Tree Improvement Activities at Michigan State University

by Jonathan W. Wright 1/

In other years Prof. P. W. Robbins has covered the various forest genetic experiments that have been started at Michigan State University. Instead of trying to bring you up to date on these experiments--after all trees don't grow too much in 2 years--I 'd like to speak about some of our plans for the future.

The teaching program at Michigan State University will include forest genetics instruction at both the undergraduate and graduate levels. In addition, I will be able to spend about half time on research. The research program has not yet been crystallized. However, I can be sure of one thing. It will include a minimum of short-term studies which are put in because they will make convenient class demonstrations. Long-term studies designed to produce the best trees for Michigan planters will make even better class demonstrations and thesis projects.

Last year Michigan nurseries produced about 88,000,000 red pine, about 66,000,000 Scotch pine, and smaller amounts of other species. Therefore, red pine is Michigan's most planted tree, and will get the greatest emphasis in our program. Our initial efforts will be aimed at producing better red pine, or at producing improved strains of other species which will outperform red pine on the ground and be as valuable as that species in the sawmill.

The European pine shoot moth is the number one problem of red pine in Michigan. It is still not serious in the Upper Peninsula or in the northern portion. of the Lower Peninsula. However, it is enough of a problem in southern Michigan to make it likely that a shoot moth-resistant strain would be appreciated more by foresters than almost anything else we could produce.

A quick glance around the Lansing area indicates that the exotic field is promising. Ponderosa pine, Austrian pine, and Scotch pine are all doing well. One or more of them might be more than a satisfactory replacement for red pine in certain areas, although probably not over the range as a whole. They are considerably more resistant to the shoot moth than is red pine. But we need to know much more about those species in particular we need to know more about their climatic adaptability and their geographic races. The small racial tests now in existence are quite inadeq-uate. Range-wide, well-replicated, and statistically analyzable provenance tests of red pine and of those three exotics are high on our must-do list for the next few years. I suspect that those same species will loom

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large in programs of other agencies, too. We will hope to carry along as much as possible of this provenance testing work on a cooperative basis, provided that the cooperative work plans call for the best possible experimental design.

Michigan State University has quite a number of statisticians and geneticists on its staff. I like that. There will be opportunities to work with them on studies to determine the best possible experimental designs for forest genetics experiments. Take the field of provenance testing. We know from past experiments on forest tree species that growth rate differences of up to 25 percent are often meaningless if based on unreplicated experiments. Therefore we must replicate. And we know from experiments ui herbaceous plants that many 1-, 2- , and 4-plant plots give 50 or 100 percent more information than two or three 100-plant plots. Also, the y are much cheaper to install and measure. But there are several things we don't know. We can't calculate beforehand whether a given experimental design will permit valid conclusions as to 5-percent, 10-percent, or 15-percent differences between origins. Every provenance test we establish obligates our employers to $25,000, $100,000, or even $500,000 worth of future work. Therefore it seems only right that we should try to eliminate all possible guesswork in planning those tests. I am hopeful that during the next year we can complete two experimental design studies, using data from existing commercial plantations, to help us in planning all our subsequent tests.

So for I've talked only about provenance testing. It may well be that an individual tree selecticn and selective breeding program in red pine holds more promise. We're going to find out. The first step of course, will be to select the plus trees. That will take a little time. There should be about 500 of those plus trees--selected for apparent resistance to the shoot moth or apparent rapid growth--to provide the basis for a new strain. With too small a number of initial selections we can get only theoretical information showing what can be done. We'd like the actual improved strain.

During the course of this preliminary phenotypic selection work we will get clues as to whether or not red pine is genetically variable in resistance to shoot moth attack. Clues, not proof. If those clues point in the right direction we will go ahead with the next step--a seedling progeny test of the selected parents.

My thoughts on by-passing the traditional clonal seed orchard ran something like this. (1) A good-sized clonal orchard would be expensive. (2) The clonal test if properly designed, would give information on the genetic variability of red pine with regard to shoot moth resistance. However, that is not the information we need. For a seed-propagated species we have to know whether superior trees give superior seedlings. (3) The clonal test can serve as a tested seed orchard only if every clone is progeny tested. It is the progeny testing--not the grafting--that will prove the superiority of certain parents. (4) At the end of one progeny-testing rotation--say 15 years from now-- the clonal test could be thinned to become a tested seed orchard. But what would happen if we thinned our F_1 seedling progeny test, leaving the best progenies? Fifteen years from now they will start to produce seed. The seed they produce will be genetically more variable than the seed produced by the clonal seed orchard, but it will have the same average productivity. (5) If our original assumption that the best parents give the best seedlings. is correct--the entire project will fall apart after 15 years if it isn't--seed harvested from the best individual trees of the bust F_1 progenies will produce even better 12 trees. The selection cycle can be repeated for at least 5 or 6 generations (if we are to judge by the results on lower plants), with a constant genetic increase per generation. On the other hand a clonal test does not offer any opportunities for further selection work.

Those five points can be summarized as follows, A seedling progeny test is cheaper and far superior to a clonal progeny test for a species that is usually propagated by seed

In these remarks I have attempted to give--not a complete summary of all our future plans--but some of the major projects that I feel reasonably certain will be started in the next biennium or at least in the next 5 years. We may start off rather slowly, but I think it's better that way. Then we can be sure that every project will furnish good theoretical information for the science of forest genetics, and improved strains which will be usable on sizable Michigan areas.