LESSONS OF THE PAST

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In the present state of our knowledge, it is probably somewhat premature, and certainly presumptuous, for anyone to make far-reaching decisions as to what should be, and what should not be, incorporated in forest tree improvement plans for any particular locality. Nevertheless, past experiences in the application of genetics in forestry, both in this country and abroad, point up certain promising courses of action, as well as pitfalls that may be avoided.

For convenience, such lessons of the past may be grouped in two general categories:

1. Those which might be termed "public relations" problems.

2. Those of a more technical nature.

The first lesson of a public relations nature is concerned with what the cigarette advertisers call "extravagant claims." I grant that a reasonable amount of promotion may be justified in securing recognition for the new application of a science. Certainly as a means of gaining financial support, the public, and especially prospective donors, must be informed of the advantages that may accrue from research in a new field. Unfortunately, however, the emphasis may frequently be so unbalanced that a completely erroneous impression is created in the minds of the uninformed. The sad consequence may be that if the promised results are not immediately forthcoming, financial support may be lost, and the whole program discarded.

I would venture to say that the average citizen, and even many foresters, if asked what was being done in the application of genetics in forestry would unhesitatingly say, "0 yes: They're creating those fast-growing superhybride."

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The fact that such mundane activities as the selection and progeny testing of wild genotypes and the development of seed certification systems are at present the chief concerns of most forest genetic programs is frequently given secondary billing. But since it is to these rather unglamorous applications of genetics in forestry that we may look for early results in tree improvement, there seems to be little point in keeping them under cover.

I do not wish, by these remarks, to discredit in any way the valuable studies in controlled intra- and interspecific hybridization now carried on by many workers in this country and abroad. I have no doubt but that such work will ultimately be richly productive. But I do feel we must discourage the popular prevailing misconception that the end of all forest tree improvement entails the indiscriminate production of F1 hybrids.

Another lesson that we may note from a study of past efforts **to** apply genetics in forestry also falls in the public relations field. I refer to the unfortunate relationship that has existed, and still exists in some quarters, between what might be called the "classical" silviculturist and the forest geneticist.

The principal area of misunderstanding appears to be centered about the old "nature vs. nurture" or "genotype vs. environment" argument. Although a popular biological bone of contention in the early part of this century, this argument was soon recognized as futile and did not long persist, except, apparently, in the field of forestry. There appear to be several contributing causes. One is that silviculture has developed under a strong environmentalist influence, and there has thus been a reluctance to acknowledge such heretical modern concepts as the genotype and the phenotype. The geneticists themselves have frequently aided little in efforts to attain a meeting of minds. We still, for example, speak glibly of "environmental variation" in contrast to "genetic variation" as if they were quite independent phenomena.

Fortunately, such fundamental misunderstandings are fast disappearing; and with the eventual incorporation of forest genetics in forestry school curricula, they will, I am sure, completely disappear.

Of those lessons concerned with the more technical aspects of tree improvement, the first I would like to mention is of a somewhat general nature: the venerable problem of "pure," "fundamental," or "basic" vs. "applied" research.

The question here, as in the case of the "genotype vs. environment" argument is not one of relative merit. Both are certainly necessary. The trouble lies in the fact that the practical cart is frequently placed far in advance of the fundamental horse. The tendency to such manipulation is common *in* other fields of science, but I am personally doubtful if the results are quite as serious in forest genetics as some people believe.

I think almost everyone would agree that the most desirable plan for a tree improvement program in the Lake States would be to place principal emphasis on basic genetical, physiological, and other allied fields of research for the next 50 years. At the end of that time we would be in a much better position to devise a tree improvement program.

Such a procedure is obviously impossible, and in many respects undesirable. In the first place, there simply aren't enough altruistic millionaires available. Basic and applied research can, I think, very advantageously share the same bed; but some caution should be exercised to prevent the basic studies from being pushed too near the edge. In may opinion, one of the conspicuous shortcomings of several tree improvement programs, both in this country and abroad, has been the almost complete neglect of studies concerned with wild intraspecific diversity. This neglect is probably traceable in large part to a persistence of the environmentalist concept of intraspecific genetic uniformity. Certainly such basic studies are a necessary prerequisite for any improvement program.

Deliberation on the relative emphasis to be placed on basic, as opposed to applied, research leads quite naturally to the contemplation of the desirable over-all plan of a tree improvement program for any particular region. My considered opinion is that one of the most important lessons to be gained from a study of European and domestic experience is the desirability of placing initial emphasis on an extensive, rather than an intensive, approach to tree improvement problems -- on what many people insist upon calling "basic" studies of intraspecific variation. Although such studies of intraspecific diversity -- the "reaction range" of genotypes, etc. -- are technically of a basic nature, they may, nevertheless, be rapidly and richly productive of important practical results.

Provenance studies, for example, which have been of such fundamental practical significance in the definition of racial diversity in pine and spruce in Europe, should unquestionably be given high priority in any proposed improvement program for species native to the Lake States.

In conjunction with such efforts to improve the genetical quality of seed on a geographic basis, attention should also be directed to the isolation of genetically superior local seed sources, both stands and individual trees. Such studies will require progeny testing on a large scale; but in areas where planting is now being done, the added costs of such tests should not be prohibitive. Actual seed costs will, of course, be somewhat increased, as will also the clerical expense of keeping adequate records. With careful planning, the requirements of a statistically sound experimental design will not add greatly to the usual planting costs.

Together with such selection and testing efforts, a major portion of initial research activity should be directed to methods of insuring regular and adequate seed production from selected stands or individuals.

Although essentially a physiological problem, as is also the important problem of seed. storage, its early solution is just as essential as is that of the strictly genetical phases of the improvement program.

In those programs designed to improve Lake States conifers, I believe that major emphasis for perhaps the next 50 years should be centered on the isolation of the best wild seed sources ar.d on methods of insuring sufficient seed production to meet planting needs. I do not wish to imply, however, that intensive breeding studies should not, in special cases, be pursued. Studies concerned with disease and insect resistance and other specific improvement problems should be encouraged and supported. in conjunction with the more extensive phases of the program.

There is one final lesson that I think should now be apparent. I have touched briefly on the matter before in connection with the still frequent misunderstandings that persist between some silviculturists and forest geneticists. This lesson is simply that so—called genetical research is actually, in large measure, so—called silvical research. For some odd reason we still insist upon separating the twos For other odd. reasons many foresters are reluctant to admit that genetical research can in any way contribute to the development of sound silviculture' management plans in the self—produced forest.

When we realize that the great majority of the forests of this country are at present, and will doubtless long continue to be, managed. under systems of self—reproduction, we must make the altogether reasonable inference that if their genetic quality is to be improved, or even maintained, we must eventually come to recognize the necessity for learning considerably more about their hereditary characteristics. I think it is a matter of little consequence whether such knowledge is labeled gene t i cal " or "silvical," or whether it is accumulated by self—styled. "forest geneticists" or "silviculturists." I feel very strongly that one of the principal and unavoidable responsibilities of forest geneticists is to contribute to this knowledge.