

THE BOUNTY OF RESEARCH

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Sixty years ago the University of California at Riverside hired a young plant breeder, Howard Frost, to work on citrus. In terms of the objectives of the experiment station, Frost's duties were to seek out promising varieties of citrus and breed them for growers. But Frost's primary interest soon turned to the genetic structure of citrus plants and their crossing behavior. Since the period from seed to fruiting in citrus ranges from five to ten years, this meant that an enormous amount of time had to be expended on research that might extend over several generations of trees before results were known.

In those days, growers weren't as enlightened as they are today about the need for a proper mix of both basic and applied research. There probably were many complaints that Frost's research had no bearing on the problems of growers, who wanted new citrus varieties they could put into production immediately. The experiment station had to hire other men to carry out the work originally intended for Frost. Both H. J. Webber, director of the station, and Leon Batchellor who followed him, however, recognized that Frost was doing research which might eventually prove significant. Less imaginative and visionary administrators would have let Frost go. But they kept him on at the experiment station. His budget was never very large and many of Frost's colleagues viewed his work as something that would never have practical implications. As for Frost, he cared only for his research and worked from morning until late at night in his laboratory and greenhouses. He was a shy, seemingly vague, humorless man, who could never even remember what day of the week it was because he was too busy to bother with non-essentials.

Then just before the start of World War II a group of Russian scientists suddenly appeared at the experiment station. They came to see Howard Frost: they showed almost no interest in anyone else or any other area of research. They had been following his work in genetic journals for years. Suddenly, it became clear that whatever Frost had been up to all those years pattering around his greenhouses might be highly relevant to the citrus industry. In 1966, Frost was presented the Wilder Medal of the American Pomological Society — the oldest and one of the most highly prized awards in American horticulture. The practical application of Frost's studies — which expanded our basic knowledge of the genetics of citrus — lay in his development of hybrids and nucellar budlines. Today more than half of all lemon and orange varieties propagated in California are vigorous nucellar selections — some of them, such as the Kara and Kinnow mandarins, originated by Frost.

If Frost were working for us today on avocados, I'd have deep reservations about bringing him to a meeting of growers. I'm sure he would be working somewhere at the

frontiers of our knowledge, and many of you might wonder if we weren't wasting our funds from your marketing order. As growers, you are practical men — like most farmers — and I'm sure you feel most comfortable with a researcher who is highly visible, who comes out to your orchards and cuts open avocados with a knife, and who can talk intelligently about your immediate problems. We need both types of scientists though — men who can carry out applied research on current problems and men who are interested only in basic research. In an age when new breakthroughs in agriculture increasingly lie in unexplored regions — when agriculture has really become applied biology — we particularly need more and more men like Frost.

I've selected Frost's work from that of a number of pioneer scientists at Riverside who appeared in their time to be out in left field as far as the needs of growers were concerned. I'm not saying sometimes we aren't out in left field. For every Frost, there are any number of researchers who fritter away their time on useless research in agriculture.

Recently, a group known as the Pound Committee conducted an intensive study for the National Academy of Sciences on the U. S. Department of Agriculture. Their report has been highly controversial, and many scientists have challenged it. The Pound Committee concluded that much of agricultural research being conducted today is duplicative and of mediocre caliber. In particular, they charged that there isn't enough basic research of the type which Frost was carrying out being conducted and such research is essential to continuing achievements in agriculture. Much of the problem lies in too tight a control on scientists the Pound committee suggested. I don't know how true this may be, but I do know that freedom of inquiry is the soundest foundation for good scientific research.

Harold L. Enarson, president of Ohio State University, recently said that there is a tempting heresy loose in the land. "Very simply," he said, "it is the dangerous notion that state universities are simply another agency of state government, a unit to be policed, regulated, and whipped into a bureaucratic mold."

Universities, as Enarson noted, are a very special kind of place: "They are fragile as the truth itself is fragile." They exist by public sufferance and it is a marvel that the American public has supported with its dollars an institution that is "independent, free-standing, openly critical of the conventional wisdom . . ." Agricultural achievements in the United States — which have stemmed predominantly from the research of the land-grant colleges — have carried us in a short period of less than one hundred years from an agrarian to an industrialized food system. In 1830, one out of every four persons in the United States was a farm worker — seventy percent of everyone who was gainfully employed. By 1920, only a few decades after the creation of the land grant colleges, that ratio was one in eight. Today the ratio of farm workers to non-farm workers is less than one in forty. Both the farmer and consumer benefited immensely from the new agricultural technology, which resulted in a predominantly urban population with the world's highest standard of living. In fact, if the benefits of research had not been distributed beyond the farmer, agricultural research on its present scale would have been impossible. It was public recognition of the bounties that flowed from agricultural research which made such research possible.

In the past few years, however, agricultural research has been drastically reduced at the very time that rising prices and tight food supplies here and throughout the world indicate it should be given major support as one of the nation's top responsibilities. We no longer have our immense crop surpluses, and much of the world is on the brink of famine. Part of the cutback has been a product of public dissatisfaction with student unrest on the campuses. Part of it has been said to have been a growing lack of confidence in science. In the long run, everyone will lose if agricultural research is atrophied. Agricultural research has to be revitalized and reemphasized to earn goodwill abroad and restore Americans a reliable supply of ample and cheap food.

I would hope that our outlook for agricultural research is beginning to improve and that the dissatisfaction of the past few years with science is diminishing. A Louis Harris poll taken late last year shows that although public confidence in people "running science" has fallen from 56 percent in 1966 to 37 percent in 1972, the decline does not support the widespread notion that the public is disenchanted with science, and that the trend may finally be reversing itself. Five percent more people in 1972 than in 1971 expressed great confidence in the men and women of science. Young people were not the main source of lack of confidence, but their elders. Highest ratings (41 percent) were given to scientists by the age group of 18 to 29, as opposed to only 33 percent for those 50 and over.

I have stressed the need for freedom of inquiry in research — that scientific accomplishments always come out of an environment which gives the researcher as much freedom as possible. I'm sure some of you wonder how you can be sure that you are getting the most out of your research money under such conditions.

The task of ensuring that research is productive falls on research administrators such as myself. The record of successes of an institution are the only criterion on which it can be judged. The research administrator has to decide in which areas the resources of his institution should be placed. He has to decide how much basic research is necessary to provide the underpinnings for applied science. It isn't always easy to recognize or foresee the possibilities in new areas of research. It may be easier for the cautious administrator to concentrate his resources and funding in safe areas, where the results will be certain but the returns are small rather than to take risks. We know today that Howard Frost deserved much more support than he received from the experiment station. At the same time, if there had not been a Webber or Batchelor in administration, he probably would have been let go. Creative scientists are rare and often misunderstood. Yet the truly creative and innovative discoveries are made mainly by a few such men — a small fraction of scientists — just as 10 percent of used car dealers makes 90 percent of all used car sales.

My task as an administrator is to help provide the environment in which scientists can achieve results. To determine how our funds — including monies we receive from you avocado growers through your marketing order — can most wisely be used. I can't command scientists to be creative or order them to come up with breakthroughs. I can only help provide them with the support they need and expect you to judge us by our results. Sometimes results come quickly. In the case of the attack of corn blight in 1970, agricultural researchers were able to control it in one year of research. More often, however, application of a research discovery has a ten to twenty year time lag.

As growers, all of you are helping to provide support to the experiment station for a five-phase program aimed at early control of the root rot fungus. I think we have made a number of breakthroughs in the past year and I'd like to briefly summarize them.

First, our expanded collecting program in Latin America has continued and in the past year 227 collections were made in Mexico, Guatemala, El Salvador, Nicaragua, and Costa Rica. Emphasis has been on collecting new species of avocado and on intensifying collection of avocado types that have shown some resistance to *Phytophthora* fungus in earlier tests. We also obtained seeds of another genus of plants related to the avocado, *Machilus*, which grows in Asia. We've run tests on 1250 cuttings and seedlings in the greenhouse for resistance to root rot. Most plants have proven very susceptible. But we are particularly encouraged by a Mexican seedling from Guatemala which was found on the slopes of a volcano. It appears to offer seedling resistance to *Phytophthora* and good graft compatibility to commercial California avocado varieties. It looks very good in our early tests, and we'll be making our first field plantings later this year. We're also encouraged by the resistance of some old California avocado trees in the field to root rot and will be planting them in the field also this year.

A second major development has been the identification of a chemical, borbonyl acetate, which may prove to be the cause of resistance in some avocados to root rot. We are now trying to synthesize the chemical in the hope that it may help us in providing resistance against root rot or in treating diseased trees.

Finally, we have begun residue studies on a presently available fungicide — Terrazole — which has shown a capability for retarding the progress of root rot disease. The residue tests are currently being made from fruit on trees treated with Terrazole this past season. In addition, four fungicides have been tested for possible frequent, low-dosage applications, particularly through drip irrigation systems.

These are the major areas of breakthrough. We have sixteen research Ph.D.'s concentrating on avocado research at present. We are also focusing a great deal of attention in areas of basic research. You won't see our basic researchers out in the field cutting avocados, because it's in these areas that the test tubes and sophisticated equipment come into play. I used Frost's research in citrus genetics earlier as an example of basic research that during its time seemed unrelated to citrus problems. The same sort of example can be given in problems related to plant pathology. It may take a great deal of basic knowledge before you can even begin to look for the solution to a plant disease problem. For example, we spent many years trying to control viruses which were uncontrollable. Finally, through electron microscopy we found that we weren't dealing with viruses at all, but with mycoplasma — an entirely different organism.

I believe that in the past two years — using funds from your marketing order backed up with University research monies — we have made remarkable progress in a number of areas on avocados. If you have questions about our research at any time I shall be pleased to try and answer them. In particular, however, I hope that I've given you some idea of why basic research is an essential component of any agricultural research program. The more we know — trivial as such knowledge may sometimes seem to an outsider — the more we are able to bring together the facts which lead to solutions of

problems.