Review of the PhD thesis of

Irena Šímová

Mechanisms of species –energy relationships across spatial scales

The thesis is based on four papers, of which two have already been published, one is submitted, and one is just intended for submission. In all but one paper, Irena Šímová is the first author. The fact that two papers have been already published in respected international journals can be considered as a kind of proof of their scientific soundness (even thought that I am far from believing that everything what went successfully through the review process in an international journal is correct and valuable). Nevertheless, based on my own reading, I am convinced that the four papers constitute important scientific achievement, presenting novel approaches and finding for the science, are scientifically sound, well written, and clearly demonstrate the ability of the author to perform independent scientific work and so are sound basis for awarding the candidate PhD. In fact, I believe that the two papers that have not been published yet will finally be accepted for publication.

Saying this, however, I should also note that the thesis is written from the macroecological point of view, whereas I am myself typical community ecologist focused on mechanisms within communities, i.e. from completely different perspective – so I will not agree with many points and approaches, or will have different opinion in many cases, or would use myself other approaches. This does not decrease the scientific values of the submitted thesis, I am sure that macroecologists would have analogical (but opposite) reservations to my own work.

So, the main conclusion of my review is the unequivocal recommendation to accept the thesis as a basis for awarding the PhD to Irena Šímová.

In the next text, I will present some comments on individual chapter, and I will pay particular attention to the papers that have not been published yet. Minor comments (not to be discussed, unless the candidate wants to) are shown in *italics*.

Chapter I

Whereas I like the approach used in the chapter 1 (i.e. the expected relationships under individual hypotheses are stated a priori, and then the relationships in real data are are compared with expectations), there are some technical, and some not so technical problems with this approach here.

The most important is the testing of the effect of species pool. Here (similarly as e.g. in papers of Zobels), the species pool is taken unequivocally as the independent variable, affecting the species richness. We (independently me, and Tomas Herben) criticized this approach (see

Herben T, 2000. Correlation between richness per unit area and the species pool cannot be used to demonstrate the species pool effect. J. Veg. Sci. 11: 123-126.

Leps J. 2001 Species-pool hypothesis: Limits to its testing. Folia Geobotanica 36: 45-525.

As a matter of fact, there was a whole forum in Folia Geobotanica about this:
Herben T. 2001. Species-pool hypothesis - Forum Folia Geobotanica 36: 1-1..

The idea is that the size of species pool estimated from the phytosociological data (as in Sádlo et al.) includes the effect of competition – one can argue that the species richness of highly eutrophic grasslands is low because their species pool is low, but also, that the species pool of highly eutrophic grasslands is low, because the communities are species poor themselves because most of the species are competitively excluded from them.

Then there are some technical problems with real testing. One is the number of individuals, which was represented by number of ramets. First, I can hardly imagine how this was estimated in the field in, e.g. tussock grasses – have you really counted the individual tillers – generally, the operational definition of ramet for so many species must be extremely difficult. Could you explain this a little? Second, rarefaction (but probably also the selfthinning hypothesis) expects random selection of ramets, which is very unrealisstic for clonal plants where the ramets of a species are clumped (thank to this, the richness is usually overestimated by rarefaction). Could you comment on this?

Data analyses: I would appreciate some info about F df (both numerator and denominator) for individual analyses presented in tables – particularly for the hierarchical models. Which Type of Sum of Squares were used? If it si Type I (sequential), I am afraid that it si sometimes default in R, some explanation of the order in which variables entered the individual analyses would be useful. Chao index: you claim, that it is independent of both, alpha and gamma diveristy. This seems to me strange: how can an index independent of both measure heterogeneity, which must be positively dependent on the ratio of gamma to alpha? Could you please explain this? Finally (need not be discussed unless you wish so): the assumption of normality applies to a stochastic part of a statistical models. Consequently, the fact that S has normal distribution (p. 16 bottom) is not relevant, because the residuals after fitting the model should be normally distributed, not S itself. Transformation of independent variables is possible, but your justification is, in my view, not correct – by transformation of independent variable you change just the shape of relationship, but not the statistical properties – there is no assumption that the independent variables should be normally distributed, because they are expected to be „error free".

Discussion – there was rather contentious discussion in forum in the September 2010 issue of Ecology. I think that at least some of the papers might be discussed here (particularly R.J.Whittaker seems to be upset that his criticism is not sufficiently appreciated).

In places, I would prefer less rigorous conclusions (e.g. in Table 4 - I would prefer the term not supported to the term rejected [just, remember the Type II error rate].

Finally, I do not like omitting the outliers in regression (which of course improves R^2, as in your regression of biomass on RPM). If there were outliers in your calibration set, there are undoubtedly also in your estimated data set. With omission, the R^2 gives too optimistic impression about the precision of estimates (would be nice to see the outliers also in Fig. S1).

Chapter II

In this chapter, the theories are evaluated on the basis of forest plots where individual trees are recorded. This makes the situation much easier, because here, each tree can be regarded as an independent genetic individual.

Here a have just one question – The correlation determination by minimum climatic values is interpreted (more or less unequivocally) as support for the climatic tollerance hypothesis. Whereas such interpretation is undoubtedly possible, many limit of distribution of many trees is very probably not determined by the low winter temperatures, but by their ability to get enough photosynthesis during the vegetation season (i.e. e.g. by growing degree days). Is really the determination by minimum support for the climatic tolerance? (As a matter of fact, I believe that the climatic tolerance is the most important factor, I am just not sure about unequivocality of the support by the importance by minimum temperature effect.) Were the growing degree days or some "sum of effective temperatures" ever used in similar analyses?

Chapter III

As one of the predictors of species richness of dragonflies, the species richness of plants was used. I would believe that the species richness of plants is affected by similar factors (not included in the model) as insects (and so these are correlated, and so one can be used as a predictor for the other one), rather than plant species richness affects the diversity of dragonflies. What mechanism would you suggest for this effect?

Fig. 2 – dashed lines are not standard errors, but predicted values $\underline{+\ s.e.}$

Chapter IV

Interesting paper asking interesting questions, nevertheless, as usual at studies at this very broad scale, there are always some methodological uncertainties.

I would like to know, how many values for traits were the missing. Use of have you any idea, how much could be the genus average misleading?

Also, I would appreciate to have numbers of units used in each analysis. Apparently, there were considerably more units for the grid cells. So, I would be extremely careful in interpretations of scale dependence – in particular, the fact that the relationship became non-significant for the SALVIAS plots can be just a consequence of decreased power of the test.

Mean of any subsample should be an unbiased estimator of the mean of the whole sample. I am thus surprised that fixing the number of species resulted in loss of significance in SLA. (p. 78). Do you have any explanation for this? Could this be just a consequence, that low number of species used for the calculation of mean increased the proportion of noise and thus decreased power of the test?

It might be a good idea to provide explanation of USGS acronym.

Please, be careful with the meaning of ln *and* log. *Apparently, both are used for natural logarithm,* ln *in chapter II,* log *in chapter IV.*

Nothing is highlighted in Table 5.

In České Budějovice, May 18, 2012   Prof. Dr. Jan Lepš