclusions that the candidate draws are in my opinion right they are strictly spoken not backed up completely by the data that are presented.
2. *The spatial scale used.* The large advantage of the scale used is that it enables comparisons based on at least complete regional species pools –or from even higher scales- instead of those based on a local –often incomplete- species pool. However, in the present analysis a large part of the explained variance is correlated to region-related parameters. In fact this suggests that different factors are dominant in different regions.
3. *Lack of experimental evidence*. Based on multivariate analyses the candidate suggests several major factors affecting the further course of succession. Some of them could have been tested experimentally relatively easily. Unfortunately she did not do this.
4. *Natural succession vs. active restoration*. Throughout the whole thesis the candidate expresses her firm belief in natural succession as a good or even better alternative to

active manipulation. I can follow her quite a bit in this belief but consider the discussion as too strong black-white. She herself sees that obviously as well and advocates a so-called *assisted succession*. Essentially this means that some manipulation is allowed –e.g. raising the water table- but not too much. It is almost classical philosophy: thesis – antithesis – synthesis!

Despite some critical remarks I consider the study as worthwhile, especially also because of the emphasis the candidate puts on its applications for restoration.

Yours sincerely,

Prof. Dr. R van Diggelen

Review of the PhD thesis „Spontaneous vegetation succession in mined peatlands" by Petra Konvalinková

The submitted thesis presents important and novel data about succession in mined peatlands. The data are appropriately analysed and presented in the form of accepted papers and submitted manuscripts. The core of the PhD thesis is formed by two papers, one accepted in Preslia, and second submitted to a journal. Other papers present synthesis and applications. Paradoxically, the thesis ends by a paper called „preliminary results" which really presents preliminary results, valid for the period before the thesis was conducted. Why it was placed at the end, and not at the beginning of the thesis?

Generally I consider the thesis as very good and I can reccommend it for the defence. I have following comments to the particular chapters:

Introduction

I would like to read in this chapter which differences in succession of mires as compared to other habitats you predict and why.

Minor notes:

- I am curious which field of research is widespread, but not popular?
- The terms *mire* versus *peatland* – could it be also a matter of differences between European and American terminology?
- I would translate *Übergangsmoore* rather as *transitional mires* than *mixed mires*
- Such a strong statement that there are no purely ombrotrophic bogs in the Czech Republic should be supported by more sound citations, based on measured hydrological and hydrochemical data.
- The question (ii) on p. 9 is unclear for me.

Chapter II

You write that „peat is mined almost exclusively in raised bogs". It holds for Czech Republic (but in one of the next chapters you are mentioning the case of Hrabanovská černava fen), but not for surrounding countries – there are many mined calcareous fens in Slovakia and Poland, with very interesting and diverse successional development. I think that the issue of succession in mined calcareous fens has deserved at least short note. This concern holds for the entire thesis.

The paper presents many partial effects of measured variables. However, I am wondering about the usefulness of these effects in the context of the study. For example, partial effects of both altitude and precipitation are not significant, but these factors are important in forward selection. It is obviously caused by the fact that they are inter-correlated, and that they are correlated also with temperature. Partial effects of water level are significant, but would they have been significant if soil moisture were measured? It simply seems that these effects, in the way how they were calculated, are strongly dependent on the selection of measured variables. I would prefer either to calculate partial effects only for variables that were selected by forward selection, or to use partial effects only for testing specific particular hypotheses (e.g. testing the pure effect of altutide independent of precipitation and temperature).

Minor notes:

- Figure 2b is missing, and there are also many typing errors (e.g., missing spaces) throughout the paper.
- Nomenclature is not united. The fact that you have used the name „*Straminergon stramineum* and *Drepanocladus fluitans*" in one species list may be confusing for a reader searching directly for a specific species.

Chapter III

This nice study has few drawback:

1) According to new belief, percentage of explained variation in variation parititioning have to be adjusted when comparing groups with different numbers of explanatory variables. Peres-Neto et al. (2006; *Ecology* 87: 2614–2625) have presented the new variation partitioning algorithm that subtracts the random effects of redundant explanatory variables (unfortunatelly this new algthoritm is not incorporated in Canoco software). However, this drawback apparently did not influence the results of this study. I would rather call into question the inclusion of the factor „Locality" (see Table 2, factor 17) into the group of „landscape factors". Why do you think that this factor cannot mask some unmeasured ecological variable?
2) The paper ends with „*reccommendations for practical application*". But are they somehow based on presented data? How the thresholds of 50 cm of peat layer and 30 cm of water-table-decrease were created? According to Chapter 6 (paper with „preliminary results" placed at the end of the thesis), these thresholds were only taken from older literature. Is it true?

Chapter V

I have only minor comments to this chapter:

1) In this chapter a reader of the thesis meets some general informations for the first time. I have in mind the existence of mined calcareous fens and the existence of wet mining technique (without drainage). These informations have deserved earlier mention in this thesis, I think.
2) Again, how thresholds of reccommended water table and peat depth were obtained? And why on page 71 the threshold of peat depth is 40 cm, while in chapters 3 and 6 it is 50 cm?
3) Herbs have usually seeds, not spores (p. 72, point (ii))
4) Is it possible to support the statement about butterfiles requiring nectar from surrounding grasslands by the references? Does it mean that presented butterfly species are the species of small temperate mires rather than of large boreal bogs?
5) By analogy, I would suggest to present exact references for the statements about Holocene history of the localities. These statements are usually based on interpretations of indirect data and it is therefore worth referring on which data they are based.
6) Why are you using subtitles like Botany or Geology? These terms refer to scientific disciplines, not to factors characterising a site.
7) *Sphagnum molluscum* is old synonym of *S. tenellum*, not used in current floras. Using this name thus can be is a bit confusing for non-specialised reader.

Conclusions

I have two comments:

1) How do you define „chance“ here? Is everything you are not able to explain by your data a chance? And why you are mentioning the chance in conclusions, when you have not discuss the matter of chance in previous chapters?

2) You are writing about „*restoration of another wetland community*“ instead of a bog – but is it still the **restoration**?

In spite of presented critique, I would like to compliment this study and to reccomend its acceptance.

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