

Review of the thesis „Gene flow and its consequences for microevolution in Taraxacum (Asteraceae)“ by Lenka Martonfiová

The thesis consists of three published papers, one submitted paper and Introduction and Conclusions. The whole thesis is consistent. The author analyses possibilities of different crosses between particular ploidy levels. For me, this has an important aspect, because she compares results of crossing experiments with the situation in the field. This fact makes the results very realistic.

Because most of the results were already published I do not discuss them in detail. I shall try immediately to discuss some aspects of the thesis.

I do not agree with the content of some terms used. The author confines the facultativeness of apomixis only to potential of apomicts to form $n+n$ hybrids. For the other category of non-maternal progeny, i.e., the so called BIII or $2n+n$ hybrids she uses the other term, residual sexuality. I cannot agree with this narrowing of concepts. The sexuality consists of two elementary processes: meiosis and fusion of gametes. While the formation of $n+n$ hybrids unites the both characters, the $2n+n$ hybridization is also a sexual process, in spite it involves only gamete fusion, and not meiosis at mother plant. In this respect, the $n+0$ progeny (i.e., the haploid parthenogenesis not detected in *Taraxacum*) is the third possibility involving meiosis and avoiding gamete fusion. In my opinion the correct concept is that the plant is facultative apomict because of residual sexuality. Can the author support her view by some other papers using the terms in the same way?

I read several times in the thesis (e.g. p. 40, 53) that self-fertilization is induced by mentor effect of incompatible pollen. Does the author think it should be always the incompatible pollen? In our experience from *Hieracium*, it can also be fully compatible pollen which induced self-fertilization, beside the hybridization of the same plant. In this respect I would ask, how the author knows that pollination of diploid by pollen of diploid produces hybrids (p. 52)? Is it possible that at least some proportion resulted from autogamy?

Also the statement that most of the diploids produced from diploid x triploid crosses are from autogamy seems strong to me. Theoretically, triploid can produce any pollen from haploid to diploid, does not it? Production of seed with tetraploid embryo and pentaploid endosperm suggests the involvement of unreduced pollen at all.

Triploids with sexual reproduction were produced from crosses between di- and tetraploids in sect. *Erythrosperma* (unpublished paper). Have such plants been found anywhere in the field?

I do not fully understand some statements:


p. 39: „The pollen of tetraploids is often subregular with larger grains, probably containing a higher amount of viable pollen grains than that of triploids“. Does it mean proportion?

p. 45: „Triploids producing diploid ovules and tetraploids producing triploid ovules are addressed here“. I cannot imagine tetraploid producing triploid ovules... I can imagine triploid embryo.

p. 82: I would be more careful in speculations about different densities of plants from different sections. Densities can change quickly during time, I think more quickly than the changes of reproductive systems (e.g. during extensive grazing).

I consider thesis submitted by Lenka Mártonfiiová as valuable contribution to the study of apomictic plants. Especially valuable are her combination of experiments and observation in field populations. In my opinion it is sufficient to obtain PdD degree.

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