Review of a PhD thesis

Vojtěch Abraham: Palynological synthesis for the Czech Republic

The thesis consists of four published papers, one manuscript and the general introductory part. For two papers and the manuscript Vojtěch Abraham is the first author. In general the thesis has a logical structure, starting with a paper describing the data contained in the Czech Quaternary Palynological Database, continuing with two methodological studies providing relative pollen productivity estimates in a lowland landscape of the Czech Republic and parameterization of the REVEALS model, and culminates in a synthetic study that reconstructs the Holocene vegetation of the Czech Republic and the Tatra region based on pollen data. However, the paper on phylogeography of *Lonicera nigra* in central Europe, in which pollen data are used to complement molecular evidence, clearly does not fit this otherwise logical structure, both by its topic (focus on a single species) and by the area studied (central Europe). No other part of this thesis builds upon the results of this study. In my opinion the thesis would be much better if this paper was excluded, and actually its inclusion was not necessary, because Vojtěch Abraham was not the lead author of this paper while the other papers are more than sufficient for a PhD thesis.

Apart from this drawback in the overall structure, the thesis is an excellent example of systematic research with logical steps leading towards a clearly defined target, starting with data collection, continuing with methodological studies and resulting in a synthesis. The establishment of the Czech Quaternary Palynological Database and the use of the Landscape Reconstruction Algorithm (LRA) for the interpretation of the Holocene pollen data at the national scale is undoubtedly a milestone in Czech and central European palynology and palaeoecology. The thesis provides a methodological example and strong incentive for further LRA studies in palynology and at the same time it provides novel insights (in some cases perhaps unexpected yet credible) for vegetation scientists, biogeographers and landscape ecologists. Based on this overall evaluation, I recommend this thesis for defence.

Questions and comments

1. Do you think that adding pollen data to the phylogeographic study on *Lonicera nigra* really helped to interpret the history of this species? The pollen of *Lonicera*-type and *L. xylosteum*-type can be produced by other species than *L. nigra*, therefore its presence does not provide any evidence of the occurrence of the target species. Conversely, the absence of *Lonicera* pollen does not imply the absence of *Lonicera.* Moreover, pollen data do not seem to indicate any pattern of *Lonicera nigra* migration that would match the pattern indicated by the molecular data. My main conclusion from this study would be that pollen data totally failed to add anything to the phylogeographic study, but I will be happy if the author convinces me at the defence that I am wrong.

2. Pollen productivity estimates (PPE) were based on vegetation composition in circles around the sites with surface pollen samples. Given that the westerly (or NW) winds prevail in the study area, wouldn't it be better to shift the circles slightly to the east with respect to the site with pollen sample? Or to use west-east (or WNW-ESE) oriented ellipses with pollen sample within their western part?

3. Why did you select a lowland area for the PPE study? As dry lowlands represent an extreme case of the Czech landscape types while the thesis aims at reconstructing Holocene vegetation of the whole country, wouldn't it be better to do such a study in a mid-altitude landscape?

4. In the calibration study of the REVEALS model, pollen spectra from the shallowest samples of different pollen profiles were compared with current vegetation. However, some of these shallowest samples were from depths over 50 cm, suggesting that they may have been quite old. Can we believe calibration which is based on comparison of such samples with surrounding vegetation sampled within last 15 years?

5. The last paper of the thesis is a monumental LRA-based synthesis of data collected by many researchers over several decades of palynological research. The result is a revised interpretation of the Holocene history of Czech vegetation with important implications for various subdisciplines of ecology and geography, and also highly relevant for applications in nature conservation and forestry. However, in my opinion this paper strongly suffers from choosing an inappropriate framework of searching for match of the potential natural vegetation (PNV) to the pollen-based reconstructed vegetation of the past. Although the authors admit that this approach has limitations due to conceptual differences between PNV and pollen-based reconstructed vegetation, it is obvious from various statements in the paper that they misinterpret the conceptual base of PNV and of 'reconstructed natural vegetation' (RNV) sensu Mikyška map.

Both PNV and RNV refer to current climatic conditions, not to any climate of the past (Tüxen 1956: 'heutige potentielle natürliche Vegetation'; Bohn et al. 2004: 'current natural site potential based on vegetation'; see also recent discussion on the PNV concept; Neuhäusl 1963 and Moravec 1998 explicitly define RNV as vegetation corresponding to current climate). RNV sensu Mikyška map is *not* the state of natural vegetation in the past before its destruction by humans, as asserted by Abraham et al. on page 118. It is climax vegetation corresponding to current climatic conditions and abiotic habitats that would be here unless destroyed by man (quarries, opencast mines, water reservoirs, drained land, ...).

In general, both PNV and RNV is a hypothetical climax (most mature) vegetation corresponding to current climatic conditions, while the vegetation reconstructed based on fossil pollen is actual (i.e. not just climax, but also disturbed and early successional) vegetation of the past. Because of this fundamental conceptual difference it makes no sense to compare PNV or RNV with pollen-based vegetation of the past.

An example of the PNV misinterpretation in the thesis is the conclusion that *Betula* and *Pinus* are underestimated in PNV. You cannot tell this based on your data. I would argue that these trees are not underrepresented in PNV, because PNV is hypothetical climax vegetation, in which these trees are very rare. In contrast, you reconstruct actual vegetation of the past, a landscape containing different successional stages with many niches suitable for these early successional trees. On the other hand, the conclusion about underestimation of spruce in PNV is correct so long as the REVEALS models and input data are correct.

An alternative (in my opinion better) framework and structure of the synthesis paper would be the focus on the reconstruction and description of actual vegetation of the past, with suggestions for correcting the published concept of PNV, especially with respect to high spruce representation at mid-altitudes. It would be much more realistic than accepting the published PNV as the truth and use it as a benchmark for asking the question 'When was vegetation natural?', as put in the current manuscript (page 128). Actually it was never natural in the sense of PNV, because never in the Holocene was the Czech landscape covered by a continuous climax vegetation.

6. Are you satisfied with the comparison of the REVEALS results with PNV units when each unit is characterized by a single 'typical' relevé from the explanatory text of the PNV map? There is a considerable variation in species composition and relative abundances of species within most of the PNV units, which can hardly be adequately described by a sample of $n = 1$. This reliance on species composition and relative covers recorded in a single relevé sharply contrasts with quite sophisticated statistical approach used for the pollen data. The results of such comparisons are clearly error-prone and can be misleading. If you would like to do such a comparison, I would recommend using summary data from sets of relevés representing each PNV unit (some kind of constancy or cover-constancy synoptic tables). However, my recommendation is not to do such a comparison at all because of the conceptual mismatch between PNV and pRNV.

7. To what extent can we trust the results of the reconstruction for the Tatras given that *Larix* pollen is not considered in the analyses?

8. You are giving a strong evidence for widespread occurrence of spruce at mid-altitudes of the Czech Republic before the establishment of spruce plantations in the early 19th century. This is a strong challenge for the current paradigm of Czech nature conservation, which considers spruce occurrences on mesic soils at mid-altitudes as anthropogenic. What would be your message and recommendation to our nature conservationists and foresters?

Technical comments (not for discussion at the defence):

1. PNV mapping is not based on preferential sampling of maximum richness. Actually many PNV units are characterized by low richness (p. 117).
2. PNV/RNV does not predict what vegetation would develop after cessation of human activities (p. 118). It is a hypothesis about how mature (climax) vegetation would look like today if it was not affected by humans in the past.
3. Legend in Fig. 2 (page 121) is difficult to follow, especially because some colours are similar. It would be helpful if you arranged the legend items in the same order as they appear in the graph, in a single column rather than two columns.
4. Please consider avoiding mixed BP and BC dating in the same paper, it is rather confusing for the reader.
5. How could *Corylus* expansion around 8650 BC be a result of Bond event 7 around 10.3 kyr BP (p. 126)? The effect would happen 350 years before the cause. Or is one of these dates a raw ^{14}C-date and the other is calibrated? If so, it should be clearly indicated.
6. Table S1 (page 152): Why are there so small total percentages of tree species for some units (e.g. units 31 and 35 have the total tree cover only 10%)?
7. There are many typos in the Latin names of vegetation units in Table S1. Please correct.

Brno, 30 January 2015 Milan Chytrý