STEPHEN DENIS GARRETT (1906–1989).
(Portrait drawing by his father-in-law, Christopher Perkins)

Annu. Rev. Phytopathol. 1992. 30:27–36

STEPHEN DENIS GARRETT: Pioneer Leader in Plant Pathology

J. W. Deacon

Institute of Cell and Molecular Biology, University of Edinburgh, King's Buildings, Mayfield Road, Edinburgh EH9 3JH, U.K.

KEY WORDS: biography, biological control, root diseases, soilborne pathogens

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Few scientists attain the international stature that Denis Garrett attained. To do so through the study of soilborne plant pathogens is all the more remarkable. Garrett was one of the foremost thinkers in plant pathology and he contributed much to its conceptual basis. He was also, in many ways, the founding father of root pathology. Although not the first in this field, he showed how it could be investigated systematically by incisive experimentation and, through a number of important books and reviews, he served to unify it. These achievements have been widely acknowledged. Cook & Baker, in the frontispiece of their book on biocontrol, described Garrett as "Scholar of Soilborne Pathogens and Dean of Root Pathologists." He received formal recognition in his election to Fellowship of the Royal Society (1967) and to a Professorship at the University of Cambridge (1971). These rare and coveted distinctions crowned a career of more than 40 years in research, but they were preceded by something that pleased him even more—his election to a Fellowship of Magdalene College, Cambridge, in 1962. Garrett, however, was more than a notable scientist: as a man he set an almost unparalleled example of courtesy, charity, and wisdom. He was so modest and unpretentious that one of his colleagues was moved to say, "He wore his greatness like an invisible cloak."

How should one write about such a man? This question troubled me for some time because a bald recounting of his life and work would reveal little that today might seem exceptional. The reason is that his methods, approaches, and ideas evolved rather than burst onto the scene and were

0066-4286/92/0901-0027$02.00

steadily incorporated into work around the globe. As a former colleague, N. Robertson, said, "His ideas are now the coinage of plant pathological thought."

The approach I have chosen was in a way suggested by Garrett himself (8). In his inimitable way he described how his early difficulties in research were "considerable enough to make something to write about" and how he learned from the example of William Brown, the eminent physiological plant pathologist whom he always deeply admired. Of Brown he wrote:

> I said to myself "If he can do that, why can't I?" So I dissected the papers to find out how Brown did it, rather as one might take a piece of machinery to pieces to find out how it worked. I could not have selected a better model . . .

Garrett was to become a superb model in his own right, as a researcher, a writer, and a man. So I can think of no more fitting tribute than to record my personal view of the machinery of Garrett, from which we all might learn.

LIFE, CAREER AND PERSONAL ATTRIBUTES

A full account of Garrett's life and work was prepared by J. L. Harley (13) so only a brief outline is given here.

Garrett was born on 1 November, 1906, at Leiston, Suffolk, England. The Garretts of Leiston had for generations been noted designers and makers of agricultural tools and, as the nineteenth century progressed, of a wide variety of steam-driven machinery, mostly agricultural. Denis Garrett was very proud of the achievements of his forebears. He never wanted to be an engineer but intended to enter business of some sort; he studied botany almost as a recreation at Cambridge University, driven by his strong interest in natural history. But he recounts (8) how, having graduated, he was deemed too unworldly for commerce and his career as a pathologist began when F. T. Brooks recommended him for a post as Assistant Plant Pathologist at the Waite Agricultural Research Institute, Australia. He worked there under Geoffrey Samuel, from 1929 to 1933, but recognized that he needed more formal training and so returned to Britain to study under Brown at Imperial College, London. Then he moved to Rothamsted Experimental Station, where he stayed for 12 years and developed much of the experimental approach that characterized his work. He also began his "publishing career" in earnest at Rothamsted, producing several papers, substantial reviews on root pathology, and his first book (2). These early achievements were rewarded by receipt of the Sc.D. degree from Cambridge University; the terms of the Leverhulme Fellowship that supported him at Imperial College had precluded him from taking a Ph.D.

From Rothamsted Garrett went to Jamaica in 1948, to study Panama

disease of bananas, but he returned in less than a year owing to ill health. He later rationalized this by maintaining that a climate conducive to the growth of bananas is most unconducive to man! This type of statement, incidentally, was so characteristic of him that it might be called a Garrettism—a pithy and often amusing observation that represented, for him, the last word on a subject. A similar one was offered when I was seeking my first permanent post and said I was considering a job in Dublin. After a moment's reflection he responded, "I have always thought it good for young pathologists to gain overseas experience, and that is overseas experience in every sense of the word."

On returning from Jamaica, Garrett became a lecturer (1949–1961), then reader (1961–1971), and finally Professor of Mycology (1971–1973) at the Cambridge Botany School, where for many years he was head of the sub-Department of Mycology and, later, deputized for the head of the School. The sub-Department was never large, consisting of only three permanent academic staff—Garrett himself, John Rishbeth, and Harry Hudson in the 1960s. But it was almost a Mecca for soil mycologists, attracting sabbatical visitors from far afield. Among the names that spring readily to mind, David Griffin, John Lockwood, Tex Baker, Glynne Bowen, Harry Stover, and Don Munnecke all spent time there, as did many others of note.

Garrett's administrative load never seemed to be onerous but, like everything, he discharged it in a competent, unfluttered way that gave the impression of sedate calm. Administration was one of his many duties, which included undergraduate teaching, supervision of research students, editing of journals (*Annals of Applied Biology* and *Transactions of the British Mycological Society*), and serving on committees—he chaired the Executive Committee of the First International Congress of Plant Pathology. In addition, he continued to write books, papers, and reviews, kept up an enormous correspondence with other scientists, because his counsel was much sought, and remained an active bench scientist throughout. There was hardly ever a time when he did not have an experiment in progress. In his retirement he continued to work from a small home-based laboratory until he was forced to stop, owing to his declining eyesight and general health—the progressive result of diabetes diagnosed some years earlier. He died in Cambridge in December 1989, aged 83 years, survived by his wife Jane, and three daughters.

He had met Jane on the boat returning from Australia in 1934, and they were married a year later. It was a very happy and mutually supportive marriage for more than 55 years. Jane had a career of her own as a psychiatric social worker. After her retirement she took to historical research and published a monograph *The Triumphs of Providence. The Assassination Plot, 1696,* detailing an early conspiracy when the Jacobites were contesting the British throne. This monograph was followed by a memoir of her father, the

artist Christopher Perkins, whose drawing of Denis is reproduced as the frontispiece to this chapter.

Garrett's physical features were both distinctive and distinguished. His face broke readily into a warm smile that instantly put people at their ease and that betrayed his deep humanity. He had no regard for any of the outward trappings of authority or success; his clothes could best be described as serviceable, and the raincoat and cap in which he cycled to work, like the cycle itself, seemed more serviceable than the rest. Yet he was regarded with almost universal respect, borne of his achievements and, in no small part, of his behavior. He was "Dr Garrett" to all but his closest friends, a sign of recognition that he was somehow different from the rest.

In his relationships with others Garrett was the epitome of kindness and selflessness. He constantly used all his skills and tact to encourage and support others. He criticized as gently as possible, invariably couching his criticism in praise of one's finer features or achievements. So, to be chastized was almost to be flattered. Of an idea that he did not like he might say "Um . . . yes . . . that's interesting and it might be worth following up, but I really think your earlier ideas could most profitably be extended." To reinforce the point he then would praise the earlier ideas. Of mistakes he would say, "Do not be discouraged. I have found that my own errors have often helped me in my thinking—it may prove to have been useful in the longer term." If, as an editor, he recognized the hand of a young person or one with poor command of English he would completely redraft a paper to make it suitable for publication. And if the work was not up to standard he would reject it in the kindest way. Of one such paper from the tropics he wrote that he had read it with much interest to see "how your work has developed," pointed to the pressure of submissions to the best-known journals, and added that he would be surprised if the paper were not warmly welcomed by "your own (country's) phytopathological journal which will be looking for papers of quality." All true in a sense, but avoiding confrontation and unkindness while cleverly steering the person in an appropriate direction. This was the essence of Garrett's approach to personal relations; it seemed to come naturally to him but, in effect, it was man-management *par excellence*. By accentuating the positive, he managed always to promote the best in people. But beneath this charitable exterior he nevertheless held strong views. He had no time for people who persistently muddied the waters of science and who were experienced enough to know better. Of one scientist he is recorded to have said, "That man casts darkness on everything he studies." It is perhaps the only unkind thing that anyone can remember him saying.

Garrett followed the example of William Brown and never put his name on the publications of his research students, though many of us would admit that the best ideas in them were his and he often played no small part in the writing. There were thus many fields to which he contributed without formal

recognition. He later told me that he thought his policy had been wrong and that it should be reserved for only the best students, to avoid distortion. Yet his magnanimity was rewarded, because he could write with authority and first-hand experience on the many topics in his books and reviews. His generosity extended to his role as a biographer of distinguished colleagues; whenever called upon to do a job he did it assiduously, as a natural duty. The material for his Royal Society memoir of William Brown (9) took seven months to assemble; a more personal tribute appeared here (12). His memoir of Percy Brian (11), noted for his work on the gibberellins, griseofulvin, and obligate parasitism, took a full three months to write. Garrett also produced a memoir of W. J. Dowson (10). All these personal qualities evoked admiration; Garrett commanded natural respect and warm affection.

Outside of work, Garrett had a life-long interest in plants and was a keen field botanist. He knew nearly all the flora of Britain and western Europe, and kept a card index of all the species he had seen—a characteristically thorough approach to his hobby. He was not so adept, nor I think interested, in identifying the larger fungi. But he was a "specialist" in at least one respect: on mycological forays he wandered alone and, spurning the most sought-after species, collected *Amanita rubescens,* which he valued for its ubiquity and particularly liked to eat.

RESEARCH

Garrett's name will always be linked with the take-all fungus, *Gaeumannomyces graminis (Ophiobolus graminis),* which he worked on throughout his career. He also studied other pathogens, including *Plasmodiophora brassicae, Armillaria mellea,* and cereal foot-rot fungi; and through his research students he contributed to work on *Helicobasidion purpureum* (violet root rot), other sclerotial fungi, *Agaricus bisporus* (cultivated mushroom), *Serpula lacrimans* (dry rot), cellulolytic soil fungi, and various biocontrol systems. In every case the work, though based in a laboratory or glasshouse, was directed towards understanding the behavior of fungi in nature and to the prospect, where appropriate, of achieving practical disease control. So Garrett was not an academic plant pathologist in the narrow sense; rather, he took his cues from problems in the "real" world and tried to tackle them through an experimental approach. It might be argued that this approach was unsuccessful because they remain as practical problems today. But, in truth, most soilborne pathogens can be controlled effectively by the methods that Garrett and others developed; the problems exist only because these measures have been abandoned in current intensive agriculture, and it is an open question for how long this practice can be sustained on environmental or social grounds. Anyhow, Garrett's contribution extended far beyond the specific problems that he investigated. He always had a view to the general applicability of his

findings and he used these to develop concepts of lasting value. Many of them have guided the development of the subject to the present day.

To trace the history and significance of Garrett's experimental approach we must return to his early days in Australia, where he was a field-based pathologist. He soon recognized that field studies could give only a limited understanding of soilborne pathogens because root disease is influenced by many interacting factors, including soil and environmental conditions, cultural practices, the crop's growth, and the activities of the pathogens themselves and other microorganisms. So he resolved to work in strictly standardized conditions in the glasshouse or laboratory and to test the effects of altering single factors against a precisely controlled background. He also recognized the need to study not only the parasitic phase but also the saprophytic and survival stages of soilborne pathogens, because all these aspects must be integrated to explain the behavior of pathogens in the field. Perhaps none of these points can be attributed uniquely to Garrett. His early reviews (e.g. 1) and his first book (2) show that he had thoroughly searched the literature and would have seen the separate threads. But his contribution was systematically to develop and pursue the experimental approaches, drawing all the threads together into an integrated body of evidence and concepts. This set him apart from others and made him a key force in the development of root pathology.

Garrett's experiments were, in fact, very simple and his materials almost crude. For most of his working life he used tumblers or jam jars as soil containers, because they were cheaper and more durable than scientific glassware but served the same purpose. He also used a narrow range of soils, to ensure standardization, but they were always field soils so that the results were biologically meaningful. Many younger root pathologists who were not trained in the Garrett mold would do well to note this. A typical experiment of Garrett's might involve no more than a dozen jars, representing four treatments with threefold replication so that the results, as always, could be analyzed statistically. To my knowledge, he never repeated an experiment and strongly advised against this: if the repeat experiment gives similar results then what is gained, if different then what is to be done? Instead, he argued that one experiment should logically follow another and serve both to reinforce and to extend the findings of the first. Garrett's experiments did just that. The commonplace materials belied the sophistication of the work. Each experiment was perfectly designed in every respect and with much intellectual forethought, and each was done with absolute care and attention to detail. So the results almost invariably were clear-cut. I was always astonished at how such simple experiments could yield so much information and, in Garrett's hands, generate a new or modified idea of general significance. The reason is almost certainly that he did few experiments, but did them well, critically to test an existing hypothesis or a new idea. His wide knowledge and mental agility also enabled him to relate his findings to work in quite unrelated areas.

Economy was something to be employed in every respect except thinking, planning, and execution.

Garrett's students were trained in the same way (although I suspect none would claim with full success!). Typically, he provided each new student with a clear synopsis of the field of study, usually extending to several pages and referring to key publications. With this came a detailed plan of the first experiments to be done—enough for many months' work. After some preliminary discussions you were left to get on with the job and Garrett held formal supervisions at approximately monthly intervals but made it clear that he was always available when needed. At first, I bombarded him with data and ideas at these formal sessions and, frankly, was disappointed by his response: he encouraged, gently corrected, and referred to other work that might be relevant, but there was little sign of a great mind at work. However, he invariably called me back the following day, having thought about the points, and more often than not he came up with a new approach that incorporated the best of my ideas but was far better than anything I had thought of. From this I learned many things, not least that a research student should prepare something for a supervisor in advance and give him or her time to digest it. Garrett's research students, of course, used the same experimental approaches as he, and it was sometimes difficult to justify them when faced with the high science of one's biochemical colleagues. But all took comfort from the fact that we had been asked to use jam jars and not the substandard black lavatory cisterns that Garrett had bought some years earlier as ideal soil containers for glasshouse work. That distinction fell to one of his last research students. In the relevant paper they are described as "soil containers made from a black plastic material and with a hole in the bottom for drainage; internal measurements . . . 46 cm long × 20 cm wide . . . depth of 23 cm!"

There is little need to discuss the details of Garrett's research; he did this in his books and reviews, where he integrated it into the wider body of knowledge. But one particular experimental system illustrates the robustness of his approach—the technique that became known as the "Cambridge method." It was designed to study the saprophytic activities of pathogenic fungi and involved burying pieces of sterile wheat straw in jars of soil containing graded amounts of inoculum of a pathogen. After usually 28 days the straws were retrieved and the percentage colonized by the pathogen in competition with the soil microflora was determined by bioassay. With this system his students compared the competitive saprophytic abilities of different cereal pathogens, providing both a quantitative basis of comparison and information that could help explain the contrasting behavior of pathogens in crop residues. Comparable experiments enabled the survival of different fungi to be studied in straws that were colonized before burial, and the comparisons were extended to include the effects of factors such as soil pH, soil water content, mineral nutrient supply, and temperature. We now know that, because of the artificial

conditions, these experiments may not precisely reflect the activities of pathogens in natural crop residues. Nevertheless, the comparative aspects of this work have not been challenged, and the approach as a whole brought order to an otherwise complex field, enabling progress to be made on a broad front.

Not all of Garrett's model systems worked so well. One of his students spent many frustrating months investigating the production of sclerotia by *H. purpureum* on moist cylinders of filter paper (simulating tap roots) with projecting cocktail sticks (simulating root laterals). The filter papers were supplied with different amounts and proportions (C:N ratio) of nutrients in an attempt to explain why sclerotia often form around small root laterals rather than on the fleshy storage roots of sugar beet, carrot, etc. The innovation of this approach, unsuccessful though it was, will always capture the imagination—how interesting our journals would be if they were filled with such original ideas.

A serious point underlies this account of the unconventional materials and approaches that Garrett used. In the best scientific tradition, he saw to the heart of a problem and devised, through a "reductionist" approach, the best way of investigating it. He was not afraid to follow his convictions, nor to use the simplest means that were appropriate.

WRITINGS

Perhaps Garrett's main influence on plant pathology stems from his writings. He published over 70 papers, books, and reviews in his own right. His first book (2) was a *tour de force* in assembling and synthesizing all the then-known information on effects of environmental factors on soilborne plant pathogens. But it is now little known and much of the information in it was not included in his later books, *Biology of Root-infecting Fungi* (4) and *Pathogenic Root-infecting Fungi* (7). These are devoted more to concepts in root pathology and are based increasingly on the experimental approaches that he and others had brought to bear on the subject. Quite apart from their scientific content, the books are testimony of Garrett's craftsmanship. He was a naturally gifted writer who conveyed his meaning in a flowing but precise style and with a flair that made even the most difficult concept easy to understand. Page after page, the books contain a surprising amount of data abstracted from the scientific literature, but presented in a way so easy to read that it is almost conversational. Perhaps because of this there has been no natural successor to his *Pathogenic Root-infecting Fungi*. So persuasive was Garrett's style and so intimate was the blend of scientific data and philosophy that the only way of updating his texts is to dismantle them and start afresh. The result, one fears, would not nearly match the original.

Again, the scientific content of these books is too well known to need much

comment here. Through concepts such as specialization of parasitism, the ectotrophic infection habit, inoculum potential, and competitive saprophytic colonization, Garrett surveyed the entire field of root pathology, setting everything in its place as he saw it. He was also a firm advocate of biological control long before it became fashionable, and many of its basic tenets and approaches were discussed in his texts. His other book, *Soil Fungi and Soil Fertility* (5), was intended primarily as an introduction to soil microbiology and mycology for undergraduates. It ran to a second edition but its content and emphasis are now dated.

Of Garrett's major reviews one of the most influential was *Ecological groups of soil fungi: a survey of substrate relationships* (3). It served for many years as a useful classification of the behavior of soil saprophytes and it separated, among others, the groupings that Garrett termed primary and secondary saprophytic sugar fungi. Such ecological groupings based on substrate utilization still have inherent appeal and may not have outlived their usefulness. But fashion—at least in Britain—now dictates that saprophytes are saprotrophs and, more important, that groupings are too artificial; instead, the favored approach to fungal ecology is based on strategy theory that was first developed for animals and then plants. Stemming from this, individual fungi can be viewed as occupying some position within a triangle, the three points of which represent the extreme expressions of ruderal strategy, stress-tolerant strategy, and combative strategy. I suspect that Garrett would have applauded this conceptual advance but rued the fact that the practical simplicity of groupings had been lost. It was always implicit that no two fungi occupy precisely the same niche—a basic ecological tenet—even if they are grouped together for convenience.

One of Garrett's most masterly reviews and the one that perhaps best reveals his character is *Toward biological control of soil-borne plant pathogens* (6), presented at the first major international symposium on this subject, in California, 1963. It can be read as profitably today as then. Garrett pays tribute to all involved in the development of this field, identifying the key players and the significance of their specific contributions. Then he surveys the state of the art and its scientific background and, dealing with the prospects for biocontrol, cautions against blind optimism. He writes, "There are no short cuts to biological control; that is the mistake that many of us have made in the past and that some of us, no doubt, will make again in the future." How right he was, and how little his words were heeded in what remains, for all this, a most promising field of development.

POSTSCRIPT

Remarkable is a term that comes readily to mind when thinking of Denis Garrett. He was remarkable as an individual, as an innovative researcher, and

as a writer. His contributions have now passed into history and, ironically, were instrumental in the developments that make them now seem rather old fashioned. But this is perhaps the fate of all who deal in concepts and approaches, unlike those who describe invariable phenomena and who will forever be credited with their discoveries. I think history will show that, as a guide, Garrett was the equal of William Brown on whom he modelled himself.

I still have a letter from 1974, after I left Cambridge where I worked under Garrett for 5 years. In it he wrote, "it is high time you stopped calling me Professor Garrett and use my christian name instead, as most of my former research students do at my request." Then he apologized for writing on such a small matter and added, "No need to acknowledge this, of course, but remember for the future." I speak for many in saying that I feel deeply privileged to have known him, as Denis, mentor and friend.

ACKNOWLEDGMENTS

I am very grateful to R. Jane Garrett and Noel F. Robertson for reading this in draft and for supplying some of the information; also to Jane Garrett for the photograph.

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