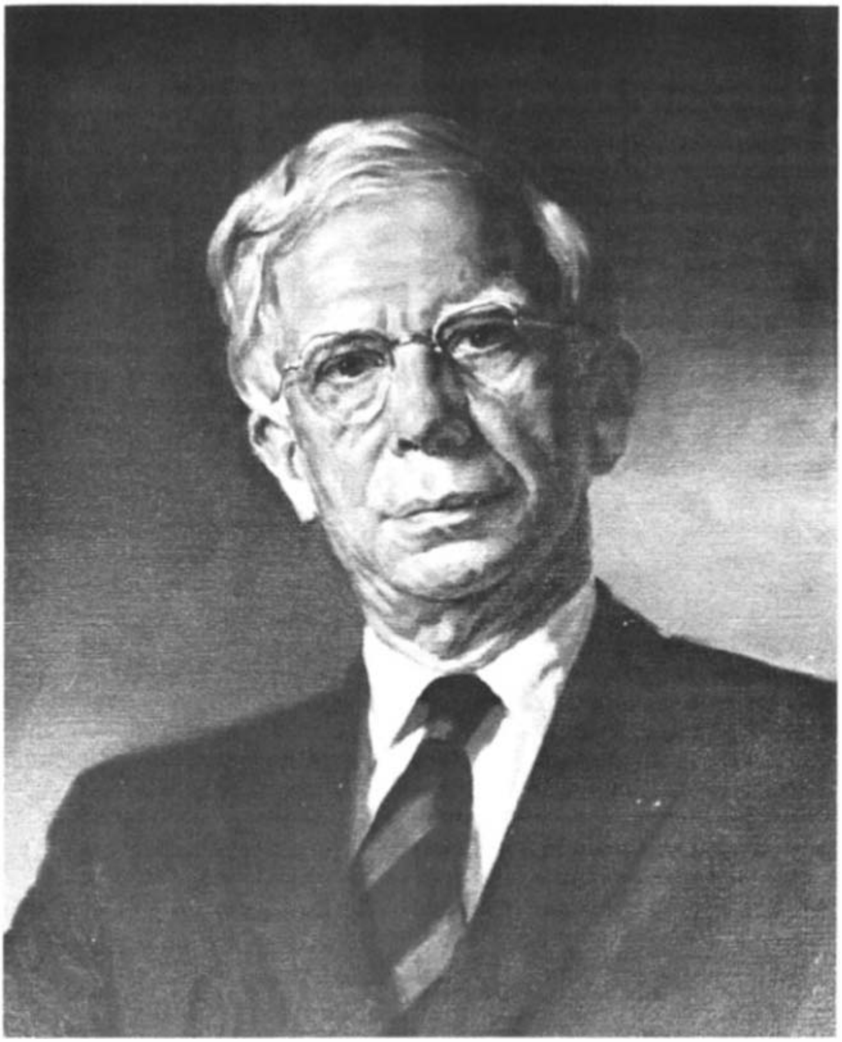

Oil painting by Deane Keller

James G. Horsfall

FUNGI AND FUNGICIDES
The Story of a Nonconformist

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The Editorial Committee has granted me the high honor of addressing you, my friends, in the pages of the *Annual Review of Phytopathology*. Had they not done so, I would never have had the courage or the necessary immodesty to do it somewhere else.

They told me to be autobiographical, philosophical, or both. Being a cornfield philosopher and a ham at heart, I cheerfully accept the challenge. I do hope you like it.

Although I have wandered around extensively over the field of plant pathology, I have specialized in fungicides, and so I shall discuss that. Being by nature and nurture a nonconformist, I shall discuss that too.

THE PHILOSOPHY OF A NONCONFORMIST

After fifty years, man and boy, I still think that nonconformity is a great asset to a scientist. We must be curious to see if what we see is what we seem to see. We must analyze it, open it up, turn it over, look underneath it, and look behind. The conformist is simply not programmed for this.

Perhaps the harshest lesson a young scientist has to learn is that while nonconformity is a great asset in science, it is counterproductive in living. It can be a joy in science. It can be a heartache in living.

The herd instinct is strong in the human animal. An old aphorism describes it. "As one banana said to the other, you wouldn't have been stepped on if you had not got away from the bunch." Society has learned from psychologists to call the isolated banana an introvert.

Apparently I didn't inherit the herd instinct. As a child I was the stereotype of an introvert. I had to learn the hard way that isolated bananas can be stepped on. I found myself in the position of the man who woke up all bandaged in the hospital. He said, "I had the right of way." The nurse gently said, "But the other driver had a truck."

I had been playing a dangerous game. It could not continue. Therefore, I became a synthetic extrovert. I couldn't go all the way, to be sure. And so, I learned to set up side shows. Mycology was crowded, so I got into fungicides. Bordeaux mixture and elemental sulfur were heavily worked. Whereupon, I got into organics. When the emphasis was on field testing, I turned to mechanisms of action. When this got crowded, I returned to epidemiology and disease assessment, a field I have fiddled with for years.

THE PLEASURES OF PLANT PATHOLOGY

Lest the above sound too lugubrious, let me say, "Cheer up." The heartaches of nonconformity aside, I have had great fun—a great life in plant pathology. Plant pathology has been kind to me. It has taken me all over the world to make friends everywhere. I take this means to thank you all for your hospitality.

J. C. Walker introduced me once as a person who had fun with fungicides. I must say I was tempted to title this piece Fun With Fungi and Fungicides, but I finally decided to be a conformist and label it simply Fungi and Fungicides.

Roland Thaxter, the first Connecticut plant pathologist to have fun with fungicides (15), later in his life teased the plant pathologists by dubbing them "squirt gun botanists." I am sure he had his tongue in cheek because 18 years later he became a charter member of the American Phytopathological Society. Following Thaxter, I often call myself a squirt gun botanist. If we can't laugh at ourselves, we are stodgy indeed.

A PHILOSOPHY OF SCIENCE

My philosophy of science derives by alchemical osmosis from the policy of my institution. Its policy derives in turn from the title of a book, *How Crops Grow,* written in 1869 by S. W. Johnson, founder of the experiment station where I write.

At that time his experiment station was only a gleam in his eye, but he knew that the *quid pro quo* must be the payoff in the fields and granaries of the nation.

His thesis, frequently expounded, was that theoretical and useful science must march together. His title, *How Crops Grow,* is "clear, curt, and complete." The *how* denotes the theoretical, the *crops* denote the useful, *grow* denotes the mechanisms. I wish I could condense as much as well.

My philosophy of research matches Johnson's completely. It was expressed in the preface of my first book on fungicides (6). "It [the book] attempts to develop the underlying theory on which the practice is based, and by which the practice may be improved."

The modern terms, pure and applied or basic and applied, are only analogue terms that are not really equivalent to Johnson's theoretical and practical. One can do theoretical research on corn or wheat that is intellectually just as deep as that on *Chlorella.* Its snob value is lower but its scientific value is as high and it *can* get one a Nobel prize or elected to the National Academy of Sciences.

A very slight shift in the wording of Johnson's title gives us How to Grow Crops. There is no theory in this, and the research under it tends to be shallow and inadequate. In the early days many experiment stations and US Department of Agriculture tended to use this phrase as policy, however.

Those in academe who consider *crops* a dirty word have rephrased Johnson's title to How Plants Grow. This rephrasing has been heavily underscored since 1950.

The rephrasing of Johnson's title to How Plants Grow knocks out the useful and eventually knocks out the societal support. In the late sixties the policy generated by this title came under intense scrutiny by congressmen. Senator Allott of Colorado writing in *Science* in 1968 (1) said that, for some time he had been warning members of the scientific community that unless they explained to the taxpayer what he was buying with his research dollars there would be a severe cutback in funds allocated for research. And there was!

The National Science Foundation listened to Senator Allott and the rising tide of antiscience sentiment and established RANN—Research Applied to National Needs. They decided a century after Johnson that the voice of taxpayers is persuasive, and that the theoretical and useful should march together.

In my philosophical talk to the Phytopathological Society in the summer of 1969 (7), "Are We Smart Outside?," I urged us to attune our ears to Senator Allott and to return to a better balance between theoretical and useful. Some said I was a renegade who had deserted basic research, but like Mark Twain's premature obituary, this was "slightly exaggerated," however.

It is fitting that this epistle will appear in the late summer of 1975 because it was in the late summer of 1925, fifty years ago, that I shook the cotton field dust from my feet in Marianna, Arkansas, put on my shoes, and took the train to Ithaca, New York and Cornell University to work with the great H. H. Whetzel. My only recollection of the trip was that as the train from St. Louis clattered its way eastward across Indiana, a fellow traveler asked my destination. I told him Ithaca, New York. "That's where Ithaca guns are made," said he. I had never heard of Ithaca guns and he had never heard of Cornell University.

A half century of plant pathology has gone by. In 1925 mycology had us in its grip. In fact, I minored in mycology, having been forced to take all the courses anyway. It shortened my graduate student days somewhat, but perhaps I should have stayed longer and minored in organic chemistry. Who knows? It would have helped the theoretical side of my squirt gun botany.

In my student days Whetzel was dusting the apple orchards of New York State, Greaney the wheat in western Canada. Viruses were still "filterable viruses"; they had hardly escaped from the phrase, "contagium vivum fluidum." Mycoplasmas were a light year away. Fungicides were mercuries, lime sulfur, and Bordeaux mixture—that holy water of plant pathology. Farlow had called it *Eau Benite*.

Some of the giants of American plant pathology were Duggar, Edgerton, Jones, Melhus, R. E. Smith, "Erwinia" F. Smith, Stakman, and Whetzel. The latter pair always arranged a well-orchestrated squabble to attract members to the business meetings of the Society. Charles Walker, Joyce Riker, Max Gardner, and James Dickson were on their way up. Cap Weston from Mt. Olympus enlivened the banquets by needling the plant pathologists. The only sally that I can still remember was his speaking of the Boyce Thompson Institute in Rompers-On-Hudson. Abroad, some of the giants were Appel, W. Brown, Butler, Gäumann, Hemmi, Miyabe, McAlpine, Sorauer, and Westerdijk.

Those were experimentally simple days in 1925. We had never heard of low-volume spraying, captan, zineb, chromatography, NMR, electron microscopes, and the double-crossed helix. LSD meant least significant difference. The causal organism was called a pathogene, not pathogen. I have never found out why the final "e" was dropped.

The Coolidge boom was on. The flappers reigned. The Charleston was the dance. Harold Cook and I built a heterodyne radio from a kit. About the only stations we could tune in were KDKA in Pittsburg and WEAF in New York, and they crackled and fried as we listened.

The Model T Ford was still alive but gasping for breath. It had two more years to go before it died.

Surely plant pathology has moved ahead with exciting developments. We have grown out of the swaddling clothes of descriptive mycology. We are now concerned with mechanisms of action. We now enlist recruits from the ranks of those who fixed their mothers' alarm clocks, not from the ranks of the collectors of stamps and butterflies.

ANTECEDENTS

There must be something earlier than 1925. Yes, of course there was.
ther Horsfall belonged to a family of ship owners in Liverpool in England. But he, being a nonconformist, too, got crossways with his father and was banished to the US as a remittance man with £1000 per year. He eventually settled in eastern Arkansas because the bird hunting was good in the Mississippi River flyway. Nonconformity cost me only a delectable job or two. It cost him a fortune. Needless to say, we pulled few plums from the family pie.

When I was born, my father, poor as a churchmouse, was working in a tiny village in a tiny independent fruit experiment station in southwest Missouri. By arranging to be so born, I automatically inherited an interest in science.

My father saw no future in it, however, and departed to become a schoolman in his home state, Arkansas. He was an agricultural schoolman. We were so far down in southern Arkansas that you could chase a stray steer into the Mississippi River to the east or into Louisiana to the south. My father maintained his interest in horticultural research, however, eventually obtaining a Master's degree at the University of Missouri.

My father was his own type of nonconformist, but to that extent, he was a great mentor in science. He was wont to say, "Son, what is often accepted as fact is often not so." I can recall sitting on the front porch one Sunday afternoon. Seeing a cow in the pasture he asked me what color it was. "Black," said I. "How do you know?"

said he. "I can see," said I. "You can see only this side. How do you know it is black on the other side?" I had to make an experiment. I had to walk down and verify that it was black on the other side. Luckily, it was.

I recognized my first plant disease when my scientist father said our pear tree was dying of fireblight. We were advised to prune out the blighted shoots. After a few years of such Draconian treatment we had only a stub left. The treatment was a success but the tree died.

He sent me off to the University of Arkansas, where he had graduated. I wanted to be an engineer, having shown an aptitude to repair Model T cars. At the University I discovered, though, that engineering is the job of a mathematician not a grease monkey. My grades in mathematics were all right, but I didn't like math.

Enter Dwight Isely, an entomologist. He loved science and he stimulated me to love science. He provided me with pin money to impale his *Chrysomelids* on pins in Schmidt boxes. This bored me, but allowed me to take my girl to the movies. It was more exciting when he sent me two summers to Marianna, Arkansas to chase cotton boll weevils on horseback. I couldn't see the boll weevils from my horse, but I could see the yellow infested "squares." I never learned why the triangular flower bud of cotton is called a square.

Dwight Isely was pioneering the use of insect counts to determine when to dust the cotton. This is now called integrated control. He had the idea; all I did was chase the bugs. You can imagine my surprise 48 years later to hear from the stage of the auditorium of the National Academy of Sciences that I was the first "scout" to do so (Smith 14, p. 53).

Pioneer or not, no entomology department would give me a graduate scholarship. My nonconformity was definitely nonproductive here. H. R. Rosen and V. H. Young found me a place at Cornell in plant pathology, and that is how it came about that I left the cotton fields, never to return.

FUN WITH FUNGICIDES

By February 1929 graduate school was over and I was out in the cold world. By great good luck a few days ahead of time I was offered a job at the Agricultural Experiment Station in Geneva, New York. The station was larger, the town was larger, and the future was larger than my father had had a generation earlier at that little experiment station in the Missouri Ozarks.

The ink was still shining wetly on my diploma when two greenhouse growers of tomato seedlings came in to see this callow PhD. "Doctor," they said, (I had difficulty answering to that august title) "Can you soak tomato seeds in a copper sulfate solution and control damping-off?" Sensing that they already knew the answer, I responded that I thought so, and proceeded to test it experimentally. To my intense delight my first experiment was successful, and thus did I discover fun with fungicides. I was on my way to becoming a squirt gun botanist.

I was so pleased that I gave a paper (5) on the results at the Annual Meeting of the Phytopathological Society. And there on the front row sat the great L. R. Jones. I was so awed I could hardly speak, rare as that occasion is. Nevertheless, here was proof I needed. Fungicides interested one of the top men. Maybe it would provide a ladder for me.

From time to time I studied the base of Jones' success. It was clear that Jones carried water on both shoulders. He could encourage theoretical epidemiology on the one hand and cabbage breeding on the other. This was several years ahead of my exposure to the same principles in Connecticut.

To control damping-off was my practical aim. Where lay the theoretical? I stumbled along in search of some basics in damping-off. To describe the dynamics of damping-off and its control I needed four new terms: pre-emergence damping-off, post-emergence damping-off, inoculum potential, and pasteurizing soil. These found an audience. Plant pathologists were indeed interested in the theoretical as well as in squirting Bordeaux.

My pattern was complete. I would try to do both theoretical and applied research, and do both on crops and diseases that were important in my state.

Bordeaux Mixture

Like many other pathologists of the day, I began by squirting Bordeaux mixture. In the late twenties I used canning crop tomatoes that were subject to foliage blights. Bordeaux didn't work very well. The dry thirties came along. I continued to spray but the disease was scanty. This turned out to be a boon in disguise.

Suddenly I could see that Bordeaux mixture was deleterious to tomatoes. What was going on? Basic research was needed. I needed to understand. We found that stomates were closed by the spray, the middle lamellas were hardened, the leaf cuticles were weakened by the alkaline spray, and the plants were dwarfed. To find all this out was great fun.

For potatoes at that time, Bordeaux mixture was still king. "Spare the Bordeaux and spoil the potatoes," was the hue and cry of plant pathologists and entomologists. "It stimulated potatoes," they said. Being a nonconformist, I didn't believe it. Since it was so deleterious to tomatoes, the cousin of potatoes, how could it be stimulatory to potatoes? It must be deleterious to potatoes, too, but this was hidden by the bug and blight control.

Since the lime in Bordeaux mixture was a potent source of plant injury, we tried to substitute red and yellow cuprous oxides. Cuprous oxide was free of lime but could not control the insects on potatoes as well as Bordeaux mixture and, hence, could not succeed on potatoes.

Nevertheless, in the mid-forties I went to one of Harry Young's fungicide forays in Ohio. I spoke up to say that Bordeaux mixture on potato was a dead horse that had not yet fallen over. This was heresy and the local aficionados almost excommunicated me that night.

About the same time, a reviewer of my 1945 book wrote that, "not all will agree with the author's apparent attitude that organic fungicides will soon replace the inorganics, and that 'Bordeaux mixture and elemental sulfurs will be turned out to pasture to spend their last years in leisure for a good job well done.' "

Bordeaux mixture wasn't dead yet, of course. I was just an ebullient nonconformist.

Organic Fungicides

My philosophy about fungicides had changed a few years earlier with a personal near-tragedy. My small daughter had had serious inner ear infections from time to time. In 1937 she was miraculously healed from a dangerous infection with a new synthetic organic compound called sulfanilamide.

My mind was made up. I was bound to find useful organic fungicides for agriculture. My colleagues were pessimistic. Farmers wouldn't buy an organic compound at $1.50 a pound when they could buy Bordeaux for 6 cents. Can you imagine 6 cents for copper sulfate? I believed they would if the compound performed.

TETRACHLOROQUINONE With the help of Walter C. O'Kane of the Crop Protection Institute we persuaded the US Rubber Company (now Uniroyal) to try. They were as ignorant then of plant pathology as we were of rubber chemistry. Upon questioning we said that the then current dogma was that copper in Bordeaux mixture killed by an oxidizing action. They said that copper acts as an oxidant in rubber as well, and causes rubber to crack and deteriorate. Why not, therefore, try tetrachloroquinone, an organic pro-oxidant? In April 1938 we did. It worked, and in a couple of years farmers of New York State were clamoring to buy it at $1.50 per pound for treating pea seed. The price myth was exploded.

I never published it. US Rubber wouldn't release the chemistry, and until they did I wouldn't write a paper.

Eric Sharvelle was in our laboratory then. Along with Cunningham (2), he published it under a code number.

Unfortunately, we couldn't make it work in a squirt gun. It was not destined to be the nemesis of Bordeaux mixture. It hydrolyzed in the sun and the dew. 2,3-Dichloro-1,4-naphthoquinone did not deteriorate on the leaf, and it found commercial adoption for some foliage diseases, but still not for potatoes.

ETHYLENEBISDITHIOCARBAMATE I remember sitting in a cheap restaurant on 42nd Street just outside Grand Central Station in New York City one day in the fall of 1939 with my friend Donald F. Murphy of Rohm and Haas Company, talking about our joint work on cuprous oxide. I felt it was time for a change. I said to Murphy, "Let us try to develop organic fungicides. Sulfur is a fungicide. Let us try organic sulfur compounds."

Murphy persuaded his company. In January 1940, they sent us 100 samples to try. He-175 was among them. This went on to have a new code, D-14, and eventually was published in 1943 as disodium ethylenebisdithiocarbamate, or nabam. A potato fungicide was born, and Bordeaux mixture was in trouble.

Dimond and Heuberger were with us then; we published it together (3). The new compound was water soluble. It should not have protected potato foliage, but it did. Heuberger decided to stick it on better with the zinc analogue of Bordeaux mixture ($CaOH_2$ plus $ZnSO_4$). His mixture worked (4).

The company chemists were sure that the zinc salt was formed in the spray tank. If so, their patent was threatened. Before our eyes they dramatically made Heuberger's mixture in a beaker. A precipitate formed. They poured it on a filter paper.

Clear water came through. Presto! The insoluble zinc salt was on the paper, they said. They recommended that the research be dropped.

I was born in Missouri. I had "to be shown." We needed a little basic research. We needed to understand the mechanism. I went home, made Heuberger's mixture, poured it on the paper. A clear liquid came through, just as before. I put my trusty fungus spores on a slide with the precipitate, on another slide with the filtrate. The filtrate killed the spores. The precipitate did not. The zinc salt was not being formed in the tank; it was zinc hydroxide instead.

To learn this was more fun. I understood nature a little better. The patent would not be invalidated, and the study went on. It was only several years later that the explanation hit me. Since Heuberger's precipitate was zinc hydroxide, I guessed that it was less soluble than the zinc salt of my fungicide. I further guessed that the carbon dioxide from the sprayed leaf would convert the zinc hydroxide to zinc carbonate the first night, and that this would be more soluble than the zinc salt. This would permit the zinc salt to be formed.

Whereupon, I made another batch of Heuberger's mix and bubbled CO_2 through it. Presto! The toxicity was in the filter cake, not in the water that went through. This was practical physical chemistry.

The plant was a chemical factory. It produced the zinc salt in situ. I had understood a little more about nature.

Zinc ethylenebisdithiocarbamate plus a good insecticide rapidly replaced Bordeaux mixture on potatoes. And the dead horse did fall over. This was still more fun.

No longer did the cry go out to potato farmers, "Spare the Bordeaux and spoil the potatoes." The new fungicide replaced Bordeaux, not so much because it controlled *Phytophthora infestans* any better than Bordeaux, but because it did not dwarf the plants as much.

The work led us down a short trail. Zinc ethylenebisdithiocarbamate and its progeny, maneb, controls *Alternaria solani* much better than Bordeaux. The mechanism was a mystery for many years, but Lukens & Horsfall (11) showed that it liberates just enough volatile material to kill *Alternaria* spores when they are germinating at high humidity without free water. We understood nature a little better.

Chemotherapy

Before I went to New Haven in the summer of 1939 I had heard how the Dutch elm disease was marching down the streets of that well-named Elm City. In my naiveté, I imagined myself as Sir Galahad. It would be I who would find the Holy Grail, the control of disease in those stately and elegant elms.

Zentmyer came and shared the enthusiasm, quietly of course! We would solve it with chemotherapy. That was a new and untried procedure for elms. The fungus lay inside the tree. We would inject our medicine and search it out as sulfanilamide had searched out the infection pocket in my daughter's inner ear.

We filled beer bottles with our magic elixirs and attached them to the trees with rubber tubes. Since we bought the bottles from the Cremo Beer Company, we claimed we were doing Cremotherapy research.

We could never make it work usefully. Zentmyer got discouraged before I did, and returned to the sunny clime and chocolate-colored smog of southern California.

Dimond came back from Nebraska and struggled with it, but as of this writing the elm disease has done its work on most of New Haven's elms. We simply could not solve the problem in time. We found large numbers of useful compounds. The trees felt better after injection but died as soon as the dosing was stopped. Apparently the tree degraded the compound and the titer in the tree diminished too far.

Benomyl looks better. We hope it lives up to its promise. I think that our compounds failed for two reasons, (*a*) the tree degraded the compound and (*b*) the elm had no phagocytes. The compounds were fungistatic, not fungicidal. Penicillin is bacteriostatic, too, not bactericidal; it reduces the infection to a reasonable level. The phagocytes clean up the last stragglers and the patient recovers.

We are convinced that our compounds reduced the infection in the elm to a low level, too, but there were no phagocytes to clean up the last stragglers; the elm degraded the compound and did not recover from the disease.

In any event we had great fun and great excitement, even though we found only experimentally workable compounds. We still have hope.

FUN WITH FUNGI

H. M. Fitzpatrick was a warmhearted teacher of mycology when I was at Cornell, and besides he ate lunch with us graduate students in the "Domecon" cafeteria. He pitched pennies with us (and always won). He added his penny winnings to his private funds, took us on picnics and provided steaks, which we could never afford in the cafeteria.

I became so enamored with Fitzpatrick's fungi that I minored in mycology and helped publish three papers on fungi.

Probably my favorite fungus, however was *Alternaria solani.* On hearing that I was going to my first job at the Geneva Experiment Station, Charles Chupp asked me to control *Septoria* on tomatoes. *Septoria* was in my plots the first year in 1929, but it slowly faded away and was gradually replaced by *Alternaria solani.* I wish I knew why.

The Prodigal Son Principle

Alternaria solani provided me with a couple of examples of the prodigal son principle of research. Here my nonconformity shows again. At heart, I had always been fascinated with the points on the regression line that did not fit the expected curve. They are the prodigal sons. As the Bible says, I would kill the fatted calf for them.

This is why I have seldom worried much about standard deviation, analysis of variance, Chi square, T test, goodness of fit, etc. I like the points that don't fit, not the odds that the others do. Sometimes statistics are needed, however, to show that the prodigal son exists.

The first prodigal son in the *A. solani* case was the very rare "bull" plant that I saw occasionally in farmers' tomato fields. There was no statistical evidence for the one bull plant in 10,000, just your eye. They were sterile, produced no fruit, and were essentially immune to *Alternaria solani.* They stood starkly alone as islands of green in a sea of brown and defoliated plants. They were prodigal sons. Let us kill the fatted calf, celebrate, and cerebrate. Why were they immune?

We were able to produce bull plants by pinching off the blossoms. Obviously bull plants are not genetic freaks. We concluded that the fruits remove something from the leaves that makes them resistant (9). Came the war; we dropped the subject. Later John Rowell in his PhD thesis (12) showed that the substance pulled from the leaves by the fruits is a hexose sugar.

The Umbrella Principle

Rowell's discovery led me to speculate, as discoveries often do. The elucidation of the role of sugar in alternarial blight suggested a general principle. Perhaps sugar is related to resistance in other diseases. Let us call this an umbrella and attempt to see what we could bring under it. With Al Dimond's able assistance we raised such an umbrella (8).

Potato leaves with leaf roll are high in sugar and low in *Alternaria.* By the same token tomato leaves sprayed with maleic hydrazide are high in sugar and low in *Alternaria;* the roots are low in sugar and high in *Fusarium.* Roots of barley plants with yellow dwarf are low in sugar and often high in *Fusarium.* Old tomato leaves are low in sugar and high in *Alternaria.* A search of the literature reveals many low sugar diseases. One is pink root of onion.

This is just observation. What is the mechanism? Horton & Keen (10) showed that sugar inhibits the destructive enzymes of the onion pink-root fungus, and Sands & Lukens (13) have shown the same for *Alternaria* in tomato.

One of my favorite critics, reviewing a book of mine, discussed my concern with the umbrella principle in these words: "He leaps from crag to crag with the nimbleness of a mountain goat." I liked that.

Occam's Razor

I believe, too, in using Occam's razor in my research. We scientists love to think that processes are complex, and we set up wide arrays of assumptions to account for an observed phenomenon.

The Reverend William Occam (or Ockham) was an English cleric of the fourteenth century who proclaimed a class of logic holding that assumptions should be sliced to a minimum. I confess that I surely failed the Reverend Mr. Occam in trying to explain a bull tomato plant. I tried everything but the obvious. Rowell took the simplest case—a fruit is composed of big quantities of carbohydrates—therefore sugar must be the substance moved out of the leaves. Rowell didn't know that he was slicing through the brambles with Occam's razor, but he was.

FUN WITH WORDS

Style

Being a nonconformist, I have always tried to say it differently. It is fun and it gets me into trouble occasionally, but I still like it.

One critic calls it "a breezy and compelling style," while another, perhaps more tolerantly says that the "use of homely analogy and exposition to enlighten some of the more knotty technical problems makes for easy reading and comprehension." I hold that scientific writing should be comprehensible and easy to read. I could never abide the stodgy stilted style of much scientific writing. The English language is an elegant medium for saying exactly what one wants to say—no need to use any of the standard circumlocutions. Say it differently!

I guess it was this propensity that got me into the editing business, beginning with the college student magazine and continuing through a three-volume treatise into the editorship of the *Annual Review of Phytopathology*.

On account of the latter, the current editor, Baker, has asked me to set down here the history of that review.

Origin of the Annual Review of Phytopathology

The story can be traced back to 1951. In that year the council of the American Phytopathological Society rescinded the rule against printing theoretical papers in Phytopathology (*Phytopathology* 42:230. 1952). No longer were the pages limited to "original research" with laboratory and field gadgets. One could now do theoretical research with pen and paper. Einstein was not an experimental physicist. His tools were his intellect, a slide rule, pencil, and paper.

In 1956 (*Phytopathology* 47:320. 1957) the Society set up a Standing Committee on Phytopathological Reviews to solicit and edit reviews to be published in the journal. Members were Stevens, Shay, and Sill. The Committee reported in 1957 (*Phytopathology* 48:116-17 1958) that Annual Reviews Inc. had appointed Horsfall to represent plant pathology on the editorial committee for the *Annual Review of Plant Physiology*. They proposed that his name be added to the Committee on Reviews.

The Committee continued in 1958 (*Phytopathology* 49:162, 169–70. 1959). Stevens complained about the difficulty of obtaining reviews. The members of the Society had clearly not overcome their longstanding feeling that the only worthwhile research was to continue to fragment knowledge, not to put it together again. A review by Gäumann on fusaric acid was accepted and printed, however.

In 1959 (*Phytopathology* 50:243–251. 1960) the committee continued to complain about the difficulty of procuring reviews for *Phytopathology*. Stevens, its dynamic chairman, published his "Bibliography of Reviews" as a supplement to *Phytopathology*.

That year the committee structure was drastically altered for reasons that were not and are not clear to me. Horsfall was made chairman. The other members were Boyce, H. J. Jensen, Ross, and Sill.

In 1960 the Society, seeing a wind change, tacked again and appointed an additional committee—a Special Committee to Study Publications and Public Relations, with Horsfall as chairman, and including Dimock, Hayden, Hewitt, McCallan, P. R. Miller, Ross, Snyder, and Zentmyer (*Phytopathology* 51:45, 53–4 1961).

This committee met during the convention of its appointment at Green Lake, Wisconsin (it should have been called Dry Lake, Wisconsin), and recommended among other things "that the Society should look into the possibility of publishing an annual volume that would attempt to synthesize the knowledge extant in various fields."

We sent to the council in early March 1961 a proposed policy for a new Society publication to be called *Perspectives in Plant Pathology.*

This proposal uncovered an arrangement being worked out by a publishing house in Holland to publish commercially a series, *Recent Advances in Plant Pathology.* This information in turn generated a proposal from Annual Reviews Inc. that they would publish at their own expense an *Annual Review of Phytopathology.*

They requested the American Phytopathological Society to appoint an ad hoc advisory committee. This was done (*Phytopathology* 52:464, 482–83. 1962). Horsfall (chairman), Holton, Kelman, Pound, and Snyder. This group met on call of Annual Reviews Inc. in Palo Alto, California in January 1962.

Ludwig was added, and the group was appointed as the first editorial committee to set up the first volume. In late 1962 K. F. Baker was appointed Associate Editor. The rest of the history is in the volumes of the Annual Review.

THE MEN IN MY LIFE

It goes without saying that no story there would be to tell, had there not been a series of great colleagues who participated in the cooperative research. They did the research, and I did the cooperating. In more or less chronological order they were Z. I. Kertesz, R. O. Magie, Ross Suit, Eric Sharvelle, J. W. Heuberger, Neely Turner, A. E. Dimond, G. A. Zentmyer, G. A. Gries, R. A. Barratt, Saul Rich, Richard Chapman, David Davis, R. J. Lukens, and Paul Waggoner. They honed the experimental designs to a sharp edge. They picked the flaws and improved the philosophy. To them a thousand thanks.

THE END

And thus my tale is told. I come to the end of my epistle. If you have read this far, you are a friend indeed, and I am grateful. Please forgive me for my immodesty. When it comes your turn to do the prefatory chapter for the *Annual Review of Phytopathology,* you will understand.

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