Review of the PhD Thesis “The effects of productivity and other environmental factors on the forest herb-layer diversity” by Irena Axmanova

Prof. Alessandro Chiarucci

Dipartimento di Scienze Ambientali “G. Sarfatti”, Università di Siena, Italy

General comments

The PhD thesis presented by Irena Axmanova contributes to the debate about species richness and productivity, by specifically investigating this relation in the herb layer of temperate and continental forests. In this type of habitat, the shape and the meaning of species richness-productivity relationships are still not well known. The thesis is organized into four research papers which are summarized into a synthetic overview.

This thesis represents an good contribution to modern ecology and vegetation science. The investigated questions are coherently framed into general theory and problems. The specific analyses are performed according to well designed field experiments and the quality of data is generally high. The results are analyzed using simple but proper statistics and are clearly expressed. The interpretation of the results and their framing into the present knowledge is correct and updated. The knowledge and use of basic and specific literature is adequate.

My general evaluation of this thesis is very positive.

A few specific comments of the four main chapters are reported below.

Paper I

Among the four chapter, this study is in the most advanced stage, being already published in *Global Ecology and Biogeography*. I already had the chance to read the paper and found it very interesting.

My main point of this paper is about the definition of what is herb layer within a forest. It is said that juveniles of trees and shrubs were not included in the analyses (I understand nor in species richness nor in biomass measurements). However, in forests there are several species which can show scarce lignification processes (e.g. some *Lonicera* or *Teucrium* species) and it is not clear to me if those were eventually included or not. In addition, these species together with the seedlings and/or saplings of woody species could represent an important proportion of the community biomass in the forest understorey. I think that it could be interesting to have some insights of the patterns here investigated but including these information (of course as a follow up of the study and not now).

The variance partitioning procedure here adopted for investigating the relative importance of productivity *vs.* tree canopy cover and soil pH is interesting since it shows that these factors can have comparable importance and complex interactions. However, the trends of the pH and canopy cover effect on species richness were not explained. A small table giving the trend of single variable (*e.g.*, linear positive, negative or unimodal) would have completed the picture.

Another important point is about the possible inclusion of litter data. In the classic study by Al-Mufti *et al.* (1977), the unimodal relationship between species richness and biomass was calculated including litter biomass. The inclusion of litter biomass in forest ecosystems could provide significantly different

information, since litter biomass is expected to be very high. In addition, the inclusion of litter data could provide a better information about the real site productivity because of the inclusion of data about tree layer.

Minor points:

I think that the term “grain” would have been a little better than “focal scale” for describing the plot size at which the data are collected. In this case, the two components of scale “grain” and “focus” are almost corresponding, but grain is more immediate.

Paper II

This paper aims to investigate the relative importance of competitive exclusion *vs.* species pool limitations in controlling the species richness-productivity relationship. The basic hypothesis is that in the specific investigated ecosystem, few species are adapted to the high productivity conditions and thus a lower number of species can expected at the higher level of site productivity.

This paper is very clear and logic. My main point is the lack of a true test for understanding the possible role of species pool pool limitation. I think that the addition of richness data (at least their estimates) of species adapted to low, medium and high productivity could have added a lot of value to the paper. I understand that this task is rather difficult and data might have not been available. However, some data could have been obtained from the lists of species sampled in the plots.

Another minor point relates to the biomass variables. Two types of biomass data were used here: 1) annual biomass of graminoids, non-graminoids herbs and leaves of deciduous trees and shrubs; and 2) the same added to 30% of the (total ?) biomass of evergreen dwarf shrubs. If these latter also consider the woody parts, maybe 30% is too much. I think that the inclusion of all standing biomass data of the species considered for species richness estimation could have been interesting. In particular I would have used the two types of available data: complete standing biomass (total biomass of all species considered for richness measures) and estimated annual productivity (total biomass of herbaceous species, leaves of deciduous woody species and 30% of evergreen dwarf shrubs).

Paper III

This chapter aims to verify a phytometer approach for assessing forest site productivity. The idea is a potentially very useful, given the difficulties associated to estimate real productivity. However, I have several doubts that a single cultivated plant species could provide reliable estimates of real productivity in natural ecosystems, especially when environmental conditions are far from those typical of agricultural or horticultural sites or spread along wide environmental gradients. In this case, the forest soils were rather acidic (mean pH = 3.8) and I guess that this can make a difference.

My points here are:

1) Do you have enough evidence that *Raphanus* productivity could be used as a reliable estimate of those forests?
2) Do you have any idea of how different could have been the results in case of adopting other species as phytometer?
3) Why not using a mixture of plants as “phytometers” in place of a single one? Could this give a better idea of productivity along an wide environmental gradient?

Another important point to me is: why did you not present any result showing the (eventual) correlation between herbaceous biomass harvested in the forest understorey and the biomass obtained by the radish phytometer? I think this could have been a fundamental evidence about the possibility of using the phytometer approach for estimating productivity in natural ecosystems.

The differences observed in the coefficients (including the signs) of the GLM of radish biomass *vs.* forest herb layer biomass are expected to reflect a difference in these two productivity parameters as function of environmental conditions. However, the concerns expressed at the above points 1, 2 and 3 could make difficult to real understand the potential use of radish as phytometer in natural ecosystems.

Minor points:

I think that the higher relation observed between biomass values and total nutrient content than between biomass values and nutrient concentration is due to the fact that the first value are calculated using biomass as multiplier. Given the fact that "Total nutrient content" is calculated by multiplying "Nutrient concentration" by "biomass", it is obvious that it is related to biomass (even in consideration of the much higher values of biomass than concentrations).

Paper IV

This chapter aims to verify the possible use of structural data of plant communities in order to predict vegetation biomass and productivity. This is a very important and basic issue in vegetation science and the insight provided by this paper are really useful. Most of the data used in this chapter are not in forest communities, so deviating a little from the main aim of the whole study (this is not a big bias).

It is not clear if specifically collected data or existing data were used for this chapter. If the data were specifically collected for this study, why not getting more accurate data on plant species cover and height than the simple ordinal values of the Braun-Blanquet scale. It is well known that the distribution of species cover is usually highly skewed and, consequently, the species cover is more likely to be close to lower than higher values of each cover class. This can create bias, especially in species with higher cover values.

In figures 1-4 the measurement units of *x*-axes are not evident. I guess they are in terms of biovolume (cm^3) or something similar but this could be made evident..

In table 2 I would add the mean column and raw values, in order to see the mean fitting value for vegetation type (column) and model (raw). This could also help to read the results (e.g. by saying that the "Biomass estimate median" model was the mean best fitting model across vegetation types.

Prof. Alessandro Chiarucci