

ERWIN FRINK SMITH

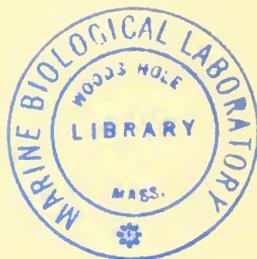
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ERWIN FRINK SMITH

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ERWIN F. SMITH

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A STORY OF
NORTH AMERICAN PLANT PATHOLOGY

ANDREW DENNY RODGERS III

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To Dr. Edgar Nelson Transeau

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Most of the material contained in this book has been quoted direct—or obtained from unpublished correspondence which has never before been generally available. Footnote references explain the previously published sources. Two years before his death, Dr. Smith planned to write an autobiography and materials saved throughout his lifetime for this purpose have provided by far the largest portion of the unpublished sources. The author acknowledges his indebtedness to Mrs. E. F. Smith for many interviews and for permission to make use of these materials including diaries, journals, correspondence, reprints, books, etc. Permission, furthermore, to use certain official documents and correspondence, copies and originals, has been given by officials of the United States Bureau of Plant Industry. The author obtained photostated copies of a collection of materials held by the Agricultural Division of the National Archives, Washington, D. C. Permission for the use of these has also been granted by officials there. Letters written by Dr. Smith to Dr. W. G. Farlow of Harvard University were photostated; and for permission for their inclusion indebtedness to officials of the Farlow Herbarium is likewise acknowledged. Dr. T. S. Cullen lent the author the originals of the letters from Dr. Smith to him. From various persons and institutions letters written by the subject of this biography have been secured; and each letter has been specially identified by a footnote or in the text.

The list of libraries and their facilities utilized by the author has included the Library of the United States Department of Agriculture, that of the United States Department of Education, the Library of Congress, the Library of the Michigan State Board of Health, the Ohio State University Library, that of its Medical School and Botany and Zoology Departments, the Ohio State Library, the Columbus and Bexley, Ohio, Public Libraries, and many libraries of botanical and scientific institutions. Indebtedness to each one is acknowledged, especially the Ohio State University Botany and Zoology Library, and to Dorothy (Mrs. E. M.) Schreck its librarian.

Many persons acquainted with the life and work of Dr. Smith have been interviewed by the author. They include various of his laboratory scientific associates and assistants and many scientists in different research fields from the Atlantic to Pacific coasts, their work or collaboration being described fully or in part in the narrative.

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CHAPTER I

BOYHOOD, EARLY SCHOOLING, AND TEACHING. *FLORA OF MICHIGAN*.
STUDY AT MICHIGAN AGRICULTURAL COLLEGE.

ERWIN FRINK SMITH was born on January 21, 1854, at Gilberts Mills, New York, the son of Louisa Frink Smith and Rancellor King Smith, a tanner and shoemaker by trade. Nine generations of the ancestry of Rancellor King Smith had lived in the United States, as early as 1628-1629 the first having migrated from England, settled at Salem, Massachusetts, and, moving on to Connecticut, attained to positions of considerable wealth and eminence in various towns, Norwalk most prominently. Toward the end of the eighteenth century, about 1784, one Noah Hoyt, the seventh of ten children, migrated to New York, lived for a while in Saratoga County, and settled permanently in Onondaga County. Lucy King, the granddaughter of Noah Hoyt, married one Charles Smith on November 15, 1821. He, it is believed, hailed from Palermo, Long Island, New York. But he took up his occupation as a tanner in Oswego County, and the couple was blessed with ten children, of whom the third was Rancellor. On September 24, 1850, and at the age of twenty-four years, this son, who had chosen his father's trade as his life work, married Louisa Frink, of Herkimer County, New York, daughter of Asa Leonard and Lydia Ellis Frink, and to them were born two children, Erwin Frink, and Lillian, some two years younger.¹

Gilberts Mills, located not far from Syracuse and in the midst of a progressive agricultural setting, was named for its principal industry—its flour mills. It was a contented, thriving village, of happy, strong folk. Many years later Erwin Smith redescribed his recollections of this serene, untroubled community where he spent his first fifteen years:

¹ This paragraph is based on materials which Erwin F. Smith himself gathered and was in part founded on data taken from David W. Hoyt's *A genealogical history of the Hoyt, Haight, and Hight families*, 2nd ed., Providence, R. I., 1871.

Big droves of kine, orchards on all the hills,
 Flower-banked white cottages in all the vales,
 Broad pastures, zigzag-fenced with hemlock rails
 The anvil's clang, the rumbling busy mills.
 Tan-vats in rows, a salt-well's heavy drills,
 The parson's Sunday face, the miller's tales,
 Mowers with scythes and maids with milking pails.
 The mill pond's mysteries! The mill-stream's thrills.

Hour after hour he watched "the giant mill-beams gloom in rows cobweb-festooned and hoar with whitest flour." Student from his early years of intellectual awareness, he meditated on the "prisoned god" he heard in the grinding of the mill-wheel. But more, he studied the "finny tribes" he saw below the mill-wheel. A stanza of quiet rapture caught his spirit:

Daily round and round the mill-wheel goes,
 Dull rumbling like a demi-god in pain;
 And endlessly like some god's ichor flows
 The crystal stream o'er its green algal plain.

He early began to find "the strong sweet life of Nature . . . the God of earth and heaven whom Science bares." Nature's processes, as well as her myriad forms, appealed to his observation. "This swarming earth," he later wrote,² "has been a trial field wherein a virile God has sown in haste a horde of struggling forms, hoping as yield to reach some consummating perfect stage—elusive ever and evermore erased." The *type* is precious to Nature; and the "rocks are full of types sketched out, reviewed, found wanting, cast away, or slowly trued to perfect forms. Ceaseless, God's winnow-fan sorts better still from best." Of all the mind's highways "the most sublime" were those made from "deeps of space" by the astronomers. He, however, lived and worked closely to the earth. In April he was often "a wandering boy, in hemlock forests dim," enjoying the "spring's sweet ferment," and conscious that "myriad million cells to one blind end, in harmony attuned, within each tree, expand and bud: absorb, consume and blend the gifts of earth and air by sun set free." He wrote:

² The Geological Record, January 2, 1913. Other quotations are taken from published and unpublished poems which have a definite retrospective and biographical significance.

The plants, the swarming life in every nook,
Bird, beast, and frog, I was their intimate,
And they my harvest home and great estate!
What lessons there I had from Nature's book! ³

Ants interested him all of his life. He watched them, studied their habits and activities, explored their habitats, and wrote of them:

I do not claim to know much about their mental processes; they may have some. They certainly do most of the things done by primitive peoples and I think they possess the rudiments of memory and reason. They are feudal robbers. They have division of labor: soldiers, workers of various sort, nurses, etc. They are agriculturists and harvest grains, honey and flesh, and gather manures for their fungous gardens. They keep chattels, make slaves, and fight desperate battles! Their nests are gynecocracies, and the males are reduced to a fugitive life, ending with the reproduction of the species. They have no tools or weapons, their fierce jaws being all-sufficient. They have games but no religion, unless it be the worship of their queen. There are more than a thousand species, but the largest are scarcely an inch long.

Of "Ants" he wrote also in poetry:

Safe housed in earth each queen gives myriads birth,
Slight reason animates the swarming band,
A hungry *plunderbund*, as one they stand,
With instinct strong as death in reason's dearth.
They are a curious race of little worth,
The tribal need alone is their command;
Had they but larger grown, born to expand,
They must have been the dominant forms on earth.

Some secret force decreed they should be small,
Else had lions and tigers no chance at all,
And men been forced to live in trees like birds.
That we are not their slaves, to fetch and bring,
Or tend their gardens and their aphid herds,
Thanks to that force: through it, man stays the king! ⁴

Rare happy hours were his along the streamlets and millponds of the countryside. Sometimes he dropped his fishing line for a catch. More often, however, his eager, inquiring mind captured fresh knowledge of the life histories and habits of the animal or plant life he observed. Of "Cat-Fish," he wrote:

³ The Brookside, September 2, 1926.

⁴ July 4, 1926.

Within a group that has so little care
 For home and its own young and conjugal life,
 To see two fishes live as man and wife
 And guard their eggs and rear the young they bear
 With tenderest solicitude is rare.⁵

Or of frogs:

The aromatic birch by pondsides grew
 And where tall hemlocks joined the cedar swamp;
 Bullfrogs splashed there and cried har-r-omp, ar-r-omp.

This boy went about the countryside studying the living forms of hills and valleys, intent only on knowledge for knowledge's sake. He revelled in the "thousand sights and scents and tastes" of nature, the "faint sweet song the August crickets sing," the "gold-green" landscapes, the green meadows, brown pastures, and dense dark thickets. He learned as much as a boy could of agriculture, how corn and oats and other farm products were grown. When in "The Meadow,"

The golden buttercups were his delight,
 Filling June meadows full of pure sun-gleams;
 Then came wild strawberries, luscious, red and white;
 Blue *Mimulus* along the winding streams.

In August blossomed stately meadow-rue;
 Slender *solidagos*, braiding golden hair;
 Gorgeous *Turks-cap* lilies, just a few;
 With purple fringed orchids here and there.

All days the bobolinks and blackbirds sang;
 Crows called and golden meadow larks flew low;
 With his shrill shout and song the meadows rang,
 For glad was the boy, beholding fair things grow.⁶

Even at home on long Sunday summer afternoons, he found beauty and dreams in his surroundings: "Then would [he] sit beneath the lifted sash; To hear the music of the falling stream, Or lie beneath the dooryard's mountain ash, Steeped in some poet's" flight of fancy. The sounds of birds ringing through the copses, the sight of blackberries ripening on the barren hillsides, of the Jo-pye weed in bloom, of the sight of *Lobelia cardinalis* the crimson flower, or sweet flag, or the "orchard crowned"

⁵ July 5, 1926.

⁶ September 3, 1926.

valley: these, and a multitude of other joyous memories, lingered with him many years. There was formed his deep consciousness of obligation to the Creator. Insights into nature were awakened there, which, with the years would mature and quicken an inspired interest in interpreting facts of creation. Erwin found God in the fields and forests about him, as well as in the church of which he was so regular an attendant. Whereas he sought ostensibly for things, fish, water-weeds, red berries, the robin's nest, and other natural objects, he discovered an elusive, but very real, cosmic kinship with a "subtile presence everywhere that lends all motion, light, and life," which comprehends all and "in one harmony all discord blends." "All tends to good, all life is one in span," he later wrote. "The Love Divine beneath its brooding wings, tender as mother's love, enfolds all things that live and move, from monad unto man; And what to us seems wrong in God's vast plan, to our disturbed and faulty vision clings, as low-hung clouds obscure the lark that sings, or blot Polaris and Aldebaran."

Probably Miss Ida Holmes, a friend and school teacher, more than anyone gave Erwin his intelligent love of nature. Years of Civil War between the North and South wrought a change in the family of Rancellor King Smith. Lucy King Smith died while serving as a nurse to Union soldiers. R. K. Smith enlisted with Company K of the 184th New York Infantry and for years was away from his home. While located with the Army of the Potomac near Washington, D. C., in May 1865, he received a letter from Ida Holmes which told of his children. "Lilly," she wrote, "has grown like a weed. Irwin too has grown, and is quite a gentleman. He reads my books with as much appreciation as anybody and is, I think, an uncommonly intelligent boy for his age." He borrowed from her books by Tennyson, Longfellow, and Dickens. By the hour he listened to her tell "of Lowell and Hawthorne, of Emerson and Thoreau, of James Freeman Clarke and Edward Everett Hale; and of what the world would be when all men should be noble and all women should come into their own."

She lent him copies of the *Atlantic Monthly*, *Harper's Magazine*, *Putnam's Magazine*, and *The Round Table*. She believed in the youth's future and, when a few years later she died, he cherished the memory of her spirit and at last offered the following tribute to her in verse:

O woman crowned! How much I owe to thee!
 For thou didst teach the boy, as best of creeds,
 Dearly to love great books and noble deeds,
 And through their subtle alchemy be free.

The torch of learning kindled thus in me
 Burns on, a sacred flame, which inward breeds
 A hope that somehow answers to my needs,
 And widens toward the universal sea.

In that large world of saints and seraphim,
 That to our mortal eyes remaineth dim,
 Would I could trace thy goings, spirit mother!

In vain! But I have learned thy lessons well,
 And when, in Death's Great Silence, muted, I dwell,
 Many whom I have helped shall call me 'Brother'!⁷

Books and journals of scientific as well as literary interest were included in his reading. Among the journals were *Appleton's Journal of Literature*, the *Journal of Applied Chemistry*, and *The American Naturalist*. Among his books were *Humboldt's Library of Popular Science Literature*, works on science and art, "Dick's Complete Edition of Shakespere's Works," and books by other masters.

On March 20, 1870, he mentioned in his diary having received a copy of Asa Gray's "School and Field Book of Botany." For many years he was undecided between literature and science in his choice of a career for himself. In fact, once deciding in favor of science, he next experienced difficulty in choosing between its various branches: botany, chemistry, physics, and geology. His interest in biology seems to have been early bound with his enjoyment of the study of plants and animals. While quite young, he received a present of a two-volume "Hydropathic Encyclopedia." The medical profession also, because it relieved suffering, appealed strongly as another possible career for himself.

One afternoon he would read Thomas Gray's poems, and the next, experiment with his crude chemical apparatus and paraphernalia. Erwin ordered his chemicals from reputable companies. His investigations covered some points of knowledge useful to him later in experimental research. For instance, he⁸ "read in

⁷ May 30, 1924.

⁸ E. F. Smith, Mechanism of tumor growth in crowngall, *Jour. Agri. Research* 8 (5): 165 ff., at p. 171, 1917.

some boys' wonder book that coffee berries, said to be proverbially slow to germinate, might be germinated overnight by throwing them into strong ammonia water. [He] bought some ammonia with [his] pocket money, tried it, and found it to be as stated, but [he] held no checks." Fifty years later, while studying ammonia or salts of ammonia as a possible determining factor in the growth of tumors in plants, he repeated these experiments and this time held controls. He connected the work of his adult years with the discoveries of his youth thus:

I have extended the experiment also to date seeds and to some others known to be hard to germinate—that is, black-locust seeds, but only with doubtful results, and even in the case of coffee seeds some germinate promptly in distilled water, so that, curiously enough, the experiment which more than any other determined the trend of these researches would have had no influence on my thinking had it been made in the first place properly—that is, with a sufficient number of controls in pure water.

While midway in his 'teens, Erwin began to study cells of plants and plant life under the microscope. On January 5, 1870, he wrote in his diary: "I studied cell life some tonight. [U]sed Craig's microscope (100 diameters). [S]aw the cells in two house plants, moss and cacti" He drew plates of what he saw. In the pith of elder he observed "six sided cells very plain" and compared these with the skin of human hands. Microscopes were rarities in those years, especially in rural America, and he longed for one of powers strong enough to carry his anatomical comparisons further. He was reading a text on anatomy and physiology, and from the "Chemical News Am[erican] Reprint" of November, 1867, he had copied a formula for preserving anatomical specimens.

He was an amateur botanist as well as a student of human physiology; and amateur botanists, interested in cellular physiology, were as rare as professional botanists whose work extended beyond taxonomic classification of plants. Most botanists, amateur and professional, were natural historians, and very few in America were genuinely interested in plant physiology.

Many of Erwin's amateur research ventures served practical purposes, of immediate use, such as the preparation of variously colored inks, medicinal prescriptions, glues, salves, stain removers, cements for leather and gas pipes, and metal coatings. Of course, his investigations required a knowledge of the few known basic

chemical elements, the simple compounds, and formulas for their utilization.

On January 7, 1870, his diary read: "Father started for Michigan yesterday. Gone to look at land and to visit friends." R. K. Smith, for some time now out of the Army, had decided to join the westward migration moving in large numbers of persons to the frontier agricultural sections of the middle western and western states. Michigan already was a land of hope and promise, and a virtual boom in farm-land values of many years standing was not entirely over. Orchards of the best quality in the larger fruits, notably apples and pear, peach, plum, and cherry, together with vineyards and other small fruit culture, were being planted throughout the state; and the stories of abundant harvests and healthful living conditions caused many to dare the dangers of the temporary lacks of civilization. While by far the largest land area was still under forest, the state's industrial future seemed assured because of the extensive business development from lumber and horticultural crops. Urban as well as rural centers were growing in population. The school system was adequate; and in its agricultural college at Lansing and its university at Ann Arbor the state had two of the best educational institutions of the nation. Early in March, R. K. Smith returned to his family at Gilberts Mills, New York, and on March 7 Erwin wrote in his diary: "We start for the west the 20 of this month. Father has bought a farm of 80 acres in the northwest corner of Clinton Co[unty] in Michigan near Maple river The Future will soon be the Past. God speed the Present."

A deep snow prevented the family's departure on March 20; but the next became the "eventful day," and until the morning of March 23 they rode "in the cars," crossing Suspension Bridge during the night of the twenty-second, and reaching Detroit the next morning. From there they went to the home of Erwin's grandmother in Ionia County and lived about two weeks at North Plains.

Erwin was so excited on reaching "the west [which had] been settled in the last half century" that he wrote nothing in his diary until April 8 and 9 when, their household possessions having arrived by freight, they moved to their own farm. He described his new home in the midst of

beautiful spring weather. . . . The farm Father has bought is somewhat stony on the front part of it, more so than he knew for, he buying it in the winter, but this can be endured. [I]t must be endured. [O]therwise I like it quite well. [I]t is pleasant, I think, the river running by our door about an arrow's flight distance away, the banks lined with trees. [T]he land rises from the river into a hill, part of which is covered with trees behind the house. [T]he farm has about 150 young fruit trees, say six years or so old. [T]he woods are mostly Oak, though there are Walnuts—Black Walnut, Sweet Walnut, Bitter Walnut—Beeches [and] a very few Red Elm.

Maple River was neither deep nor wide. In the spring the overflow of its banks would cover large land areas. But its valley was beautiful, and he learned to love it as much, if not more than, the countryside around Gilberts Mills, New York.

Immediately after uncrating their furniture and settling their home, Erwin went to work woodchopping and picking up stones. On April 14 they manured and plowed their garden. Ten days later he and his father went into the nearby village Hubbardston and purchased a cow and another plow. All that spring and summer he helped his parents with the farm work and household chores. They harvested hay, corn, beans, apples, potatoes, and other garden truck. They cut acres of clover for seed and reaped a good financial return per bushel. They threshed buckwheat, sawed wood, sold oats, killed hogs, hunted for wild game including turkeys, gathered hazel nuts and, all in all, their profit that year seemed to vindicate the wisdom of casting their lot in a frontier state.

Erwin was affectionate by nature and he longed for the loved ones left at Gilberts Mills. Soon after arriving in Michigan he had begun to write letters to friends and relatives, and by April 16 there was a prospect of answers. He read botany part of the day and in the late afternoon walked through a wet drizzling snow-storm to Hubbardston after the mail. As he trudged along the icy and snow-covered road to town, his spirits were buoyant for he was happy in the thought that, regardless of the years of hard work before them, his family had a good home and "a good farm as far as soil [was] concerned." Many persons would have been discouraged because of the land's stoniness and stumpiness. The sizable mortgage on the farm could have occasioned misgivings for the future, even despair. He, however, was always happy

when out-of-doors. His parents had gone to the county seat on business matters and left him with no special work. He, therefore, gave himself over to searching for natural objects to add to his collections of Indian relics, plants, stones, and animals. After he had explored the three and one-half mile area to the village, he went to the drug store where was located also the post office. Probably on this journey he became acquainted for the first time with Charles F. Wheeler, the village druggist and postmaster who, for the next sixteen years, would be one of his closest friends.

Erwin was attracted to him "first because he was very congenial and one of the few distinctly intellectual people of the village, and because he was a born teacher and a man who rapidly and effectively got hold of young people for their improvement. Next to the church and perhaps more so than the church, the drug store in that village was the intellectual center."⁹ At least two obvious reasons account for Wheeler's and young Smith's immediate interest in each other. Wheeler also had been born in New York State. Educated in the famous Mexico Academy located in the town of his birth and bearing the same name, he had journeyed westward and entered the University of Michigan where he completed one year but left to enlist during the Civil War on the Union side. Contracting pneumonia, he had been rendered unfit for further active service. He then married and settled at Hubbardston where in the drug business in partnership with a cousin, Henry Wheeler, he remained for twenty years.

It is not improbable that when on April 16 Erwin walked to the village drug store to obtain the family mail he had in his pocket his copy of Gray's *Field, Forest, and Garden Botany*, the second edition of which had been published that year. He had heard of "a man in the village by the name of Wheeler who was interested in botany." Quite likely, therefore, he inquired for him, made his acquaintance, and "from that time on" they "got together frequently to compare notes and specimens." As the spring enlivened the earth with greenness and blossoms, they began to collect together and separately. Erwin studied botany that year from spring through autumn and by December he was

⁹ Quotations are taken from a memorandum prepared by Smith many years later, but left undated and unsigned.

referring in his diary to botanizing expeditions with "Charlie Wheeler." They had decided to explore "the whole state as far as [they] could do so and to make a catalogue of the plants of the state which should be something more than a mere catalogue, that is, [it was to] include geographical and ecological notes." Ecology was a word not yet in use in America; and only in rare instances were plants and plant societies studied in the light of their physical and biological factors.

Geology, entomology, and medicine each appealed to him as study subjects. But, most of all, he enjoyed the study of plants and reading the books on botany. A "great variety of strange and beautiful plants and shrubs" were to be found around Hubbardston and the environs of his father's farm. He brought plants and shrubs from the woods to his mother's flower garden. Years later he recalled his mother's garden in a poem:

There lilies opened white and creamy wells;
There daffodil and tulip flaunted gold;
Rathe primrose, crocus, hyacinth, made bold;
There larkspurs grew with Canterbury bells,
Dark peonies where no sweet odor dwells,
Snow-ball, mock-orange, iris yellow-stoled,
June-rose, tall hollyhock, pinks manifold,
With half a hundred more fond memory tells.

And there the boy each spring and summer saw
The lithe shoots push from beds of warm brown earth,
Then leaf and flower appear, by some fixed law;
And wondered o'er it all, as seasons rolled—
The yearly miracle, the strange new birth—
And wonders yet the more, now he is old.¹⁰

When the family moved to Michigan, the central parts of the state were covered with huge forests of white pine. Many of the pines were more than one hundred feet high and had trunks of three to four feet in diameter. Some towered one hundred and thirty to one hundred and forty feet and bore diameters of five to six feet. The lumbering industry already had started to devastate wide areas of valuable white pine forests. Yet many a summer day he roamed the forests, searching for the dainty, creeping, twin-flowered *Linnaea*, *Clintonia*, or orchids. Often he visited the chestnut and beech woods, climbed high into their branches, bare

¹⁰ April 6, 1913.

footed and bare handed, and returned proud of his store of golden burs and meaty nuts. He learned the life habits of the field mouse and timid hare, the brown bats which flew about on velvety wings, the tawny foxes which lived among the hills. He found shells, bones, and other fossiliferous evidences of ancient life in rocks of the region, and prized them "more than gems." Of nights he read on the region's natural history, examining his "treasure trove" of corals, crinoid stems, and broken reefs from glacial drift, and learned of the far past epochs and eras of the geological calendar, from the period of Devonian seas and shores through the Carboniferous and Cretaceous ages to the coming of man on the earth. His imagination must have dwelt especially on the "Strange herbs, and forests of the weirdest trees; Low shores that swarm with life" where were no flowers or bees, and before the coal was massed. Because, later in life, in 1925, he wrote a group of poems on these subjects, and in each the reactions of an "eager boy," obviously himself, were presented. Of "Evolution" he was to say:

Out of the Earth itself came man and beast,
 What's anchor'd, swims, or flits on painted wings,
 The endless multitude of creeping things,
 Tall trees, green herbs, gray lichen, fungus, yeast,
 All eager to partake of Life's great feast—
 What runs and leaps and soars, or merely clings,
 What rustles, murmurs, hisses, roars or sings,
 Yea, whatsoever living thing is least!

Earth swarms! So be it, there is room for all!
 And still they come! As on creation's day,
 Triumphant rise new forms while old ones fall.
 But the God in sentient things, the urge divine
 That vivifies—That too must be in the clay:
 Each atom of it throbs, a living mine!¹¹

Even before he knew much of Charles Darwin's *Origin of species by means of natural selection* (1859), or Darwin's or Alfred Russell Wallace's theories on evolution as presented July 1, 1858, before the Linnean Society of London, he had observed evidences of the relentless struggles for survival among natural forms. One day he watched a large hornet battle with three flies, take the juice of one and at the same time try to catch another;

¹¹ February 26, 1923.

and he recollected that he had read somewhere that wasps kill insects and place them in their cells to feed on their larvae when they hatch out. He was unafraid of reality and the facts of nature, yet he adored legend and fiction and all things of the imagination. In the forests he could reconstruct in fancy the sacrificial rites of the Druids, the while he was watching a woodsman chop down a tree. He could be at one and the same time a Robin Hood in imagination and a woodchopper in actuality.

Late in October 1870 the Smiths added one hundred new apple trees to their orchard, and Erwin journeyed joyfully with his father to a nearby railroad station to get them when they were shipped from a Monroe nursery. When picking apples, high on a ladder, a basket on his knee, he was prone to sail his romantic ships far beyond the thoughts of apples and relive the poets' legends which haunted his brain and murmured on his lips. A rumor spread that soon a railroad might be built to the village. He ardently hoped this would be. He was a born enthusiast. When driving the family's cattle and horses home from pasture, or hogs and other domestic animals from the woods where they fed on acorns and leafy foliage, he would mark out new plants, return to the localities after evening chores to gather them; and far into the night study them with the aid of a manual. Things of both the earth and sky entranced him. He could agree with John Ruskin that there was no need of picture art galleries when gloriously colored ever-changing skies were near. Equally much he could love with his whole heart the sight of a meadow filled with sky-blue gentians where a forest was the background. In autumn he would watch the "gray oaks cast afield their rippling spray on long straight arms to greet the autumn ray And make of light and shade the background meet For picture painted by the moment fleet." To him, as also to Thoreau, "Olympus is but the outside of the earth everywhere," whether one gathered the pale anemone, blue rathe violet, or harbinger-of-spring, climbed adventurously the hilltop hemlocks to the open sky, or fished through the ice of Maple River amid the barren trunks of maple, ash, and elm.

On September 4, 1870, Rancellor King Smith and his family united with the First Congregational Church of Hubbardston. That autumn thoughts of the family turned to Erwin's schooling, a matter which owing to circumstances had been neglected since their departure from Gilberts Mills. As late as December 21, the

youth wrote in his diary: "I am on the Farm, have worked on it all the time since we came here. I am studying Arithmetic, Orthography, Gramar, and Botany. I do not go to School this winter. I have made good progress in Botany this summer and expect to do much more next Summer." He wished that he might own books on materia medica, agricultural chemistry, astronomy, and other subjects. Evidently as yet Charles F. Wheeler had not started him on his study of French. The older of the two young men was about two years ahead of Smith in botanical study; and so he had first to catch up in botany. Nevertheless, Wheeler, also interested in languages, gave him his first lessons in French, an acquirement believed next in importance to German. They read together. Wheeler lent young Smith books; among them, works by Rousseau, his "Confessions" and "Emile," as well as Madame De Stael's "L'Allemagne." These, with Scott's Waverley novels, the writings of Bulwer-Lytton, and other literary masterpieces, placed the Maple River farm youth in advance of the learning of even the adult townspeople of Clinton or Ionia counties.

Erwin completed most of his grade schooling while living on the farm. By long hours of study at home, the youth educated himself. Schooling often had to be secondary to the farm work. He may have attended some of the county schools before entering at the advanced age of eighteen years the schools of Hubbardston.

During the winter months, when the heavy snows and cold lessened his farming duties, R. K. Smith earned part of his family's livelihood by making and selling medicines prepared from mostly herbs and roots of plants. Foremost of the products which he vended was a preparation known as "Burdock's Bitters." Sincere and honest, devoutly religious, but poor, Erwin's father fought valiantly against adversity. But rains ruined their wheat crop during two successive years, and, because of this and other reverses, the farm in Clinton County was lost; and the family was forced to move to one in North Plains Township, one mile north of Hubbardston. To walk the distance to the village was possible. In 1876, when twenty-two years of age, Erwin went to the high school at Ionia, the county seat.¹²

The year 1876, when the Philadelphia Centennial celebrated one hundred years of American independence, was an important

¹² Based on letters and materials collected by Miss Florence Hedges in 1932.

one in the life of young Smith. It took courage for a full bearded youth to enter the Ionia High School, two weeks late, with few, if any, acquaintances, and the difficult prospect of supporting himself during each of the years till graduation.

Exceptional circumstances often give rise to exceptional solutions. A wise principal, Anson P. DeWolf, educated at the University of Michigan, seems to have realized this. Impressed by Smith's "unusual intelligence, courteous bearing and evident acquirements,"¹³ he granted him "full permission to come and go as the spirit moved." The eager young scholar-farmer from Hubbardston "had acquired a fine knowledge of the French language." Indeed, he already "read extensively the French scientific books" which laid part of his "foundation for the scientific work of his later life."

On October 27, 1876, Wheeler, by letter, encouraged him to continue his botanical studies while going to school in Ionia. "We must begin," he wrote, "to think of getting the materials in shape for our Catalogue, *Catalogus Plantarum circa Hubbardstonensem Nascentium*. How will that do for a title page? You are studying Latin I believe. Are there any errors?" By 1878 they had prepared a "List of Cent[ral] Mich[igan] Plants."

Principal DeWolf never complained when his young student dropped everything, filled his collecting can with food, and went into the woods and fields to search the region for uncommon and new species.

Joseph Charles Arthur, student of Charles Edwin Bessey of Iowa Agricultural College at Ames, was among the nationally known botanists with whom they exchanged specimens of plants. As early as 1870 he had sent a list of his duplicates and, both then and in 1876, offered copies of his catalogues of plants from the Iowa flora. On October 16, 1876, from Philadelphia, he wrote of the Centennial where he had an exhibit of Iowa plants for which he received a medal. Thomas Jonathan Burrill, botanist of the Illinois Industrial University, displayed a collection of woods.¹⁴ Other botanists from various quarters of the nation supplied exhibits. Federal Commissioner of Agriculture Frederick Watts had

¹³ L. R. Jones, Erwin Frink Smith 1854-1927, a biographical memoir, Nat'l Acad. of Sci. of the U. S. of Amer., 21: 3, first memoir, 1939.

¹⁴ J. T. Barrett, Thomas Jonathan Burrill, *Phytopathology* 8 (1): 1, Jan. 1918.

arranged that each division of his Department—Entomology, Chemistry, Gardens and grounds, Botany, Microscopy, and Statistics—should contribute displays in acknowledgment of the “universally-accepted” belief that “to agricultural science and industry belong the attribute of characterizing the leading feature of the world’s improvement.”¹⁵

The Microscopical Division supplied a collection of “finely-executed water-color drawings” representing a wide variety of subjects, six hundred specimens in all, among which were shown the more common molds, those destructive to vegetation and those of value in preparing cheese, bread, and jellies. Approximately one hundred varieties of vegetable starches were presented in one series; edible and poisonous mushrooms were differentiated; and one series of drawings illustrated such important diseases of plants as pear-tree blight, the black knot of cherry and plum, and the rot of potato. For the first time a “large collection of well-executed drawings of cryptogamic plants” had been brought together: and the collection, it was believed,¹⁶ would prove “of permanent value to mycological science in America.” In 1873¹⁷ Thomas Taylor, the Department’s microscopist, had reported examinations made of several plant diseases: hawthorn-blight, potato blight and rot, black knot, orange blight, apple-speck or rot, and onion-rust. Pear blight was omitted, perhaps because he believed that its suspected fungous origin had not been proven.

Arthur, in his Philadelphia letter to Smith, mentioned no exhibits except his own. He had spent six months at the Centennial; yet his next letter of January 6, 1877, told nothing of interest on mycology but did compliment the young Michigan student “on the very careful and *scientific* manner in which you have preserved your last season’s spec[imen]s. They are,” he wrote, “a hundred times more valuable than the ones sent me previously.”

Before he had graduated in 1872 from Iowa Agricultural College, a compound microscope, a Tolles instrument, had been used in courses there. In the spring (March-June) of 1871, Bessey had

¹⁵ *Rept't of the Comm'er of Agric. for 1876: 7, 12, Washington, Gov't Print. Off., 1877.*

¹⁶ Thomas Taylor, Microscopic investigations, *Rept't of Comm'er of Agric. for 1876: 74-76.*

¹⁷ *Rept't of the Comm'er of Agric. for 1873: 183-210.*

lectured on the fungi "to what was then the class in advanced botany, composed of members of the Junior Class. It naturally follows," he later wrote,¹⁸ "that in the first course of lectures the subject was not very profoundly treated and yet, since Ames College is an agricultural institution, I found it necessary to deal with the matter somewhat 'economically,' and this made it necessary that I should discuss fungi in such way as to bring out the life history of each plant so discussed. . . . I had lecture demonstrations on various spores and other parts of the more interesting fungi. That first course of lectures was followed the next year by a similar one, considerably improved," and ultimately these lectures became the basis for his "treatment of the fungi" in his important text, *Botany for High Schools and Colleges*, which was published in August of 1880.

In 1877 had been published at Beloit G. D. Swezey's *Catalogue of the Exogenous Endogenous and Acrogenous Plants of Wisconsin*. Smith thought this "very neat"; and he added to his small number of correspondents a very important trio of southern Indiana botanists, publishers of the recently established *Botanical Gazette*. In 1878 Charles Reid Barnes of Madison, Indiana, and Smith began exchanging species of plants and county and state floras. Wheeler and Smith had a printed "List of Phaenogams, Ferns, and Mosses indigenous to, or naturalized in, the Counties of Clinton, Ionia, Gratiot, and Montcalm in Central Michigan." Barnes had prepared a "Catalogue of the Phaenogamous and Vascular Cryptogamous Plants of Jefferson County, Indiana."

In 1878 Professor John Merle Coulter of Hanover College, Indiana, founder and editor of the *Gazette*, also arranged with Smith to exchange "desiderata." Within a year Professor Coulter and his brother, Stanley, were announcing plans for a state-wide Indiana flora; and two years later, these editors of the *Gazette* and Barnes presented their *Catalogue of the Phaenogamous and Vascular Cryptogamous Plants of Indiana*.¹⁹ In 1882 a supplement was added; 1,432 species and 577 genera were described under four principal regional classifications: those plants common to the river valleys, those of the lake borders, those of the prairies, and those of the "barrens," so-called.

¹⁸ Letter, C. E. Bessey to E. F. Smith, December 17, 1901.

¹⁹ See chapter III, "The Indiana Flora," in the author's biography of *John Merle Coulter, Missionary in Science*, 29-37.

Similarly in 1881 appeared Wheeler's and Smith's *Catalogue of the Phaenogamous and Vascular Cryptogamous Plants of Michigan*, indigenous, naturalized, and adventive, in which 1,634 species and 113 families or orders were described.

These state catalogues brought each of their authors into prominence, and every one of them became later leaders in American botany. The work of the Michigan flora provided an introduction for Smith to two of the state's professors of botany, William James Beal at the agricultural college at Lansing and Volney Morgan Spalding at the state university at Ann Arbor. Wheeler and Smith, during fourteen years of exploring and gathering specimens from other herbaria, systematized more than 1,100 species of flowering plants and ferns. Earlier catalogues of Michigan plants, some originating from studies of specimens gathered on the first scientific exploratory expeditions of the Great Lakes and upper Mississippi River regions, had been consulted. Michigan, for many years, had been among the leading states in the study of plants. As early as 1838, when the creation of the state university was still a matter of contemplation, a Gothic building to house natural history and the appointment of Asa Gray as professor of the subject were foremost among the plans. Valuable herbaria, as, for examples, Dr. D. Cooley's at the Agricultural College and Dr. John Wright's and N. H. Winchell's at the state university, together with about a dozen formal plant lists, were available and made use of. Wheeler and Smith paid tribute to the pioneering accomplishments of "the little band of indefatigable naturalists of the past generation. . . . First among them," they said, "stands Dr. Douglass Houghton, while around him, among others, may be grouped the botanists, Dr. Zina Pitcher, Dr. Abram Sager, Dr. Dennis Cooley, and Dr. Daniel Clark, of whom alone Dr. Clark remain[ed], full of years and still active in the pursuit of his favorite science." Indebtedness was acknowledged to George Thurber, a contemporary of Torrey and Gray and still influential as editor of the *American Agriculturist*, to Professor Albert Nelson Prentiss and Professor J. C. Holmes, formerly with Michigan Agricultural College. Once, while on a visit to Hubbardston, Smith became acquainted with Liberty Hyde Bailey, junior, who, while a student at Michigan Agricultural College had met Wheeler and accepted his invitation

to visit him. Bailey was to lead in the movement for a "new horticulture." During these years, he was more interested in botany, though plant life of whatever form fascinated him. He had become interested in the work of the catalogue, furnished lists and specimens of plants growing in the vicinity of Lansing and South Haven, his home. He would praise the catalogue in open meetings of students and faculty at the agricultural college.

Smith had been fortunate in enlisting the interest of a number of botanists over the country. In the east was Mary Treat of Vineland, New Jersey, known for her collections along the Atlantic seaboard as far south as Florida and as an early biographer of Asa Gray; also, S. N. Cowles of Otisco, Onondaga County, New York. Among other collectors within near ranges of latitude were correspondents in Canada and as far east as Sweden. In North Carolina was M. E. Hyams, and at Washington, D. C., Lester F. Ward who sent Smith copies of his *Flora Columbiana*. Others might be mentioned. No one, however, rendered more valuable assistance to Wheeler and Smith than William James Beal.

Beal, born in 1833 at Adrian, had been educated at the University of Michigan where he received his B. A. degree in 1859 and his M. A. degree in 1862. Going to Harvard to study under Gray and obtain in 1865 his degree in science, he had excelled as a student of botany and come under the influence of Louis Agassiz. He left Harvard imbued with Agassiz's educational philosophy; and passionately urged his students to learn every observable fact about nature and natural objects. Book learning was not enough. The book of nature itself must be opened and carefully examined.

Logically enough, he emphasized investigations in physiology and pathology without at any time neglecting the fundamental importance of taxonomy and morphology. Beal was one of America's first great plant histologists; and one of the first teachers, if not the first, to take his students into the garden and farm field to study crop improvement and into the forest and scenes of wild nature to study the natural flora. Crop amelioration by selection and hybridization was studied experimentally under varying conditions of soil and climate. His work in seed testing was valuable. First president of the Society for the Promotion of Agricultural Science, he presided at its organizing meeting held in

Boston on August 24-25, 1880. In June 1879 Dr. E. Lewis Sturtevant, then of South Framingham, Massachusetts, and editor of *Scientific Farmer*, had published such a strong "Plea for Agricultural Science" that Beal had begun corresponding with several scientists with a view to forming a nation-wide society dedicated to stimulating scientific study in agriculture. At the Rochester, New York, meeting in September of that year of the American Pomological Society, a preliminary organization had been effected, and at the Society's first formal meeting Beal outlined a program designating the objects and directions the Society should follow:

- I. To encourage the formation, cooperation and support of agricultural experiment stations.
- II. To try to ascertain what experiments in agriculture are most needed, and indicate the methods of conducting them.
- III. To discover and define the best methods for uniform standards in the analysis of soils, fertilizers and vegetable products.
- IV. To discover and define the best methods of stamping out parasites and contagious diseases of all domestic animals.
- V. To endeavor to find the best combination of foods for growing and fattening animals in the various parts of our country.
- VI. To make discoveries and extend the application of science to dairying.
- VII. To experiment in fish culture.
- VIII. To investigate insects which are injurious or beneficial in agriculture, and discover improved remedies for those which are injurious.
- IX. To learn and point out the best methods for testing each kind of agricultural seed, to ascertain its vitality and purity.
- X. To make investigations in vegetable physiology, especially with reference to learning how to keep plants in healthy and productive conditions. To study fungi which infect cultivated plants and point out remedies.
- XI. To advance the subject of improving crops by the selection, cultivation, crossing and hybridizing plants for seed.
- XII. To encourage agricultural surveys in the states and the nation, and to discover improved modes of conducting them.
- XIII. To encourage agricultural education, to encourage and approve good work done by any one in the United States Department of Agriculture.
- XIV. To encourage collecting and improved methods of arranging and presenting statistics in agriculture.
- XV. Finally in every way to encourage and help each other and others who are not members in original research in all that pertains to science in agriculture.²⁰

²⁰ *Proc. First, Second and Third Meetings of the Society*, ed. by W. J. Beal and L. B. Arnold, 12-13, Syracuse, N. Y., Farmer and Dairyman Print. 1883.

In 1881 Beal's famous address, "The New Botany," was published, and its presentation before a Michigan teachers' convention became an important event historically in the science. The "new botany" signified new methods of study and instruction. A maxim which Dr. Beal taught all of his life was "Details and facts before principles and conclusions." He, before beginning his forty years of teaching at Michigan Agricultural College, had taught for two years and obtained a master's degree in science at the University of Chicago. His doctorate of science was later secured at Michigan Agricultural College. But by 1880 his alma mater, the University of Michigan, had invested him with an honorary degree of doctor of philosophy.

When Wheeler and Smith had their catalogue of Michigan plants ready to be published, they went to Beal who consulted Charles W. Garfield, a graduate and former teacher at the agricultural college, now a farmer, banker, and business man still vitally interested in horticulture, agriculture, and forestry, in fact secretary of the state horticultural society. Beal told him that "two Hubbardston boys had something that was very valuable but they had no money." Garfield saw in their work "just what [he] wanted [for his] next horticultural volume and it was printed in the 1880 report"²¹ of the society. Reprints at a small cost were made and Smith and Garfield became warm friends. So recognized for its merit was the flora that copies containing 105 pages of descriptive material, corrections, and a preface which elaborated "main features" of the vegetation of the upper peninsula, areas around the Great Lakes, in river valleys, upland regions, marshes, pine country, the lower peninsula, and other special localities, were published in bound form under the title, *Flora of Michigan*.

How much of a part Volney Morgan Spalding had in its preparation before the year 1881 cannot be said positively. He had published a "List of native medicinal plants of Michigan," and its use as source material was acknowledged by Smith and Wheeler. Spalding also was an easterner. In 1849 he had been born at East Bloomfield, New York. But his advanced education had been acquired at the University of Michigan where in 1873 he had been graduated with the degree of Bachelor of Science. Soon

²¹ Letter, Garfield to William Alton Taylor, April 22, 1927.

appointed an instructor in zoology and botany, he was by 1879 assistant professor of botany.

During the early part of this year Anson P. DeWolf, principal of the high school at Ionia, was taking advanced study at his alma mater and wrote Smith of his interest in his work, mentioning " 'identification of vertebrates' and Structural and Physiological Botany" as his main courses. He believed that, while Smith was earning his high school education under difficulties that would defeat many a valiant, hardy spirit, his "*actual progress* [was] much greater than that of the average student in the University"; indeed, better "than two thirds of the students in our best colleges." He liked Smith's "cheerful outlook," and, believing his health holding well against severe odds of arduous toil and study, urged:²²

I wish very much that you could have the advantages of the U[niversity] of M[ichigan]. Your last letter contained some things of interest respecting your work with the plants you are receiving and your plans, and it being a very excellent letter I took the liberty to show it to Prof[essor] Spalding. He remembered you well and the letter inspired him with an unusual interest in you and your work. I supplemented the letter with a detailed account of the admirable work you had done and the beautiful spirit you had manifested, and I am sure you have not a warmer friend or one more deeply interested in your success than he. He asked me about you again yesterday and said "he should wish you could be here next year." When you do come you will find in him an excellent instructor and an earnest friend. I think a great deal of him.

Yes indeed you will have a fine herbarium when you come, and I should conjecture that you had one already. I should like to see it, but should know altogether too little to appreciate its worth. I know but little about systematic Botany but mean to know more about it sometime. My work in botany has been structural. . . . I am pleased at the grit you manifest in taking up German by yourself. I hope you may succeed with it as you did with French.

Smith this year evidently was earning his livelihood by teaching in a district country school, Prairie Creek School by name. DeWolf congratulated him on his progress in this, as well as his individual studies. "You certainly have done remarkably well to take care of so large and advanced a school," he wrote. Erwin had planned that during the year 1879 he would complete his high school education, and then go to college. In all candor he had

²² March 30, May 25, 1879.

written to Viola Holmes on March 28, 1878, in answer to her inquiry as to what he proposed to do in life, that the following year he would finish his courses at Ionia High School and by then would have had "four years drill in Latin, four in French, together with the usual amount of Algebra, Geometry, Rhetoric &c," and had made up his mind to take a four years course

in our state University, or, perhaps at Cornell or Cambridge [Harvard University] in order to fit [himself] for teaching Natural History. Perhaps you wonder [he continued] where I get my money and how I can afford to belong to the non-producing classes so many years. Father helps me some and for the rest I help myself. Sometimes I am ragged, and often I am pinched for funds. . . . Besides my usual school work this winter which has been confined, chiefly, to reading Latin and French authors, I have been very much occupied evenings and Saturday with botanical work. Last summer I pressed about 3000 plants and this winter I have been looking them over and putting them into bundles to send to my botanical friends. I keep up an active correspondence with a dozen botanists in as many different states. . . .

During the summers he had worked on the farm at home. In winters, while at school, he had to study so diligently that, struggling against the preciousness of time, he could not be much with friends, could not often write letters to them or his family. Faithful adherence to the tasks before him, earnestness of purpose, and reverent thankfulness for his opportunities, were characteristics of him and parts of his gospel.

Smith's every writing on these and kindred subjects reveals a simple, sincere dedication of himself to God's work. He was no believer in putting "saints into theological strait-jackets." "'The groves were God's first temples,'" he later said,²³ "To me all days are God's days, all men his children, and every devout heart pagan or Christian his shrine. It goes, therefore, without saying that 'materialism' does not seem to me to embody the last word either in natural science or philosophy." He chose to regard his life and his work as efforts expressive of true religion. Should attainments not crown his labors with what the world called success, his faith pointed to a "diviner success in the great *beyond*." Since childhood he had regularly attended singing school, Sabbath school, morning, evening, and prayer services of his church. That gradually "Unitarian proclivities" gained ascendancy during his young

²³ Letter written September 9, 1901.

manhood meant no more, so far as the reality of his religious nature was concerned, than that his high sense of honor and intellectual integrity compelled him to reject all narrow orthodoxy. On August 23, 1877, he wrote Viola Holmes concerning his activities in the church; but he added, "it matters not so much where we work, as how." He read sermons of the great pulpit orators. David Swing especially to him presented "such manly expositions of Christianity, *as it really is*, freed of incrustations."

He was no more the "pale faced little fellow whose delight was to talk with Ida and Viola [Holmes], and to drink entranced at the wells of knowledge." His sister Lillie had grown to be his "friend and companion,—rare times," he wrote, "we have had together reading Shakspeare and the Poets." He was confessedly a hero-worshiper, idolizing persons for "the nobility of their souls" and not "the beauty of their garments." Later he explained, "In my high school days Carlyle had more influence on me than any other prose writer and I am still thankful to the crabbed old Scot for having impressed me so thoroughly with the gospel of work. It has stood me in good stead many a time." The English and American poets were "favorites" and helped to make his high school experience "three delightful years at a time when I should have been through the University." An essay written by him in 1877 points to a considerable influence from the nature symbolism of the poet Coleridge.

While a student at Ionia high school, Smith edited a small journal, *The Castalia*. Miss Josephine Williams, his teacher of literature and English, inspired him with her belief in his abilities and his future. During one or two years he was one of several young persons from the country districts who lived together in a rented home in order to defray the expenses of their education. One especially dear friend was Eva Walker. He was a leading debater in the Philomathean Society of the high school, and enjoyed the acquaintance of many students, young and old, who were interested in cultural subjects.

Science, however, more than literature, was his pole-star in humanity's service. Only a very few of his closest friends and classmates could share his proficiency. The years in Michigan had been busy ones, full "of hope . . . full of good . . . full of toil . . . and more . . . they [had] given [him] a love and a

reverence for Nature, and a knowledge of [his] own innermost self beyond all price."²⁴ His studies of plants were not his only strong scientific interest.

One afternoon Professor DeWolf announced that a course in zoology would be offered in the school. So enthused was Erwin Smith that, collecting his thoughts "upon the real value of this much neglected but interesting and important study," he prepared and read before the students a theme entitled "A Plea for Zoölogy." Outlining a bit of the interdependence and interrelationships of the "animal and vegetable kingdoms," he urged that comparative mechanisms and life histories of natural forms should be studied not only because of the many interesting facts already known and yet to be correlated with future discoveries but also to understand man's own interrelationships "to each other and to the earth, to himself and to the Author of all."

"The older and lower forms of life must be known," he insisted, "before its highest manifestations can be clearly understood." Recent disclosures now rendered it "imperative that we accept Darwin's doctrine of the origin of species, or some similar theory of Descent to account for a multitude of facts brought to light since 1859." Science for Smith purposed through systematic examination of nature's forms and the deducing of natural laws to present an adequate conception of "the sublime, inspiring fact that we and all forms of life, whether spoken into being by a word, or slowly wrought into present forms through secondary causes, are intimately related to one all wise God whose work we are."

He was not a believer in Louis Agassiz's "theory of special creations [of forms] in pairs upon various parts of the earth, a theory," Smith said,²⁵ "no more, if indeed as much, in harmony with the account in Genesis, than is the doctrine of development." He believed that "the great foundation principle of natural and sexual selection [had been] first clearly enunciated by Charles Darwin [and would] remain intact." He hailed "the time when Natural History shall be taught in all schools not cautiously as an enemy of truth, but boldly as a foundation of all morality and religion." A knowledge of "some branch of Natural History,"

²⁴ Letter, Smith to Viola Holmes, August 23, 1877.

²⁵ Undated memorandum, found among Dr. Smith's papers, and from which this and the next quotation are taken.

the twenty-four year old student urged, "is indispensable to a sound education," and the investigation of our fauna through the learning of zoology was as important as the study of "a kindred science" of plants, botany.

Geologists, in accounting for the origin and age of the earth and the formation of its large-scale physiographic features, were still ridding their science of the last remnants of the older dogmas of a cold earth, catastrophic change, and special creation.²⁶ Smith evidently studied Sir Charles Lyell's *Principles of Geology* and found developmental theories of slow, gradual change concerning rock strata and other phenomena written into the English geologist's chapters on geological succession. This appealed to the youth as more proof of the truth of evolution.

Asa Gray's *Darwiniana: Essays and Reviews pertaining to Darwinism* had been published in 1876; and by 1879 the Harvard botanist's "Structural Botany or Organography on the basis of Morphology," a sixth and revised edition of his *Botanical Text-book*, was to appear. Smith knew that since the American botanical authority's *Introduction to Structural and Systematic Botany* (the fifth edition of the *Text-book*) had been published in 1857, he, like Lyell, had become more than ever a believer in the principle of orderly development. During this period, Smith, so far as is known, wrote nothing on the germ-layer hypothesis in embryonic development or on protoplasm and the cell theory, both "cornerstones of scientific biology"²⁷ scarcely less than a half a century old. His preoccupation appears to have been with the third great doctrine of biological science—that of organic evolution; and his few preserved writings on the subject include mention of the evolutionary beliefs of Herbert Spencer, Thomas Huxley, and Jean Baptiste de Lamarck.

Already he had begun some studies in entomology. Students came to the high school from the nearby towns of Saranac, Smyrna, and other localities, as well as Hubbardston and Ionia, the county seat. While exploring this and other regions for scientific materials, he probably became acquainted with interested persons who helped him with his studies. One E. H. Hunt aided him. He in 1875, while at Michigan Agricultural College, took

²⁶ Joseph Mayer, Modern geology, *The seven seals of science*, 276-277 ff., N. Y. and London, Century Co., 1927.

²⁷ *Idem*, 324-325.

Smith's specimens of insects to Professor Cook, and the professor supplied valuable information concerning some of them. Occasional communications passed between Cook and Smith for at least two years.

In 1867 Albert John Cook, professor of zoology and entomology at the college, had begun the first formal course in any American college, devoted exclusively to the study of insect pests and supplemented by collections of specimens.²⁸ Lectures and some previous instruction had been given earlier by C. V. Riley whose nation-wide reputation had been gained while State Entomologist of Missouri by reason of his efforts to stop the spread of the Colorado potato beetle during the 1860's from the Rocky Mountain plateau regions to the central and eastern states. He studied the life histories and habits of insects and the insecticides which might be used to exterminate them. The ravages of the Colorado potato beetle were controlled by a substance known as Paris Green which was dusted on the plants or the pests. Scientific agriculture now included economic entomology in the study of diseases of crops. The value of insecticidal applications was being studied in Europe and a comparatively untried field was being opened to demonstrations here. Riley, Cook, John Henry Comstock of Cornell University, B. H. Mudge and Edward A. Popenoe of Kansas, Burrill of Illinois, and many other able men began to build this useful science into the fabric of scientific American agriculture. In 1876 a United States Entomological Commission was appointed to study Rocky Mountain grasshoppers and, under Riley's leadership, published notable volumes on Rocky Mountain locusts and cotton insects, some incidental papers, and a volume on forest insects by A. S. Packard. During the 1850's Townend Glover had served the United States Commissioner of Patents as the first federal entomologist. In June 1878 Commissioner of Agriculture William G. LeDuc appointed Riley chief of the Division of Entomology of the United States Department of Agriculture, and in his office was employed another scholar who was to become one of America's foremost entomologists, Leland O. Howard. Entomologist Riley reported that year that, since taking office, he had "more particularly" concerned himself with

²⁸ Herbert Osborn, *Fragments of entomological history* including some personal recollections of men and events, published by the author, 95-97, 103, Columbus, Ohio, 1937.

"four classes of insects that seriously affect American productive industries for good or for evil. These [were]: 1st. Insects affecting the cotton plant. 2d. Silkworms. 3d. Insects affecting the orange and which so seriously threaten orange culture in Florida. 4th. Insects affecting the cranberry."²⁹ As yet, federal research had not been coordinated with instruction such as was being developed in colleges of agriculture over the nation in the 1870's, notably by Burrill in Illinois, Charles H. Fernald in Maine, Comstock in New York, and Popenoe in Kansas.³⁰

Cook, like Beal, had been influenced by study under Agassiz. He generously advised youthful students of natural history, especially those interested in entomology. On June 13, 1877, he wrote Erwin Smith concerning a larval specimen of what he thought might be Apple-tree Canker-Worm. He suggested the desirability of securing the mature insect since its origin was in doubt and a matter of importance.

Plants, nevertheless, remained his favorite study. In 1879 E. H. Hunt of Saranac told him that in the library of a nearby grange could be found "a full set of the [Michigan] State Pomological and Board of Health reports," together with four volumes of reports of the United States Department of Agriculture. Smith began to make use of these. An interest in forestry, encouraged by Beal probably, was aroused when the early reports on this subject became available. During his senior year in high school, however, much of his time for study and reading was taken by necessary employments. In 1880 to provide finances to complete his schooling and prepare for college that year, he worked as a prison guard at the Ionia State Reformatory.

On June 25, 1880, he was graduated from the Ionia high school, and for the graduation exercises he composed the school's "Alumni Song," which expressed characteristically his love of, and search for, truth. Fourteen students made up the graduating class.

That summer he attended the summer school of Michigan Agricultural College at Lansing. He roomed with G. H. Failyer, later professor of chemistry at Kansas State Agricultural College.

²⁹ Report of the Entomologist, *Ann. Rpt. of the Comm'er of Agric. for 1878*: 209, Washington, Gov't Print. Off., 1879.

³⁰ *Fragments of entomological history, op. cit.*, 95. Concerning the Entomological Commission, *idem*, 35-36.

His two or three main courses were plant histology under Beal and zoology and entomology under Cook.

Since his and Wheeler's *Catalogue of the Phaenogamous and Vascular Cryptogamous Plants of Michigan* was soon to be published, some of his time had to be given to proof-and-copy-reading. Some of his time, furthermore, was occupied by a part-time employment with the United States Weather Bureau. Smith became acquainted with William Arnon Henry, a recent graduate in Agricultural science from Cornell University, who the next year was to become professor of botany and agriculture at the University of Wisconsin and in 1887 director of the state agricultural experiment station. Each was interested in teaching science and literature, and had taught in secondary schools. Henry pronounced Smith "a pleasant gentlemanly person in conversation"; and as the next half dozen years went by and he read Smith's scientific papers and saw notices of his communications, his first opinion that he was "a good botanist and a clear careful writer on botanical subjects" was supplemented by a belief that he had "done good work in studying the lower forms of plant life."

Beal regarded Smith as "a very clear and very capable student," who had "a great liking for botany [and] a practical turn which would be of great value to him." Cook spoke for his accomplishments in zoology and entomology. He estimated Smith "as a very close enthusiastic student, a man of rare attainments, natural and acquired," and, as late as 1886, believed he knew of no young man whom he could "more highly, and gladly recommend" for a position as professor of natural history. Failyer was sure Smith was both diligent and efficient, and of exceptional ability and worth.³¹

When Wheeler's and Smith's *Catalogue of Michigan Plants* was published, Henry immediately offered congratulations, saying, "It is a credit to your state as well as yourself that such a list is published." Charles Sprague Sargent, Director of the comparatively new Arnold Arboretum of Harvard University, characterized the catalogue as both "interesting and most instructive. . . . Such publications," wrote Sargent, "are most important, and should be prepared for every State." Professor N. H. Winchell of the University of Minnesota, who was in charge of a botanical survey of that state and had been a resident of Michigan, compli-

³¹ Quotations taken from letters written in May 1886.

mented the authors on the thoroughness of their work. Another former resident of Michigan, Albert Nelson Prentiss, Professor of Botany and in charge of the botanical laboratory of Cornell University, wrote Smith:

Please accept my thanks for the "Catalogue of Michigan Plants." It bears evidence of painstaking care, and is altogether a commendable work. There is no flora in which I feel quite so great an interest as in that of Michigan, as it was among its plants that I made my first ventures in botanical studies.

Many congratulations were received. Byron David Halsted, now associated with George Thurber and the *American Agriculturist* but in 1885 to become professor of botany at Iowa Agricultural College at Ames, tendered his "most sincere thanks." He was a holder of degrees of bachelor and master of science from Michigan Agricultural College and had been Farlow's first graduate student of plant diseases at Harvard University. Lester F. Ward called the catalogue "a model of neatness and convenience and well gotten up." Norton S. Townshend of Ohio State University, answering a request for Ohio geological survey reports, thanked Smith especially for a copy of the flora. Other scientists acknowledged receiving copies. In two leading centers of plant research, Washington, D. C., and the environs of Boston, the work was recognized. Farlow and Sereno Watson of Harvard and Commissioner Loring of the Department of Agriculture sent communications. Beal, knowing that the list was verified from a collection of 1,700 plants, sold or distributed at Lansing twenty-five copies among faculty and students of the agricultural college.

More than ever Erwin Smith was being confronted with his unrelenting problem of poverty. After his summer spent at Michigan Agricultural College, he found that he could not enter, as planned, the University of Michigan that autumn. He returned to Ionia and again accepted employment at the Ionia State Reformatory—this time as a keeper of the prison at a salary increased to fifty dollars a month. Not much more is known of his life during the year 1881 than that his and Wheeler's catalogue was published and that he lived most of the time at Ionia. Student he remained, however, and, while not neglecting his study of plants, he read more and more on public health, sanitation, and human diseases.

CHAPTER II

BACKGROUND OF WORK AND STUDY IN PUBLIC HEALTH, HYGIENE,
AND SANITATION, PATHOLOGY AND BACTERIOLOGY, PREPARATORY
FOR RESEARCH CAREER IN THE PLANT SCIENCES.

MICHIGAN was among the first states to create a Board of Health. Its Board was established in the early 1870's, and its Secretary, Dr. Henry B. Baker, was for many years a leader of the movement for public hygiene and sanitation within the state and nation. For a decade a national movement to improve public health had been gaining strength. Since the years 1857-1860, national sanitary conventions had been assembling in Philadelphia, Baltimore, New York, and Boston. During the 1860's the Civil War had temporarily interrupted progress. But once the war was over, the campaign to combat the ravages of dangerous communicable diseases, especially those which had been reaching epidemic proportions, began again to gather momentum. In 1865 Dr. Stephen Smith and others had conducted a sanitary survey of New York City and exposed the wide-spread prevalence of diarrheal disease, typhus fever, smallpox, etc., among its crowded tenement sections and elsewhere. By 1872 the influence of his work had gathered a sufficient following to bring about the appointment of a preliminary committee to form the American Public Health Association; and with its first meeting modern public health practice was given an impetus. The first volumes of its *Proceedings* are said¹ to contain

a mine of information, for they indicate the vital matters that concerned the leaders of that day. Many of the principles that were presented in the very first report have become the foundation stones on which public health in America has been built. The broad viewpoint and scope of interest of these pioneers may be indicated by the subjects that were discussed at the first meeting. Among others one notes: 1. Public health education, which was considered even at that time to be of primary importance in public health matters. 2. Vital statistics—race and nationality, rural and urban factors in relation to mortality. 3. The germ theory of disease. 4. The epidemiology of typhoid fever and particularly the relationship of water

¹ Wilson George Smillie, *Public health administration in the United States*, 17, N. Y., Macmillan, 1940.

supplies to this disease. 5. Control of specific diseases, particularly small-pox, yellow fever, and cholera. 6. Quarantine and disinfection. 7. Water purification, street cleaning and scavenging.

The first state to create a health board had been Massachusetts, and its action appears to have been taken largely as a result of a report of a sanitary survey made by Lemuel Shattuck and his associates. The exemplary work of a short-lived health board under Edwin Chadwick was partly responsible for the survey; and the English influence was again revealed in 1876 when, within seven years after its creation, the Massachusetts board placed before its constituency a reprinting of a booklet by John Simon on *Filth Diseases and Their Prevention*.² Simon had been one of the first municipal health officers of England. A medical officer of London and the British government, he in 1878 had been honored with the presidency of the Royal College of Surgeons, and for his accomplishments and other important writings on public health and sanitation, he was knighted in 1887.³

Other states followed the example of Massachusetts. As early as 1870, a state health board had been organized on the Pacific coast in California. In 1872, among the southern states, Virginia established a health board. Either just before or during 1872 or 1873 when Michigan did, Minnesota created a state health board. The District of Columbia in 1871 had established such an official agency, and in 1874 Maryland, and in 1875 Alabama, were added to the list.⁴

Many years before, large municipalities had begun to organize health departments. Near the turn of the century Baltimore officials had been convinced of what might be accomplished by a municipal health service and organized the nation's first city health department. Others began to do likewise about two decades later: Philadelphia (1818), Providence (1832), Cambridge, Massachusetts (1846), New York (1866), Chicago (1867), Louisville (1870), Indianapolis (1872) and Boston (1873). Not until 1888 was a first diagnostic laboratory for the control of communicable diseases established. This was located in Providence, Rhode Island, under the pioneering leadership of Dr. Charles

² E. O. Jordan, G. C. Whipple, C.-E. A. Winslow, *A pioneer of public health William Thompson Sedgwick*, 5-6 f., New Haven, Yale Univ. Press, 1924.

³ *New International Encyclopedia* 21: 121.

⁴ W. G. Smillie, *op. cit.*, 15 f.

Value Chapin. He, after serving in 1879-1880 as house physician of Bellevue Hospital in New York City, had become in 1884 superintendent of health at Providence and in 1886 professor of physiology at Brown University. New York also a few years later established such a laboratory under Dr. Hermann Biggs, a "statesman of public health"⁶ as well as physician and an outstanding leader in the American movement.

Sometime around the year 1870 health boards had begun to require the reporting of the more important contagious diseases. At first, primarily administrative agencies, these boards had to spend much time educating a disbelieving public in the practical values of sanitary science; and for the purpose often authentic statistics concerning diseases and epidemics had to be acquired. Often their rules and regulations required the passing of laws to secure their enforcement, and frequently hostility to the measures was encountered. With the advancement of modern preventive medicine and the practical introduction of vaccines and antitoxins against human and animal diseases, their task in public education became less difficult. But their official duties were greatly expanded. "Environmental sanitation," W. G. Smillie says,⁶ "became a secondary activity of the health department, and control of communicable disease a major function." Public confidence in the work of the health boards increased as their successful conquests of disease began to outnumber their failures.

The forces of public hygiene and preventive medicine united efforts, and as early as 1879 a National Board of Health was organized. This lasted but a few years. Yet its discontinuance did not undermine the American Public Health Association. It became the leading national organization in public health matters. Dr. Smillie suggests⁷ two reasons why the health board failed: its inherent weakness, but mainly that "the country as a whole was not yet ready for a nation-wide health promotion service."

The humanitarian phase of the movement in which Erwin Smith first became interested was that directed toward improving hygienic conditions within prisons. For more than a century in several European countries, notably England, reforms had been

⁶ Charles Edward Amory Winslow, *The life of Hermann M. Biggs M. D., D. Sc., LL. D.*, with bibliography, Phila., Lea and Febiger, 1929.

⁶ *Op. cit.*, 18-19.

⁷ *Op. cit.*, 19.

correcting evils in prisons and almshouses. In English prisons, infirmaries and medical care had been provided for the sick. Rooms were required to be regularly cleaned and ventilated, walls and ceilings scraped and whitewashed, and generally such measures had to be taken "as would tend to restore and preserve the health of the prisoners."⁸ It was part of an era of providing better treatment and care for the indigent and helpless ill; and toward this end more public hospitals were built. Similar reforms were needed in America, but not to the same extent as in the older countries. For example, such a requirement as to eliminate underground dungeons was rarely applicable in prison reforms here. There were, however, some conditions in need of amelioration; and on one of these—proper ventilation—Erwin Smith spoke at a sanitary convention held in December 1883 at Ionia.⁹ He said:

Those who have carefully examined our poorly ventilated State prisons have repeatedly had demonstrated to them by the sense of smell, if not otherwise, that the upper layer of air in a room occupied by large bodies of men become soonest charged with foul emanations. In sleeping wards where cells rise above each other in from three to five tiers, the impurity of the air, as shown by the sense of smell and general feeling of oppressiveness, increases as one ascends from one gallery to another, until, towards morning, on the uppermost gallery near the ceiling, the stench and oppressiveness of the air becomes almost intolerable; and this, too, in spite of the fact that in our State House of Correction, at Ionia, numerous openings have been provided in the outer walls near the floor for the express purpose of ventilation. It is also true, as I know by observation, that prisoners who sleep in ranges of cells near the ceiling complain more of headache, have less appetite, and eat considerably less food than those sleeping in cells near the floor. From these considerations I think it is apparent that a room cannot be properly ventilated by simply making an out-door opening at or near the floor.

This was not said as part of a formal address, but in discussion which followed an address on "Ventilation."

Smith's hours on duty at the reformatory while a guard were from one o'clock in the morning until one o'clock in the afternoon. At night he was on duty along the cell block and in the mornings when the prisoners went to breakfast he went on the

⁸ *A Pioneer of Public Health, op. cit.*, 3-5 (quoted address by Dr. Sedgwick on "The rise and significance of the modern movement for public health").

⁹ *Supplement to the 12th Annual Report of the Sec'y of the Mich. State Board of Health* for year ending Sept. 30, 1884, report of convention in part prepared by assistant secretary Erwin F. Smith.

wall with a shotgun. The suffering and confinement of prisoners was a melancholy sight to him. Often while on duty he would repeat to himself an old couplet: "Be the day weary, be the day long, It ringeth at last to evensong."

He took this position because, since he had to work to go to school, there was an opportunity to study between rounds. He was working then on the catalogue of Michigan plants. He was probably reading also in sanitary science.

Smith began on October 30, 1878, what he called his "Index Rerum." His introduction read: "In every book I read, I find some valuable things along with a great deal of to me useless matter. By keeping an Index book the wheat can be separated from the chaff and made available for future use"

Into this was written no abstract or material from state health board reports. In 1881 when a school journal, the *Michigan School Moderator*,¹⁰ published his review of the Michigan board's report for 1880, he suggested that these reports were "valuable for study and reference and ought to be in every teacher's library. The school room," he said, "is the proper place for instruction in Hygiene." Nor was anything of public school interest included in his "Index Rerum." From Ionia, on November 17, 1881, this young scholar, himself once a school teacher, supplied to the *School Moderator*¹¹ a short, informative argument on behalf of the teaching of "Physiology in Common Schools." "Now is the time," he urged, "for the district teacher to agitate forming a class in physiology [to] understand the laws of health." Physiology without hygiene was like Hamlet without the Prince. He recommended several texts on human physiology—Gray's Anatomy; Flint's, Dalton's, or Carpenter's; and said, for "various reasons" he preferred Dalton's.

Erwin believed,

It is not so much the possession of great libraries as it is the faithful use of small ones, that makes men learned. I cannot think the Bible, Shakespeare, and the dictionary sufficient for a man's education, as some have declared; but I have learned that a few books carefully read are better than hundreds of volumes carelessly thumbed. Someone says of Carlyle's library that the books were few but "worn and battered as if thrown at the ages."

¹⁰ Founded at Grand Rapids, Michigan, October 27, 1881, as *The School Moderator*. Smith's review, "Health Report for 1880," was published in vol. 1, no. 11, p. 803.

¹¹ 2 (12): 820, December 1, 1881.

Early in 1882 the editor and publisher of the *School Moderator* arranged with Smith to have charge of a "Scientific and Sanitary" department of the publication. In June of that year he was employed as a correspondence clerk by the Michigan State Board of Health, and moved from Ionia to Lansing. He was probably recommended for this position by Dr. Robert C. Kedzie who, although professor of chemistry at Michigan Agricultural College, was also a doctor of medicine and an influential figure in the state medical society and the state health board; a past president of both organizations.

In 1876 Dr. Kedzie had been chairman of the section of public hygiene and state medicine of the American Medical Association. He was a graduate of the first class of the medical department of the University of Michigan, had distinguished himself as a student then, practiced his profession for a time, but, because of his predilection for chemistry, had accepted a teaching position in that subject at the agricultural college, and become one of the founders in America of the science of agricultural chemistry. Smith valued his acquaintance with him and secretary Henry B. Baker of the Michigan health board, both men doctors of medicine and workers for the cause of public hygiene and health. In 1881 Dr. Baker had a column in the *Michigan School Moderator*. This was entitled, "Health in Michigan," and was usually based on health bulletins recently published by the board.

Smith's "scientific and sanitary" department of the *Michigan School Moderator* covered many subjects. Two of his early contributions were on "Cheap aquariums" and "Meteoric fossils." But soon many notes on botany, astronomy, physics, chemistry, geology, and zoology began to appear. Those on botany ranged from comments on Asa Gray's *Synoptical Flora of North America* to Edward Tuckerman's study of lichens; from cross-fertilization of plants by insects to insectivorous or "carnivorous" plants; from the Michigan flora including its ferns to garden plants and seed catalogues. Paragraphs or brief articles dealt with such subjects as the solvent power of water, silk production in Italy, stellar immensities of space, comets and superstitions, light velocity, the earth's motion, the electric light, minuteness of particles of matter, fire prevention in buildings, and much else.³⁷ Smith's interest in

³⁷ "Scientific and Sanitary" department notes, data, and articles began in January,

facts about human diseases and their etiology was evident throughout the two volumes when the department appeared. He included references to documents on diphtheria and scarlet fever, to prevention and treatment of other diseases; and, once warning against a threatened epidemic of smallpox, he urged that the current prejudice against "vaccine points" was unfounded. Later, during the summer of 1885, when a serious outbreak of typhoid fever occurred in Ionia, Smith addressed a letter to his former fellow-townsmen in which he advanced impure water supplies and lack of proper sewage disposal as probable causes.¹³ It is not known definitely when his studious interest in malaria was first aroused. But it was soon after the year 1881 that he began to read on the subject and sometime during or before 1883 when he learned that Edwin Klebs, noted German physician and pathologist and one-time assistant of the great Rudolph Virchow, was believed to have discovered in 1879 "the cause of malarial fever." Smith described in his "Index Rerum" the manner in which the "*Bacillus malariae*" was cultivated.

In 1873 Klebs, in an important treatise, "Beitrag zur Kenntniss der Micrococcen,"¹⁴ had outlined his fractional method of culture study. No reference to this was made by Smith in his valuable index entries, nor to any other of Klebs's discoveries. Émile Duclaux, in his book *Pasteur: The History of a Mind*,¹⁵ accredits Klebs with having found bacterial organisms in purulent nephritis (1865) and with having in 1872 "shown how, starting from a wound, bacteria could penetrate the lymphatics or the veins by means of the interstices of the connective tissue, and from there infect the thrombi of the blood vessels or produce abscesses. Then," Duclaux concludes, "came the discovery of bacteria in erysipelas, hospital gangrene, puerperal fever, diphtheria and other diseases."

Erwin F. Smith's interest in the bacteria may have been started by his reading of Dr. Sternberg's translation of Antoine Magnin's

1882, and were concluded May 31, 1883. Citations are dispensed with because of their abundance. See volumes 2 and 3 of the publication.

¹³ *Report of the Sec'y of the Mich. State Board of Health* for the year 1885; 278.

¹⁴ *Arch. f. exp. Path. u. Pharmacol.* 1: 31-64, with four plates, 1873.

¹⁵ Translated by Erwin F. Smith and Florence Hedges, 248-249, Philadelphia and London, W. B. Saunders Co., 1920.

treatise on *The Bacteria*. The first edition of this work was published in French in 1880; and by November 30, 1882, he had prepared for the *Michigan School Moderator*¹⁶ "a short account of the bacteria." He recommended the translation as the "best book on this subject for English readers. . . . This, however," he cautioned, "was published two years ago, since which time there have been important discoveries and many changes of opinion."

By this time the alert, young student was much interested in physiology and was keeping abreast of new discoveries in pathology. His review of a book on the physiology of digestion took into account the value of permanently recorded experimental data for the building of scientific truths. He believed that the science of human physiology had made "wonderful strides" since the year 1833.¹⁷ To him, moreover, the learning revealed by the microscope was like exploring "a strange, diminutive wonderland, more startling and vastly more real" than any stories of Aladdin or Grimm or Gulliver or Munchausen. To him the microscope provided "a genuine *terra incognita* of wonders."¹⁸

Dr. George M. Sternberg had been graduated in 1860 from the College of Physicians and Surgeons of Columbia University in New York City, and the following year been appointed Assistant Surgeon of the United States Army. In 1892 he was to present his classic work, *A Manual of Bacteriology*,¹⁹ which, prepared by "one of the pioneers" in the science in this country, was to be regarded as the English-reading public's "first adequate survey of the bacteriological field." The work was divided into four main parts: classification, morphology, and bacteriological technology; its general biological character; pathogenic bacteria; and saprophytes. In a chapter on susceptibility and immunity, Dr. Sternberg said: "The experimental evidence detailed gives strong support to the view that acquired immunity depends upon the formation of antitoxines in the bodies of immune animals." A reviewer in *Science*²⁰ found that he, "while disposed to accord to phagocytosis an important rôle in some diseases, [was] profoundly impressed—as [were] most bacteriologist—by the remarkable evi-

¹⁶ 3 (13): 199.

¹⁷ *Mich. Sch. Mod.* 2 (19): 938, Jan. 26, 1882.

¹⁸ 2(31): 162, Apr. 20, 1882.

¹⁹ New York, William Wood & Co., 886 pages.

²⁰ 21: 107-108, Jan. 24, 1893.

dence adduced during the last few years in support of the 'antitoxine' theory." By 1892 Élie Metchnikoff's "brilliant phagocyte theory" was believed to embody "at most only a partial explanation of the facts of immunity."

Some idea of the thrilling quarter-century in which Erwin Smith was to be educated and acquire his early learning in pathology and bacteriology may be deduced from another quotation from this review. The reviewer, seeing in the growing science of bacteriology a partial, if not complete, vindication of the practical worth of "pure science," said of this period: "To have given to the world for the first time a rational theory of infectious disease, and to have indicated the therapeutic possibilities of the future are achievements that may well make the last quarter of the nineteenth century memorable in his history of human progress."

Before Dr. Sternberg published his manual which became a standard one for the new science, he enlarged and revised to the year 1884 his translation of Magnin's treatise on bacteria. His career as an army surgeon had provided him with exceptional opportunities for studying epidemic diseases.²¹ In 1867, while stationed at Fort Harker, Kansas, he had been on the scene of a serious cholera epidemic; in fact, lost his wife from the disease. In 1871 at Fort Columbus, New York, and a few years later at Barrancas, Florida, he had observed the ravages of yellow fever; and in 1879 served as a member of the Havana yellow fever commission. For a decade thereafter he worked on the etiology of this disease, and years later when an army board was sent to Cuba to study the contagion and its transmission he, as Surgeon General, was primarily responsible for the government's sponsoring the commission.²²

The study of scientific disinfection is believed²³ to have been started by Dr. Robert Koch in Germany and Dr. Sternberg in America. In 1878, while stationed at Walla Walla, Washington, the young American army surgeon had begun experiments to test

²¹ Drs. Howard A. Kelly and Walter L. Burrage, George Miller Sternberg (1838-1915), *Dictionary of American Medical Biography*, 1158-1160, N. Y. and London, D. Appleton & Co., 1928.

²² E. F. Smith wrote of this: "Sternberg, the bacteriologist, was Surgeon General at the time and without his warm support nothing would or could have been done." See, Dr. Smith's address, Fifty years of pathology, given in 1926, *Proc. Internat. Congr. of Plant Sciences* 1: 31, 1929.

²³ Drs. Kelly and Burrage, *op. cit.*

the practical value of disinfectants, and in these used putrefactive bacteria to determine germicidal activity. His experiments were continued, under the auspices of the American Public Health Association, at the army medical laboratories in Washington and still later at the laboratory of pathology at Johns Hopkins Hospital.

He was also a skillful photomicrographer. In 1881 he exhibited to the American medical profession one of the first photographs of the tubercle bacillus, cause of tuberculosis. By 1886 he possessed a photomicrograph of the causal bacillus of typhoid fever. In 1884 he made available in America a photomicrograph of Alphonse Laveran's plasmodium, believed to be a parasitic protozoan of the red blood corpuscles and the cause of malaria fever.²⁴

Erwin Smith, while reading about the bacteria and public hygiene, became acquainted with the work of Dr. Sternberg. Entries in his "Index Rerum" as of 1882-1883 show a definite interest in the etiology of yellow fever²⁵ and considerable knowledge, as of 1881, concerning the proof of a "schizomycete" or "bacillus" of malaria. In 1880 Laveran, a French army surgeon stationed in Algiers, had found in the blood cells of patients suffering from malaria certain pigmented bodies, and reported his discovery to the Academy of Sciences in Paris.²⁶ American medical scientists, however, had not fully begun their studies of elements in the blood of malaria patients; and a belief in bacilli, as possible causes of both yellow fever and malaria, was fairly prevalent.

Young Smith's interest in the "Bacillus malariae" seems to indicate more an experimental research scientist in-the-making than a medical doctor. He appears to have read of bacteria at this time equally as much as a student of plants as a student of diseases afflicting mankind. The "bacillus malariae" was referred to as a "plant," but, what is more important, was his interest in the environmental conditions (temperature, humidity, oxygen requirements, etc.) necessary to the organism's "development and

²⁴ *Idem.*

²⁵ Smith's entries cited: Monsieur Victor Babes's paper, descriptions, and figures in *Comptes Rendus*, 97: 682-685, Sept. 17, 1883; and Dr. Joseph Jones's report (New Orleans) to the Board of Health, reprinted in the *London Practitioner*, July and Aug., 1882.

²⁶ See, Dr. Harvey Cushing, *The life of Sir William Osler* 1: 270 and n. 1, Oxford at the Clarendon Press, 1925.

multiplication." He had begun some reading in plant physiology. A lecture, published in *Nature*,²⁷ on the "Excitability of Plants" fascinated him. In his "Index Rerum" he outlined the results of irritability experiments on *Mimosa* and some experiments, made with the use of an electrometer, on *Dionaea*, an insectivorous plant. The similarities in movement phenomena and other changed physical conditions observed in these two genera of plants were compared; and comment extended to resemblances and differences which take place in animal tissues, for example, when muscles contract.

In 1882, when he prepared his "short account of the bacteria" for the *Michigan School Moderator*, he knew something of the bacilli which were supposed to explain putrefaction and fermentation. He knew something of the bacilli which were believed to cause anthrax (or splenic fever) in animals, consumption in man and animals, and diphtheria in man. The July 1882 number of the *London Practitioner* was important because it contained a paper by a Swedish doctor on the etiology of scarlet fever, and because of another which told of the presence of micrococci in mumps. Earlier that year, in his "scientific and sanitary" column for the *Moderator*,²⁸ he had written on angleworm infestation as possibly involved in the etiology of typhoid fever. Soil, water, and air parasites always interested him. In his "Index Rerum," his references to plants contained some mention of works on fungi, but none specifically concerning diseases of plants. The North American fossil flora, including such current interest topics as the genesis and migration of plants, interested him. But equally strong were his interests in matters dangerous to human welfare and health: unsanitary house drainage, sewer gas, street filth, and the like.

Before Smith moved from Ionia to Lansing to accept a position as correspondence clerk of the Michigan State Board of Health, he had learned of Louis Pasteur's discovery in France of a vaccine remedy against anthrax in animals. Immediately following an editorial on the death of Charles Darwin, who he believed had since 1859 "completely revolutionized scientific thought," he pub-

²⁷ August 10, September 14, 1882. Lecture by J. S. Burdon Sanderson.

²⁸ 2 (22): 18, Feb. 16, 1882.

lished in the *Michigan School Moderator*²⁹ another editorial on Pasteur's vaccine from an attenuated culture.

Great is M. Pasteur, the French savant. Nothing since [Edward] Jenner's [discovery of smallpox vaccination³⁰] is more remarkable than his recent discovery of the cause of splenic fever, and of simple means for its prevention. He has been able to check the ravages of this fatal disease by means of a peculiar vaccination. Germs of this disease are grown in the laboratory and under certain conditions, the key to which M. Pasteur has discovered, a mild form of virus is developed. Sheep vaccinated with this virus suffer a slight sickness and thereafter enjoy complete immunity from the dreadful scourge. As an example of what he could do, M. Pasteur selected 50 sheep, 25 of which number he vaccinated with the modified virus. These were but slightly indisposed, and soon recovered. After fourteen days he inoculated each of the 50 sheep with splenic fever virus. The 25 vaccinated animals resisted the infection; the unvaccinated ones all died of splenic fever within 48 hours. The practical value of this discovery will be realized when it is known that the loss to the flocks of France from splenic fever has annually exceeded \$4,000,000. Vaccinations have become general in the departments around Paris.

M. Pasteur has opened a great field for study. He is practical and his researches are wonderfully suggestive. What if we shall be able to discover and stamp out the germs of yellow, typhoid and miasmatic fevers, by and by, and to reduce the dreadful mortality of diphtheria and scarletina. It now seems probable! M. Pasteur is evidently on the right track, and a multitude of trained observers both in America and Europe will follow his leadership.

The Michigan Board of Health, interested in preventing the outbreak, or controlling the spread, of contagious diseases, continued to publish each year compilations of the geographical distribution of diseases throughout the state. Very likely Erwin Smith helped to prepare the Board's weekly reports and summaries which analyzed the diseases of increasing or lessening prevalence, their supposed or known causes, and any valid remedies or cures therefor. In the secretary's report for the year 1882³¹ was contained a reference to Robert Koch's experimental studies of "consumption," and it was indicated that tuberculosis, whether in man or animals, is a communicable disease caused by a "microscopic bacillus." In November Smith, in his "short account of the bacteria," called this the "*Bacillus tuberculosis*."

²⁹ 2 (35): 235, May 18, 1882.

³⁰ See, Dr. Warren T. Vaughan, *Strange malady, The story of allegory*, 25-27, Doubleday, Doran and Co., 1941.

³¹ P. 518.

From February 28 until March 1, 1882, a Sanitary Convention had been held at Ann Arbor. This was presided over by the Michigan jurist, Thomas M. Cooley; and the secretary of the convention was Dr. Victor C. Vaughan, a graduate of the University of Michigan's medical department and later to be its dean. Not yet had Dr. Vaughan been made a full professor of hygiene and physiological chemistry at the University; and not yet had he gone aboard to study, and come under the influence of, among others, Max von Pettenkofer. "No king ever did for Munich and Bavaria what this model scientist did," Vaughan wrote³² of von Pettenkofer.

About the middle of the nineteenth century Munich was a hotbed of typhoid fever. From 1857 to 1867 the annual death rate from this disease in that city averaged two hundred and three per one hundred thousand. The city was honeycombed with privy vaults and shallow wells. The contents of the former leaked into the latter, from which the people drank. About the later date there came to this city a young, intelligent epidemiologist, one of the first of his kind, by the name of Pettenkoffer. He induced the people to abandon their privy vaults and cesspools, to build a system of sewers and to bring a pure water supply from a mountain lake. By these means the prevalence of typhoid fever was within a few years reduced to almost zero.

Erwin Smith has described³³ the North American and European situation in epidemic diseases prior to 1876 thus:

Typhus fever, typhoid fever, diphtheria, tuberculosis, syphilis, cholera, yellow fever, and the plague destroyed hundreds of thousands of persons every year, and the causes were not known and there were no remedies. There was a shot-gun quarantine and disinfection of clothing (now known to be useless) for yellow fever, and with stench in the houses there were prayers in the churches against yellow fever, cholera, and other epidemics. Malarial fever destroyed or invalidated multitudes of persons every year and rendered a large part of the tropics uninhabitable to the white man, and nothing was known as to its cause or as to how it was contracted. Many supposed it was due to breathing night air. Following Pettenkofer, the cause of typhoid fever was supposed to be a miasm in some way related to the depth of the ground water. No one suspected that deadly diseases could be transmitted by flies, mosquitoes, ticks, bedbugs, fleas, lice, cats, rats, rabbits, goats, antelopes, and other animals, or that an apparently healthy human being (one recovered from a disease) could remain the carrier of its germs deadly to another. Such ideas were all

³² *A doctor's memories*, 152, Indianapolis, Bobbs-Merrill Co., 1926.

³³ *Fifty years of pathology*, *op. cit.*, 15-16 (address given in 1926).

on the knees of the gods. The world was not ready for them. It was too much absorbed in money-making, pleasure, politics, war, and other-world religion to pay much attention to causes of disease, nor was the scientific man properly equipped for the study of minute parasites. Apochromatic glass and cultural equipment were things of the future . . . [T]here was no science of bacteriology, no immunology, cytology, no genetics, no plant or animal pathology worthy of the name, no public health service founded on exact knowledge, and in nearly every case no well-ascertained remedies. In fact, there was no medicine such as we now have based on the circle of the sciences

In America, before 1870, educational entrance requirements were not in force in even the best medical schools. Laboratory work and practice were almost unknown.³⁴ "The teaching of medicine as a science, as something of larger scope than its practice," Garrison has said,³⁵

began with the foundation of laboratories and with the gradual assemblage of specialties as units in university instruction It was only toward the end of the 19th century, under the direction of Eliot at Harvard, Billings, Welch, and Osler at the Johns Hopkins, and Pepper in Philadelphia, that medical teaching began to be true university teaching, in the sense of training a student to make use of his own mind as a substitute for blind acceptance of dogma.

Until that time, medical teaching had been mostly by lectures and demonstrations, supplemented by text-book reading.

Erwin Smith participated in discussions on hygiene at the Ann Arbor Sanitary Convention.³⁶ During the following April,³⁷ another similar convention took place at Greenville, and at this Dr. John Harvey Kellogg spoke. He was a graduate of Michigan State Normal School and the Bellevue Hospital Medical College of New York City, and had not only founded but was also superintendent and surgeon of the Battle Creek Sanitarium. Smith may have heard his address on "Disposal of decomposing organic matter," since, when in 1881 he reviewed³⁸ the eighth annual (1880) report of the Michigan State Board of Health he chose as

³⁴ W. T. Sedgwick, *A pioneer of public health, op. cit.*, 13-14.

³⁵ Fielding H. Garrison, *An introduction to the history of medicine*, 4th ed., 755-756, Phila. and London, W. B. Saunders Co., 1929.

³⁶ *10th Ann. Rep't Sec'y St. Bd. of Health of Mich.* for 1882; 202.

³⁷ *Idem*, acc't of meetings at Greenville, April 11-12, 1882. See also, *Mich. Sch. Mod.* 2 (37): 262; 2 (38): 278.

³⁸ *The school moderator (Michigan)*, 2 (11): 803, Nov. 24, 1881.

a paper "of interest" another by Dr. Kellogg on "Decaying wood a cause of disease." At that time he found of especial interest also Dr. Vaughan's "brief but valuable paper" on "Contamination of drinking-water by filtration of organic matter through the soil."

By 1882 the Michigan State Board of Health had been functioning almost a decade, and Dr. Vaughan and Dr. Kellogg were both, at one time or another, among its members and prominent in its deliberations. Dr. Henry Brooks Baker,³⁹ its secretary since 1872, had received his medical education at the University of Michigan and the Bellevue Hospital Medical College. He employed Smith for work with the health board immediately following the Greenville convention. Smith probably was acquainted with each of these leaders in American sanitary science.

At the Bellevue Hospital Medical College, Dr. William Henry Welch, professor of pathological anatomy and general pathology, had within a very few years past established "the first laboratory course in pathology ever given in an American medical school."⁴⁰ Following this, another similar laboratory had been founded under Professor Theophil Mitchell Prudden⁴¹ at the College of Physicians and Surgeons of Columbia University; and thus two leading American medical schools had recognized pathology "as a subject of independent merit that should be taught practically, in the laboratory."⁴² Both men had studied abroad: Prudden at Heidelberg, Vienna, and Berlin, part of the time taking work with Rudolph Virchow and Julius Arnold; and Welch at Strassburg, Leipzig, and Breslau under many of the greatest masters of pathology, physiology, physiological chemistry, and other subjects. Both Welch and Prudden were of Connecticut birth and graduates of Yale University; Prudden of Sheffield Scientific School and Welch of the College of Arts and Sciences. Before going abroad, Welch had obtained his degree of doctor of medicine from the College of Physicians and Surgeons of Columbia. Prudden had taught at Yale and studied at the medical school there before beginning work with Dr. Francis Delafield at the College of

³⁹ Henry Brooks Baker, *Sanitary News* 5 (55): 79, Feb. 1885.

⁴⁰ Simon and James Thomas Flexner, *William Henry Welch and the heroic age of American medicine*, 114, New York, Viking Press, 1941.

⁴¹ Francis Carter Wood, Theophil Mitchell Prudden, *Dict. of Amer. Biog.* 15: 252-253, Chas. Scribner's Sons, 1935.

⁴² *William Henry Welch etc., op. cit.*, 116-117.

Physicians and Surgeons. Welch's first European journey has been characterized as "in its results perhaps the most important ever taken by an American doctor."⁴³ He had planned to study with Virchow but, at Leipzig, while working with Ernst Wagner in pathology and Carl F. W. Ludwig in physiology, he was advised by Ludwig to go to Breslau and study with Julius Cohnheim, a brilliant pioneer in the "application of physiological methods to pathology."⁴⁴ At Strassburg he had studied normal histology with Waldeyer, physiological chemistry with Hoppe-Seyler, and before he returned home he studied pathological histology with von Recklinghausen, Virchow's "most brilliant pupil."

While in Europe, Welch and Prudden became acquainted. Toward the middle 1880's both men went to the University of Berlin to study with Dr. Robert Koch. The advancement of experimental hygiene by improvements in bacteriologic technique has been called "the chief glory of Koch's Institute."⁴⁵ Prudden and Welch went to Koch's famous laboratory to learn this, and more, to learn everything possible of bacteriology and the master's new study methods. Both men, while in Europe, worked with other leading scientists: Prudden with Ferdinand A. T. Hueppe, and Welch with Wilhelm Frobenius and Max von Pettenkofer at Munich, Karl Flügge at Göttingen, and, among others, again at Leipzig with Ludwig and Weigert.⁴⁶

During his first journey, at Leipzig, Welch met Dr. John Shaw Billings⁴⁷ from America who was in Germany to study laboratories and methods with a view to organizing a hospital and medical department at Johns Hopkins University. Dr. Billings,⁴⁸ after distinguished service with the United States Army Medical Corps during the Civil War, had been transferred to the Surgeon-General's office in Washington, where he remained for the next thirty years. He was a graduate of the Medical College of Ohio, "the tenth medical college founded in this country and, following the medical school of Transylvania University (Lexington, Ken-

⁴³ *Idem*, 76.

⁴⁴ *Idem*, 78-110 (account of Welch's first European journey), quotation at p. 94.

⁴⁵ F. H. Garrison, *Intro. to the hist of med.*, *op. cit.*, 582.

⁴⁶ *William Henry Welch et al.*, *op. cit.*, 102, 107, 138-149.

⁴⁷ *Idem*, 92-93.

⁴⁸ F. H. Garrison, *John Shaw Billings*, 136, N. Y. and London, G. P. Putnam's Sons, 1915.

rucky), the second to be established west of the Alleghenies (1819)."⁴⁹ His years of service with the Medical Corps had proved fortunate, since, while located at important government hospitals, he acquainted himself with the latest and most approved ideas of hospital construction and administration. In time he was to plan the construction and administration of at least seven great buildings—the Barnes Hospital or Soldier's Home of the District of Columbia, the Army Medical Museum of Washington, D. C., the Johns Hopkins Hospital of Baltimore, the Laboratory of Hygiene and the William Pepper Laboratory of Clinical Medicine in Philadelphia, the New York Public Library, and the Peter Bent Brigham Hospital of Boston.⁵⁰

More than ten years before Pasteur and Koch had discovered the proper methods to investigate the infectious diseases of animals and man, Dr. Billings had been interested in the germ theory. During the 1860's and early 1870's, he had reported on cattle diseases and, in the course of his investigations with Dr. Edward Curtis, had examined "fluids of diseased cattle with reference to the presence of cryptogamic growths."⁵¹ Contagious pleuropneumonia and the "Texas cattle disease" were two of the animal maladies experimentally studied. Their conclusions were, of course, in the negative, but, it must be remembered that, at that time, fevers and infectious diseases of man had been studied for possible cryptogamous origins.⁵² After graduating from medical school, Dr. Billings had been appointed to the faculty as a demonstrator of anatomy. In Washington his interest in microscopy was supplemented by an interest in microphotography.⁵³ He learned to read the German language by studying Rudolph Virchow's three volume treatise on *The Pathology of Tumors*. Part of his future greatness was to lie in the field of medical bibliography, and at the Johns Hopkins Hospital medical school he was to be a lecturer on medical history.

Dr. Billings was among those leaders of American medicine who early realized the need for more adequate laboratory facilities

⁴⁹ *Idem*, 4-18, quotation at p. 8.

⁵⁰ *Idem*, 339.

⁵¹ *Report of the Comm'r of Agric. on the Diseases of Cattle in the United States*, 156-170, Washington, Gov't Print. Office, 1871.

⁵² *John Shaw Billings, op. cit.*, 150-152.

⁵³ *Idem*, 138.

in medical schools. Medical students in large numbers were going to Berlin, London, Paris, or Vienna to complete, or take all of, their studies, and the main reasons were superior teachers and superior laboratory and clinical facilities.⁵⁴

Erwin Smith, while yet in Ionia, might have read of the new laboratories of pathology established by Dr. Welch and Dr. Prudden. More likely, he became interested in their work, after their founders returned from their studies with Dr. Robert Koch. He probably knew something of the work of Dr. Billings after he became connected with the Surgeon General's office. Not yet had Dr. Sternberg become president of the American Public Health Association. Many years would pass before Sternberg would be president of the American Medical Association, and not until 1893 would he be Surgeon General. Smith's learning about pathology was in 1882 still a matter of books and scientific papers he could obtain. In Lansing he had access to better libraries. But his learning about the works of Pasteur and Koch was still quite meagre.

Since the latter half of the 1870's, Pasteur, "the pioneer of modern preventive inoculation against disease," and Koch, to whom "we owe the development of the correct theory of specific infectious diseases,"⁵⁵ had been at work in their laboratories. We do not know whether Erwin Smith knew anything of Pasteur's earlier studies of fermentation and biogenesis. We do know, however, that by 1882 he believed that the "researches of [John] Tyndall and Pasteur" had "settled in the negative" the question of the spontaneous generation of plant and animal forms.⁵⁶ We know also that he knew something of the medical research work of Edwin Klebs of Prague, who, along with Pasteur, did much to win over pathologists to the bacterial theory of infection.⁵⁷ Young Smith at this time appears not to have known of the great Frenchman's important studies of alcoholic fermentation and diseases of wine and his investigations of microorganisms in beer. This work, for the most part, preceded the year 1875. But, during the last half of the 1870's,⁵⁸ Pasteur's studies of the virulent diseases of

⁵⁴ F. H. Garrison, *Intro. to the Hist. of Med., op. cit.*, 756.

⁵⁵ F. H. Garrison, *Introd. to the hist. of med., op. cit.*, 575. See also to the same effect, *William Henry Welch etc., op. cit.*, 146.

⁵⁶ *Michigan School Moderator* 3 (13): 199 f., Nov. 30, 1882.

⁵⁷ F. H. Garrison, *Intro. to the hist. of med., op. cit.*, 580-581.

⁵⁸ *Idem*, 576-577. Also, Dr. C. A. Herter, The influence of Pasteur on medical science, *Bull. Johns Hopkins Hospital* 14 (153): 327-330, Dec. 1903.

anthrax and chicken cholera took place, and by 1882 Smith had learned of his immunological experiments against anthrax. This was important because during the next decade a firm biological basis for the modern public health movement would be supplied, and the way be opened for the development of bacteriology and immunology. New discoveries would bring about astonishingly rapid advancements in medical science, and indeed in the animal and plant sciences; and the public health movement would begin to pass from an emphasis on "sanitation of the environment, through the conquest of the insect-borne pestilences and the scientific use of vaccine and serum therapy, to the great social and educational movement of the present day,—boasting of successes which involve the prolongation by [many] years of the average period of human life."⁵⁹

Infiltration of the new learning from Europe to America was gradual, and in many quarters tardy. Communication facilities then were not what they are now. Because of the exact technical proficiency required for proper understanding and introduction of the new research methods, American scientists had to await the return of trained students from abroad and the extension of educational and laboratory facilities. Students acquainted with the new research skills were in demand, and book learning prepared some brilliant, capable workers.

Science in America was still conquering frontiers. Charles Darwin's *Origin of Species* (1859) and the evolutionary doctrine were gaining adherents, but complete acceptance of the new views was belated and confined to the most advanced scholars. Science in nearly all branches was analytic rather than synthetic. On this continent the important economic and scientific explorations of western United States to determine our mineral resources and other objects of national and state wealth had not been entirely completed. Geologists, while probing more deeply into the "scheme of the rocks," their stratigraphy and paleontology, had been also understandably slow to appreciate the full value and significance of so important a work as Sir Charles Lyell's *Principles of Geology*.⁶⁰ Important theories of the earth's transforma-

⁵⁹ *A pioneer of public health William Thompson Sedgwick, op. cit., 7.*

⁶⁰ Brief History of the Association 1848-1948, 7, 9, *Amer. Assoc. for the Advancement of Science*, Washington, 1948. Lyell's *Principles* was published 1830-1833.

tions and elemental composition had been advanced here before gatherings of scientists; but the necessary emphasis of the science was still on the minerals and wealth-producing resources. In a few words, geology was still largely mineralogy. Zoologists had still before them the task of completing their initial systematic arrangements of the American fossil discoveries of vertebrate and invertebrate life. Belief in man's antiquity was asserted,⁶¹ but final proof for the view had not yet been established. In 1869 Joseph Leidy's monograph on the "Extinct Mammalian Fauna of Nebraska and Dakota" had appeared, and during the next decades studies by Edward Drinker Cope⁶² and Othniel Charles Marsh would increase knowledge of the life histories of many forms of fossil vertebrates. Among the more interesting of the discoveries were toothed birds and horses, and in some of the work Darwin found the best proof of evolution to appear in twenty years.⁶³

Entomology, too, was primarily taxonomic. John Lawrence LeConte,⁶⁴ "the greatest of American entomologists," devoted his life especially to systematic work. Investigating "as far as practicable" life histories of the Coleoptera and other groups of insects, he defined more than 1,100 of the higher groups, and formed nearly 250 synoptic or analytic tables. Before his death in 1883, he had described "for the first time" half of the Coleoptera of the United States.

Most of the early important classification studies made by Charles Leo Lesquereux on the paleobotany of North America appeared during the 1870's and 1880's.

In the physical sciences, astronomy, chemistry, physics, etc., large segments of study represented more the work of analysis than synthesis. It is of interest to learn that not until almost the middle of the nineteenth century were the first American volume of astronomical observations and first catalogue of stars published.⁶⁵

⁶¹ *Idem*, 10.

⁶² Marcus Benjamin, Edward Drinker Cope, *Leading American men of science* ed. by David Starr Jordan, 313-340, N. Y., Henry Holt and Co., 1910.

⁶³ George Bird Grinnell, Othniel Charles Marsh, *ibid.*, 283-312 at p. 301.

⁶⁴ Samuel H. Scudder, John Lawrence Le Conte, *Biog. Memoirs Nat'l Acad. Sci.* 2: 261-293, 1886; *A hist. of the first half-century of the Nat'l Acad. of Sci.*, 157, Washington, 1913.

⁶⁵ B. A. Gould, James Melville Gilliss, *Biog. Mem. Nat'l Acad. Sci.* 1: 135-179, 1877; *A hist. of the first half-cent. of Nat'l Acad.*, *op. cit.*, 136.

In pathology the 1880's brought forward distinct advances in learning over the 1870's. For instance, the germ theory of disease, still under questioning scrutiny in the 1870's, became firmly established during the 1880's, improvements in research technique making possible astonishing accumulations of proof as to micro-organisms as causes of disease. Erwin Smith, in health work which required some knowledge of the latest triumphs of animal pathology, was bound to become familiar with the more important "high-lights" of a transition period which reached this country between the last years of the 1870's and, continuing throughout the 1880's, influenced greatly the course of development of plant pathology as well. He believed: ⁶⁶

Three main currents of thought and pathological influence flowed into this . . . period from earlier times. These three streams were (1) the purely systematic mycological influence of such men as Corda, Elias Fries, and the Tulasne brothers; (2) the experimental and observational work on the morphology and etiology of human diseases by Henle, Cohnheim, and Virchow and of plant diseases by such masters as Farlow, Berkeley, Kühn, and DeBary; (3) the bacteriological and animal pathological researches of Louis Pasteur and his school. The last influence was greatest of all, but each current interacted with the other two, and all of them on the men of this period. . . .

We know that Erwin Smith by 1882 knew of the famous discussions "fought so bitterly pro and con" ⁶⁷ between Pasteur and his pupils, Joubert and Chamberland, on the one side, and "Dr. Bastian and others" on the opposing. These discussions, according to Smith, had been going on "for the last dozen years." Émile Duclaux says, ⁶⁸ "All our present technique has arisen from the objections made by Bastian to the work of Pasteur on spontaneous generations." In other words, it was Pasteur's defense of his conclusions by repeating his experiments that established in the minds of other scientists the fundamental validity of his laboratory technique and the truth of his assertions. From his studies of fermentation and specific ferments emerged his discovery of anaërobic organisms, a discovery which Smith later ranked third in his list of "milestones in the progress of bacteri-

⁶⁶ Fifty years of pathology, *op. cit.*, 14-15.

⁶⁷ *Mich. School Moderator* 3 (13): 199 f., Nov. 30, 1882.

⁶⁸ *Pasteur, The history of a mind, op. cit.*, 114. See also, Third Part.

ology.”⁶⁹ The “overthrow of the doctrine of spontaneous generation” was placed first among “some of the milestones,” and, second, was the “discovery that putrescible fluids (exclusive of milk) will not decay after boiling, if protected from the bacteria of the air by means of cotton-plugs.” Each was a proof of the value of the fundamental knowledge revealed by Pasteur.

Only the invention by Robert Koch of a method to obtain pure cultures for the isolation of bacteria and fungi on solid media, a technique employing a poured plate,⁷⁰ later improved by the Petri dish,⁷¹ and gelatin for streak cultures, was regarded by Smith as possibly more important.⁷² This was believed with full knowledge of the influence in developing antiseptic surgery which Pasteur’s method of protecting his flask cultures from air contaminations had on the great English surgeon, Joseph Lister. Lister applied the principle to protect wounds in surgery, and he took also from Pasteur his dilution or mineral solution method for obtaining cultures and elaborated a method⁷³ to obtain pure cultures of “the lactic acid bacillus by gradual isolation of a single bacterial cell with a clever syringe of his invention, the only method of consequence,” Dr. Garrison has said,⁷⁴ “between Klebs (1874)⁷⁵ and Koch (1877).”⁷⁶

At the great International Medical Congress of London, held in August, 1881, when Pasteur’s discovery of a vaccine remedy against anthrax was explained amid loud cheering from the audience, Robert Koch’s poured-plate method for obtaining pure culture colonies of microorganisms on solid media was also described and demonstrated before Lister, Pasteur, and other world-known celebrities in pathology. Koch’s original sterile nutrient jelly or gelatin bacteria beds were soon to be displaced or supplemented by vegetable jelly derived from seaweed, or agar-agar; and

⁶⁹ E. F. Smith, *Bacteria in relation to plant diseases* 1: 152-153, Carnegie Institution of Washington, Sept. 1905.

⁷⁰ Robert Koch, Zur untersuchung von pathogenen organismen, *Mittheil. a. d. kaiserlichen Gesundheitsamte* 1: 1-48, 14 pl., Berlin, 1881.

⁷¹ R. J. Petri, Eine kleine modification des Koch’schen plattenverfahrens, *Centralblatt für Bakteriologie* 1 (9): 279-280, 1887.

⁷² See, Fifty years of pathology, *op. cit.*, 18.

⁷³ *Trans. Pathological Soc. of London* 29: 425-467, 1878.

⁷⁴ *Introduction to the history of medicine, op. cit.*, 590.

⁷⁵ *Op. cit.* See *Bacteria in relation to plant disease, op. cit.*, 1: 226.

⁷⁶ Koch’s 1877 publications will be mentioned and cited a few paragraphs, post.

the new methods of work would constitute a vast improvement over the procedures employed by Brefeld in the study of common moulds and fungi, and, indeed, on the culture methods of Pasteur, Klebs, and others.⁷⁷ Duclaux reminds us that, nevertheless, Pasteur, while isolating the bacteridium of anthrax from the "myriads of germs which accompany it in the soil," employed the principle of securing his "pure cultures in the medium best fitted to the physiological needs of the anthrax bacillus,"⁷⁸ and these experiments date from the year 1881. This does not imply that Pasteur did not accept Koch's better laboratory method. It is told⁷⁹ that at the International Medical Congress, when he saw Koch demonstrate his plate cultures, Pasteur rushed forward, exclaiming, "C'est un grand progrès!" It was about this time that Koch and his assistants perfected steam sterilization as a procedure in laboratory investigations.⁸⁰ In 1881, furthermore, John Tyndall (1820-1893), an Englishman, friend of Pasteur, a physicist and student of heat, light, sound, and fermentation, published his *Essays on the floating matter of the air*, and⁸¹ the study included a consideration of air floating matter in relation to putrefaction and infection.⁸² Smith accredited Tyndall⁸³ with discovering intermittent steam sterilization. "[H]e it was," Smith later said, "who recommended discontinuous sterilization, another superb simple procedure," and listed "Tyndall's discontinuous moist sterilization" eighth among the "milestones" of progress in bacteriology.⁸⁴

The year 1881 was considered by Smith "the proper starting point" for workers determining the systematic priority of bacterial species. Saying this in the first of his great three-volume work, *Bacteria in Relation to Plant Diseases*,⁸⁵ he reasoned that this year represented "the time when bacteriologists were first able

⁷⁷ E. C. Large, *The advance of the fungi*, 210 f., N. Y., Henry Holt & Co., 1940.

⁷⁸ Pasteur, *The history of a mind*, *op. cit.*, 286.

⁷⁹ F. H. Garrison, *Introd. to the Hist. of Med.*, *op. cit.*, 579.

⁸⁰ *Idem.*

⁸¹ *Idem.* 858. See, Fifty years of pathology, *op. cit.*, 18.

⁸² Fifty years of pathology, *op. cit.*, 18.

⁸³ See, "Annotated list of persons mentioned in this book," *Pasteur: The history of a mind*, *op. cit.*, 350.

⁸⁴ *Bacteria in relation to plant diseases* 1, *op. cit.*, 153; Fifty years of pathology, *op. cit.*, 18.

⁸⁵ Published by the Carnegie Institution of Washington (September, 1905), *op. cit.*, 155.

to make and easily maintain pure cultures of any given organism, namely, from the discovery of the poured-plate method of isolation in the year 1881." Not once in pages of his department of science and sanitation in *Michigan School Moderator* did Smith refer to Koch's invention of a poured plate method nor to Koch's canons or rules of laboratory procedure, published in 1882-1883.⁶⁶ These postulates, in great part a more perfect statement of desiderata taught Koch by Henle, required that the disease be reproduced and unvaryingly demonstrate the presence of the suspected organism, exclusive of all other matter which might cause the disease, and not once but many times was the isolation procedure to be repeated.⁶⁷ Of course, the *Michigan School Moderator* was not addressed to an audience of experimental scientists. But in 1882⁶⁸ Smith exulted that 221 persons in Michigan were actively engaged in studying science, although of this number how many were actual or aspiring pathologists was not revealed. During 1882-1883 Smith was, at best, a beginner in bacteriology. Having no research laboratory affiliation, he had not reached the point of learning laboratory procedure.

This was less true of generic nomenclature in bacteriology, which later was believed by him to have been really begun with Ferdinand Cohn's first morphological classification of the bacteria, which was prepared between the years 1870 and 1875. Some classification of different forms of the bacteria was contained in Dr. Sternberg's translation of Antoine Magnin's treatise, *The Bacteria*. Presumably, Smith in 1882⁶⁹ drew from this source his popularly intended definitions of *Bacillus subtilis* as "found in rancid fats and . . . the cause of the butyric ferment" and *Bacillus anthracis* as occurring "in the blood of animals sick of splenic fever." In 1872 Cohn had formed his new genus *Bacillus*, founded chiefly on *Bacillus subtilis* and including *Bacillus ulna* and *Bacillus anthracis*, the last representing, Smith believed, "a sort of afterthought," since he had never studied the organism

⁶⁶ Robert Koch, Ueber die milzbrandimpfung, eine entgegnung auf den von Pasteur in genf gehaltenen vortrag, Kassel und Berlin, Theodor Fischer, 1882. The same in French. Theodor Fischer, 1883.

⁶⁷ Cecelia C. Mettler, *History of medicine*, ed. by Fred A. Mettler, 263-264, Toronto, Blakiston Co., 1947; see also, F. H. Garrison, *Introduction to the history of medicine*, *op. cit.*, 579, and authorities cited.

⁶⁸ *Michigan School Moderator* 2 (19): 938, Jan. 26, 1882.

⁶⁹ *Michigan School Moderator* 3 (13): 199, November 30, 1882.

causing anthrax.⁹⁰ In 1868 a genus had been used to describe the anthrax organism, *Bacteridium* Davaine, so named in honor of Casimir Davaine, who studied anthrax before Pasteur and who Duclaux has said "perfectly demonstrated the coexistence of the bacteridium and of the anthrax."⁹¹ Smith noticed this genus when he discussed "Nomenclature and Classifications" in *Bacteria in Relation to Plant Diseases*,⁹² but he also stated his belief that "In the way of generic nomenclature there is not much of value prior to Cohn's first great paper in the year 1872." Cohn in 1876 discovered the spores of *Bacillus subtilis*,⁹³ but he is best remembered for his great studies which appeared in *Beiträge zur Biologie der Pflanzen*, edited by him at Breslau 1870-1892 and comprising five volumes. An eminent authority on algae and fungi, he was accredited by Smith with two "milestones" in bacteriology: first, his system of classification, and second, his discovery of endospore-bearing organisms resistant to heat.⁹⁴

Robert Koch, the modest district physician of Wollstein, Germany, more interested in his research than in medical practice, chose the erudite botanist of Breslau before whom to demonstrate his conclusions as to the life history and sporulation of the anthrax bacillus. As early as 1870, it is said,⁹⁵ Cohn had called attention to the vegetable nature of bacteria. In 1876, at the Botanical Institute of Breslau, before Cohn, Weigert, Leopold Auerbach, Ludwig Traube, and Julius Cohnheim, Dr. Koch followed his preliminary report to Cohn with a three-day demonstration of his culture methods and results,⁹⁶ and his proof as to the role of the spore in the etiology of anthrax won from Cohnheim the famous declaration:

Let everything stand and go to Koch! This man has made a great discovery which, in its simplicity and exactitude of method, is the more remarkable as Koch is separated from all scientific associations, and everything which he has done is absolutely complete. Nothing remains

⁹⁰ *Bacteria in relation to plant diseases, op. cit.*, 1: 158, 159.

⁹¹ Pasteur, *The history of a mind, op. cit.*, 238-240.

⁹² *Op. cit.*, 154-176. Quotation at p. 156.

⁹³ *Introduction to the history of medicine*, by F. H. Garrison, *op. cit.*, 575.

⁹⁴ *Bacteria in relation to plant diseases, op. cit.*, 1: 153.

⁹⁵ S. and J. T. Flexner, *William Henry Welch and the heroic age of American medicine, op. cit.*, 100.

⁹⁶ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 578.

to be done. I regard it as the greatest discovery yet made in bacteriology, and I believe Koch will astound and shame us all with discoveries yet to come.⁹⁷

Koch, at this time scarcely thirty three years old, had spent less than ten years in medical practice since getting his degree of doctor of medicine at the University of Göttingen. Cohn immediately published Koch's memoir which demonstrated that the "anthrax bacillus is the cause of the disease, and that a pure culture grown through several generations outside the body can produce it in various animals."⁹⁸ Moreover, Pasteur completely confirmed Koch's results. Of Koch's immediately following triumphs, Dr. Garrison⁹⁹ has commented summarily:

In November, 1877, Koch published his methods of fixing and drying bacterial films on cover-slips, of staining them with Weigert's anilin dyes, of staining flagellae, and of photographing bacteria for identification and comparison.¹⁰⁰ In 1878 appeared his great memoir on the etiology of traumatic infectious diseases ("Untersuchungen ueber die Aetiologie der Wundinfektionskrankheiten," Berlin,) in which the bacteria of six different kinds of surgical infection are described, with the pathological findings, each microorganism breeding true through many generations *in vitro* or in animals. These three memoirs elevated Koch to the front rank in medical science, and, through Cohnheim's influence, he was appointed to a vacancy in the Imperial Health Department (*Kaiserliches Gesundheitsamt*), with [Friedrich] Löffler and [Georg] Gaffky as assistants, in 1880. Here, in 1881, he produced his important paper upon the method of obtaining pure cultures of organisms by spreading liquid gelatin with meat infusion upon glass plates, forming a solid coagulum.

Apparently, the first written references by Smith to these scientific discoveries appeared in the May 31, 1883, issue of the *Michigan School Moderator*¹⁰¹ and dealt with Koch's discovery and proof that tuberculosis is a highly infectious bacterial disease

⁹⁷ S. and J. T. Flexner, *William Henry Welch etc., op. cit.*, 100. See also, Émile Duclaux, *Pasteur, The history of a mind, op. cit.*, 242.

⁹⁸ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 578, and authorities cited. The title of Koch's paper was "Aetiologie der Milzbrandkrankheit begründet auf die Entwicklungsgeschichte des Bacillus anthracis," Cohn's *Beitr. z. Biol. d. Pflanzen*, 2: 277 ff., Breslau, 1876.

⁹⁹ *Introduction to the History of Medicine, op. cit.*, 578-579.

¹⁰⁰ Robert Koch, *Verfahren zur Untersuchungen, zum Conserviren und Photographiren der Bacterien*, Cohn's *Beiträge* 2 (3): 399-434, Breslau, with 24 photographs on 3 plates.

¹⁰¹ 3 (36): 653.

affecting both man and animals. We do not know whether Smith purposely awaited such time as Koch's conclusions had been corroborated by further research. But such an intention seems somewhat indicated by the following language:

The researches of the German savant, Dr. Koch, concerning the nature and cause of consumption, have attracted world-wide attention. As a result of experiments upon lower animals, and of exhaustive microscopic study, the Doctor concludes: 1. That consumption is contagious, and 2. That it is caused by a *bacterium*. The latter may be set down as one of the most striking pathological discoveries of modern times. So novel, indeed, is this view of tuberculosis that many have doubted its trustworthiness, preferring to await the verification of Dr. Koch's researches by other observers before giving full credence. Such verification we now have in the experiments of Dr. W. W. Cheyne and other competent observers, and we may accept Dr. Koch's views as substantially proven. Dr. Cheyne's paper is published in *The Practitioner*, London, England, April, 1883. A good abstract of Dr. Koch's original article may be found in *The Philadelphia Medical Times* for September 9, 1882, and a full translation of it in *The Cincinnati Lancet and Clinic*, May 12, 1883.

By 1883 Koch, in charge of the important German Cholera Commission which visited Egypt and India, was strengthening still more the germ theory of disease and the doctrine of *contagium animatum* (Henle, 1840)¹⁰² upon which the theory rested. His most valuable studies of Asiatic cholera were performed in Egypt and India, while residing in those countries when the epidemic was violent. He reported from time to time his conclusions as to the disease, its contagious nature and cause. An outbreak of another epidemic in southern France enabled him, moreover, to verify these conclusions which he presented in a formal address at Berlin in July, 1884.¹⁰³ During the spring of that year and soon after the return of the expedition, Dr. Koch had been honored by an official dinner for his "series of brilliant observations from the discovery of the spores of the bacillus anthracis to that of the bacillus of cholera." Koch, in response, is said to have "claimed only to have discovered improved methods of observation."¹⁰⁴

¹⁰² F. H. Garrison, *Introduction to the history of medicine*, *op. cit.*, 575.

¹⁰³ *Appleton's Annual Cyclopaedia* and register of important events of the year 1844, n. s., 9: 143-145, N. Y., D. Appleton and Co., 1885. See also, *idem*, for the year 1883, 8: 298, on cholera in Egypt; 7: 798-799, 1882, concerning Koch's discovery of the tubercle bacillus; also, 4: 442-444, 1879, as to the germ theory of disease.

¹⁰⁴ Harvey Cushing, *The life of Sir William Osler*, *op. cit.*, 1: 212-213, quoting a letter from Osler.

Koch found the so-called "comma bacillus," and for his courage and persistence was compared to another heroic figure in medicine, Rudolph Virchow, who had gone to the scene of a typhus epidemic and risked his life "as much as the man who examined the bodies of cholera patients in the dirty huts by the Ganges." Dr. William Osler, then in Berlin, wrote of Koch: "His career is particularly pleasing, and it reminds one of that other country physician who nearly a century ago made the memorable observations on cow-pox." This represented a considerable shift in Osler's attitude toward the discoveries of Koch and Pasteur, and appears to have been quite typical of the reaction of many medical scientists in America. Dr. Harvey Cushing, in his two-volume study of *The Life of Sir William Osler* discloses that in 1881

Osler, like many other physicians, did not appear at this time fully to grasp as Lister did the significance of Pasteur's work, or to show great interest in Koch's remarkable contributions; and in [a letter written from the International Medical Congress of London at which Pasteur was called on to describe his recent experiments, which showed that animals could be protected "against certain scourges" by vaccination, Osler] dismissed the subject with the mere statement that "there was an abundant discussion on germs" in the Pathological Section. Indeed [continued Dr. Cushing in his discussion of the attitude of the medical profession in 1881 toward Pasteur's and Koch's discoveries,] the editorials in most of the British and Canadian journals of the time intimate that M. Pasteur saw germs everywhere, and his views regarding their prevalence as a cause of disease were regarded as rather horrible, if not mirth-provoking.

It seems that Pasteur's anthrax vaccine was interpreted, at first, as of primary value to agriculture and veterinary science, what there was of the latter. When, however, in 1882 Koch's discovery of the tubercle bacillus was reported, the attitude toward the work started by Pasteur changed with amazing celerity. This discovery became, as Smith recognized, one of the great finds of modern pathology. It became acknowledged as "one of the most important discoveries bearing upon the relation of micro-organisms to disease, a subject which," Dr. Cushing has said,¹⁰⁶ "made this particular decade stand out above all others in the history of medicine."

Greater recognition came to Dr. Koch after his work on Asiatic

¹⁰⁵ *Idem*, 189-190.

¹⁰⁶ *The life of Sir William Osler, op. cit.*, 1: 198-199.

cholera. He "discovered the cholera vibrio, its transmission by drinking water, food, and clothing, and incidentally found the microorganisms of Egyptian ophthalmia or infectious conjunctivitis (Koch-Weeks bacillus, 1883), for which results he received a donation of 100,000 marks from the Prussian"¹⁰⁷ government, and in 1885 was appointed Professor of Hygiene and Bacteriology at the University of Berlin. Under his tutelage some of the world's most brilliant medical research scientists were to be developed: Emil von Behring, Shibasaburo Kitasato, Friedrich Löffler, Richard Pfeiffer, Albert Fränkel, William Henry Welch, and others. Each gained eminence in his own right, and Koch was accompanied on the Egypt-India Cholera Commission journey by, among others, Georg Gaffky, who took Koch's place in charge of the expedition and was later succeeded by Löffler.¹⁰⁸

Erwin Smith, presumably in the year 1884, abstracted for his "Index Rerum" a translation of a report (or abstract of a report) made by Koch from Calcutta, India, to the German government. His abstract, entitled "Asiatic Cholera" and based on material taken from the *Journal d'Hygiène*, read as follows:

Dr. Koch now thinks he has established what seemed probable from his Egyptian researches, but not then certain, i. e. that cholera is caused by a minute transparent highly refractive bacillus. This bacillus resembles a comma, or bow in shape, and is endowed with active motion. He has made 17 dissections of cadavers and examined 27 other cholera patients, and in each case found this specific bacillus in the rice water discharges, as well as in the intestines. In no case was he able to discover it in the dejections of persons suffering from other diseases, even diarrhea, or by most careful examination of cadavers of persons, dead of other diseases.

The most important practical points of the report are: 1. A moderate amount of heat kills this bacillus. 2. It grows rapidly in artificial culture fluid, under proper conditions and on moist clothing, blotting paper, or moist earth. 3. It requires an alkaline solution in which to develop. [A]cids, even weak solutions, kill it. The acids of a healthy stomach offer to it an impossible barrier. Koch further notes in this connection the fact that persons suffering from disorders of the stomach are most likely to be attacked by cholera.

Soon we shall again consider research in cholera, and Smith's interest in methods to prevent epidemics of the malady by sanitary

¹⁰⁷ F. H. Garrison, *Intro. to the hist. of med., op. cit.*, 579.

¹⁰⁸ *Idem*, 579. See also, V. C. Vaughan, *A doctor's memories, op. cit.*, 146; also, *William Henry Welch etc., op. cit.*, 147-149; also, Douglas James Guthrie, *A history of medicine*, Phila., J. B. Lippincott Co., 287-288, 1946.

science. Koch had received his commission to study cholera largely because of his work in tuberculosis, and the German pathologist's studies of both diseases greatly stimulated the scientific aspirations of the young Michigan student. He said:

In 1882 [Koch] demonstrated in laboratory animals that tuberculosis could be produced by inoculating pure cultures of a microorganism cultivated from tubercles.¹⁰⁹ In 1883 he announced his discovery of the cause of Asiatic cholera, followed the next year by a long paper on the same subject, with all necessary proofs.¹¹⁰ In 1884 Koch also published his magnificent long paper on the cause of tuberculosis, a model for all time.¹¹¹ What a year that was! and what enthusiasm these two papers evoked! I remember it all as if it were yesterday, and much of the old thrill comes back! I knew very little about Pasteur in those days, but Robert Koch was my hero, and his influence more than that of any other man decided me to enter pathology and bacteriology.¹¹²

During these years there were other important discoveries¹¹³ which stimulated study. Three of the most celebrated, among many others, were: Löffler's and Emil Schütz's discovery of the bacillus of glanders (1882);¹¹⁴ Carl Eberth's¹¹⁵ and Gaffky's¹¹⁶ discovery of, and work with, the bacillus of typhoid fever (1883-1884); and Arthur Nicolaier's discovery of the cause of tetanus (1884).¹¹⁷ Notable, too, was the work of several on lobar pneumonia: Pasteur, Sternberg, Albert Fränkel, and Carl Friedländer.

Smith, when writing of medical research, did not presume to be

¹⁰⁹ For a more elaborate discussion, including an allusion to Koch's famous address given before the Physiological Society in Berlin March 24, 1882, see *The life of Sir William Osler, op. cit.*, 1: 198-200. This address was published in *Berliner klinische Wochenschrift* 19: 221-230, 1882.

¹¹⁰ Robert Koch, Vortrag ueber die Cholera, *Berliner klin. Woch.* 31 and 32: 477-483, 493-503. *Deutsche med. Woch.* 32 and 32a: 499-507, 519-523, 1884. Reprinted in *Fortschr. der Med.* 2(16): 121-134; (17): 141-168, 1884.

¹¹¹ Die Aetiologie der Tuberkulose, *Mitteil. a. d. K. Gesundheitsamte*, 2: 1-88, with 10 plates, 1884, Translated for New Sydenham Society, CXV.

¹¹² Fifty years of pathology, *op. cit.*, 18.

¹¹³ See, for instance, F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 582 ff.

¹¹⁴ Vorläufige Mittheilung über die Arbeiten des K. Gesundheitsamtes, welches zur Entdeckung des Bacillus der Rotzkrankheit geführt haben, *Deutsch. med. Wochenschr.* 8: 707-708, 1882.

¹¹⁵ Der Typhusbacillus und die intestinale Infection, Vortrag 226, *Volkmann's Sammel. Klin. Vorträge.* Innere Med. 77: 2,033-2,050, Leipzig, Breitkopf und Hartel, 1883.

¹¹⁶ Zur Aetiologie des Abdominaltyphus, *Mittheil. a. d. K. Gesundheitsamte* 2: 372-420, 1884.

¹¹⁷ Ueber infectiösen Tetanus, *Deutsche med. Wochenschr.* 10: 842-844, 1884.

a student of each subject from beginning to end. Often the "milestone" or "highlight" interested him most. His point of view was that one could not know plant pathology thoroughly "without paying some attention to the diseases of animals and of man, for," said he,¹¹⁸ "the subject is really one subject with the same general principles of procedure in research and the same underlying laws. Moreover," he believed, "plant pathology is tremendously indebted to animal pathology in many ways."

The fact, therefore, that a bacillus of leprosy had been found outweighed in importance the fact that Armauer Hansen discovered it, that Hansen was a Norwegian, or the exact time when the real discovery took place. On one occasion Smith gave the year of discovery as 1880.¹¹⁹ Friedrich Fehleisen published in 1883 on the etiology of erysipelas,¹²⁰ and showed by pure cultures, as a result of many years of study, that the disease is caused by streptococci. On this subject he had written before,¹²¹ but this evidently was to Smith the paper of final importance. About the same time Friedländer discovered the pneumococcus,¹²² and to this Smith later in writings referred.

It would be some years after Nicolaier's find of the tetanus bacillus before Von Behring and Kitasato would discover an antitoxin for tetanus. It would be more than half a decade before Von Behring's valuable antitoxin for diphtheria would be either discovered or made available. Before 1884 Edwin Klebs had seen the bacillus of diphtheria, but not until this year did Löffler remove doubt as to the organism's causal relation to the disease. Since Löffler secured pure cultures, Smith must have believed his demonstration tantamount to an original discovery.¹²³

Prior to 1884, half a dozen or more years were required to

¹¹⁸ Fifty years of pathology, *op. cit.*, 14.

¹¹⁹ *Idem*, 18.

¹²⁰ Die Aetiologie des Erysipels, Verlag von T. Fischer, Berlin, 1883.

¹²¹ F. H. Garrison, *Intro. to the Hist. of Med.*, *op. cit.*, 858.

¹²² Fifty years of pathology, *op. cit.*, 18; *The life of Sir William Osler*, *op. cit.*, 1: 216 (to effect Fränkel in 1884 described the pneumococcus but its relation to the disease was not generally accepted). See also, C. Friedländer, Ueber Pneumonie-Micrococccen, *Fortsch. d. Med.* 3: 91-93, 1885.

¹²³ Fifty years of pathology, *op. cit.*, 19; F. Löffler, Untersuchungen über die Bedeutung der Mikroorganismen für die Entstehung der Diphtherie beim Menschen, bei der Taube und beim Kalbe, *Mitteil a. d. K. Gesundheitsamte*, 2: 421-499, 1884; *Intro. to hist. of med.*, *op. cit.*, 858-859, also p. 581; Justina Hill, *Germs and the man*, 95, 210, G. P. Putnam's Sons, 1940.

differentiate the staphylococcus from the streptococcus and prove the differentiation by isolations in pure culture.¹²⁴

The importance of the year 1884 was shown also by Watson Cheyne's find of *Bacillus alvei* in foul brood of bees.¹²⁵ The distinguished veterinarians, Saturnin Arloing, also a physician, and Auguste Chauveau, an anatomist and pathologist, one of France's great physiologists, and said¹²⁶ to have been "abreast of Pasteur in his conception of pathogenic viruses (1868) and their attenuation (1883-1884)," published in 1884 their important study of septicemias, "Etude expérimentale sur la spéticémie gangréneuse."¹²⁷ A few years earlier, Arloing, with Charles Ernest Cornevin and Philippe Étienne Thomas, a pathologist and a veterinarian of France, had studied symptomatic anthrax of cattle; and a German pathologist, Otto von Bollinger, had produced Actinomycosis, or lumpy jaw of cattle, by means of the vegetable parasite. *Actinomyces bovis*, or the ray fungus.¹²⁸ The half-decade immediately preceding the year 1884 had been of vast importance to both animal and medical pathology, and not alone because of the epoch-making discoveries of Pasteur and Koch. As illustrations of this, it may be noticed that, while Friedrich Löffler did not really publish on swine-erysipelas until 1885,¹²⁹ he had discovered the bacteria of this disease about the same time he and Schütz discovered the bacillus of glanders. Albert Neisser of Breslau discovered in 1879 the gonococcus, cause of gonorrhoea and other serious ailments,¹³⁰ and it will be recalled that in this year the

¹²⁴ D. J. Guthrie, *A history of medicine, op. cit.*, 288; F. H. Garrison, *Intro. to hist. of med., op. cit.*, 577, 857; Justina Hill, *Germs and the man, op. cit.*, 46. It should be pointed out that Smith, in his address, Fifty years of pathology, did not include under the year 1884 any of the studies being made of the staphylococcus, although in other connections he referred to the work of Pasteur (1878-1892) on streptococcus and staphylococcus pyogenes.

¹²⁵ Fifty years of pathology, *op. cit.*, 19.

¹²⁶ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 563. See also "Annotated list of persons" in *Pasteur, The history of a mind, op. cit.*, 323 (concerning Arloing), 328 (Chauveau), and 349 (Thomas).

¹²⁷ *Bull. de l'Acad. de méd.*, 2e sér., 13: 604-615, May 6, 1884.

¹²⁸ Fifty years of pathology, *op. cit.*, 18. Concerning Arloing, Cornevin, and Thomas's study of symptomatic anthrax, see, also, *Pasteur, The history of a mind, op. cit.*, 315. Also, F. H. Garrison, *Intro. to the hist. of med., op. cit.*, 583.

¹²⁹ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 581, and citing Löffler's publication on this, *Arb. a. d. K. Gesundheitsamte*, 1: 46-55, Berlin, 1885.

¹³⁰ Fifty years of pathology, *op. cit.*, 18. See also, Garrison, *Introduction to the*

many years of study of malarial fever and its cause came to a focus. Laveran's discovery in 1880 of protozoan parasites in malarial fever of man stimulated new research which extended over many years. Ettore Marchiafava, Italy's leading pathologist,¹³¹ and Angelo Celli, among other eminent scientists of the same nationality, studied malaria, and Smith said of them:¹³²

In 1884 Marchiafava and Celli saw the meningococcus of meningitis and correctly described its location, but [Anton] Weichselbaum was the first to isolate it in pure culture (1887).¹³³ Marchiafava and Celli subsequently differentiated the parasite of tertian malaria, and Celli wrote much on the occurrence and prevention of malaria in Italy.¹³⁴

Another important discovery originating from studies of diseases occurring around the Mediterranean Sea and elsewhere was that of the micrococcus of "Malta fever," so-called because of its prevalence on the island of Malta. "Malta fever" is now known as "undulant fever," and is caused by microbes now known as brucella, a derivation from the name of the discoverer, Dr. David Bruce, a young English medical officer, who found the causal organism in the spleen of patients.¹³⁵ Unpasteurized milk and other foods from cows, pigs, and goats are now known to be agencies which transmit the microbic cause of this disease to man. Bruce's discovery followed by several years the period in Erwin Smith's life of which the year 1884 was a climax.

Until the year 1883 Smith had been a text-book student of botany at night, a botanical explorer and systematist on Sundays and holidays, but every day of every week during working hours

history of medicine, op. cit., 582 and 857. Also C. C. Mettler, *History of medicine, op. cit.*, 645, and authorities cited.

¹³¹ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 744.

¹³² Fifty years of pathology, *op. cit.*, 19. See also, F. H. Garrison, *Intro. to the hist. of med., op. cit.*, 744, for the names of other Italian scientists who studied malaria.

¹³³ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 582, gives the year 1887 as when Weichselbaum proved the cause of cerebrospinal meningitis.

¹³⁴ Garrison, *idem*, 583, writings of Laveran's discovery of the plasmodium of malarial fever, adds: "These hemocytozoa were accurately described by Ettore Marchiafava and Angelo Celli (1885). . . . In 1889, Marchiafava and Celli showed that the organisms of the pernicious and the tertian and quartan forms are different." Other important malarial research results are also defined.

¹³⁵ Fifty years of pathology, *op. cit.*, 25. Smith and Garrison agree that the year of this discovery was 1887. See, F. H. Garrison, *Intro. to the hist. of med., op. cit.*, 582. See, also, Justina Hill, *Germs and the man, op. cit.*, 78.

and very often at night he studied health matters. In September 1883 he added to his regular duties with the State Health Board his employment by *The Sanitary News*, an illustrated daily and weekly journal believed by him to be the best of its kind in the west. This journal reported the latest work being done in Europe as well as America. Before proceeding to the further years of this employment, however, we must acquire some idea of the status of plant pathology, the branch of science to which ultimately he would dedicate his life's work.

During the late 1850's Julius Gottlieb Kühn of Germany had published an important, and perhaps "the first distinct treatise on plant diseases worthy of mention."¹³⁶ He is regarded by some authorities as the "Father of Modern Plant Pathology."¹³⁷ Because of his work and that of Heinrich Anton de Bary, "founder of modern mycology," on the entophytic fungi, the past quarter of a century had been "characterized by an almost complete devotion to the study of the causal relations of fungi to the diseases with which they are found associated."¹³⁸ Kühn's treatise was entitled *Die Krankheiten der Kulturgewächse, ihre Ursachen und ihre Verhütung*, or The diseases of cultivated plants, their cause, and their prevention. Dr. Whetzel has said of this work: "It is in several respects an epoch-making book. It is the first phytopathologic text to appear based upon the remarkable and far-reaching discoveries and researches of de Bary, the Tulasne brothers, Pasteur, and other workers of the first half of the 18th century in the field of parasitology."¹³⁹

DeBary, Dr. Whetzel believed,

did not concern himself extensively with the pathology of the diseases with which he worked, except from the point of view of the physiologist undertaking to discover the nature of the life of the parasite, its mode of attack, its method of feeding, and its life history. With the many other aspects of the disease, especially the economic, he concerned himself little.¹⁴⁰

DeBary, born in January 1831, at Frankfurt am Main, Germany, became interested in botany while a student at the Gymnasium, and so strongly did his enthusiasm for its study persist that, despite his medical degree secured in 1853 at the University of

¹³⁶ Cornelius L. Shear, *Phytopathology* 3 (2): 77, April 1913.

¹³⁷ Herbert Hice Whetzel, *An outline of the history of phytopathology*, 47, Phila. and London, Saunders Co., 1918.

¹³⁸ *Idem*, 44.

¹³⁹ *Idem*, 52.

¹⁴⁰ *Idem*, 47.

Berlin and a few months of practice at Frankfurt, he gave up active participation in this profession to devote himself to research in botany. In December of his graduation year he began as an instructor, or privat docent, at the University of Tübingen. He published his classic work, *Die Brand Pilze* (1853), in which, though only twenty-two years of age, he "established beyond doubt the causal nature of the fungi found associated with rust and smut diseases."¹⁴¹ Two years later he became Karl Wilhelm von Nägeli's successor at the University of Freiburg, and there, as a professor, he remained until 1867 when, after refusing a call to the University of Leipzig, he accepted a professorship at the University of Halle formerly held by D. F. L. von Schlechtendal. From 1872 until his death in 1888 he occupied the chair of botany at the University of Strassburg.¹⁴²

Gifted with brilliance and the instincts of a cautious experimental scientist, one who refused to admit or advance any truth as fact until proved by exact, technical procedures, he studied the lower plant forms. In 1861 he published "his investigations on the cause and nature of the late blight of potatoes in which he proved the causal relation of *Phytophthora infestans*. In 1865 he published his work, establishing the relation of the aecidium on barberry to the rust fungus on wheat."¹⁴³ These were studies on which many predicate their belief that he was "the father of modern plant pathology," but, whether or not, no one denies that he supplied a foundation for future work in mycology and plant pathology. Smith said: "DeBary's work cleared the way for all that has followed in plant pathology, and we must ever think and speak of him with that reverence due to a great master." In his series of monographs on algae, fungi, myxomycetes, bacteria, and higher plants, Smith saw that "biological adaptations were always kept in mind" and that "his work and that of his students . . . had much influence on human and animal pathology, since his very successful infection experiments with fungi on plants suggested many things to those who were trying to determine the cause of human and animal plagues."¹⁴⁴

In 1865, the same year DeBary demonstrated heteroecism

¹⁴¹ *Idem*, 45.

¹⁴² Erwin F. Smith, Anton de Bary, *Phytopathology* 1 (1): 1-2, Feb. 1911.

¹⁴³ H. H. Whetzel, *op. cit.*, 45.

¹⁴⁴ Anton de Bary, *op. cit.*, 1-2.

among the Uredinales,¹⁴⁵ he made use of careful culture experiments to determine life histories and cycles of development of fungi.¹⁴⁶ Michael Stepanovitch Woronin and Anders Sandøe Oersted soon made use of the procedures, new then to botanical investigations; and eventually, we shall see in Chapter III, the new methods would be brought to America by one of DeBary's students. Although DeBary died at the age of fifty-seven years, no less than sixty-eight men later distinguished in science studied under him at Strassburg.¹⁴⁷

On one occasion at least, Erwin Smith divided the history of plant pathology into two periods—before and after the year 1880.¹⁴⁸ Others¹⁴⁹ who have written on this subject seem fairly in accord that a pre-modern era ended during the early 1880's, about the time that DeBary, after supplying a scientific interpretation of the cereal rusts, was completing and ready to publish his classical work on the comparative morphology and biology of the fungi and other forms of the lower plant orders.

Smith chose as his year of division, 1880, for many reasons. In his presidential address before the Society for Plant Morphology and Physiology in 1902 he emphasized that before that year laboratory methods for the study of fungi and bacteria

were not well developed. In the first place, there was no exact and convenient method for obtaining pure cultures and, in the second place, the microscope was still the principal instrument of research. . . . The main thing considered was the parasite rather than the host plant, and the technique for the study of both was of the simplest sort. We had no precise fixing and staining methods, no fine microtomes with their yards of serial sections, no synthetic culture media, no elaborate sterilizing ovens and broad chambers, and no apochromatic glass for lenses. "Pure cultures" were practically unknown, and photography and photomicrography had not yet become arts of daily use in the laboratory. . . . Only the crude beginnings of bacteriology were earlier than 1880. Prior to that time we had, it is true, the fractional and dilution methods of isolation,

¹⁴⁵ Joseph Charles Arthur, History and scope of plant pathology, address before the Congress of Arts and Science, Universal Exposition, St. Louis, 1904, ed. by H. J. Rogers, 5: 154, Houghton, Mifflin and Co., 1906.

¹⁴⁶ W. A. Kellerman, Index to Uredineous culture experiments with list of species and hosts for North America, *Jour. Mycology* 9 (4): 244, Dec. 1903.

¹⁴⁷ E. F. Smith, Anton de Bary, *op. cit.*, 2.

¹⁴⁸ Plant pathology: a retrospect and prospect, *Science* n. s. 15 (381): 601-612, at pp. 602-605, 1902.

¹⁴⁹ J. C. Arthur, *op. cit.*, where a period 1850-1880 was considered.

but these, although capable of yielding good results, are troublesome and have never appealed very strongly to the mass of workers.

Before 1880 the available literature had been mostly works on the classification and description of fungi, with some few books of merit on plant diseases. "In the time of which I speak," continued Smith,

there were already many excellent helps in the way of treatises on fungi. We had, for instance, the splendid volumes of the "Selecta Fungorum Carpologia" by the brothers Tulasne, and if we were not always sure of the Latin construction, we could at least read the magnificent copper plates which embellish these volumes. There were also books by Persoon, Corda, the Nees von Esenbecks, de Notaris, Rabenhorst, de Schweinitz, Fuckel, Bonorden and Montagne. There were numerous volumes by the Swedish mycologist Frieze. We had also Berkeley's "Outlines," Cooke's "Handbook of British Fungi," and many scattered descriptions by Oudemans, Magnus, Schroeter, Winter, Berkeley, Cooke, Ellis, de Thuemen, Rehm and others, in *Hedwigia* and other journals. The Italian Saccardo had not yet begun his monumental compilation of all known species of fungi, but he was printing the first parts of his "Fungi Italici." Several parts of Brefeld's "Untersuchungen" also appeared prior to 1880, and there was an excellent "Handbuch" of cryptogamic plants by Luerssen. There were also some good exsiccati, including, in this country, the first centuries by Ellis. . . .

In the matter of plant diseases, we were much less well provided. In fact, there was scarcely anything in English in the nature of a general treatise. The nearest approach I can recall was a brief chapter on diseases caused by fungi in Berkeley's "Outlines of British Fungology" (1860), and a little book by M. C. Cooke entitled "Rust, Smut, Mildew and Mould" (1865). A knowledge of foreign languages was even more essential in that day than it is now for the study of diseases of plants. Even in European tongues there were comparatively few useful general works on diseases of plants. We had, it is true, the rare, largely neglected, and generally negligible, crude, early works of Re, Unger, Meyen, Hamel and Hallier. There was also the first edition of Sorauer's "Pflanzenkrankheiten" (1874), and Winter's little book of a dozen chapters, which appeared in 1878. This book, which described some of the commonest diseases of plants, is now quaint and old-fashioned reading, but it then seemed a model in its way. In 1878 there also appeared a little book by de Jubainville and Vesque on "Les Maladies des plantes cultivées, des arbres fruitiers et forestiers, produites par le sol,—l'atmosphère,—les parasites végétaux, etc., d'après les travaux de Tulasne, de Bary, Berkeley, Hartig, Sorauer, etc." There was also an earlier and very good book for its time by Kühn (1858).

As to bacterial diseases of plants, Smith believed that the litera-

ture prior to 1880 had been almost wholly inadequate. Before 1874 it had been an "ill-digested mixture of fact and speculation."¹⁵⁰ He pointed to Sorauer's *Handbuch der Pflanzenkrankheiten* of that year in which was "no mention of bacterial diseases of plants. A great many diseases are included," Smith said,

but none of this type. De Jubainville and Vesque writing in 1878 mention a "cellular rot" of potatoes, radishes, carrots, and beets, occurring in the soil or in cellars, but they attribute it to soil ill-adapted to the plants or to improper cultivation. No mention is made of bacteria either as the cause of this rot or of any other disease mentioned by them. There is nothing on this subject in Winter's little book, published in 1878. . . . The first edition of [A. B. Frank's *Die Krankheiten der Pflanzen*], published in 1880, contains a brief chapter on root-tubercles of Leguminosae, but nothing on bacterial diseases of plants.

J. Reinke's and G. Berthold's *Zersetzung der Kartoffel* (1879) was also discussed since a potato rot was described, but their work was criticised, especially for a lack of clarity on some points and the failure to make use of pure culture experiments.

Practically contemporaneous with Woronin's announcement¹⁵¹ in 1866 of his discovery of bacteria in the root-tubercles of lupins, the Frenchman Davaine,¹⁵² calling his organism *Bacterium putredinis*,¹⁵³ had shown that certain plants would rot when inoculated with infusions containing bacteria. Ph. Van Tieghem in 1879 claimed to have rotted land plants with his *Bacillus amylobacter*.¹⁵⁴ There were other miscellaneous discoveries. Smith's bibliography on "thermophilic bacteria"¹⁵⁵ began with a publication by Pierre Miquel of this year. But to an American, Thomas Jonathan Burrill, belongs the honor of having found and proven (1877-1880) the first real parasitic bacterium in a plant disease. His discovery in pear blight, and proof by direct infection experiments, of "minute moving organisms,"¹⁵⁶ bacteria¹⁵⁷ and not fungi,

¹⁵⁰ *Bacteria in relation to plant diseases* 2: 9-10, Carnegie Inst. of Washington, 1911.

¹⁵¹ See bibliography, *Bacteria in relation to plant diseases*, *op. cit.*, 1: 239.

¹⁵² *Bact. in rel. to pl. dis.*, *op. cit.*, 2: 89.

¹⁵³ *Idem*, 9, 77.

¹⁵⁴ *Idem*, 77.

¹⁵⁵ *Bact. in rel. to pl. dis.*, 1: 247.

¹⁵⁶ T. J. Burrill, Blight of pear and apple trees, 10th *Annual Report of the Board of Trustees of Illinois Industrial University* for two years ending Aug. 30, 1880, 62-84, Springfield, 1881.

¹⁵⁷ *Trans. Ill. State Horticultural Soc.* for 1877: 114-116; for 1878: 79-80.

extended the study of bacteria to plant diseases and the study of plant diseases to include bacterial causes. His discovery and his experiments, which withheld the test of subsequent investigation, will be elaborated more fully in Chapter III.

Burrill was a student of the German and French languages, as well as a botanist who early realized the need, especially in the agricultural colleges, for studying vegetable diseases. In 1880 a short treatise on bacteria by Georg Winter, appearing as one of the parts of *Die Pilze* of Rabenhorst's *Kryptogamenflora Deutschlands* (1884), was translated by Burrill and published in 1882 as part of a paper entitled simply "The Bacteria."¹⁵⁸ Smith reviewed the "neat," sixty-five page pamphlet the next year in the *Michigan School Moderator*.¹⁵⁹ It contained, he told his readers, "a very satisfactory account of the nature and organization, effects, and classification of these microscopic plants." In this paper on "The Bacteria," Burrill described the organism of pear blight and entitled it *Micrococcus amylovorus*.

Smith at this time believed that the bacteria are "cellular organisms of a vegetable nature." His belief was based on Magnin's treatise which considered, among many other points, their structure, the nature of the substances contained in their protoplasm, their forms, multiplication, and "their rôle in fermentations, in putrefactions, in contagious diseases, and in surgical lesions." In his "Index Rerum," Smith devoted several pages to points about bacteria; and one, "The actual state of our knowledge of bacteria," was taken directly from Magnin's work as translated in 1880 by Dr. Sternberg.

Among the pages of Smith's "Index Rerum" appeared no mention of the work of Burrill on pear blight, no reference to the work of Anton de Bary, none to the work of Kühn or his book on plant diseases, no allusion to culture study of fungi or bacteria, no reference to the early plant disease study of Dr. William Gilson Farlow of the Bussey Institution and cryptogamic laboratories at Harvard University, and none to Woronin's researches on the life histories of the lower plants, chiefly fungi. Woronin¹⁶⁰ was a former student of DeBary at the University of Freiburg and

¹⁵⁸ 11th *Ann. Rep't Ill. Indus. Univ.*, reprint.

¹⁵⁹ 3 (36): 653, May 31, 1883.

¹⁶⁰ Erwin F. Smith, Woronin, *Phytopathology* 2 (1): 1-4, 1912, quotation and papers tabulated at p. 2.

a graduate of the University of St. Petersburg. He studied also at the University of Heidelberg; and he spent some time in 1860 with Gustave Thuret and E. Bornet at Antibes, France, studying Mediterranean algae. Smith later wrote of him: "As early as 1880 his name was known in every quarter of the globe as a very original and distinguished investigator, possessed not only of unusual powers of observation, but endowed with a genius for painstaking, so that his papers remain models of lucid presentation and convincing illustration." He tabulated the famous Russian scientist's papers, those done alone and those with DeBary, and among those which he regarded as "wonderfully illuminating" was his paper on the club root of cabbage (*Plasmodiophora*) which we shall consider again later.

If, during the early 1880's, Smith was studying the accessible literature on diseases of plants, his interest in the subject could not have been primary and was quite unlike his enthusiasms for systematic botany and the literature of animal and human pathology. The last was a part of his daily work. His botanical interest had never failed him; but botany, either as an independent instructional entity or a practiced profession, was almost unknown in America, especially in the "west." Botany was still an adjunct of "natural history" or the natural sciences. Even in the eastern states, for the most part it was a taxonomist's science. As late as 1883 one major laboratory equipped for real experimental research in plant physiology existed, and that was at Harvard University under George Lincoln Goodale. Some other laboratories had been equipped for undergraduate instruction. Professor Beal's laboratory at Michigan Agricultural College was one, and from there Smith could have acquired some modern knowledge of laboratory procedures and methods. Beal's and Dr. Bessey's laboratories were modelled after those at Harvard.

No laboratory in the United States for the study of human, animal, or plant diseases, however, was yet comparable to the laboratory of the Imperial Health Department in Berlin, to which in 1880 Robert Koch had been appointed. During the early 1860's Louis Pasteur had held a professorship at Lille.¹⁶¹ But, mainly, his work had been done in Paris at the *École Normale*, and the

¹⁶¹ The influence of Pasteur on medical science, *op. cit.*, 327.

Sorbonne where his professorship was in chemistry.¹⁶² He was beginning to exercise a great influence on animal and human pathology, and, indeed, on medical and health matters generally. But Emile Duclaux warns that ever we must keep in mind "the fact that Pasteur was neither a physician nor a veterinary surgeon, and that the history of any disease, *as a disease*, did not interest him deeply. That which he studied in the anthrax bacteridium was not the anthrax, but the mode of reaction of the microbe toward the organism in which it developed. Every bacterial cell able to become pathogenic in any way or by any means whatsoever, and thus to throw light on the mechanism of the struggle with the cells of the host organism, was welcome in his laboratory."¹⁶³

We do not know precisely when Erwin Smith learned of Pasteur's vaccine against fowl cholera. But one so enthused about the vaccine made from an attenuated culture against anthrax would learn soon, it would seem, of another similar and practically contemporaneous discovery important to animal pathology, especially since he believed that these "laid the foundations of immunology."¹⁶⁴ During the middle 1880's Joseph Meister, a youth nine years of age, was vaccinated successfully for hydrophobia by a remedy found by Pasteur in an attenuated virus; and this Smith called "the greatest triumph of all." Years later, in Paris, he would become acquainted with Meister, by that time an adult.¹⁶⁵

We do not know, furthermore, precisely when Erwin Smith first learned of the new anilin stains begun about 1875 to be used by Carl Weigert and others to demonstrate bacteria in tissues. We do know that Smith regarded their introduction and that of photomicrography as an important "milestone" of progress in bacteriology;¹⁶⁶ and that he believed staining in tissues was "worked up carefully for bacteria causing animal diseases" more than a quarter of a century before much was known of the "best methods of staining bacteria in vegetable tissues."¹⁶⁷ The effective tech-

¹⁶² *A pioneer of public health William Thompson Sedgwick, op. cit.*, 7; F. H. Garrison, *Intro. to the hist. of med.*, *op. cit.*, 575, 579.

¹⁶³ *Pasteur, The history of a mind, op. cit.*, 253-254.

¹⁶⁴ *Fifty years of pathology, op. cit.*, 17.

¹⁶⁵ *Idem, Journal of Smith's third European tour, February 2, 1925.*

¹⁶⁶ *Bacteria in relation to plant diseases, op. cit.*, 1: 153.

¹⁶⁷ *Idem, 29, 153.*

nical proficiency demonstrated by Koch as early as his publishings on bacterial wound infections and sepsis must have been a strong, abiding reason why he in time appealed to Smith as a "hero" in science.

The great distinguishing feature between the work of Pasteur and Koch—what has been called "the central jewel in the diadem of Pasteur's achievements"¹⁶⁸—was the epoch-making principle of experimental immunity to pathogenic bacteria. Émile Duclaux has said¹⁶⁹ that Pasteur, but for this, might have been only a "precursor" to some other scientific man, "a Koch for example," since

his pathological work was the development and the complement of his work upon the fermentations. . . . He remains the equal of many when he demonstrates the bacterial origin of anthrax or of other diseases. Where he is without equal is when he discovers the attenuation of viruses, and when he introduces into science that fertile notion that allows us to act upon a disease by acting, not upon the sick person, as up to that time one had been in the habit of doing, but upon the pathological bacterium.

Smith, like Duclaux, found in Pasteur's work the origins of bacteriotherapy. Whereas Darwin's work had given wide currency to the phrase "struggle for existence," Pasteur's experiments had placed another phrase, the "struggle for oxygen," into the scientific foreground. Duclaux explained¹⁷⁰ thus:

Some common bacteria sown with the anthrax bacteridium, in neutral or alkaline urine, prevent its developing because they take possession of the ground more rapidly and exhaust the oxygen. They can, in the same way, arrest its development in an animal. "It is possible" [said Pasteur] "to inject great quantities of the anthrax bacteridium into an animal without its contracting the disease, if some of these common bacteria have been present in the culture used." Here we have the first example of *bacteriotherapy*, to which Cantani returned later, and which has not spoken its last word. The interpretation of these facts has changed, and we know now that it is less simple [than it seemed at that time] but the idea of the struggle for existence was nevertheless then introduced into pathology, in the domain of cellular antagonism: and it has remained there.

Pasteur began to study fermentation and disease before Koch secured his medical degree from Göttingen. For some time, how-

¹⁶⁸ Dr. C. A. Herter, *The influence of Pasteur on medical science*, *op. cit.*, 329.

¹⁶⁹ *Pasteur, The history of a mind*, *op. cit.*, 232.

¹⁷⁰ *Idem*, 256.

ever, Pasteur's lack of pathological training stood in his way. For instance, during the 1860's, while studying the butyric acid ferment (1863) and later a disease of silkworms known as flâcherié, he had observed the formation of spores; but he was not as prepared as Koch to study sporulation in anthrax. Koch began to study the disease in 1876. He forced the rods of the anthrax to produce spores and proved their fundamental significance in the disease's etiology. By experiments, furthermore, he proved the transmission of infection through spores in the earth and the food of animals.¹⁷¹

Pasteur, on the other hand, knew how to perform an indefinite series of cultures, something which it is said¹⁷² Koch as yet had not learned to do. He proved that the virulence of anthrax belongs to the bacteridium and, examining the microbe's reaction to the organism, brought forth proof of a bacterial secretion which produces a symptom of the disease—agglutination and massing together of the corpuscles of anthrax blood. Koch had realized that the anthrax bacteridium is aerobic and, to survive, has to have oxygen.¹⁷³ But he had not actually found anthrax spores in the earth, nor did he know how long these spores live. Pasteur discovered that anthrax spores are distributed by earthworms and, moreover, could survive for many years where the conditions were the slightest possible for bacterial growth. An analysis of the comparative contributions to learning made by these two great scientists might be extended over many pages.

During the early 1880's, Erwin Smith, in his "Index Rerum," wrote of Pasteur in another connection. Concerning antiseptic treatments for wound infections, he abstracted the substance of three pages from Sternberg's translation of Magnin's *The Bacteria*. Monsieur Guérin had caused

many bad wounds [to heal] by keeping them closely covered with cotton wadding. Following Pasteur, he attributed this to the power of batten to strain out or hinder germs floating about in the air from gaining access to the wounded surface. Lister had equal success . . . by the use of carbolic acid spray, and of dressing saturated with this acid. He attributed his success to the antiseptic properties of the acid—thought it destroyed

¹⁷¹ Pasteur, *The history of a mind*, *op. cit.*, 241-244.

¹⁷² *Idem*, 250-255. "the first example introduced into science of a bacterial secretion producing one of the symptoms of a disease."

¹⁷³ *Idem*, and pp. 286-287. Also, Smith, *Fifty years of pathology*, *op. cit.*, 17.

the germs. Virchow and other microscopists have proved that bacteria occur quite generally in such wounds in spite of this treatment. The cause of their more rapid recovery must be sought elsewhere according to Magnin.

In 1874 Dr. Lister had sent Pasteur his "celebrated letter acknowledging the value of his work in relation to antiseptic surgery." Pasteur was then being "virtually transformed," Garrison says,¹⁷⁴ "from a chemist to a medical man, particularly in his mode of attacking the problem of infectious diseases." Even as the science of physiological chemistry is written before and after Pasteur, and medical bacteriology before Koch and after Koch, so surgery is divided into two great periods: before and after Lister.¹⁷⁵ In France, Germany, and England, world renowned institutions for the study of infectious diseases and their prevention have been established, each to honor the work of a countryman.

During the 1870's and 1880's Lister was reforming surgical practice on the bases of antiseptics and chemotherapy. This was the period when Pasteur was "writing and speaking on wound infections and the cause of puerperal or childbed fever and on the necessity of an entire change in surgical procedure in order to avoid the frightfully prevalent hospital infections."¹⁷⁶ In the matter of puerperal fever studies, Pasteur had predecessors, among them, Dr. Oliver Wendell Holmes of Boston and Dr. Ignaz Semmelweis of Vienna.¹⁷⁷ Lister was influenced by this part of Pasteur's work, and by the latter's studies of air-borne bacteria, bacterial wound infections, fermentation and putrefaction including the ferments of lactic and butyric acids and, in fact, the whole range of his investigations. At the microscope he himself had studied the life histories of fungi and bacteria and their parts in fermentation and putrefaction.¹⁷⁸

In his first volume of *Bacteria in relation to plant diseases*¹⁷⁹ Smith evaluated the early work of Pasteur and Koch:

¹⁷⁴ *Intro. to the hist. of med., op. cit.*, 576-577.

¹⁷⁵ Harvey Graham, *The story of surgery*, with a foreword by Oliver St. John Gogarty, 361, N. Y., Doubleday, Doran and Co., 1939.

¹⁷⁶ E. F. Smith, *Fifty years of pathology, op. cit.*, 17.

¹⁷⁷ *Idem*, n. 2. See also, C. C. Mettler, *History of medicine, op. cit.*, 975-977, account of studies before and by Pasteur on puerperal fever; F. H. Garrison, *Intro. to the hist of med., op. cit.*, 576-577, 435.

¹⁷⁸ Harvey Graham, *The story of surgery, op. cit.*, 342-360, at p. 351.

¹⁷⁹ *Op. cit.*, 152.

France has had many great sons, none greater than [Pasteur]. His refutation of the doctrine of spontaneous generation cleared the air of many misconceptions and laid the foundations for exact experimentation. His demonstration of the nature of pébrine and flacherie, two destructive diseases of silk worms, brought again into vivid light the assumption that the origin of a great variety of human and animal diseases should be sought in the activities of microscopic organisms. His studies of anthrax and other diseases of warm-blooded animals confirmed this suspicion and set a great many persons thinking and working. His investigations of the problems connected with fermentation were similarly fertile in discovery and in suggestion.

The publication of Robert Koch's great paper on tuberculosis in 1884 marked another distinct advance. The same memorable year Koch published in full his discovery of the cause of Asiatic cholera, only a brief announcement of it having been made in 1883. The whole world was interested, and from this time on experimenters began to multiply in every civilized land, boards of health, universities, and private citizens vying with each other in the establishment of laboratories for the study of these minute organisms endowed with so much power for good or evil.

In a sense, these two paragraphs must have been, on Smith's part, somewhat autobiographical. They must have represented his memory of the evolution of bacteriological science in America and throughout the world. Pasteur's early studies on crystallography find little place except indirectly through the great one's mastery of experimental technique. Dr. Christian A. Herter,¹⁸⁰ at the opening of the medical department of Johns Hopkins University in 1903, appraised in a valuable address "The Influence of Pasteur on Medical Science,"¹⁸¹ and, among other points, estimated that the "crystallographic researches were the bridge over which the far-seeing investigator passed on the way to lay the foundations of a new biological science, a science which has effected a veritable revolution in our conceptions of medical problems." From these studies Pasteur went on to his study of the micro-organic life involved in various kinds of fermentation. Pasteur's saving of the silkworm industry in France ranks as a great scientific and economic feat. Against one disease which was killing silkworms—the pébrine or corpuscle disease caused by the psorosperm *Nosema bombycis*—he devised a method of breeding new and healthy silkworms by carefully selecting eggs shown by the microscope to be

¹⁸⁰ Concerning Herter, see, Dr. J. C. Hemmeter, *Master minds in medicine*, 346-348, N. Y., Medical Life Press, 1927.

¹⁸¹ *Op. cit.*, 326-327.

free from infection.¹⁸² His study of the disease of the morts-flats, or flâcherié, took him into "the domain of pathology [and] led him to examine a host of new problems" which, Duclaux¹⁸³ indicated, "had clearly a reflex action on his later discoveries." To employ Dr. Herter's interpretation, Pasteur, by these researches, entered¹⁸⁴

the realm of animal pathology, and . . . thus the vestibule of modern medicine. For it is true that the laws governing the propagation and development of the flâcherie disease have the most striking analogies to those of the diseases of man. The varying susceptibilities of different individuals to the same microorganisms, the influence of the path of infection and the fact that flâcherie organisms acquire increased virulence after passage through the bodies of living silkworms foreshadow discoveries in human pathology. The two volumes dealing with the diseases of silkworms and dated 1870, are works whose contents should be familiar to every independent student of the infectious diseases.

Pasteur's researches were not without other practical and applied value. Notably from his study of acetic acid fermentation came his recommendations to the wine industry of methods to prevent the spoiling of wine. Valuable scientific information was also supplied vinegar manufacturers. But for our purposes, his contributions to pure science are of more moment. Considering the subject of the mediation of living organisms in fermentation, Dr. Herter stated:¹⁸⁵

[Pasteur's] first notable paper in the long series which solved one of the most pressing questions in biology deals with lactic acid fermentation. . . . This research ended, as is well known, in the discovery of a specific lactic acid organism or ferment, and in the cultivation of this and other organisms in an artificial medium free from albuminoids. Pasteur was not slow in forming the hypothesis that different types of fermentation are dependent on different types of microorganisms and this idea of specificity soon established in relation to the ordinary decompositions ultimately became the basis of our modern knowledge of the infectious diseases.

The research on lactic acid fermentation thus gave the *coup de grâce* to the chemical theory of fermentation at the same time it marked the birth of the promising science of bacteriology. The development of a method designed to secure pure cultures from fluid media, the use of culture media

¹⁸² *Idem*, 329.

¹⁸³ *Pasteur, The history of a mind, op. cit.*, 179 f.

¹⁸⁴ The influence of Pasteur on medical science, *op. cit.*, 329.

¹⁸⁵ *Op. cit.*, 328.

of known composition and the careful chemical study of products of decomposition all belong to this early period of Pasteur's life and were achievements of the deepest significance for the future of the great department of knowledge which has revived the biological sciences.

Another research on fermentation deserves more than passing notice on account of the extraordinary discovery which appears as its almost accidental by-product. This is the investigation on butyric acid ferments (1861). This research brought to light the fact that there are motile organisms capable of inducing a decomposition of sugar with the production of butyric acid. In the course of this research Pasteur saw that these organisms (whose motility was most puzzling on account of its suggesting animal life), behaved very differently according to their position with reference to the cover-glass, those at the center being active, while those at the periphery and exposed to the air were checked in their movements. From this casual observation came the fundamental conception of anaerobic life.

Smith's text-book reading was mostly on botany. But by 1884 he was keeping informed of the latest in bacteriology. In a letter to his Swedish correspondent, G. Hyltén-Cavallius, he told him how "extremely interesting" he was finding Julius von Sachs's *Vorlesungen über Pflanzen-Physiologie*, and added: "In this country we are now greatly interested in the study of the bacteria. All foreign publications are speedily put before our people by the journals, and some original work is also being done." It was this year that Smith made available to the Michigan State Board of Health a digest or report—said to have consisted of some 180 pages¹⁵⁶—of European and American publications on sanitation, hygiene, pathology, and bacteriology. Since September 1883, he had been supplying to *The Sanitary News* of Chicago translations and notes culled from foreign literature. Within a few months at least four translations, articles, or notes had been published: "The Sanitary Improvement of Paris";¹⁵⁷ "The Sanitary Condition of Damietta (Egypt) and the Cholera";¹⁵⁸ "Sanitary Inspection of

¹⁵⁶ L. R. Jones, Biographical memoir of Erwin Frink Smith 1854-1927, *Nat. Acad. Sci. Mem.* 21: 4, first memoir. The author has not been able to find where this was published, if it was published at all. He, however, has been informed by Dr. Frederick V. Rand, who prepared the bibliography and part of the subject matter of this memoir that Dr. Jones's source of information probably was Dr. Vaughan.

¹⁵⁷ *Sanitary News* 2 (23): 129-130, Oct. 1, 1883.

¹⁵⁸ *Idem*, 2 (24): 137, Oct. 15, 1883. See also, Erwin F. Smith, The influence of sewerage and water-supply on the death rate in cities, supplement to the *Annual Report of the Michigan State Board of Health* for the year 1885, and reprint, pp.

Lodging Houses in Paris";¹⁸⁹ "Paris Water Supply."¹⁹⁰ Other "foreign gleanings," as, for examples, on "Cholera in Egypt," "Cholera in Calcutta," and "Preventable Disease in Ireland," were prepared and submitted by him. These show that his sources were official health officer reports, the *Journal d'Hygiène* of Paris, hygienic society bulletins, and government publications. In October, 1883, he purchased Littré's *Dictionnaire de la Langue Française*, a five volume work of about 5,000 closely printed pages, and he had decided to devote the year 1884 to the reading of German, the while continuing his reading of French both by way of scientific publications and a study of Pierre Étienne Duchartre's *Éléments de Botanique*.

Smith found learning the German language "a very great task." "It will take me a year or two more at the very least," he told his mother in a letter of March 15, 1884, "and I have been working faithfully for fully as long as this. I now recite, or rather read, once a week to a German minister, who gives me excellent instruction." And in this letter he expressed his happiness in his work.

The more I learn of public health work, the more cordially do I find myself in sympathy with it. There is no question but that this Board in the past ten years of its existence has saved this State in sickness and death, by its active efforts in the prevention of the spread of communicable diseases alone, a hundred times more than all it has cost. I had the same impression before I entered this Office, but did not then have as great familiarity with the facts as I now have.

In a letter to Professor Volney Morgan Spalding of the University of Michigan, he disclosed he was reading Goethe's "Faust" with much pleasure but planned to be soon at his botanical studies again in real earnestness. Liberty Hyde Bailey, junior, was then an assistant at the Gray Herbarium of Harvard University. He was assistant curator of the herbarium, in entire charge of the nomenclature of the botanic gardens and greenhouses and of the students' and garden herbaria, and assistant in physiological experiments. Smith's letter of April 18 to him asked how *Das Botanische Centralblatt* compared

28-29, where a copy of Smith's translation, Cholera in Damietta, has been set forth almost in full.

¹⁸⁹ *Sanitary News* 3 (26): 22, Nov. 15, 1883.

¹⁹⁰ *Idem*, 23.

with other German botanical journals? I think I wrote you about buying Sachs' "Vorlesungen über Pflanzen Physiologie." It is a great book in more senses than one. I think I shall send for deBary's "Vergleichende Anatomie der Vegetationsorgane der Phanerogamen und Farne," and [Karl von] Goebel's "Grundzüge der Systematik und speciellen Pflanzenphysiologie." I want both very much. I am half through the first volume of the third edition of Duchartre's "Éléments." The style of the book pleases me. It is more interesting than a novel.

The previous year Bailey had written that he was enjoying the "privilege of attending Dr. Goodale's lectures" at Harvard. By August 1884 Smith was reading Sachs's *Vorlesungen über Pflanzen-Physiologie* and comparing its subject matter with the last edition of the great German botanist's *Lehrbuch der Botanik*. He found that the author had "changed views considerably." This he said in a letter to G. Hyllén-Cavallius of Lund, Sweden, and since plant specimens were sent to this correspondent through the Gray Herbarium, another exchange of letters with Bailey took place. For some time he had been urging Smith to spend a year in study at Harvard. Smith had confessed he would "be only too glad to study at Cambridge," but it would not be possible this year. Someday, he promised, he would leave the "tread-mill" and "drudgery" of much of his work, and go to college, but till such time as he could afford it, he would have to be content.

Late in August 1884 a Reverend Mr. Rork, who was moving his school from Sherwood, Michigan, to Lansing, asked Smith to teach classes in botany and French. By mid-October, instead of teaching botany and French, Smith had a class of thirty-two young men and women in physiology and hygiene. This work, also, was in addition to his regular duties with the Board of Health. Jerome Walker's *Anatomy, Physiology, and Hygiene* was the text used and he recommended it to readers of the *Michigan School Moderator*.¹⁹¹ Smith believed Martin's *Human Body* (Henry Holt and Co., New York) "the best Physiology yet approved" by school boards, but Walker's book was "one of the best" and published that year. Smith's teaching, a "labor one of love," he told Charles F. Wheeler, took place each day after dinner and lasted forty minutes. "I use a text," he wrote his mother December 14, 1884,

But we have dissections in the class room (cats, etc.) and I get as much material for study as is convenient from the butcher shops. I also make

¹⁹¹ 5 (20): 384, Jan. 29, 1885.

good use of the microscope, get the pupils to do as much outside reading as possible, and when we are done with each subject I require a written summary of it, made without use of the textbook. Most of my pupils have become very much interested in the study. We shall continue it about three months longer.

Early in March Smith had examined and shown to Dr. Baker of the Michigan health board *trichinae* in "a small piece of muscle taken from" the body of a boy who had "died of trichinosis, caused by eating uncooked pork . . ." Material for the examination had been sent to the health board by a doctor, and, in part, Smith's memorandum read:

Use[d] Bausch & Lomb's Physician's Microscope C eyepiece and $\frac{3}{4}$ inch objective. Studied the sections about 2 hours. Never before saw any meat so literally filled with the parasites. Counted 16 of the worms in one field, 20 in another and 30 in another, seemed to be more abundant in some parts than in others. In only a few cases did I detect any well defined *cysts*. The worms were among the muscular fibers doubled and coiled into various shapes. I also re-examined some of the sow's flesh sent. . . . Found usually two or three encysted trichinae to a field. Only one hog was diseased in this way. . . .

During the summer of 1884 he edited, and supervised the publishing of, the proceedings of the Ionia state sanitary convention. In the spring a sanitary convention had been also held at Hillsdale; and by summer its proceedings were being printed. The Board's annual report for the year 1883 was soon to appear, and to contain "a compilation of all the laws relating to Public Health in Michigan, for the use of local boards of health. You see," Erwin wrote to his mother, "there is no end to work in this Office. I am so busy, as a rule, that I do not mind the flight of time." In 1884 Secretary Baker reported Michigan to have between three and four thousand physicians and fourteen hundred health officers.¹⁹² Smith gathered and put together not only data concerning their education and practice but also names and addresses of secretaries of state boards of health, twenty-seven in number, and the names and addresses of state medical societies, forty two in all, including Canadian references. Moreover, he sent "fre-

¹⁹² *Sanitary News* 4 (37): 11, May 1, 1884. See also *San. News* 3 (29): 67, concerning Ionia convention; concerning Hillsdale convention, p. 10. Data as to Smith's work taken from his correspondence.

quent and valuable contributions . . . upon various scientific subjects"¹⁰³ to *The Sanitary News*.

In 1884 he served in Lansing as secretary of the Citizens' Law and Order League. Since 1882 he had been connected with the United States Weather Bureau there. For one year he was the weather observer, and had "continued [his] readings in that department." He was "familiar with all the details" of the work and "with the general plan of work of the U. S. Signal Service,"¹⁰⁴ and he edited the bureau's meteorological data for the year 1883, a task which did not reach the printer until 1885.

He was saving his money to enter the University of Michigan the following September. "By that time," he told his mother, "I shall have a pretty full knowledge of German, and some additional knowledge of Botany,—all of which will be useful." He had kept Professor Spalding advised on his studies. One of the reasons he gave why he liked Duchartre's *Éléments de Botanique* was the fullness and usefulness of the foot-note references to "continental authorities." As he read, he said, "I am more than ever surprised at the amount of original research done by the Germans. The authority for seemingly three-fourths of the newer statements is some German paper or monograph."

In August 1884, at an eastern publisher's request, he criticised a book on physiology and hygiene. In four pages of criticism, Smith cited conclusions by Galton, Denton, Huxley, Hartley, Miquel, Parke, Buck, von Pettenkofer, Koch, and others. Prominent among his points was the subject of disinfectants and germicides. But that sanitary science had not won him away completely from botany was evident throughout the year. When others urged that he could make more money from law, medicine, real estate, or business pursuits, Smith unvaryingly replied that already he had "chosen, and given a good many thousand hours of hard study to the preliminaries" of natural history, and he was "not inclined to change [his] plans."

January 26, 1885, he began a study which was to last some two years, and more. It was to be an economic, and not scientific, survey of the influence of civic cleanliness in lowering the death

¹⁰³ Letter from editors G. P. Brown and John K. Allen, May 13, 1886 concerning his work for *Sanitary News*.

¹⁰⁴ Letter, Smith to Prof. M. W. Harrington, Detroit Observatory, Ann Arbor, June 14, 1885 concerning work with the weather bureau for past three years.

rates of cities. Primarily it was to focus on the availability of adequate sewerage and pure water supply in reducing the annual number of deaths from several epidemic diseases, principally typhoid fever and cholera, and on the theory that diphtheria is a contagious disease transmissible from person to person and from place to place, like smallpox and scarlet fever, statistics were gathered with regard to it and other diseases. Smith addressed a letter to Dr. Max von Pettenkofer of Munich which read:

Most esteemed Sir:—In studying the reduced death-rate from typhoid fever and similar diseases, resulting from improved sanitary measures in cities I have been much struck with the Munich statistics quoted from you by Dr. Douglas Galton in Transactions of the Sanitary Institute of Great Britain, Vol. IV., 1883. It is stated that enteric fever decreased in Munich, with increase of sewerage, from a death-rate of 24.2 per 1,000,000 inhabitants for the quinquennium 1854 to 1859 to one of only 8.7 for the quinquennium 1875 to 1880. If it is not asking too much of you, will you have the kindness to inform me *what has been the average* enteric fever mortality in Munich for the four years 1881 to 1884—the period since the Vienna Congress.

Smith had kept watch of the international congresses. Early in 1884 he had offered to *Sanitary News* a summary of a translated "interesting communication in French relative to the International Sanitary Congress to be held at the Hague," the Netherlands.

Dr. von Pettenkofer must have responded to his letter, and either referred him to the source of publication or sent a reprint of an article by him, which Smith translated and had published in the *Sanitary News* of September 26, 1885, under the title, "Does Hygiene Pay?" There it was argued, and with statistics supporting the conclusion that "Hygiene is health-economy as national economy is the science of economics applied to other possessions."

Among many other scholars, health officers, bureaus, societies, and municipal, state, and national governmental agencies, Smith wrote to Dr. John S. Billings of the Surgeon General's Office at Washington. His requests ranged all the way from loans of literature to gathered statistical information. From leading cities of Germany, France, Italy, Austria, England, Russia, Mexico, and the United States, information and data were obtained, and by March 19-20, 1885, Smith was presenting a preliminary discussion labelled, "Sewerage and Water Supply,"¹⁹⁵ at a sanitary conven-

¹⁹⁵ *Annual Report of the Michigan State Board of Health*, suppl., 104-106, 1885.

tion held at Lansing, in the course of which he argued that soil cleanliness, pure water supply, and adequate sewage disposal by sewerage and proper drainage deter and reduce mortalities due to especially cholera and typhoid fever. Curiously, not one reference to Koch's proof of the comma bacillus of Asiatic cholera was made. The principal presentation, entitled "The Influence of Sewerage and Water-Supply on the Death Rate in Cities,"¹⁹⁰ occurred at Ypsilanti in July of that year. Neither at this sanitary convention, in the main discourse, did Smith attempt to discuss the etiology of any one of the diseases, although as to each some attention was paid to the point.

Sanitary News in 1883 published a preliminary announcement of Koch's discovery of the cholera bacillus, in 1884 a full announcement, and in 1885 a summary. Smith had access to these and the foreign literature. By this time, he probably had read Koch's original reports. Part of Dr. Baker's recommendation of Smith for future employment was based on the fact that he read and wrote "French easily, and [was] a fair student in German. He is a fine student in the natural sciences, having in Botany more than a State reputation," also said Baker. "[W]hile here, he read a great deal on sanitary science, and wrote a paper on 'The Influence of Sewerage and Water-Supply on the Death-Rate in Cities.'" This would convince a reader of "Mr. Smith's carefulness and true scientific spirit. I consider the paper a valuable contribution to sanitary science. . . . I know of no young man who gives so large promise of valuable work in the field of science as Mr. Smith." As late as 1894 Dr. G. L. Magruder, dean of the medical department at Georgetown University, wrote Smith that he regarded it as a "most valuable paper." Further on, we shall see that this paper became one of the reasons why Dr. William H. Welch of Johns Hopkins Medical School interested himself in Smith and his studies in plant bacteriology. His paper was typical of the sanitary science of that day. A need was evident for more and better vital statistics on which to found study. Development of the science was "the soundest social and financial economy"; and it was "the coming science . . . most recently developed . . . because," wrote Smith, "it is based upon all the others. Mathematics, physics, chemistry, biology, statistics are all tributary to it.

¹⁹⁰ Reprint no. 243, *op. cit.*, 84 pp.

What has been done is but a tithe of the beneficent work which can be done and will be done in the years to come." He prophesied that "some of us will undoubtedly live to see chairs of sanitary science established in all our universities, and the fundamental principles of the science taught in all the common schools." His paper was not a study in the medical aspects of the subject but a proof of the value and importance of accurate statistics on birth, deaths, and movements of populations and how death-rates in cities were lowered by proper sewerage and pure water-supply.

Smith, as early as December, 1884, may have discussed with Dr. Spalding his studying plant diseases when he should have accumulated sufficient funds to finance a year or more at the University of Michigan. During that month he had journeyed to Ann Arbor, had "a long and friendly chat" with the University's "acting professor of botany," and their conference probably was about more than taxonomy and the Michigan flora. It must have included plant morphology and plant physiology, and something of plant pathology. For, that summer Smith had purchased, and surely was reading, DeBary's *Vergleichende Anatomie der Vegetationsorgane der Phanerogamen und Farne* (1877); and, immediately upon his return to Lansing, he either looked up or verified, and sent to Spalding, the title of DeBary's "new book," *Vergleichende Morphologie und Biologie der Pilze, Mycetozoen und Bacterien* (1884). Smith had been acquiring several "German botanies to mouse on," he told friends, but the fact was he was building, or adding to, his botanical library. Karl von Goebel's *Grundzüge der Systematik und speciellen Pflanzenmorphologie* (1882) had also been received, along with four volumes of Charles Darwin's works and Lesquereux and James's *Manual of North American Mosses* published that year. Since for many years the following were in Smith's library, the Darwin works must have been: (1) *The Power of Movement in Plants* (1883); (2) *Insectivorous Plants* (1883); (3) *The Various Contrivances by which Orchids are Fertilized by Insects* (1884); and (4) *The Movements and Habits of Climbing Plants* (1883). The last two were second editions. Sachs's *Geschichte der Botanik* (1875) also had been ordered during the summer and was later in Smith's library for many years. At least twice he had tried to acquire a copy of the fourth edition of Sachs's *Lehrbuch der Botanik* and once the second part of Duchartre's *Éléments de Botanique*.

Wolf's *Naturwissenschaftlich-mathematisches Vademecum* had been ordered early in the spring and during the summer the four-volume dictionary, M. H. Baillon's *Le Dictionnaire de Botanique*, may have been added. It was ordered but, since Smith asked Bailey at Harvard to consult with Dr. Gray with regard to the set's merit for his purposes, he may have countermanded the order, awaiting word from Bailey. In later years Smith possessed an eleven-volume edition of this work.

While in Ann Arbor, Smith had dinner with Dr. J. B. Steere, professor of zoology at the university who had contributed to the science department of *Michigan School Moderator*¹⁹⁷ two articles, "Lessons in Zoology," one on birds and one on reptiles. Dr. Steere thought highly of Smith and later recommended him as "an indefatigable student in Botany," whose published Michigan flora was "the most valuable and complete work in existence." "I consider him excellently fitted," wrote Dr. Steere, "both by training and by natural tastes and talents for Scientific teaching." Smith also called on Dr. Victor C. Vaughan, then an assistant professor and soon to be full professor of hygiene and physiological chemistry in the university's medical department. While on this visit, he was probably told by these leaders of the faculty of science that, conditioned upon his passing each of the requisite examinations and with one year of resident study, he would be granted a degree. In the next chapter will be described more fully the circumstances of Smith's enrollment, but that the arrangement was perhaps unparalleled was later indicated by Professor Spalding, who commented that accrediting Smith "with the work of three years [was] a thing that [he did] not remember to have occurred in any similar case since [he had] been" with the university. Obviously, the arrangement was a tribute to Smith's workmanship and thoroughness of preparation. Dr. Vaughan believed in Smith's superior worth and erudition in sanitary science. By June 5 of the following year, Smith had in possession a terse letter from him, which read, "Dear Mr. Smith, I can pass you on the course in Sanitary Science."

Erwin, however, was not to specialize in sanitation and hygiene,

¹⁹⁷ 3 (27): 473, March 22, 1883; also, the June 7 issue of the same volume and year. Smith would have accepted more of these articles for publication, but the next was submitted after he had resigned from this position and was giving his spare time to *Sanitary News*.

nor in pathology and bacteriology, but in the natural sciences, and particularly botany. When he consulted Dr. Vaughan, he may still have been contemplating the medical profession. To prepare for the practice of human medicine or surgery, however, would probably have required more than one year of resident study at the university. Dr. Spalding may have turned Smith to the idea of studying parasitic fungi, or Erwin evolved his plan from the abundant preparatory reading which he pursued the summer previous to his entrance to the university. July 9, 1885, in any event, he tendered to Dr. Baker his resignation from his position with the Michigan State Board of Health. It was to take effect on July 21, and Smith felt constrained to say: "I am led to do this because I need the time for proper preparation for my University work next year." He thanked the secretary for his courtesy to him during his three years of service, and closed his letter by reiterating his "lively interest in Public Health work in general" and especially such work as was being done in Michigan.

A week before his resignation became effective, he ordered DeBary's *Vergleichende Morphologie und Biologie der Pilze, Mycetozen und Bakterien* (1884), Dr. W. Zopf's *Die Spaltpilze* (1885), Dr. E. Klein's *Microorganisms and Disease* (1885), and W. B. Grove's *A Synopsis of the Bacteria and Yeast Fungi* (1884). Early in February he had sent for Eduard Strasburger's *Das Botanische Practicum* (1884) and on July 21, 1885, he ordered Dr. Ferdinand Hueppe's *Die Methoden Bakterien-Forschung* (1885). Hueppe, a Prussian army surgeon, had done important work on fermentation (1883), had begun to study the bacteriology of milk (1884), and, besides being a principal collaborator with Koch, had prepared this "well-known manual on bacteriological methods (1885)."¹⁹⁸ During the spring Smith had examined Dr. Germain Sée's *De la Phtisie Bacillaire des Poumons*, a work published in Paris in 1884, and had written to Dr. William Oldright of Toronto that he had found it "very interesting, especially some chapters." It seems apparent that Smith, while preparing for the University, read bacteriology as much as botany, and that he himself, or with Dr. Spalding's help, began to realize he might combine these two interests by studying plant pathology. That summer he became convinced that bacteria cause diseases of

¹⁹⁸ F. H. Garrison, *Introduction to the history of medicine, op. cit.*, 582.

plants as well as of animals including man. In August, at Ann Arbor, occurred the thirty-fourth meeting of the American Association for the Advancement of Science and the sixth meeting of the Society for the Promotion of Agricultural Science. Smith either attended these meetings or learned, through reading, of certain research proofs made by Dr. J. C. Arthur at the New York Agricultural Experiment Station at Geneva to the effect that a plant disease known as pear blight is caused by a bacterium.

In October of that year was published by the *Michigan Horticulturist*,¹⁹⁹ edited by C. W. Garfield, an article by "Erwin F. Smith, Lansing" entitled "Recent Literature concerning Pear Blight." This began: "Nothing has interested me more as connected with the subject of horticulture recently than the discussion upon pear blight," and reference was made to a "valuable contribution" by Dr. Arthur at the Ann Arbor meeting from which Smith "gathered facts that may be of interest to the readers of the *Michigan Horticulturist*." Arthur had confirmed Burrill's experiments and "conclusively proved" that a micrococcus is "the real cause of the blight" and not merely a "concomitant" of the disease. Burrill's and Arthur's studies on pear blight will be compared more fully in Chapter III. Suffice it to say now that Smith appeared to believe himself sufficiently acquainted with the laboratory procedures of bacteriologists to be convinced that Dr. Arthur, making use of culture methods similar to those used by DeBary and an improvement on the inoculation experiments used by Professor Burrill, had proved the bacterial origin of pear blight "beyond doubt." Smith's article reads as if to warrant this conclusion, and what he later said of the work accomplished on pear blight by each of these scientists will be set forth in the next chapter.

Some other work which Smith had done was preparing him for the university. May 30, 1885, he had forwarded to John Merle Coulter, editor of the *Botanical Gazette*, "a translation of a very recent and somewhat interesting note on the continuity of vegetable protoplasm," which became, when published by the *Gazette*,²⁰⁰ a leading article entitled, "On the perforation of cells

¹⁹⁹ 1: (2), 34-36.

²⁰⁰ 10(8): 322-324, August 1885. See also, E. F. Smith, Trimorphism in *LitospERMUM canescens*, *Bot. Gaz.* 4(4): 168, 1879.

and the continuity of protoplasm in vegetables." Prepared by M. L. Olivier, it had been presented to the French Academy of Sciences by Monsieur P. E. Duchartre, professor at the Institut Agronomique of Versailles, to whom in 1884 Smith had sent a copy of the *Flora of Michigan*. Sachs had been among the first to insist on the continuity of protoplasm throughout the vegetable organism.²⁰¹ Probably Smith acquired his original interest in this fundamental conception for investigations in physiology from some writing by Sachs. But, whether or not, the new study was based on examinations of living plant tissues and utilized modern techniques of photography, stained sections, and microscopic analysis, and was proof of an interest in plant physiology in addition to plant pathology and botanical taxonomy.

Coulter the next year was to say of Smith: "He has my unqualified recommendation for any position which has to do with Natural History . . . He will bring to it the strength of modern thought and methods and a large acquaintance with the leading workers in the department." Similarly positive would be another botanist's opinion as to Smith's capability. Charles Reid Barnes was a professor at Purdue University and he, Arthur, and Coulter were co-editors of the *Botanical Gazette*. Each was now interested in "physiological botany." By 1885 Arthur had finished translating Anton de Bary's "article on bacteria" from his recent *Vergleichende Morphologie und Biologie der Pilze, Mycetozen, und Bacterien*, a translation which he evidently never published. Smith did not begin to read this book by DeBary until July 7, 1886, but, when he did, he determined to "master the details" of it.²⁰² That his interest in human physiology, as a teaching subject, had lasted was shown by brief descriptive reviews of textbooks in the *Michigan School Moderator*,²⁰³ and by his promise of March, 1885, to Commissioner of Education John Eaton of Washington to complete, as requested, an article on "Physiology and Hygiene in our Public Schools." The study of plants, and of plant physiology and pathology, was now, however, his main absorbing interest; and, with this enthusiasm in mind, he made ready to enter the university.

²⁰¹ William A. Locy, *The growth of biology*, 408-409, N. Y., Henry Holt and Co., 1925.

²⁰² Fifty years of pathology, *op. cit.*, 19.

²⁰³ 5 (20): 384, Jan. 29, 1885; 5 (39): 775, June 18, 1885; etc.

CHAPTER III

EARLY WORK ON PLANT PATHOLOGY AND BACTERIOLOGY IN NORTH AMERICA. STUDY UNDER PROFESSOR SPALDING AT THE UNIVERSITY OF MICHIGAN

IN 1848 the American Association for the Advancement of Science had been organized, and by the 1880's was the oldest general, national scientific society on the North American continent. The Association of American Geologists and Naturalists in 1842 had effected a national organization from several attempts at forming societies devoted to various branches of science. Academies of science had flourished for many years in the larger cities, notably New York, Philadelphia, Boston, and St. Louis. But, during the middle years of the century, even before the National Academy of Sciences was organized at Washington, the American Association for the Advancement of Science had become the federated national unit, created to serve the interests of the two major divisions, *natural philosophy* or the physical sciences, and *natural history*, the biological sciences as then known.¹ Under its mantle, numerous other scientific societies would become associated and affiliated, and the Association itself would be organized in sections. Not until 1892 would the science of botany have its own section G, although while yet it was combined with zoology under section F botanists of North America established a botanical club. Dr. Beal, presiding at the Minneapolis meeting in 1883 of the Association's section of biology, made the theme of his presidential address the relation of agriculture to science, and Dr. E. Lewis Sturtevant spoke on "Agricultural Botany." The American Pomological Society, organized in 1848, was the most influential organization in the American fields of agriculture and horticulture. Its members had been principally responsible for organizing in 1880 the Society for the Promotion of Agricultural Science. The American Medical Association had been organized two years before the Pomological Society, at New York City in May 1846. Its membership including delegates from the

¹ *Science*, 463, 478, Nov. 14, 1947.

state medical societies, this organization had held its first convention the next year at Philadelphia.

Sometime in the early 1880's Charles Edwin Bessey, while yet at Iowa Agricultural College, had been shown a rough draft of proposed legislation to be presented to Congress to "establish National Experiment Stations."² Bessey, asked to define the purposes of such stations, shaped at once a clause to include, among their leading objectives, "original researches on the physiology of plants and animals and the diseases to which they are severally subject with the remedies for the same." In 1880 agricultural experiment stations were organized in only four states: Connecticut under Dr. Samuel William Johnson; California under Dr. Eugene Woldemar Hilgard; North Carolina under Dr. Charles William Dabney; and New Jersey under Dr. George H. Cook. At some universities, notably Cornell and Harvard, experimental work in agriculture was in progress. At Cornell University, station work had been maintained since 1879 as an adjunct of a department of agriculture. At Harvard University, as early as 1870 the President and Fellows had commenced to organize a school of agriculture and horticulture pursuant to the provisions of the will of Benjamin Bussey, and a grant from the trustees of the Massachusetts Horticultural Society "for the support of a laboratory and for experiments in agricultural chemistry to be conducted on the Bussey estate."³ In the last week of the year 1871, the laboratory of the Bussey Institution directed by Professor F. H. Storer, professor of agricultural chemistry, had become available. Research both there and at Sheffield Scientific School of Yale stressed agricultural chemistry, the field of investigation in which S. W. Johnson also was a leading American authority.

About 1863 the Connecticut legislature, by appropriating funds from the sale of land-grant scrip under the Morrill Act of 1862, had made possible the creation of chairs of agriculture at Sheffield. Since 1846, when John P. Norton had been appointed to a professorship, the instruction there had included some work in agricultural chemistry and vegetable and animal physiology. About

² "The Development of Plant Pathology in the University of Nebraska," address delivered by Bessey at the University October 24, 1910.

³ A. C. True, Origin and development of agricultural experiment stations in the United States, *Report of the Commissioner of Agriculture for the year 1888, ibid.*: 541-558, at pp. 541-542.

1865 William Henry Brewer had accepted a professorship in agriculture. He was a botanist, an authority on farm animals and animal breeding. But he was not a student of plant diseases. Nor was Johnson who was the author of the two small but important books, *How crops feed*, a treatise on the atmosphere and soil as related to the nutrition of agricultural plants, and *How crops grow*, a treatise on chemical composition, structure, and life of the plant for all students of agriculture. Nor did the creation in 1875 of an agricultural experiment station in Connecticut, first at Middletown and two years later on a land-grant basis at New Haven, bring about immediately in that state a definite program of research in diseases of plants.

At the United States Department of Agriculture, William Saunders, superintendent of gardens and grounds, had written as early as 1876⁴ on *Phylloxera vastatrix*, the "grape-root louse." In the Department reports appeared numerous miscellaneous mentions of crop diseases prevalent or threatening in this country. But there was no well defined research program for their study. In January 1869, at an agricultural conference in Illinois, T. J. Burrill had constructively suggested that a "new field of labor" was "opening before the student of botany, that of the vegetable diseases. Perhaps," he said, "nothing pertaining to plants is so little understood"; and he prophesied that with "the increased facilities of later years for microscopic observations" more attention would be given to the subject. His work, and that of a few other men, gave the work, as we shall see, its start in America.

William Gilson Farlow⁵ in 1866 had received his bachelor of arts degree from Harvard College and in 1870 a degree of doctor of medicine from the university medical school. Before entering the medical school, he had studied anatomy under Dr. Jeffries Wyman; and before graduating he had been honored by an appointment as a surgical interne at the Massachusetts General Hospital under Dr. H. J. Bigelow. He had chosen to study medicine because, as he later told Smith, Dr. Gray had advised him to do so,

⁴ *Rept. of the Comm. of Agric.*, 70-73. Washington, Govt. Print. Office, 1877.

⁵ William Albert Setchell, William Gilson Farlow 1844-1919, *Nat'l Acad. of Sciences, Biographical Memoirs* 21 (4), 22 pp. See also, Anniversary Number Commemorating the 100th Birthday of William Gilson Farlow 1844-1944, *Farlowia, A Journal of Cryptogamic Botany* 2 (1): 1 ff., Jan. 1945.

since that was the only way of getting into any part of a natural history career. . . . When I began to study, [he said], there were few persons who pretended to study botany at all but most of them were well trained as far as training went in those days. There were few inducements to study botany beyond the natural desire of those naturally interested in the subject. . . . The death of Horace Mann Jr. just as I took my M. D. made it possible for me to begin as an assistant in botany.

Farlow secured his appointment at the Gray Herbarium soon after graduating from the medical school and continued in that position for two years. As an undergraduate he had been taught by Gray in the regular required sophomore course; and in his junior year he had been one of several students who requested special advanced instruction in systematic botany. During his senior year he had been one of two voluntary workers at the Gray Herbarium.⁶ He doubtless acquired from Gray an extensive knowledge of the flowering plants and vascular cryptogams, including marine algae of which there was a considerable collection at the Herbarium. His summer of 1871 was spent at Woods Hole, Massachusetts, with a corps of naturalists under Spencer F. Baird of the United States Fish Commission. Gray, overburdened with the work of his and John Torrey's *Flora of North America*, planned to make Farlow the curator of the Herbarium, and Farlow did some work arranging the thallophytes of the Herbarium. But in June 1872 he, as America's first real botanical student to seek advanced study under DeBary, began a tour of England and continental Europe which during two years would include travels in the Scandinavian countries, Russia, Switzerland, France, and Germany. At DeBary's laboratory at Strassburg he received instruction in "the management of cell cultures and the techniques required" in studies in mycology, and of the anatomy of vegetative organs of the flowering plants and ferns. Six years earlier DeBary had published his work on the morphology and physiology of the fungi, lichens, and myxomycetes, and he was now at work on his *Vergleichende Anatomie der Vegetationsorgane der Phanerogamen und Farne* (1877). While in his laboratory, Farlow studied and described the first known case of apogamy in ferns. He also read the 1870, or second German, edition of Sachs's *Lehrbuch der Botanik*.

⁶ W. G. Farlow, The change from the old to the new botany in the United States, *Science*, n. s. 37 (942): 82-83, Jan. 17, 1913.

At one time Farlow had thought of studying with Sachs at Würzburg. Gray, however, believed he would go to DeBary, and wrote Alphonse DeCandolle so. America's leading botanist had shown a steadily increasing interest in plant physiology and plant pathology. Both by writings and in his lectures to students during his last years before retiring from teaching, he had sought to stimulate interest in Europe's "Scientific Botany." For years he had placed confidence in men medically trained. He himself, his close collaborator Torrey, his two appointees Goodale and Farlow, and many of his botanical correspondents were doctors of medicine. George Engelmann of St. Louis, whose abilities as a botanist he esteemed second to none in America, was by profession a medical doctor. His inaugural dissertation at the University of Würzburg dealt chiefly with the monstrosities and aberrant forms of plants.⁷ These men knew the rudiments of anatomy, physiology, and pathology. While in this country study-methods of these subjects had not been widely extended from the animal side of biological investigation to that of plants, the beginners of modern plant science research (excluding taxonomy and an older morphology) were to borrow some investigation-techniques from zoologists especially as research in cytology and embryology would get fully under way;⁸ and, conversely, zoologists adapted some of the study methods of botanists to their inquiries. In Europe Eduard Strasburger was just coming on the botanical scene. DeBary's *Vergleichende Anatomie* was being prepared, partly to keep a promise made to Wilhelm Hofmeister. Farlow met many botanists of prominence, heard W. P. Schimper lecture on bryology, worked on algae at Antibes with the French phycologists G. Thuret and E. Bornet, studied lichens at Geneva, Switzerland, with the help of Johann Müller-Argoviensis; and very important were his consultations with J. G. Agardh, Elias Fries and his son, and other cryptogamists, as well as his work at various scientific academies where valuable collections of cryptogamia were.⁹

In June of 1873 Gray reported to DeCandolle:

⁷ Charles A. White, George Engelmann, *Natl. Acad. Sci. Biog. Mem.* 4: 1-21, 1902; *Hist. of first half-century of the Natl. Acad.*, 130, Washington, 1913.

⁸ W. G. Farlow, *op. cit.*, 83-84.

⁹ W. A. Setchell, *op. cit.*, 4f. See also *Amer. Naturalist* 3: 1, 112, 295. See also, Hilda F. Harris, The correspondence of William G. Farlow during his student days at Strasbourg, *Farlowia* 2 (1): 11 ff., 1945.

Professor Goodale, under appointment as assistant professor of vegetable physiology, will take the whole work of instruction in botany off my hands; but if a former assistant and pupil, Dr. Farlow, now with DeBary, proves capable of it, as I hope, he will, I trust, take up the work in systematic botany. His fancy, however, is for Cryptogamia.

Gray had written Farlow that what was "*most wanted*" in the United States was instruction and research in the cryptogamia, fungi especially. They arranged to acquire the late M. A. Curtis's collection of fungi; and Farlow, himself an enthusiastic student of the algae, co-worker with D. C. Eaton, S. T. Olney, Edward Tuckerman, and other American correspondents of Gray, was given on his return charge of cryptogamic botany at the Bussey Institution as an assistant professor of botany. The catalogue for the year 1874-1875 announced a course to be given by Farlow, "Vegetable anatomy particularly the microscopic study of wood. Rudiments of cryptogamic botany. Fungi, especially those injurious to vegetation. Special investigations on the diseases of plants will be pursued."¹⁰ Until 1879 when his gradually enlarged course of instruction at the Bussey Institution was transferred to the Cambridge campus of Harvard, Farlow gave instruction also at Cambridge two days a week and in the summer school. His laboratory for this teaching was in Lawrence Hall; and, continuing his study of the marine algae on which he had published before going abroad, he maintained an "improvised laboratory" for this purpose at Woods Hole. About 1875 laboratory practice, in addition to lecture attendance, became a requirement in the Natural History curriculum under Goodale and Farlow; and this applied in the course in Advanced Botany in which the study of cryptogamic botany was an elective. Farlow's laboratory and herbarium were moved several times: from Lawrence Scientific School to Boylston Hall and finally to several locations in the Museum of Comparative Zoology including the Agassiz addition and botanical quarters of the total building. Still later the Farlow Herbarium was established on Divinity Avenue in its own building.

By 1874 the botanical department at Harvard was said¹¹ to have "a thoroughly furnished laboratory, garden and greenhouse"

¹⁰ Ernst A. Bessey, The teaching of botany sixty-five years ago, *Symposia Commemorating Six Decades of the Modern Era in Botanical Sciences* 1: 1-3, reprinted from the *Iowa State Coll. Jour. Sci.* 9(2 and 3): 15-17, 1935.

¹¹ Ernst A. Bessey, quoting the university catalogue, *op. cit.*, 16.

with the largest library and herbarium in America. Before Farlow had gone to Europe, Gray may have strengthened in him an interest in plant reproduction among algae and fungi, since he lectured on these subjects in 1872 and perhaps before. DeBary, however, also a doctor of medicine, more than any other scholar probably, helped the young student to apply his knowledge of physiology, anatomy, and pathology to the study of plants and plant diseases; at least, DeBary quickened his zeal for research in the life histories of the lower organisms. W. A. Setchell, one of Farlow's biographers, believed that "since DeBary paid much attention to the parasitism and saprophytism of fungi and the reactions of host plants to their parasitic forms, we may readily infer that Farlow received much inspiration for the work he instituted on his return to America on phytopathology." His work at the Bussey Institution became "primarily directed toward the fungi of economic interest . . . [H]e laid there, firmly and efficiently, the foundations of what has come to be known as phytopathology."¹²

His accounts of asexual growth from the prothallus of *Pteris cretica* and *Pteris serrulata*, published¹³ partly here the year of his return, created notice. During the next decades no scholar of plant evolution among the lower orders was to write on this continent with more authority than he. Moreover, his articles which appeared in the first volumes (1876-1880) of the Bussey Institution's *Bulletin* won for him instantaneous acclaim and, from the point of view of the plant pathologist, have been regarded as "epoch making"¹⁴ since not only were life histories of disease-producing organisms shown but also from his descriptions of how host plants are attacked methods were suggested whereby to eradicate and prevent the causes of disease. In 1875 he published on the potato rot, and immediately Charles H. Peck urged, "We need more such papers. . . . Let me say by all means follow up this line of investigation."

In 1876 appeared Farlow's article, "On the American grape-

¹² *Op. cit.*, 4, 5.

¹³ *Bot. Zeit.* 32: 181; *Quar. Jour. Micr. Sci.* 2 (14): 266; *Proc. Amer. Acad. Arts and Sci.* 9: 68.

¹⁴ Beverly T. Galloway, Progress in the treatment of plant diseases in the United States, *Yearbook of the U. S. Dept. Agric.* for 1899: 191-200, at p. 193.

vine mildew,"¹⁵ a disease studied for many years as a part of investigations of downy and powdery mildews. DeBary discovered the germination of oospores of *Plasmopara* [*Peronospora*] *viticola*; and to Farlow's work has been traced discovery of the germination of the conidia of the fungus.¹⁶ Dr. Benjamin M. Duggar¹⁷ has classed this work of Farlow and the "incomparable studies of DeBary upon *Cystopus*, and other forms" as among the "models of mycological research." Research in plant pathology, Duggar said in 1910, "has gone but little farther." David Fairchild, writing¹⁸ of Farlow's first graduate student, Byron David Halsted, recollected that Halsted always was "proud of having been Dr. Farlow's first pupil in plant diseases." Fairchild believed that "of the pioneer work on *Plowrightia morbosa* [black knot of plum and cherry] which Dr. Farlow did, [Halsted] was . . . an enthusiastic witness." In 1876 was published,¹⁹ well illustrated, his report on the black knot, another disease which engaged his and other workers' attention for many years. This same year he reported "On a disease of olive and orange trees occurring in California in the spring of 1875."²⁰ Ten years later he was to present before the Society for the Promotion of Agricultural Science of which he became a member in 1880, "Notes on some injurious fungi of California."²¹ Some of his work was for the Massachusetts Board of Agriculture as well as the Bussey Institution. In 1877 he presented before the Board a paper on "Diseases of fruit-bearing trees"²² and that same year published in the Institution's *Bulletin*, "Notes on some common diseases caused by fungi."²³ The year before, he had published a "Synopsis of the Peronosporae of the United States,"²⁴ and the total number of species of *Peronospora* and *Cystopus* amounted to ten. In 1883,

¹⁵ *Bull. Bussey Inst.* 1: 415-425 (*Peronospora viticola*). Farlow's article on the potato rot appeared in the same volume, pp. 319-338.

¹⁶ *Phytopathology* 2 (6): 235, Dec. 1912.

¹⁷ *Phytopathology* 1 (3): 72, June 1911.

¹⁸ *Phytopathology* 9(1): 1-7, Jan. 1919. Halsted's thesis subject was "A classification and description of the American species of Characeae," *Bost. Soc. Nat. Hist. Proc.* 20: 169-190, March 1879 (W. A. Setchell, *op. cit.*, 6).

¹⁹ *Bull. Bussey Inst.* 1: 440-454.

²⁰ *Idem*, 404; *Jour. of Bot.* 14: 287.

²¹ *Proc. 6th Ann. Meet. Soc. Prom. Agric. Sci.*, 29-31, 1885.

²² *Rept. Mass. Board Agr.*, p. 218, 1878.

²³ *Bull. Bussey Inst.* 2: 106, 1877.

²⁴ *Bull. Bussey Inst.* 1: 426.

for the Society for the Promotion of Agricultural Science, was prepared an "Enumeration of the Peronosporae of the United States,"²⁵ in which he reviewed species of the genera in his collection, together with those added since 1876. Specimens were furnished him by botanists over the nation, many of whom—Arthur, Bessey, Halsted, H. W. Harkness, Peck, A. B. Seymour, Spalding, and William Trelease—were at one time or another his students or regular correspondents. To this study he appended an index of host plants of the species of Peronospora and Cystopus. The rusts became one of his specialties in plant disease study, and in 1878 he began to publish on these, presenting in the *Proceedings of the American Academy of Arts and Sciences*²⁶ a short paper "On the synonymy of some species of Uredineae."

During his lifetime, Farlow accumulated a vast fund of knowledge of lichens, mosses, algae, and fungi. In 1920 G. P. Clinton said²⁷ of this,

We have, or have had, botanists who surpassed him in knowledge concerning each of these groups, save the fungi, but none who equalled him in familiarity with them all. He was truly entitled Professor of Cryptogamic Botany. Just as surely as the old time Professor of Natural History has passed out of existence, so, to the mind of the writer, he represents the last of those who were able satisfactorily to cover the wide field of cryptogamic botany.

Farlow's knowledge of the fleshy fungi became second only to his knowledge of parasitic fungi, and he was an authority on algae.²⁸ He studied the deteriorating effect in water supply of certain lower algae and related organisms, and, his conclusions demonstrating importance on the matter of water purification, his reports to the Massachusetts Board of Health and the water board of the city of Boston were recognized as "notable contributions"²⁹ on the subject.

²⁵ *Proc. 4th Meet. Soc. Prom. Agric. Sci.* (combined with *Proc.*, 5th Meet.), 17-20.

²⁶ 13: 251.

²⁷ *Phytopathology* 10 (1): 1-8, Jan. 1920.

²⁸ W. A. Setchell, *op. cit.*, 13; Wm. Randolph Taylor, William Gilson Farlow, promotor of phycological research in America," *Farlowia* 2 (1): 53-70, Jan. 1945.

²⁹ *A pioneer of public health* W. T. Sedgwick, *op. cit.*, 60; W. A. Setchell, *op. cit.*, 5; W. G. Farlow *et al.*, Algae: in Report on a peculiar condition of the water supplied to the city of Boston 1875-76, *Rept. Cochituate Water Board, Boston* 10, 1876; W. G. Farlow, Remarks on some algae found in the water

The establishment in 1871 by the United States Fish Commission of an investigation base at Little Harbor, Woods Hole, not far distant from historic Penikese Island where Louis Agassiz had had his celebrated Anderson School of Natural History, was followed a few years later by the founding at Woods Hole of the Marine Biological Laboratory.³⁰ In 1878 Farlow made for the Fish Commission an important report "On the nature of the peculiar reddening of salted codfish during the summer season,"³¹ and within a few years his earlier reports on seaweed and marine algae were outdone in significance by a 210 page elaboration of "The marine algae of New England and adjacent coast."³² Of the seven original trustees of the Marine Biological Laboratory, Farlow was the only plant scientist; and largely through his efforts botany was early recognized as a main research subject.

His most far reaching contribution, nevertheless, was in introducing in this country careful culture practice and experiments to describe life histories and developmental cycles of organisms which cause plant diseases. His work of 1876 on "Onion smut"³³ was important because of its disclosure of the disease's fungous origin and, also, in that his work prepared the way for Roland Thaxter to establish its manner of infection and a suggested method of prevention. In 1880, as the first of the Anniversary Memoirs of the Boston Society of Natural History, Farlow began to publish on "The Gymnosporangia or cedar apples of the United States,"³⁴ a research of several years which dwelt on the relationships of *Roestelia* and *Gymnosporangium*. During the next decade and a half, some of the ablest talents of early American plant pathology—Thaxter, Halsted, Louis Hermann Pammel, and Fred Carlton Stewart, each a former student of Farlow—would work at the problem of establishing satisfactorily the connection between several species of *Gymnosporangium* and asso-

supplies of the city of Boston, *Bull. Bussey Inst.* 2 (1): 75; On some impurities of drinking water caused by vegetable growths, *Rept. Mass. Board of Health* etc. 1, suppl.: 131; In Rensen, 1., Report on a peculiar condition of the water, etc., City of Boston, Dec. 143: 15, 1881.

³⁰ Frank R. Lillie (and E. G. Conklin), *The Woods Hole Marine Biological Laboratory*, 24-26, 35, Univ. of Chicago Press, 1944.

³¹ *Rept. U. S. Fish Comm.* 1878: 969, 1880.

³² *Rept. U. S. Fish Comm.* 1879: 1-210, 1882. Separates printed in 1881.

³³ *Rept. Mass. Board Agric.* 1876, pt. 2: 164, 1877.

³⁴ *Ann. Mem. Bost. Soc. Nat. Hist.*, 1-38, 1880.

ciated *Roestelia* occurring in this country.³⁵ Professor Spalding has evaluated as of that time the value of Farlow's work on the black knot of plum and the gymnosporangium or cedar apples. He said:

Besides numerous other papers by Dr. W. G. Farlow³⁶ . . . that on the Black knot of the plum and cherry is one of the most complete and satisfactory. In this, for the first time, the complete history of the nature and development of this disease and the means of checking it were fully discussed. Still more important from a scientific as well as practical standpoint, are his later researches on the orchard rusts that in some portions of the country have proven highly destructive to apple trees. The fungi producing these rusts are now known to infest cedar trees during a portion of their cycle of development and to pass from these to apple trees; and it has also been shown that certain varieties, notably wild crab apples, are much more liable to infection than other kinds. From these facts, which it has taken the labor of many years to establish, the following preventive measures are indicated: 1. The cutting out of red cedars where they have been allowed to grow in the vicinity of apple orchards. 2. The destruction of wild crab apple trees that harbor the disease. 3. The selection of varieties for cultivation that are least susceptible to its attacks.

Thomas Jonathan Burrill, whom Gray recognized as early as 1872 as a cryptogamic botanist of extraordinary erudition, thanked Farlow in 1881 for his recently published "*Gymnosporangia* or cedar apples of the United States." He praised the work, saying:

Your résumé of the literature is of much interest and value while the critical examination and comparison of species is, to my mind, the best work of the kind so far produced in America. Not less interesting though less conclusive is the account of your culture experiments. I sincerely hope these may be repeated by yourself and others stimulated by your example.

Burrill³⁷ had been born in 1839 at Pittsfield, Massachusetts, and when a youth nine years of age had migrated with his family to a farm in Stephenson County, Illinois. During the summers he learned farming and during the winters attended a country school. At an older age than usual he entered high school

³⁵ W. A. Kellerman, *Journal of Mycology* 9 (4th quar.): 244, Dec. 1903.

³⁶ Prepared sometime between 1888 and 1890. Quotations taken from the original manuscript found among the papers of Dr. Smith.

³⁷ Charles F. Hottes, Personal recollections of Thomas J. Burrill and his work, *Illinois Alumni News*, 6-7, Feb. 1940; J. T. Barrett, Thomas Jonathan Burrill, *Phytopathology* 8 (1): 1 ff., Jan. 1918; also unpublished manuscript material by Dean Eugene Davenport concerning Burrill.

at the nearby county seat of Rockford and earned most of his way through. Twenty-six years old when he graduated from the State Normal University, he began to serve two years as superintendent of the Urbana public schools; and in 1867 Major John Wesley Powell, appointed to the chair of natural history and geology of the Illinois Industrial University, selected Burrill to accompany him as botanist of his first expedition to the Colorado Rocky Mountains.

His teacher in botany had been Dr. J. A. Sewall who was also curator of the museum of the State Natural History Society at Normal. Dr. George Vasey, who in 1872 became botanist of the United States Department of Agriculture, was presiding officer of this society the year Burrill graduated from college. Two other members were Dr. B. D. Walsh, state entomologist, and Professor Jonathan B. Turner of the state university to whom often has been accredited a role of leadership in securing enactment of the Morrill Act of 1862. Burrill's "interest in systematic botany stimulated by contact with these men," Dr. Hottes has said,³⁸ "no doubt was responsible for the collection and identification of the local flora of Champaign county, made while he was superintendent of the Urbana Public Schools. This collection, together with a small part of that of the Powell Colorado expedition, formed the nucleus of the present University [of Illinois] herbarium."

Powell's first Colorado expedition proving the need of a further exploration and survey of the Colorado River of the West, the Major chose this over a career of teaching and Burrill received in 1869 his appointment as assistant professor of natural history at the university. His first appointment in 1868 was to teach algebra but within a few months his subject was changed to natural history and botany; and by 1870 he was promoted to professor of botany and horticulture.³⁹

Eugene Davenport, Dean of the College of Agriculture of the University of Illinois from 1895 until 1922, and closely associated with Burrill, has written that he was a man

of the Darwin type, indeed he resembled him closely in many of his features. He had the same attitude toward new facts and possibilities. He was essentially university-minded, and as vice-president and for a time

³⁸ *Op. cit.*, 6.

³⁹ J. T. Barrett, *op. cit.*

Acting Regent (President) it was he who made the University of Illinois out of what had been but a small college with a small college outlook. . . . Had he not been, almost from the first, charged with heavy responsibility for the general University welfare he would have been one of the world's foremost bacteriologists. As it was, his discovery of the cause of pear blight marked a new step in the study of microscopic life.

Burrill's own account⁴⁰ of his early teaching was as follows:

in my first public address at the University here in the winter of 1869-70 I pointed out the need of studies in the diseases of plants and referred slightly to what was then known on the subject. The following year I introduced in the course of instruction a term's work called "Cryptogamic Botany": this was most surely curious teaching but parasitic fungi was made a part of the work. By 1873, however, we had gotten better into the subject. . . . [I]n the Transactions of the Illinois Horticultural Society, volume 7, page 217, 1873, you will find a paper entitled "Aggressive Parasitism of Fungi" in which the claim is made that these vegetable parasites are the cause not simply the results of diseases. In the same series, volume 9, page 139, 1875, you will find a paper entitled "Lettuce Mold, or Leaf Blight" showing something of what I was at at that time. From this on in those earlier days my students always had as a part of their botanical instruction this matter of fungous parasites.

According to Dr. Hottes, the text used in the course on cryptogamic botany was Mordecai Cubitt Cooke's⁴¹ *Rust, Smut, Mildew, and Mould, An introduction to the study of Microscopic Fungi* (1865), a little volume considerably the product of study at the Kew Gardens, England. He equipped a botanical laboratory and furnished microscopic instruction to his students who were "given the opportunity of seeing *and studying* from actual specimens the characteristics of the mildews and other forms of plant disease." His first microscope was imported from J. Moeller, Giesen, Germany, 1868. The second was purchased in 1869 from Spencer and Eaton. A Newton Binocular with all accessories was next bought; and in 1876, under his direction, "the University Mechanical Shops made eight microscope stands, and furnished them with oculars and objectives imported from J. Moeller. . . . Prof. Burrill's deepening interest in the bacteria demanded increasingly better lenses giving greater magnification and higher resolving powers. He purchased for \$120 a R. S. Tolles, Boston, Mass., $\frac{1}{15}$ inch, from Spencer Sons, Canastota, N.Y. (1869), a $\frac{1}{18}$ inch

⁴⁰ Letter written by Burrill to Smith, December 17, 1901.

⁴¹ H. T. Güssow, *Phytopathology* 6 (1): 1 ff., Jan. 1916.

(\$70), a Beck of $\frac{1}{40}$ inch, and finally with the discovery of the apochromatic by Abbe, a 2 mm. Zeiss oil immersion (\$130) was imported. The glass in these objectives was not stable, and the one he used was returned to Jena a number of times where it was reconditioned free of charge."⁴² One microscope, constructed by W. H. Bullock of Chicago (1879), was planned by Burrill, and other equipment for microphotography was obtained.

Before considering his study of parasitic bacteria which cause diseases in plants, we must examine a little further his work on parasitic fungi. In 1882, at the third meeting of the Society for the Promotion of Agricultural Science,⁴³ he described three fungus-caused maladies: apple scab (*Fusicladium dendriticum*); orange rust of blackberry (*Cæoma luminata*, Schw.); and blackberry and raspberry cane rust. Spalding believed that he had gathered

a good deal of valuable information concerning certain common and destructive though as yet insufficiently known parasitic diseases. One of these is the orange rust of raspberry and blackberry leaves, common enough about Ann Arbor, and of wide occurrence elsewhere. Another is the cane rust of the same plants, less conspicuous but in some localities still more destructive. For both of these the remedies already indicated by experience are recommended, viz., cutting out the canes as soon as the berries are picked, keeping the fields clean, and choosing for cultivation such varieties as have proven less liable to attack.⁴⁴

Burrill's "Notes on Parasitic Fungi" and a paper given at the same meeting on the day previous by Charles A. Goessmann of Massachusetts Agricultural College on "Observations regarding the Yellows of the Peach"⁴⁵ appear to have been the first discussions on plant pathology read before this Society; and among the first before any national scientific organization in America. The next year, at the Philadelphia meeting of the American Pomological Society, Halsted presented a paper on "The white mildews" or Erysipheae. In this, for the first time in America or elsewhere, the word "fungicide," an adaptation from "insecticide," was formally used before a gathering of scientists.⁴⁶

⁴² C. F. Hottes, *op. cit.*, 7.

⁴³ *Proc. 3rd Ann. Meet. Soc. Prom. Agric. Sci.* at Montreal, 103-110.

⁴⁴ *Op. cit.* This address, when revised, appears to have been entitled, "Recent Progress in the Study and Treatment of Diseases of Plants."

⁴⁵ *Proc. 3rd Ann. Meet. Soc. Prom. Agric. Sci.*, 63-66, 1882.

⁴⁶ Letter, April 5, 1893, Halsted to Galloway. See also Halsted's paper, A

Spalding believed that "some interesting and valuable" research was also being performed at the University of Wisconsin. "Of these," he said,

may be specially mentioned the spot disease of strawberry leaves, which within a few years has proven highly destructive to certain varieties of strawberries in common cultivation. The description of the parasite, the indication of varieties most susceptible to the disease and others that are practically exempt, and the directions for checking the difficulty by burning over the beds are the main features of the report for which we have to thank Professor [of Botany and Horticulture William] Trelease.

In the second annual report⁴⁷ of the Wisconsin Agricultural Experiment Station had been published with bibliography Trelease's "very full account"⁴⁸ of the white rust of strawberry. His report traced the vegetative growth of the fungus *Ramularia tulasnei* within the leaf tissues of the plant, the production of white spots and summer spores or conidia, the distribution by wind or other agencies of the conidia on short branches of the mycelium that project through the stomata of the leaf, and an overwintering stage of the fungus. His undergraduate work in botany and mycology had been taken at Cornell University under A. N. Prentiss and W. R. Dudley; and, under Farlow at Harvard, he had secured his doctorate of science. Since becoming an instructor at the University of Wisconsin, he had been much influenced by the work of Burrill in Illinois. When he "was working as a beginner on the parasitic fungi of Wisconsin, [Burrill], as a master, [was working] on those of Illinois."⁴⁹ The Illinoisan's discovery that pear blight is caused by a bacterium was in part responsible for Trelease's early interest in bacteriology and for his teaching of plant bacteria in his university courses in botany.⁵⁰ He, while at Wisconsin, built a valuable herbarium which included cryptogamic specimens. In 1885, however, he was offered, probably on Dr. Asa Gray's recommendation, the professorship of

tomato disease, *Proc. 5th Meet. Soc. Prom. Agric. Sci.*, 42, 1884. At p. 44 the word "fungicide" is used. This meeting took place at Philadelphia.

⁴⁷ Pp. 47-58.

⁴⁸ F. L. Scribner, Fungous diseases of plants, *Rep't of the Comm'er of Agric.* for 1885: 82.

⁴⁹ Wm. Trelease, Thomas Jonathan Burrill, *Botanical Gazette* 62: 153, 1916.

⁵⁰ Papers on bacteriology and allied subjects, by former students of Harry Luman Russell, *University of Wisconsin Studies Science* 2: 10, 1921.

a new department of botany in Washington University and to direct the Shaw School of Botany in St. Louis. He accepted and, when in 1889 the Missouri Botanical Garden was established, he became its director also.

Strawberry diseases were being studied elsewhere in the central west. Before the Mississippi Valley Horticultural Society meeting at New Orleans on January 16, 1885, Franklin Sumner Earle read a paper on the white rust and other diseases of strawberry. He had been born at Dwight, Illinois, in 1856, and in 1886 he began special work in mycology at the state university. Collaborating with Burrill, he became a joint author of a report on *The Erysiphaceae of Illinois*.

Burrill's discovery of the bacterial origin of a plant disease (1877-1882) plus Farlow's application of foreign culture methods to the study of fungi (1875-1880) had encouraged the few American vegetable pathologists to believe that a new science of plant pathology, embracing both pure and applied phases, was imminent. American medical pathologists for some years had been acquainted with the work of the great living physiologists of Europe, Carl F. W. Ludwig and Claude Bernard.⁵¹ In 1879 had appeared in Paris Bernard's *Leçons sur les phénomènes de la vie commune aux animaux et aux végétaux*⁵² which, as the title implied, treated of the phenomena common to plants and animals, and by the middle 1880's had greatly augmented interest in the study of physiology, including plant physiology. Among the plants which Pasteur studied experimentally at one time or another were grape-berries. From a plant's sound interior, he had extracted the juice, and, making sure that no surface bacteria became commingled, proved the juice free from micro-organisms. Flasks treated to washings from the grape-surface developed "growths of some sort." In 1879-1880 Chamberland, working in Pasteur's laboratory, demonstrated that beans taken directly from

⁵¹ William Henry Welch etc., *op. cit.*, 85. Concerning Bernard as France's "greatest physiologist" and "founder of experimental medicine, i. e., the artificial production of disease by means of chemical and physical manipulation," see F. H. Garrison, *Introd. to the Hist. of Med.*, *op. cit.*, 544-545.

⁵² Annotated list of persons, Pasteur, *The Hist. of a Mind*, *op. cit.*, 325 (Ludwig), p. 339; *Intro. to Hist. of Med.*, *op. cit.*, 555-558. F. R. Lillie in *Woods Hole Marine Biol. Lab.*, *op. cit.*, 139, characterizes Bernard's book as a culmination of knowledge in "general and comparative physiology." See also, J. M. D. Olmsted, *Claude Bernard*, 55 ff, 122 ff, N. Y., Harper, 1938.

the interior of their pods were free from bacteria, that is, did not contaminate culture-media when put into them.⁵³ These were among the first experiments tending to establish the belief that bacteria (excluding saprophytic bacteria) do not occur normally in the interior of sound plants. Parasitic bacteria enter through wounds or wilting of the plant.

During the 1880's a "new botany," featuring in the science's extended orbit laboratory research in "vegetable diseases" and the physiology of plants, was gaining headway in the United States and Canada. Since the 1870's Goodale and Farlow had been teaching natural history, including "vegetable physiology," at Harvard: at the botanic garden as a part of the undergraduate instruction, and in the Lawrence Scientific School. The requirements for laboratory practice applied to undergraduates and advanced students. The instruction, biological and histological, traced from the lower to higher plant orders, and considered in the lower orders not only fungi but also bacteria.⁵⁴ The main botanical laboratory had been established at the garden and Gray Herbarium in 1872, and during the 1880's the laboratory for cryptogamic botany became well equipped quarters in the Agassiz Museum.⁵⁵ In 1883,⁵⁶ with the equipping of a laboratory for plant physiology in Harvard Hall replete with new and valuable apparatus obtained with the advice of Sachs, Pfeffer, Pringsheim, Wiesner, Frank, and others, an unexcelled laboratory center for research as well as instruction was available to American students. Plant research, the nation over, began to emphasize the comparatively new subjects of plant physiology and plant pathology.

Farlow's laboratory became more and more devoted to research in mycology and algology, although the earlier interest in pathology was by no means lost sight of. Goodale's laboratory was rated among, if not, the best in the United States for its purpose. He had visited the model laboratories of Europe, brought home laboratory exercises and technical equipment, but at no time

⁵³ Erwin F. Smith, On the supposed normal occurrence of bacteria in plants *Bacteria in Relation to Plant Diseases* 2: 23, Carnegie Inst. of Washington, 1911.

⁵⁴ Ernst A. Bessey, *op. cit.*, 16; Botany at Harvard University, *Botanical Gazette* 8 (4): 205, 1883.

⁵⁵ J. C. Arthur, Some botanical laboratories of the United States, *Botanical Gazette* 10 (12): 395-396, 1885.

⁵⁶ *Botanical Gazette* 8 (4): 213, 1883.

planned that it should do more than prepare advanced students for work in Europe if they could go there. Every real American student studied the texts and articles which came from the great masters abroad. Each year during the first half of the 1880's, as Farlow in the *Smithsonian Reports*⁵⁷ and other publishing media evaluated the annual progress of botany, a "great activity in the department of vegetable physiology" was observed. Only a casual examination of these accounts is required to show the strength of the European influence. If a year brought forth no "especially striking discovery in regard to the physiology or morphology of plants," the steady increase of other valuable knowledge from the foreign laboratories made up for the temporary deficiency. So vast became the new learning that by 1883-1885 some practical distinction between the "countless papers on bacteria" had to be drawn: those discoveries "having a purely medical bearing" and those more strictly allocated to botany.

Bacteriology was soon to gain a foothold in several universities. For example, Trelease, before he left the University of Wisconsin, had ordered from abroad research equipment to develop bacteriological study there, and from the standpoint of botanical investigation. When the apparatus arrived, the zoologist Edward Asahel Birge, as a part of the regular curriculum of the university's pre-medical course, added to the fundamental studies of chemistry, physics, and biology, laboratory courses in comparative anatomy, embryology, and bacteriology, and a lecture course in physiology. Dr. Birge himself taught bacteriology and in one of his classes was a young student, Harry Luman Russell.⁵⁸ Born at Poynette, Wisconsin, in 1866, Russell was graduated from the university in 1888; he was to be granted his Master's degree in 1890, and, aware of the importance of the new research field in bacteriology, persuaded his father to send him abroad to study in Koch's laboratory. In Europe he found plant bacteriology almost totally unrecognized, and so, while on the continent he completed studies in animal bacteriology, he returned to the United States to prepare his doctoral thesis on bacteria in relation to plant tissue. American scientists, following Burrill, led in the study of plant bacteria, and for reasons.

⁵⁷ For 1879-1880, p. 313; 1881, p. 391; 1882, p. 551; 1883, p. 681.

⁵⁸ Based on an unpublished manuscript (1942) by Dean Emeritus Russell, Getting started in Bacteriology, and an interview with him August 1947.

In 1879 Edouard Prillieux, "founder of phytopathology in France,"⁵⁹ had published on a bacterial disease of plants—his red-rose, or micrococcus, disease of wheat kernels. Although his account of his microscopic examination was explicit and his figures "not obscure,"⁶⁰ he had not made any pure cultures or inoculations. In 1880 a young pathologist, J. H. Wakker, then working in the laboratory of Hugo de Vries at Amsterdam, Holland, had begun, under a royal grant secured by the hyacinth growers of Haarlem, to investigate the yellow disease of hyacinths. Publishing five papers on the subject between the years 1883-1888, he established its cause to be of bacterial origin.⁶¹ Subsequent study was to show him "entirely right in his main contentions."⁶² But, though his studies attracted wide attention, scholars were neglectful or skeptical of his conclusions. Methods of isolating the causal organism thus far depended largely on direct infection experiments. No standard form for describing an organism, of course, had come into being, owing to the plausible reason that no one knew exactly all of the characteristics requisite to establish its pathogenicity. No studies, either to confirm or dispute Wakker's findings, were made for some time.⁶³ Orazio Comes⁶⁴ of Naples, Italy, was a widely known plant pathologist of this time. Smith⁶⁵ regarded him as

one of the earliest workers in this field. He recognized bacteria in the tissues of various diseased plants in southern Italy as early as 1880, and published a number of papers on *Bacterium gummiis*, which he believed to be a widely distributed parasite attacking many plants. He did not, however, grow the organism properly in pure cultures and secure infections, nor describe it so that one can now be certain of its identity.

Burrill's studies of plant bacteria began in the middle 1870's, preceded the work of all other students, and were slow to be fully

⁵⁹ H. H. Whetzel, *Outl. hist. of phytopath.*, *op. cit.*, 93-94.

⁶⁰ E. F. Smith, *Bacteria in relation to plant diseases* 2, Carnegie Institution of Washington, 1911, "History," p. 7.

⁶¹ *Idem*, 7-8; "Wakker's Hyacinth Germ," historical, *Bulletin* 26: 9-10, U. S. Dep't of Agric., 1901.

⁶² *Bact. in Rel. to Plant Dis.*, *op. cit.*, 2: 7.

⁶³ *Idem*, 335-353; E. F. Smith, The bacterial diseases of plants: a critical review of the present state of our knowledge, *American Naturalist* 30: 797-804; 912-924; 1896; Wakker's Hyacinth Bacterium, *Proc. Amer. Assoc. Adv. Sci.* 46: 274, 1897.

⁶⁴ H. H. Whetzel, *Outl. Hist. of Phytopath.*, *op. cit.*, 96.

⁶⁵ *Bact. in Rel. to Plant Dis.*, *op. cit.*, 2: 8.

recognized in Europe. In 1882 Dr. Robert Hartig, in his *Lehrbuch der Baumkrankheiten*, expressed his doubt that any plant diseases are caused by bacteria. Two years later, Worthington G. Smith, in his *Diseases of field and garden crops*, omitted all reference to bacteria as a cause of disease in plants. In 1884 Dr. Anton DeBary, whom Erwin F. Smith believed "unquestionably one of the most learned and critical botanists the world has ever known and the foremost student of cryptogamic plants,"⁶⁶ yielded some ground on this point, but refused to go further than to admit the rare occurrence of such diseases. In his *Vergleichende Morphologie*, he said: "As Hartig has already pointed out, bacteria living in plants parasitically have scarcely been observed. The generally acid reaction of plant parts may be a partial explanation of this. Recently, however, Wakker has described as the yellow sickness, a disease of hyacinths in Holland, in which the characteristic symptom consists in the presence of slimy yellow bacterial masses in the vessels, etc. . . . More exact investigations upon this phenomenon must be awaited."⁶⁷ Even as late as 1885 in his *Vorlesungen ueber Bacterien*, DeBary summarized: "According to the present state of our knowledge parasitic bacteria are of little importance as the contagia of plant diseases. Most of the contagia of the numerous infectious diseases of plants belong to other animal and plant groups, principally . . . to the true fungi."⁶⁸ He still awaited "successful infection experiments and the exact following out of the life history of the bacterium" in the hyacinth disease. Burrill's work on pear-blight and apple-blight was mentioned, but "without other comment," Smith later pointed out, "than that 'in Europe this phenomenon, so far as I know, is not known, or at least has not been carefully investigated.'"⁶⁹ The importance of Prillieux's micrococcus was doubted by DeBary. "It may turn out," he said, "to be only a saprophyte appearing in consequence of other injuries." DeBary mentioned also Reinke and Berthold's wet-rot of potatoes and admitted that in exceptional instances potato tubers were shown to rot when the fungus *Phytophthora infestans* was not present and when healthy potato

⁶⁶ The bact. dis. of plants: a critical review, *Amer. Nat.* 30: 626, 1896.

⁶⁷ *Bact. in Rel. to Plant Dis.*, *op. cit.*, 2: 10.

⁶⁸ The bact. dis. of plants: a critical review, *op. cit.*, 626, n. 3.

⁶⁹ *Bact. in Rel. to Plant Dis.*, *op. cit.*, 2: 10.

tubers were inoculated with bacteria.⁷⁰ "As a rule," he said,⁷¹ "saprophytic bacteria may, *under special conditions*, attack, make sick, and destroy living plant tissues as facultative parasites."

Burrill began to publish on pear blight in December 1877, when as chairman of the Illinois State Horticultural Society's committee on Botany and Vegetable Physiology he presented a paper, styled simply, "Pear Blight."⁷² Even his second paper on this subject preceded Prillieux's first paper, and Burrill's study of the disease, "extending over several years," had anticipated, in fact searched for, an "entirely different kind of parasite" than the fungi, or a "fungous origin of the malady." He did not call the "very minute moving particles" he saw in the blighting tissue and exudate *bacteria*. He was mindful of fungi which later appeared in some of the blighted stems. In the cambium of blighted branches and in the exudate he observed "very minute moving particles, very similar to those known as Spermatia in fungi and other low plants." But he left to future experiments the clearing up of their origin, relationship, and development. "Are the two in any way connected in origin or growth?" he asked. "Are the later forms products, through germination or otherwise, of their forerunners?" He had observed the blight in winter and summer; and he recommended the "careful removal and burning of every dead limb or twig as soon as observed, winter or summer."⁷³

By the next year he was more confident of his conclusions. His inoculation experiments yielding definite results, he ascribed the cause of pear blight to bacteria—a conclusion confirmed by all subsequent experimentation. Again in December 1878, he went before the Illinois State Horticultural Society, meeting at Springfield, and presented a paper on "Fire blight of the pear."⁷⁴

If we remove the bark of a newly affected limb and place a little of the mucilaginous fluid from the browned tissues under our microscope, the field is seen to be alive with moving atoms known in a general way as bacteria. Sometimes a thick, brownish fluid oozes from the bark of dying limbs and spreads over the outside or falls in drops. This is apparently made up of the living things, myriads of them are to be seen at once.

⁷⁰ The bact. dis. of plants: a critical review, *op. cit.*, 627; also note 69.

⁷¹ *Bact. in Rel. to Plant Dis.*, *op. cit.*, 2: 11.

⁷² *Trans. Illinois St. Hort. Soc.* for 1877, 114-116, Chicago, 1878.

⁷³ Based on a memorandum prepared by Smith, perhaps for *Bacteria in Relation to Plant Diseases IV* which was never completed.

⁷⁴ *Trans. Ill. St. Hort. Soc.* for 1878: 79-80, Chicago, 1879.

A particle of this viscous fluid introduced upon the point of a knife into the bark of a healthy tree is in many cases followed by blight of the part, but with me not in every instance. The result if such it be, is not so uniform as the infection of warm milk; for this, however, there are very possibly other explanations. It is not improbable that certain trees in certain conditions are able to withstand the destroying influences of lowly organized plant enemies. Plants and parasites are forms of vegetable life; in the struggle for existence one or the other is subdued and the vanquished perishes. If we look once more to the affected branch we find the disease spreads more or less rapidly from the point of origin, and upon examination the moving, microscopic things are discovered in advance of the discolored portions of the tissues, but not very far ahead—an inch perhaps. Does it not seem plausible that they cause the subsequently apparent change? It does to me, but this is the extent of my faith; we should not say the conclusion is reached and the cause of the difficulty definitely ascertained. So far as I know the idea is an entirely new one—that bacteria cause disease in plants—though abundantly proved in the case of animals. . . . The so-called germ-theory of disease in animals, especially in infectious and contagious diseases, is rapidly gaining support and credence. It is not impossible that we are now making a beginning of the application of the theory to the diseases of plants which have heretofore been mysterious and inscrutable.

In August 1880 Burrill went before Section B, the Natural History division, of the American Association for the Advancement of Science with a paper on "Anthrax of Fruit Trees: or the so-called Fire Blight of Pear and Twig Blight of Apple Trees."⁷⁵ In his second paper before the state society he had "clearly identified as bacteria [the] minute, moving things . . . detected in every examination made";⁷⁶ and he now offered an explicit statement on the morphology and pathogenic nature of the bacterial organism. The organism, he later wrote,⁷⁷ "was compared with *Bacillus amylobacter* . . . of Van Tieghem, but was not otherwise named. The name *Micrococcus amylovorus* first appeared in print in the Eleventh Report of the Board of Trustees of the Illinois Industrial University for 1882 in connection with a short but, as it has proved, sufficient description."

⁷⁵ *Proc. Amer. Assoc. Adv. Sci. for 1880, Salem, Mass.*, 29: 583-597, 1881. Smith listed Burrill's papers on *Bacillus amylovorus* in the *Centralblatt f. Bakt.* II, 5(8): 276, 1899. See also *New York (Cornell) Agric. Exp't Sta. Bull.* 329: 369, 1913.

⁷⁶ T. J. Burrill, Bacteria as a cause of disease in plants, *American Naturalist* 15: 527-531, 1881.

⁷⁷ *Phytopathology* 4 (1): 31, 1914.

Burrill continued his studies of pear blight and by 1883⁷⁸ was "able to confirm the observation of Mr. Pieffer, of Wisconsin, that blight may be introduced through the flowers and probably without wound of any kind."

In the spring of 1884 J. C. Arthur, recently appointed botanist of the New York experiment station at Geneva, began experiments on pear blight which confirmed the work of Burrill. Dr. Bessey, his former teacher of botany at Iowa Agricultural College and now at the University of Nebraska, may have suggested this investigation, since on April 16 Arthur had written him:

I have hardly got to work yet: of course there is no out of door work yet. I am beginning some experiments on the absorption of oxygen by germinating seeds and accompanying phenomena. I find I am free to plan and execute any investigations I see fit, with the resources of the station at my command. What I shall do in physiological botany I have not determined, but have however concluded to look into the life and habits of some of the plants that cause diseases of cultivated crops, such as the rusts etc.

He told Bessey that he was very much pleased with his position and would "be very glad for suggestions." On May 6 he again urged Bessey to suggest "subjects for investigation. . . . My work here," he wrote, "is strictly botanical, and I am at liberty to determine what fully nine tenths of it shall be."⁷⁹

On a visit that year to Dr. Farlow's herbarium, he had decided to specialize in the rusts. In 1879 he had been an honorary fellow at Johns Hopkins University and studied under Farlow who was on leave from Harvard. Some work that year was also taken at Harvard under Dr. Goodale.⁸⁰ His doctorate in science, however, was obtained from Cornell University and by March, 1886, he was writing Bessey: "I am now making cultures of pear blight with improved appliances and material and hope to have considerable new matter for my thesis to be presented to Cornell University in June."

On January 30, 1885, Arthur had written Bessey: "I have just

⁷⁸ T. J. Burrill, Pear blight and peach yellows, *Trans. Ill. St. Hort. Soc.* for 1883: 46-49; see also *American Naturalist* 17: 319, 1883.

⁷⁹ Letters lent author by Dr. Ernst Bessey.

⁸⁰ Facts taken from memoranda by Arthur: (1) prepared at Lafayette, Indiana, Feb. 15, 1916; (2) "The Purdue Herbarium basis of the Rust Project"; Arthur's report as a member of the class of 1872, Iowa State College. Permission to use given at the Arthur Herbarium.

begun the cultivation of bacteria on solid substratum and hope soon to arrange culture tubes for their cultivation in fluids. I only today planted my pear bacteria and so do not yet know how I shall succeed with them." By August 17 he was announcing that he would "present only one paper to the American Association, which [would] be on the absolute proof that pear blight is due to bacteria." This was entitled "Proof that Bacteria are the direct cause of the disease in trees known as Pear Blight,"⁸¹ and, being read at Ann Arbor, supplied the basis for Erwin Smith's review of "Recent Literature concerning Pear Blight."⁸² In 1884 Arthur had found that he "could transfer the disease more certainly by cutting up a little of a diseased limb in a small amount of water. The water would become slightly milky with the liberated germs, and to inoculate a limb [he] used a small drop of this milky water."⁸³ During 1885 he used "sterilized solutions of starch, corn meal, etc." to grow the "micrococcus" and, after filtering the culture liquid through porcelain, reproduced the blight with the residue and not the filtrate.⁸⁴ This artificial culture technique, followed by pure culture experiments which as we shall see Smith later believed were "probably" pure cultures, was the reason why he believed that Arthur had "carried the work considerably farther" than Burrill. Smith not only studied Arthur's paper but also had "personal conversations with him" on the subject. In January the New York experiment station botanist had visited Professor Spalding's "good department" at the University of Michigan; and he had written Bessey that Spalding was building the department "in the right manner and in the right direction. He intends," Arthur wrote, "to visit Europe in a year or so to become more familiar with the German methods."⁸⁵

On August 26, in University Building at Ann Arbor, Dr. Bessey addressed the Society for the Promotion of Agricultural Science on "The Demands Made by Agriculture upon the Science of Botany."⁸⁶ Three coordinate extensions of laboratory research

⁸¹ *Proc. Amer. Assoc. Adv. Sci.* 34: 295-298, 1885; *Botanical Gazette* 10 (9 and 10): 343-345, 1885.

⁸² *Michigan Horticulturist* 1(2): 34-36, 1885.

⁸³ J. C. Arthur, Pear blight, *Proc. Amer. Pom. Soc. for 1885*: 44-47, 1886.

⁸⁴ E. F. Smith, *Michigan Horticulturist* 1(2): 34-36.

⁸⁵ Letter, Arthur to Bessey, January 30, 1885.

⁸⁶ *Proc. 6th Meet. Soc. Prom. Agric. Sci., of 1885, Manhattan, Kansas*, 16-18, 1886.

were called for: first, in nomenclature and classification of cultivated and other farm plants of value, similar in scope to Beal's efforts to systematize the apples on the basis of their floral characters and Sturtevant's enumeration and description of species of Indian corn included in which were important beginning studies of the origin and history of various cultivated vegetables; and second and third, full-scale investigation in vegetable physiology and pathology. "The vegetable pathologist," said Bessey, "must build his science upon that of his fellow worker in vegetable physiology, and the results of the labor of both must be laid before modern agriculture for its use. That botany which hopes to satisfy the demands of the advanced agriculture of today must include a knowledge of pathology."

In September 1884, following the annual convention of the American Association for the Advancement of Science, an official committee had been appointed composed of Arthur, Bessey, Farlow, Burrill, Beal, Peck, and Joseph Trimble Rothrock to secure research and a nation-wide program within the United States Department of Agriculture to study and treat diseases of plants.⁸⁷ The maintenance of a sound national economy was being endangered by the increasing menace of diseases among agricultural crops. Agriculture was still the nation's basic industry, and immediate action was believed required because of the alarming prevalence and severity of some diseases. The leaders of this movement were Arthur and Bessey, but other members of the committee were in full sympathy with the proposals of a formal memorial presented from the Association to Commissioner of Agriculture Norman J. Colman in the spring of 1885. By that time Charles V. Riley had been added to the committee.

Rothrock, like Farlow, had been a student of Gray and DeBary. He had returned from Europe imbued with the new methods of botanical investigation and, appointed to a chair of botany at the University of Pennsylvania, developed one of America's first important botanical laboratories.⁸⁸ He in his classroom instruction and laboratory work, while not neglecting adequate fundamental study in taxonomy, put forward research in plant physiology and

⁸⁷ J. C. Arthur, History and scope of plant pathology, *op. cit.*, 158-159.

⁸⁸ See, Botany in the University of Pennsylvania, *Botanical Gazette* 14 (1): 1-5, 1889.

anatomy patterned after his learning in Europe. His instruction, while more closely linked to medical botany than other laboratories subsequently established in various universities of the nation, helped to train not only medical students but plant pathologists also. A cryptogamic botanist like Farlow, he early appreciated the need for studies in forest mycology and pathology; and, had not the forestry movement in Pennsylvania drawn him almost wholly into its service, he might have gone far in this specialty.

Arthur reported to Bessey in May, 1885: "The avidity and cordiality with which [the Commissioner] acceded to all demands quite took my breath away, and I haven't yet recovered. He says, ' . . . I fully appreciate the importance of such investigation, and heartily sympathize with the scientists of the country as to the nature of the work. I have further to state that I have recently appointed F. Lamson-Scribner to the position of Assistant Botanist with special reference to this work.' " Arthur commented, " It looks to me as if our movement was tabled. I have no hope that any creditable work will be done. But I see no way to offer a protest. Scribner is a good man, but I don't know that he has any knowledge of fungi or methods of investigation."

Scribner⁸⁹ had been born in Massachusetts in 1851. Early in life, he had lost his parents and, being adopted into another family, combined his real name Lamson with that of his adopters Scribner. He was brought up near Augusta, Maine, and soon manifested his inclination toward natural history and his special interest in botany. At the age of eighteen years, he prepared for the state board of agriculture an illustrated pamphlet on the "Weeds of Maine." From 1870 until 1873 he was a student at the agricultural college at Orono, from which institution he graduated. Dr. George Vasey was in charge of the federal Division of Botany when Scribner was appointed assistant botanist, and the New Englander's herbarium in grasses and other plants, later acquired by Bowdoin College, was valuable.

In July 1885 Commissioner Colman assembled in Washington a convention⁹⁰ of influential agriculturists; and, in his address, made known his determination to encourage investigations by the Department "into the health and diseases of plants." He com-

⁸⁹ Based on a biographical sketch of Scribner prepared by the Division of Records and Editing, U. S. Dept. of Agric., March 29, 1894.

⁹⁰ *Botanical Gazette* 10 (8): 325, 327, 1885.

mented on the difficulties frequently encountered in distinguishing between fungi and bacteria as causes and not simply consequences of diseases in the vegetable economy, notably diseases of the peach, pear, and grape. Concerning pear blight, he said: "It is well known that the profitable culture of some of our best fruits is rendered very problematical on account of their liability to fatal diseases. The pear blight is an example. Many apparently conflicting opinions have been published from time to time in regard to the cause of this malady, some attributing to it fungi, and others to bacteria, but withal, no remedy is suggested by microscopists so far as I am aware." Agricultural colleges, he urged, could "do a noble work" by helping to determine the causes and prescribing remedies for some of the perplexing maladies: leaf blister of the peach; grape mildew for which as yet no remedy was known except possibly to grow species resistant to the ailment; and the whole fabric of blights, molds, rusts, smuts, and other diseases of crops.

Congress appropriated monies "for the investigation of the fungous diseases of plants, such as mildew, smut, blight, grape-rot, &c., and for experiments necessary to determine suitable remedies for those diseases."⁹¹ Accordingly, in 1886, a mycological section was organized within the Division of Botany, and Scribner, placed in control of the section on July 1st, began investigating "diseases of fruit and fruit trees, grains, and other useful plants, caused by fungi."⁹²

Bacterial diseases of plants were then scarcely known, and viruses, as known today, were unheard of except in connection with diseases of animals and man. Occasionally the word "virus" appeared in the literature of plant diseases but its use was non-specific and mainly as a synonym for poison. Burrill, for instance, when writing on pear blight,⁹³ said: "The introduction of the virus introduced the cause of the disease, and the potency of the virus was quite positively due to the living bacteria." This was a statement made in 1881 and he, during approximately a decade of study in cryptogamic botany, had passed from a doubt whether

⁹¹ *Rep't of the Comm'er of Agric. for 1886: 80.*

⁹² *Rep't of the mycological section, idem, 95.*

⁹³ Bacteria as a cause of disease in plants, *op. cit.*, 527-531; also, *10th Ann. Rep't Ill. Indus. Univ., op. cit.*, 62-84.

fungi cause disease in plants to a belief that not only fungi, but also bacteria, are "vegetable parasites" which cause diseases of agricultural crops. Burrill's inoculum appears to have been the "sticky, half-fluid substance" exuded from blighted branches of pear and apple trees. In 1881 he reported that sixty-three per centum of inoculated twigs became diseased in contrast to less than two per centum when similarly punctured "with sterile knife and solution."⁹⁴ The point of even greater importance than his technique, whether with or without the use of pure cultures, was that he had established that a bacterium, and not fungi in any way, was the cause of pear blight.

Burrill, in ascertaining the cause of pear blight, had performed a great service to his state and his nation. After his work, intelligent restrictive efforts toward its control became possible. The states immediately south of the Great Lakes were not the only valuable fruit-growing areas to become affected. Later in California economic losses from the malady were figured over a period of years as in the millions of dollars.⁹⁵ Smith believed⁹⁶ that Burrill had proved

four things conclusively; 1) The absence of any fungus in the blighting pear twigs; 2) The constant presence of a motile bacillus in enormous numbers in the freshly blighted twigs, which bacillus, moreover, could always be found pushing into the sound tissues some centimeters in advance of the visible browning and death; 3) The infectious nature of the freshly blighted material; 4) The identity of the blight on pear, apple and quince.

Arthur in 1886, after his studies of pear blight were practically completed, mapped the disease's distribution for Scribner's report of the mycological section. Smith believed that not only was Arthur's technique better than Burrill's but also that Burrill's student, M. B. Waite, finally isolated and worked out the cultural characters of the "right" causal organism.⁹⁷ Arthur in 1899⁹⁸ found Smith's evaluation of his work an "excellent and discriminating statement. . . . I have known for some time," he wrote,

⁹⁴ Charles F. Hottes, *op. cit.*, 7.

⁹⁵ E. F. Smith, *An introduction to bacterial diseases of plants*, 54-55, Phila. and London, W. B. Saunders 1920.

⁹⁶ *Centralbl. j. Bakt.* II, 5 (23): 812, 1899; *idem*, 5 (8): 276-277.

⁹⁷ Letter, Smith to L. R. Jones, February 24, 1914.

⁹⁸ Letter, Arthur to Smith, December 30, 1899.

"that I had confounded in my published accounts a non-pathogenic with the true pear blight bacillus, but that chiefly affected the morphological part of my work. What you have said of my work is certainly true, and could not have been better stated by myself." Smith had written:⁹⁹

Dr. Arthur repeated Prof. Burrill's experiments and carried the work considerably farther. In 1884, at the Geneva Experiment Station, in New York, he made 121 inoculations into . . . pear, apple, quince, hawthorn, June-berry, mountain ash, blackberry, peach, grape, etc. The infectious material was obtained from freshly blighted twigs and fruits and consisted at first of the extruded gum, and subsequently and in most cases of drops of water which had been rendered slightly milky by cutting it into sections of freshly blighted twigs swarming with the bacteria, the milkiness being due to the enormous number of bacteria held in suspension. These inoculations were made in the station orchard under peculiarly favorable circumstances, there being an abundance of material of all sorts for experiment and scarcely any natural blight nearer than a mile and a half. On the Station farm the only occurrence of blight on uninoculated trees was confined to a few twigs of one of the quince trees and to a few branches on a pear tree a half mile distant, which twigs and branches were cut out promptly. Under these circumstances Dr. Arthur obtained 53 successful infections, the first symptoms appearing usually in 3 to 8 days, but sometimes in less favorable tissues not until after 10 to 23 days. The failures were due to inoculations into too old tissues (old stems, leaves and fruits), to unfavorable weather conditions, and to inoculations into non-susceptible plants—peach, grape etc. . . . At this time it was still believed by many scientific men that infectious diseases were due primarily to chemical substances and not to the bacteria which were associated with the diseases. With this in mind, Dr. Arthur on six different occasions separated the juice of blighted tissues from the bacteria by filtration through unglazed baked clay cups. In every instance the filtered fluid failed to produce the disease when inoculated into susceptible tissues. In every case the same tissues blighted promptly when inoculated with a tiny quantity of the bacterial residue. Subsequently he made pure cultures or what he supposed to be such, and which probably were such, by inoculating tubes of sterile corn meal broth with bacteria from the interior of freshly blighted tissues. When well clouded, a drop from one of these tubes was transferred to another tube and so on through six tubes. Under careful control conditions pear blight was twice produced from such cultures.

In 1886 Arthur discussed¹⁰⁰ "preventives and remedies" for

⁹⁹ *Cent. f. Bakt.* II, 5 (23): 812-813, 1899; 5 (8): 276-277.

¹⁰⁰ J. C. Arthur, Pear blight, Rep't of the Myc. Sect., *Rep't of the Comm'er of Agric.* for 1886: 125-129.

pear blight, but admitted no "genuine remedies" existed. Germicides and sprays were being experimentally investigated. Certain pear varieties were known to be less blight-susceptible than others. Immediate removal and burning of affected branches had been early recommended, as well as clean orchard culture and other practical measures. Smith accredited¹⁰¹ both Burrill and Arthur with observing blossom-blight caused by the bacteria; and Arthur suggested that the bacteria entered through the nectaries, a conclusion later proved by M. B. Waite when establishing the insect-transmission of the disease. Smith's comparative account¹⁰² in 1885 of the work of Burrill and Arthur in pear blight conformed substantially to his more elaborate description of fourteen years later, when, to the contributions made by them, he added the results obtained by Waite by 1899.¹⁰³

In 1886 Burrill, with the use of pure culture inoculations, was studying "A Disease of Broom-Corn and Sorghum."¹⁰⁴ Interest in this study is heightened by the fact that Waite assisted him in part of the investigation. In July bacteria were believed found associated with the tissues and in August some unsuccessful inoculation experiments were performed. A pure culture of a bacillus was saved and the next year Burrill believed that he had reproduced the disease with the artificial culture. With material from diseased stalks Waite commenced experiments on young plants. At the eighth meeting of the Society for the Promotion of Agricultural Science, Burrill reported on the disease.

Waite graduated in 1887 from the University of Illinois and spent a year as an assistant to Burrill. His work in pear blight began later after becoming connected with federal agricultural work. He made "successful inoculations of *Bacillus amylovorus*, using modern methods and pure cultures obtained from poured plate colonies. . . ." He and Smith became acquainted in 1888, perhaps earlier, while both men were employed in the Section of Vegetable Pathology of the United States Department of Agriculture.

¹⁰¹ *Centralblatt f. Bakt.* II, 5 (8): 277, 1899.

¹⁰² E. F. Smith, Recent literature concerning pear blight, *Michigan Horticulturist* 1 (2): 34-36.

¹⁰³ *Centralb. f. Bakt.* II, 5 (23): 813-814. Describes experiments begun at Washington in 1894 and which Smith watched, inoculations and results.

¹⁰⁴ *Proc. Soc. Prom. Agric. Sci.*, 30-36, 1887, at p. 32.

Waite was an Illinoisan by birth, as was another student of forthcoming renown, Arthur Bliss Seymour, trained by Burrill during this pioneering period of plant pathology in America. Seymour graduated in science in 1881 from the University of Illinois and in 1886 was given a Master of Science degree after he had spent some time as an assistant at the Gray and Cryptogamic Herbaria of Harvard University. During 1879, and again in 1881-1883, he had served as botanist of the Illinois State Laboratory of Natural History. When William Trelease was called to the Shaw School of Botany and Washington University, St. Louis, he wrote to Gray, "After giving the subject a good deal of thought, I have recommended Seymour for trial one year as Assistant Professor for I have more confidence in him than in any other available man." Seymour was placed in charge of the botanical department of the University of Wisconsin during 1885-1886, but in 1886 he again became an assistant to Farlow in the Cryptogamic Herbarium of Harvard, and together over a period of years, 1888-1891, they published their famous *Provisional Host-Index of the Fungi of the United States*.

During the year that Erwin Smith was a student at the University of Michigan, the only plant pathologists who satisfied Professor Spalding's definition of a professional investigator in the subject were: Farlow and Seymour at Cambridge, Burrill of the University of Illinois, Trelease of the Shaw School of Botany, and Scribner of the United States Department of Agriculture. Smith's name was the next to be added. Spalding did not include himself.

September 27, 1885, Smith, still at Lansing, wrote to Mrs. P. H. Worden of Scriba Corners, New York, "I leave on Tuesday next for the State University, where I shall remain a year or two, after which I hope to secure a chair of Natural History in some first class institution." On May 18 he had advised Spalding that he had

fully decided to enter the University this fall, and in order to get off the regular work and confine myself to a specialty as soon as possible, I am anxious to pass as many studies as I honestly can. I am deficient in mathematics and will make up all that is required in that, but in the natural sciences and the modern languages I believe I can pass off a number of courses. Can you consistently pass me on some of the botanical work? My plan is to spend three years at Ann Arbor, or so long as may

be necessary to secure my second degree, and then go to Germany. I have not given up my first love, but propose to continue my botanical studies. Will there be any opportunity to do any remunerative work in the botanical laboratory, or in the herbarium?

Once before he had taken and passed entrance examinations. On June 2 he told Dr. Vaughan,

I prepared for the University and passed most of my entrance examinations six years ago this month, securing advanced standing in some branches. Circumstances over which I had no control compelled me to give up my plans, for the time, and seek business which should at once bring me money. I did not however give up my scheme, but have continued my literary and scientific studies.

He entered the university with advanced standing in English, English and American literature, the German and French languages and their literature, the science and art of teaching, and the natural sciences of zoology, entomology, botany, and probably chemistry and physics. He had had the "prescribed elementary course in physics," and his work with the federal weather bureau and his years with the state board of health evidently gave him advanced standing also in meteorology and sanitary science. His years in school work were his qualifications in the science and art of teaching.

During his first semester, he was permitted to substitute Latin for the requirement of additional mathematics, and during the year he had a semester's work in histology under Instructor Howard Ayers. On November 5, 1885, he wrote Jay P. Lee, one of his roommates while at Lansing and now a lawyer:

Trig[onometry] comes twice a week in the mornings. Advanced Physics comes every day in form of lectures with written examinations once a week. All the rest of the morning I spend in the Botanical Laboratory. I have taken an interesting group of parasitic fungi and am learning all I can about its members by microscopic examinations of fresh and alcoholic material. The literature of the subject is mostly in French and German and once or twice a week Spalding and I meet and read together. He is the better German. I have the advantage in French. This work is very agreeable and as I am the only one taking the course, I have things all my own way,—something we all like, you know. What additional time I get is given to that everlasting paper on "Sewerage and Death rate in Cities," which drags on interminably. . . . The members of the Faculty so far as I have met them, are very pleasant, and the same may be said of the people who frequent the Unitarian Church. The church is so pleasant and

homelike, the people are so cordial, and doctrine is so much to my liking that I shall go to church regularly.

The four members of the faculty with whom Smith was best acquainted were Professors Vaughan, Steere, Harrington, and Spalding. For the purposes of this book, his work under Spalding was, of course, most important. Burrill and Arthur had placed before scientists studies of not only pear blight but also of peach yellows¹⁰⁵ and other serious crop maladies. Smith soon knew what his specialty in botanical research and his life work were to be. He first finished the required courses for his degree, and with the completion of the first semester he looked forward to the end of the "grind."

The "interesting group of parasitic fungi" which he studied were the Peronosporaceae. At the twentieth annual sessions of the American Pomological Society which had met in September of that year at Grand Rapids, Bessey, Burrill, Arthur, and Riley had discussed various aspects of plant disease research. Smith and Spalding were acquainted with many of the delegates who attended these meetings. Arthur, after he had returned to Geneva, had written Smith on October 1 and expressed special interest in his university studies. He or Spalding, Garfield, Bailey, Beal, or Cook might have recommended the Peronosporas for study.

In December 1885 Smith published in the *Michigan Crop Report*¹⁰⁶ the results of his microscopic study of "The Potato Rot," caused by the fungus *Phytophthora* [*Peronospora*] *infestans*. He based his study of the fungus' life history on the work of DeBary, and so "very carefully prepared and concise [was his] statement of this disease, and preventive measures" he thought possible that it was reprinted, at least in part, in the third annual report¹⁰⁷ of the Wisconsin Agricultural Experiment Station where W. A. Henry was now director.

¹⁰⁵ See, T. J. Burrill, Bacteria as a cause of disease in plants, *American Naturalist* 15: 527, 1881. In this he added "'yellows' of the peach with much confidence," admitting, however, that, as yet, the "full investigation" given to pear blight had not been completed for peach yellows. Also J. C. Arthur, Pear blight, its cause and prevention," *Proc. New Jersey State Hort. Soc.* 11th Ann. Meet., Dec. 29, 30, 1885: 133-147, 1886. In a letter to Smith of December 4, 1890, Arthur said that his and Goff's studies at Geneva of peach yellows were mentioned at several points (pp. 44, 45, 51).

¹⁰⁶ 50: 3-5.

¹⁰⁷ (1886), pp. 56-59.

The plant diseases caused by the Peronosporaceae were among the most serious known at this time. About forty "members of the small order"¹⁰⁸ were known to occur in the United States; and, among the more important of the maladies, were the potato rot and the grape-vine mildew caused by *Peronospora viticola*. In New York State, during 1884, one-third of the potato crop had been destroyed by the rot, and no positive remedy for the disease was established. The grape-vine mildew had done "much injury to the grape-vine in this country, and more recently to the vines of Europe."¹⁰⁹ Since the 1870's¹¹⁰ Americans had been watching with interest the efforts of the French nation to conquer the grape phylloxera, and C. V. Riley, at the recent pomological society meeting,¹¹¹ had told his audience that grape mildew due to *Peronospora* was now regarded as more serious in France than the grape phylloxera. It was hoped that the phylloxera would be combated by grafting certain American vine stocks resistant to the pathogene onto the more susceptible European vines. An "immense traffic"¹¹² in American cuttings had resulted. But with the shipments the *Peronospora*, until within the past few years unknown to the grape-growers of France, had been introduced from this country.¹¹³

The hope for solutions of the *Peronospora* diseases of the grape and the potato lay in studying out the life-histories of the fungi and finding some point in their development-cycles when attack by a fungicide or other remedy would prove effective. The University of Michigan had no college or department of agriculture. Professor Spalding's idea of botanical research, however, was broad and included investigations in plant physiology, plant pathology, and mycology. By no later than October 9, 1885, Erwin Smith, in the university botanical laboratory, was sowing conidia of *Peronospora viticola* in water on a slide; and, after an hour had gone by, was finding "what seemed to be, and undoubtedly were, swarm spores swimming in the water. At first [he] saw only a few but in course of [the] next half hour saw

¹⁰⁸ F. L. Scribner, *Rep't of the Comm'er of Agric. of U. S. for 1885*: 83.

¹⁰⁹ *Idem*, 82-84.

¹¹⁰ *Rep'ts of the Comm'er of Agric. for 1873*: 160, 289, 389; for 1876: 81.

¹¹¹ *Proc. 20th Sess. Amer. Pom. Soc.*, for 1885: 52.

¹¹² C. V. Riley, *Rep't of the Comm'er of Agric. for 1884*: 406. See also p. 288.

¹¹³ *Proc. 20th Sess. Amer. Pomol. Soc. for 1885*: 52.

many swimming about. At 10:20 [o'clock he] found a conidium the protoplasm of which appeared to be segmenting." He observed this conidium until 10:50 o'clock, made six drawings "of the changes which occurred during this period. In five minutes after I began to watch," he wrote in a memorandum, "all at once one end of the conidium gave way and the swarm spores came out rapidly one after another or two or three together. There were 17 in this conidium," and he drew

the appearance of the cell two minutes after the rupture. Four of the swarm spores remained inside the conidium, and apparently could not get out. The cell walls appeared to be elastic and by contraction of the rupture so narrow the opening as to make escape impossible. The spores moved about actively inside the cell for some time but did no[t] so crowd or get into each other's way as to block the passage out. Several times a spore was alone and actively moving at the ruptured end, but apparently could not get out. The movement of the spores inside ceased in about 25-30 min[utes] after the rupture of the conidium. Those which escaped soon became active and swam away. I could not see any movements of cilia inside the cell before its rupture, as I was easily able to in a prior observation.

On October 16 another adventure in research took place. His memorandum read:

While examining *Peronospora* found on my slide several fine patches of zoögleæ form of *Bacterium termo*. At first all appeared to be motionless but in course of an hour several patches began to break up the individuals becoming very active. I several times fixed my attention on motionless individual cells of the zoögleæ mass and saw them become detached after a time and begin actively swimming about. By the end of an hour the field was full of actively moving figure-eights—thousands of them, and some of the zoögleæ masses were half broken up.

On October 30 he sowed in a watch-glass of water "conidia of *Cystopus candidus* from *Capsella Bursa-Pastoris*." This was another species of fungus of the order Peronosporaceae and the cause of the "white rust disease of cabbages."¹¹⁴ Again he took data on time, temperature, the appearance of swarm spores, and the break-up of conidia. This time he observed the protoplasm break down

into an amorphous or slightly granular mass. . . . Another swarm spore

¹¹⁴ F. L. Scribner, *op. cit.*, 85.

[he] saw come to rest at 10:20 gradually acquired a well-defined wall and two large vacuoles with a thin central wall of protoplasm between them. This cell remained nearly the same till about 11:00 when the protoplasm gathered over to one side from which a small, short protuberance pushed out. This protuberance enlarged slightly but showed no further change up to 12:00 o'clock.

He found on November 2 in a petiole of *Geranium maculatum* great numbers of round crystals. Boiling in HCl [hydrochloric acid] entirely freed the tissues of them. Treatment with strong Acetic acid had no effect." He concluded "them to be oxalate of lime."

Field, as well as laboratory, investigations were combined in his investigations. Early the next spring Garfield wrote Smith and offered to arrange to have published in August a paper by Smith on the grape rot. Smith replied that it was "a difficult subject and we shall not feel like publishing until we have gone to the bottom of it if it takes two years." Their observations had begun in October and he wanted to continue them at least until fall.

We find a half dozen or more sorts of fungi on the grape vine and frequently several together on the same vine. The problem is to tell which ones rot the berries i. e., to discriminate between the parasites and the saprophytes which follow the ferment very closely. In due time I trust we may be able to separate the sheep from the goats or at least to tell them apart. We shall not be ready to publish till we have settled a number of points now in dispute.

Three fungi were known to cause "rot" in grapes: *Peronospora viticola* causing mildew; *Glæosporium amelophagum* causing anthracnose, a malady studied by Burrill in Illinois and by others; and *Phoma uvicola*, the cause of black rot. Smith discovered an ascosporus form of *Phoma uvicola*, and this find was mentioned that year by F. Lamson Scribner, chief of the Mycological Section of the United States Department of Agriculture, in a paper on "Black Rot—*Physalospora Bidwellii*."¹¹⁵ In grapes, destroyed the previous year by black-rot, gathered from the vine, and kept for a week in a moist chamber, Smith found "an abundance of the ascosporus form," and Scribner said, "His camera drawings also clearly show the presence of paraphyses, organs not observed by Mr. [J. B.] Ellis nor myself."

¹¹⁵ *Proc. 7th Ann. Meet. Soc. Prom. Agric. Sci.*, 82, 87 footnote, 1886.

Trelease in Wisconsin had also studied the causes of grape rot.¹¹⁶ More than studying parasitic fungi which cause diseases in plants, in 1884 his thesis for the doctorate at Harvard under Farlow had been entitled "Observations on Several Zooglaea and Related Forms" and involved a cultural and systematic study of some bacterial forms on potato.¹¹⁷ In 1888 he spent a summer in study at Koch's laboratory in Berlin and obtained cultures and apparatus for a class on bacteria planned to be given at the Shaw School of Botany at St. Louis that autumn. We do not know that the course materialized but we do know that, during the early years of the school, Trelease held special classes and that this was planned for young medical doctors and included within its scope "the botany of the subject."¹¹⁸

Be that as it may, the classical instruction of DeBary and Farlow was reaching botanical laboratories of middle west United States and interest in plant disease study was being stimulated among both scientists and the public. Spalding lectured before the Washtenaw Pomological Society on "The Potato Rot,"¹¹⁹ and when this was published Smith added comments based on their microscopic and statistical analyses.

Nearly all of Spalding's life had been associated with a farm, and agriculture. Although he was born at East Bloomfield, Ontario County, New York, his parents, when he was an infant, had moved to Gorham in the same county, and there for fifteen years his father owned and worked a farm. Volney attended a district school and learned the rudiments of reading, writing, spelling, arithmetic, grammar, and geography, and some "higher" learning in history and algebra. In 1864, his father took his family west and settled on a farm "within easy ear-shot"¹²⁰ of the Ann Arbor high school and university bells. Greek, Latin, Mathematics, some study of English and modern languages, and sciences, were taught young Spalding while in high school. Delayed from completing

¹¹⁶ F. L. Scribner, *Rep't of Comm'er of Agric. for 1885, op. cit.*, 84, citing *Trans. Wisc. State Hort. Soc.* 15: 196.

¹¹⁷ *Univ. of Wisconsin Studies*. Science # 2: 10.

¹¹⁸ Letters, Trelease to Farlow, March 1, 17, August 23, 1888; December 7, 1893. Found at the Farlow Reference Library of Harvard Univ.

¹¹⁹ *Michigan Farmer*, March 9, 1886.

¹²⁰ Taken from notes dictated by Spalding and transcribed by Mrs. Effie Southworth Spalding January 1, 1918, and sent by Mrs. Spalding to Smith April 9, 1920.

his high school course by "hindrances due to the possession of a large farm," he completed his pre-university work in 1869 and entered the University of Michigan that year. After graduating, he served as a high school principal at Battle Creek and Flint, Michigan, and probably during these years had little to do with farming. Two years later, he was called to the University of Michigan, there to find the beginnings of a modernized instruction in science. In high school he had studied "two or three of the sciences, taught as 'information subjects.'" Greek, Latin, and mathematics were then regarded the best preparation for "a really substantial college course." Alexander Winchell, learned in botany, zoology, and geology, did not leave the University of Michigan to become chancellor of Syracuse University until 1873, the year of Spalding's graduation. Consequently, Spalding's work in natural history was mainly under Winchell, but also he took advanced work in the sciences at Cornell, Yale, Harvard, and the University of Pennsylvania. Late in the 1880's he spent a summer at the Physiological Institute at Jena, Germany, under Professor W. Detmer and still later in 1894 was to obtain his doctorate in philosophy under Wilhelm Pfeffer at Leipzig.

University Hall, the well equipped "noble structure [which stood] on the north side of the Campus," Spalding prophesied, would "doubtless long stand as an encouraging reminder that achievement is very largely a matter of keeping everlastingly at it." Probably no one of the several university buildings, library, museum, or other, housed in 1885-1886 any student and teacher in whom mutual respect and esteem were stronger than between Spalding and Smith. Spalding evaluated Smith's work at the university thus:

I have never been associated with one who had more of the spirit of investigation and whose results I would accept with more confidence. He is widely and favorably known to the botanists of the country and in all cases I think has inspired the utmost confidence as the result of his patient, thorough and conscientious treatment of every subject that falls into his hands. . . . [H]e has devoted all the time that he could command to [botany] for ten or fifteen years. He has, however, a practical knowledge of entomology and has done some close and creditable work in histology. In the other sciences he has done about what is accomplished by our most thorough students in physics, chemistry, zoology etc. He could not be called a specialist in those branches, but has done his work in them well.

He has a special interest in practical agriculture and all the questions pertaining to it. The present year he has done some excellent work on the potato rot and is now making a special study of the grape rot. The State Horticultural Society stands ready to print the results of this investigation and to go to the expense of furnishing suitable plates. [He is] a rare man, one of the very few genuine investigators who turn up in the course of a decade who are at the same time possessed of the other qualifications so essential to the proper discharge of the duties of a university professor. He is at heart and in bearing a *gentleman* in the truest sense. I would trust him *in every particular* as a *genuine man*. . . .¹²¹

Spalding believed that Smith's rare talents combined both teacher and investigator. He was possessed of "a natural taste for agriculture and a desire to study problems of practical utility such as plant diseases and other related subjects." Most of all, he was "a *man* of character and worth," and all these qualifications were his "in an unusual degree, in a far higher degree than numerous college professors. . . . He uses excellent English, reads French at sight with a fluency rarely met with, knows and reads men as one who has had the experience of a man of the world, and has the instincts, the full feeling, the kindness of heart, the nobility of purpose and the manly bearing that characterize a true gentleman." This was a letter of recommendation addressed to the Trustees of the University of Georgia, located at Athens.

Smith applied for the chair of Natural History and Agriculture there. He was aware that southern schools were known to prefer southern men. The likelihood was that a man who knew practical farming in the south and had been educated in a southern college of agriculture would do better than one whose practical farming experience was in Michigan. C. W. Garfield, secretary of the American Pomological Society, addressed another recommendation. Smith was, he wrote, "an earnest worker, public spirited, a careful student of Natural History, a candid practical man. I can not tell how he would do in the place he seeks, but if any man we have in Michigan would do well, I think he will." L. H. Bailey, Professor of Horticulture and Landscape Gardening at Michigan Agricultural College, recommended Smith "as a young man of unusual acquirements and singular natural ability in biological investigations. In all the range of my acquaintance," he wrote, "I know of no one who could better fill the chair of natural

¹²¹ Letter dated May 24, 1886.

history and agriculture in your institution." An experimental farm, maintained in connection with the University of Georgia, Smith believed, would permit him an "opportunity for the study of plant diseases and kindred topics of great interest." F. Lamson Scribner interceded for him. "I wish to say," he wrote, "that among the available men, I know of no one better qualified for such a position. From a slight personal acquaintance I know him to be a gentleman who will command the respect of his associates and from his published papers I know him to be a man of high scientific ability, eminently practical, and well able to impart his knowledge to others. What he has done has been *well done* and will reflect creditably upon any institution that may secure his services." Other recommendations were sent from the editors of *The Sanitary News*, Professor of Agriculture Norton S. Townsend of the Ohio State University, W. A. Henry, Dr. Cook, Professor of Horticulture and Entomology J. Troop of Purdue University, G. H. Failyer of Kansas Agricultural College, Dr. Beal, M. W. Harrington, J. M. Coulter, C. R. Barnes, and Lieutenant John P. Finley of the United States Army Signal Corps. Of course, Dr. Baker also recommended him.

At thirty-one years of age, Smith graduated with honors and a degree of Bachelor of Science in Biology from the University of Michigan on July 1, 1886. In the same class Fred George Novy of Ann Arbor was graduated with a degree of Bachelor of Science in Chemistry, and within two years he and Dr. Vaughan would hasten to Berlin to take Koch's course in bacteriology and prepare for the work of Michigan's new laboratory of practical hygiene.¹²²

Smith did not go to the University of Georgia nor did he apply for a position with any other state university or experiment station. His plan was to remain at the University of Michigan and secure a degree of Doctor of Philosophy. He still purposed to teach and do experimental research.

In 1885 an agricultural experiment station had been organized at Maine Agricultural College, Orono, under the direction of Dr. William H. Jordan who later followed Dr. Sturtevant as director of the New York station at Geneva and who before going to

¹²² *A Doctor's Memories, op. cit.*, 146-153; A. C. McLaughlin, History of higher education in Michigan, Circ. 4, *Bull. of Education*, 86-88, Washington, Gov't Print. Office, 1891.

Maine was at State College, Pennsylvania. State organized stations, moreover, had been established at Massachusetts Agricultural College under Dr. Goessmann; at Rutgers University, New Brunswick, New Jersey, under Dr. George H. Cook, leading authority on soils research; at Ohio State University, Columbus, under Dr. William R. Lazenby, professor of horticulture and forestry; at Alabama Polytechnic Institute under Director J. S. Newman; and in Wisconsin the university station under W. A. Henry. State stations of independent status were maintained at New Haven, Connecticut, under Dr. Johnson, professor of agriculture at Sheffield Scientific School; at Geneva, New York, under Dr. Sturtevant; and at Raleigh, North Carolina, under the direction of Dr. Charles W. Dabney. All of the stations received, directly or indirectly, appropriations from the states, and the amounts ranged from \$5,000 to \$11,000 (New Jersey) and \$20,000 (New York). A license tax on fertilizers helped finance work in the south.¹²³ In December 1886 a station was founded as a department of the University of Vermont at Burlington. In 1887 experiment stations would be established at Purdue University, Indiana, and at the University of Nebraska where Bessey, in addition to being professor of botany, would be the station's director and dean of the Industrial College, and, in the east, at Pennsylvania State College. In 1888, the year a state organized station would be combined with a station located at the University of South Carolina in 1887, federal funds under the Hatch Act of 1887 were made available to each state for a station, and the number of stations was increased to at least one, and in a few instances two or more, in almost every state of the Union.

Since 1879 a privately supported agricultural experiment station had been functioning at Mountainville, Orange County, New York. By 1885, however, neither this nor any one of the state stations had brought the movement toward scientific agriculture into the prominence it achieved after the state stations became nation-wide. By 1885 in most instances, the available land areas were inconsiderable—five, ten, twenty, or thirty acres, although in New York, Alabama, and Wisconsin, between more than a hundred and two hundred acres were usable as experimental areas.

¹²³ Agricultural Experiment Stations in America—Condensed Facts, 1885, *Proc. 4th and 5th Meet's Soc. Prom. Agric. Sci.*, 95, 1885.

Some stations occupied laboratory rooms of the college buildings; some, home dwellings made over for their purposes. Some possessed barn, office, greenhouse, and storage facilities. Nearly all had a chemist and assistants, but only four—Massachusetts, New York, Ohio, and Wisconsin—had botanists as such, unless the director or other official was botanically trained. The botanist at Houghton Farms was an official. Station bulletins, annual reports, and pamphlets were recognized occasionally for the quality of their research. Agriculture, nevertheless, was still largely a teaching subject.¹²⁴ Instruction, and systematic experimental investigations, in agriculture, were also under way at Cornell University, at the agricultural colleges of Kansas, Iowa, Michigan, and Pennsylvania, at the universities of Tennessee at Knoxville, of Missouri at Columbia, of South Carolina at Columbia, of Georgia at Athens, and of California at Berkeley.

Smith, however, did not apply to any other university during the summer of 1886. He was living at Lansing in August when Scribner wrote:

I should like to see your notes and drawings on the "Phoma" [Black rot of grape], and should you have any notes or observations relative to the fungus more than what I have made, or, if your observations may serve to verify mine, I should be very glad to state the fact in a paper I am about to publish on this subject. I saw Prof. Spaulding's letter to you and consequently know that he touched upon the matter relative to your coming to Washington to assist in the investigation of the fungus diseases of plants under my direction. As the finances of this Section now stand, I would not at present like to recommend the engagement of anyone for the entire year, but I would be glad to secure your assistance for, say, six months. The disease you mention, the *plum rot*, should be followed up and all possible knowledge gained respecting it. I want some one to go to work on *Entomosporium maculatum* [Pear leaf blight] and *Fusicladium dendriticum* [Apple scab]—fungi which cause the cracking of the pear and apple. I haven't the time to give these the personal attention required for their thorough investigation.

Immediately he procured Commissioner Colman's signature to a letter which authorized Smith's employment for six months at \$1,000 a year; and by September 3 the offer was accepted and Smith directed to report for duty.

¹²⁴ Agricultural Experiment Stations—Condensed Facts, 1885, *op. cit.*; A. C. True, Origin and development of Agric. Exp't Sta's in the U. S., *op. cit.*, 547-553.

That year Scribner in his paper on "Black Rot"¹²⁵ read before the Society for the Promotion of Agricultural Science mentioned Smith's work on the ascospore form of the fungus. Smith, in Washington, roomed at the home of Benjamin Pickman Mann, assistant entomologist of the Department of Agriculture, a linguist conversant with "as many as ten languages,"¹²⁶ who knew botany having taught the subject for a year at Bowdoin College and at one time edited a catalogue of the phanogamous plants of the United States, and who later was an examiner of the federal patent office. He was the son of Horace and Mary Peabody Mann, a graduate of Harvard University, member of national and local scientific societies of Cambridge and Washington, a bibliographer, author, philanthropist, and Unitarian, whom Smith esteemed always as "an upright and useful citizen."

In the same home roomed a young doctor of medicine, Theobald Smith, director of the pathological laboratory of the Department's Bureau of Animal Industry. In 1881 he had been graduated from Cornell University with the degree of Bachelor of Philosophy, and in 1883 obtained his medical degree in the city of his birth at Albany Medical College. During a spring semester while a medical student, he had studied in the biological laboratories of Johns Hopkins University; and, after securing his degree, had taken some graduate work at Cornell.¹²⁷

At Johns Hopkins courses of instruction in biology and physiology were given by the able and distinguished English biologist, Henry Newell Martin, a man imbued with the teachings of Thomas Henry Huxley, and by William Keith Brooks, a graduate of Hobart and Williams Colleges who had taken special work with, among other eminent scientists, Louis and Alexander Agassiz.¹²⁸ W. T. Sedgwick¹²⁹ was then an associate in the department. Going to Johns Hopkins with E. B. Wilson, each having been granted fellowships, Sedgwick had been persuaded by

¹²⁵ Black Rot—*Phylospora Bidwellii*, *op. cit.*, 87, footnote.

¹²⁶ Smith's diary, March 23, 1926.

¹²⁷ Hans Zinsser, Theobald Smith 1859-1934, *Memoirs, Nat'l Acad. of Sci.* 17: 261-288; Circular, Johns Hopkins Univ., 48 (April, 1886), p. 78; also, data, U. S. Dep't of Agric., Nat'l Archives, Washington, D. C.

¹²⁸ E. A. Andrews, William Keith Brooks, *Leading American Men of Science*, 438, ed. by David Starr Jordan, N. Y., Henry Holt and Co., 1910.

¹²⁹ *A pioneer of public health, etc., op. cit.*, 15, 19, 28-30.

Martin to change from his plan to become a doctor of medicine and be a teacher and investigator. While a Yale medical student, he had taught physiological chemistry at Sheffield Scientific School, and after the year of study in physiology at Johns Hopkins he had purposed to complete his medical education at the College of Physicians and Surgeons in New York City.

Theobald Smith and Erwin Smith became acquainted, and may have discussed the availability of advanced instruction in biology along the eastern seaboard. In 1883 Sedgwick had been appointed Assistant Professor of Biology at Massachusetts Institute of Technology, and the course he planned for pre-medical and other students included chemistry, physics, general biology, comparative anatomy, histology, and physiology.¹³⁰ In 1884 Dr. Joseph Leidy, the last of the great naturalists to cover the "whole field of nature" in studies ranging from the Protozoa and Infusoria up to man,¹³¹ had been appointed director of the Biological Department of the University of Pennsylvania.¹³² At Harvard University, since the middle of the century, the instruction of such men as Jeffries Wyman in anatomy and physiology, Louis and Alexander Agassiz in zoology and paleontology, and Asa Gray,¹³³ Goodale, and Farrow in botany had been sustaining qualities of leadership. In American science, the centuries-old supremacy of geology, chemistry, and other branches of the physical sciences was yielding with every year to important gains being made by the biological sciences. Theobald Smith's graduate study at Cornell had centered largely around the increasingly important subject of histological technique. In fact, his appointment to a position in Washington had been to some extent due to his knowledge of physiology as well as pathology. When Dr. Daniel E. Salmon, chief of the Bureau of Animal Industry, had consulted Dr. Simon Henry Gage, associate professor of physiology at Cornell, to recommend someone to help him in the laboratory to solve problems of some seriously widespread diseases among domestic animals, Gage recommended Theobald Smith. He, twenty-five years of age and

¹³⁰ *Idem*, 29.

¹³¹ Henry Fairfield Osborn, *Impressions of great naturalists*. 155, N. Y. and London, Charles Scribner's Sons, 1928.

¹³² Joseph Leidy, *A History of the First Half-Century of the National Academy of Sciences*, 159, Washington, 1913.

¹³³ Jeffries Wyman, *Leading Amer. Men of Sci., op. cit.*, 178 (by B. G. Wilder).

knowing no bacteriology and little animal pathology, accepted the position and began to study the subjects thoroughly. In 1884 reports were made of "important experiments and investigations of the Veterinary Division." This report, treating of work done before the Bureau of Animal Industry was created by legislative enactment, described pleuro-pneumonia, enzootics of ergotism and "foot-and-mouth" diseases, southern cattle fever, and swine plague. This, and reports of the next years, showed the strong influence of work and discoveries from the laboratories of Europe.¹³⁴

Statistical studies of the geographic distribution of domestic animal diseases throughout the United States were being made. Preventive, remedial, and control measures were being tried and tested. The experience of other countries was being examined, and the published writings of Pasteur, Koch, Löffler, Schütz, Roux, Klein, and many others were studied, their recommended procedures being applied when found effectual. Original investigations were started, notably that of contagious pleuropneumonia of cattle under Salmon, one of the main reasons why the Bureau was established.¹³⁵ With Gage, Theobald Smith prepared some papers on investigation techniques in pathology. As late as 1889, however, Secretary Rusk complained that, owing to inadequate laboratory facilities, modern scientific study of some diseases could not be undertaken because of the danger of communicating the maladies to employees of the Department. "The laboratories," he said,¹³⁶ "are not sufficiently isolated." Even so, animal pathology, as a research science including the study of bacteria, gained substantial recognition in the work of the Department before vegetable pathology became an important factor. Plant pathology was still a field, rather than an indoor laboratory, science. In Washington a few small rooms in the spacious red brick building of the Department was given over to the study of plants and plant diseases. But the quarters, nearer the roof than the main floors, were by no means commensurate with the needs.

During the academic year 1886-1887 the trustees of the Johns Hopkins Hospital of Baltimore had granted permission to use the

¹³⁴ *Rep't of Comm'r of Agric. for 1884: 181-267; 1885: 431-521; 1886: 593-686; 1887: 457-522; 1888: 145-220.*

¹³⁵ D. E. Salmon, *Yearbook U. S. Dep't of Agric. for 1899: 111-113.*

¹³⁶ *First Rep't of Sec'y of Agric. for 1889: 40 ff.*

Pathological building, so-called, for instruction and experimental research in pathology and bacteriology. W. H. Welch had been since 1884 Baxley Professor of Pathology there, and was recently home from study in Koch's first public course in bacteriology and had been an attendant or auditor of other courses in the new science, based on Koch's early instruction.¹³⁷

Welch's able associate at Johns Hopkins was W. T. Councilman, and among the seventeen physicians enrolled in 1886¹³⁸ as advanced students or special investigators in the laboratory was Dr. G. M. Sternberg who in 1887 published in the *American Journal of Medical Science* on the thermal death-points of pathogenic organisms. While in Europe, Welch had learned much about diseases of animals, and not infrequently he was consulted by officials of the Bureau of Animal Industry to help diagnose some of the more puzzling maladies or determine the efficacy of some remedy.

In 1886 Theobald Smith was appointed a professor of bacteriology in the Columbian (now George Washington) University, and from this institution in 1890 would be graduated as a doctor of medicine another graduate in science from Cornell University, Veranus Alva Moore, who that year also began a fulltime employment as an investigator of infectious diseases with the Bureau of Animal Industry.

They were the men who in 1889 and 1890 were to help shape the directions of research when M. B. Waite and Erwin Smith in the Section of Vegetable Pathology would equip laboratories and begin investigating diseases of plants in those cases where a bacterial origin either was proven or was suspected.

In 1887 Erwin Smith's paramount wish was to add "to the sum total of human knowledge, and the world's progress," and to make people happier.¹³⁹ His accomplishments in science were to be the media by which to achieve these ends.

¹³⁷ *William Henry Welch, etc., op. cit.*, chap. VIII, pp. 139-147.

¹³⁸ *Johns Hopkins Circulars* 51: 123, 1886; 55: 64, 1887.

¹³⁹ Letter from Smith to Viola Holmes, January 23, 1887.

CHAPTER IV

VEGETABLE PATHOLOGIST WITH THE UNITED STATES DEPARTMENT
OF AGRICULTURE. FUNGOUS DISEASES OF PLANTS,
AND PEACH YELLOWS, HIS SPECIALTIES.

WITH THE YEAR 1887 was begun another decade of sustained progress in the history of American plant pathology—a period which Smith later described as one of “readjustment and of taking stock of the new discoveries . . . also of great research activity and there were distinct advances,” he said, “in several directions.”¹

When Smith entered the United States Department of Agriculture at Washington in 1886, research in plant diseases “had only recently been separated from the ordinary botanical work of the department, which then consisted principally of answers to correspondents, and species descriptions of grasses. At that period, and for some time to come,” he later explained, “we had no laboratory facilities and scarcely any place we could call our own. A little cubbyhole was apportioned off for the chief, Professor Scribner, and his assistant,” Erwin Smith, “was allowed, by courtesy of Dr. Marx, the department artist, to occupy a desk in his room. We had very few books, and nothing in the way of apparatus beyond the simplest sort of microscopes. . . . The amount of money appropriated by Congress for this line of work in 1887 was \$5,000. . . . Of special journals devoted to plant pathology there were none,”² although in 1887 the *Centralblatt für Bakteriologie und Parasitenkunde* was founded and after 1895 was to provide “a vast storehouse of the new knowledge.” In 1883 the *Berichte d. d. botanischen Gesellschaft* had been started and was affording “a mine of information on botanical subjects,” in addition to DeBary’s journal, *Das Botanische Zeitung*, which had been in existence more than four decades. There were also the *Botanisches Centralblatt* founded in 1880 and the influential *Annals of Botany* begun in 1887. In 1882 Saccardo had begun to

¹ Fifty years of pathology, *op. cit.*, 20.

² Plant pathology: a retrospect and prospect, *op. cit.*, 606-607.

publish his *Sylloge Fungorum*, "destined," Smith said, "to be a summary of everything known about fungi," and in 1887 A. Engler and K. Prantl "commenced their vast publication 'Die Natürlichen Pflanzenfamilien,' of service to everyone"³ and one of the first great monumental works which sought, as the title implied, to organize all of the families of plants into natural sequences and orders.

While at the University of Michigan, Smith may have read with Professor Spalding the Austrian von Thümen's "little book on the control (Bekämpfung) of fungous diseases of cultivated plants," *Die bekämpfung der Pilzkrankheiten unserer Culturgewächse*, published in 1886 at Vienna. It is, of course, also possible that during the summer at Lansing, Smith read this book, which *Gardeners' Chronicle* in England complimented as a bold attempt to summarize the modest results thus far achieved in plant therapeutics. More likely, however, neither he nor Spalding studied this book until, at least, the last months of 1886 or the early part of 1887. Probably, furthermore, this book became available in America about the time "the first and second volumes of the greatly improved second edition" of Sorauer's *Pflanzenkrankheiten* appeared, and which Smith praised as

a godsend to all of us who could read German. We had at this time in English very few serviceable books. There were not many papers written on plant diseases at that time, nor had we any journals. . . . In English there were few books of any sort, good or bad. I recall Berkeley's "Outlines," with a chapter on plant diseases which was good but old (1869) and very incomplete, and Worthington G. Smith's "Diseases of Field and Garden Crops," newer (1884) and more extensive but less dependable. There was also a little book on "Rusts, Smuts, Mildews, and Moulds," by M. C. Cooke (1865), which did service of a sort in default of something really good. I used also a little German book by Georg Winter (1878). We had to be content with books and papers in Latin, French, and German, and largely with systematic treatises on fungi. So we read Berkeley and Tulasne, Corda and Fries, Montagne and DeBary, and all the older writers. . . .⁴

After 1887 English and American plant pathology became "more than a sublimated mycology."

Spalding referred to von Thümen's book "as an illustration of

³ Fifty years of pathology, *op. cit.*, 19, 21, 23, 24, 26.

⁴ *Idem*, 20.

the growth of definite scientific knowledge" which began soon to displace "vague theories and uncertain experiments."

By 1886 the Department of Agriculture's herbarium was "one of the largest and most valuable in the country, and contain[ed] a representation of nearly" 12,000 native phanogamous plants, as well as large numbers from Mexico, South America, and other countries.⁵ Smith had been reading about American and English collections of fungi for many years. The Commissioner's report for 1886 mentioned no such collection at the Department, but he did announce that experimental investigations were under way to study fungous diseases of plants. More than 200 species of fungi were believed to attack the grape vine. Annual losses from diseases of corn and wheat were estimated at approximately \$200,000,000. Potato rot in wet seasons had been so destructive that the loss in 1885 was placed at from ten to forty per cent of the entire crop. Very serious crop losses were reported also for cotton, orange, apple, peach, pear, plum, quince, and other industries.⁶

Scribner believed that the "most important work" of his section that year had been the issuance of his and Smith's Bulletin 2.⁷ His account of "Fungus Diseases of the Grape-Vine," occupying practically one-half and the title of the bulletin, was to have such a demand for copies that in 1890 he would publish a book⁸ on the subject, and in 1886 included in his report abstracts of the leading topics: the downy and powdery mildews, black rot, anthracnose, etc. So widespread and abundant were the inquiries that from them in part the workers were enabled to map the distribution and severity of the diseases. Professors S. M. Tracy had written the Department concerning celery-leaf blight. This subject, together with material on orange-leaf scab and the potato-rot, a summary by Arthur of his and Burrill's study of pear blight, and a discussion by Trelease of an orchard grass spot disease, comprised the main parts of the section's report for that year.⁹

In 1885 Scribner, while Vasey's assistant, had prepared for the annual report of the Division of Botany a treatise of some twelve

⁵ Report of the Botanist, *Rep't of Comm'er of Agric. for 1886*: 70.

⁶ N. J. Colman, *Comm'er, Rep't of Comm'er of Agric. for 1886*: 28.

⁷ U. S. Dep't Agric., *Bot. Div. Bull.* 2: 1-136, 1886.

⁸ *Fungus diseases of the grape and other plants and their treatment*, J. T. Lovett Co., Little Silver, N. J., 136 pp.

⁹ *Op. cit.*, 96-131.

pages on "Fungous Diseases of Plants,"¹⁰ in which he outlined several maladies and "the leading characters of some of the chief groups or more destructive species of fungi." Remedies for some were described. His footnote citations indicate that he profited considerably from reports of studies made by Farlow, Halsted, Trelease, Peck, Henry, William Saunders of the Department, and other students of diseases of cultivated plants. He did some experimental work. For instance, in his study of the downy mildew fungus, he showed in 1886 that the most favorable temperatures for the germination of its spores were between 25° and 35° centigrade. At lower temperatures the germination was slower and at 0° C. the vitality of the conidia was not destroyed.¹¹

Erwin Smith had not been studying fungi many years; in fact, on April 19, 1888, he wrote Farlow,

My attention has not been given to Fungi until within the last few years, and I have never published anything of any consequence. The year of our great potato rot, at the request of the Bureau of Crop Statistics, I prepared a brief account for his use. I have sent you a copy. This was reprinted in the Trans[actions] of the Wisconsin Exp[eriment] Station for 1885 or 1886, and somewhere in Canada. To Mr. Scribner's Report on Fungous Diseases of the Grape Vine I contributed the Index and Appendix [A]. I also translated Appendices C and D.¹² . . . To the Department Annual for 1886, I contributed ostensibly two maps and a short account¹³ of *Phytophthora* accompanied by a plate. To the Michigan Farmer in 1885, I now remember that I also contributed some statistics of the potato rot of that year, but have no copies left. For a special Department Report¹⁴ not yet published, I went over this ground much more

¹⁰ *Rep't of the Comm'er of Agric.* for 1885: 76-88.

¹¹ *Phytopathology* 9 (9): 391, Sept. 1919.

¹² U. S. Dep't Agric., Bot. Div., Bull. 2: 1-136, 1886.

Appendix A, 45-53, Synopsis of replies to a circular relative to grape mildews and grape rot in the United States.

Appendix B, 54-63, Remarks on grape rot and grape mildew.

Appendix C, 65-118, Prevention of mildew—results of experiments with various fungicides in French and Italian vineyards in 1885.

Appendix D, 119-121, Results of the Congress on Parasitic Diseases of the Vine held at Florence, Italy, October, 1886.

¹³ The Potato-rot. *Phytophthora infestans*, De By. (Plate VII), *Rep't of the Comm'er of Agric.* for 1886: 121-124. Maps showed the distribution of *Peronospora viticola* and black rot of the grape. See p. 122 concerning Smith's work on the potato-rot report and other "valuable assistance" to Scribner on his annual report of that year.

¹⁴ See, Special reports on peach blight and potato rot, Sect. Veg. Path., U. S. Dep't Agric., 1889.

exhaustively and prepared a map for the lithographers showing the distribution of the potato rot. The first from the figures of the Tenth Census, the second from 2000 returns to a circular sent out by the Section at my suggestion.

In 1886 Smith published also a "Partial list of parasitic diseases observed in Michigan,"¹⁵ chiefly in Ionia, Clinton, Ingham, and Washtenaw Counties. Arthur compiled and published a similar list of "Plant diseases. Observed at Geneva, New York."¹⁶

Smith's translation of Appendix C, "Prevention of mildew—results of experiments with various fungicides in French and Italian vineyards in 1885," was very important. In it was presented Pierre Alexis Millardet's own description of his celebrated discovery of Bordeaux mixture which was soon thereafter introduced into the United States and from which sprang "probably one of the most remarkable series of investigations and experiments ever witnessed in this or in any other country. Fungicides of many kinds," Galloway has said,¹⁷ "were proposed and tested on a large scale, and extensive lines of investigation were inaugurated not only by the Department of Agriculture, but also by private individuals in various parts of the country."

Millardet¹⁵ had studied botany with Wilhelm Hofmeister at Heidelberg and with DeBary at Freiburg, and, while like DeBary he obtained a doctorate of medicine, he chose to teach botany and was a professor at Bordeaux when he and his co-workers made their famous discovery. While studying *Peronospora viticola* in the "hope of discovering a weak point in its development which might enable [them] to become master of it,"¹⁹ he chanced to notice that a mixture of copper sulphate and lime, at first sprinkled on grapevines along a road to discourage the stealing of fruit, had a prophylactic effect against the fungus of the downy mildew. It had been known for some time that copper in various forms destroyed fungous spores. But this chance observation, made in October 1882, in a vineyard of St. Julian in Medoc, was followed by extensive experimental investigation, and by 1886 the United

¹⁵ Report of the Mycological Section, *Report for 1886, op. cit.*, 133.

¹⁶ *Idem*, 132.

¹⁷ B. T. Galloway, Progress in the treatment of plant diseases in the United States, *Yearbook of the U. S. Dep't of Agric.* for 1899: 195.

¹⁸ H. H. Whetzel, *Outline hist. of phytopathology, op. cit.*, 63-64.

¹⁹ Appendix C, *op. cit.*, 9: 94-96. See also, *idem*, 10: 97; 12: 108-112.

States Department of Agriculture was publishing not only translations of the papers by Millardet and others but also sending out circulars to encourage trial experiments on the remedies for *Peronospora* and black-rot of the grape in this country. Scribner reported²⁰ that the results in France from

the use of cupric fungicides for *Peronospora viticola* fully confirm previous statements and experiments. Those detailed by Mr. Millardet in *Journal d'Agriculture Pratique*, November 25, 1886, are especially interesting. The experiments at Dauzac and Beaucaillou were conducted either by himself or by Mr. David. Eighteen remedial mixtures, dry or fluid, were tried very carefully with the necessary control experiments, and full memoranda were made from time to time of the condition of the various plots. The experimental fields covered in all about 5 acres.

About this time Millardet was investigating another vine pathogene, the phylloxera. For this there was introduced American vine stock, resistant to the disease, which, when grafted onto susceptible European varieties, saved the vineyards and the industries dependent on them.²¹ His Bordeaux mixture, however, which "remained for a quarter of a century the most efficient and most universally applicable fungicide known,"²² became "a sovereign remedy not only for the ravages of the grape *Peronospora*, but also for many other diseases of cultivated plants, including black rot of the grape and the devastating mildew (*Phytophthora*) of the potato. This," Smith believed,²³ "was the first great advance in plant therapeutics." At one time this treatment was "hailed as a general panacea for all plant diseases," and later it was used effectively against other maladies, among which were leaf spot of pear, and apple scab.²⁴

We do not know when Smith first became definitely interested in European and American researches in rusts and smuts of grains. His review²⁵ in 1889 of two new volumes of Oscar Brefeld's *Untersuchungen aus dem Gesamtgebiete der Mykologie. Fortsetzung der Schimmel und Hefepilze* preceded by almost a year

²⁰ Report of the Mycological Section for 1886, *op. cit.*, 99-101.

²¹ H. H. Whetzel, *op. cit.*, 65.

²² *Idem*, 58-59; 63, quotation at p. 65.

²³ Fifty years of pathology, *op. cit.*, 19-20.

²⁴ Plant pathology: A retrospect and prospect, *op. cit.*, 608; H. H. Whetzel, *op. cit.*, 59.

²⁵ *Journal of Mycology* 5 (2): 98-102, 1889.

his translation and publication of the Munich professor's address in 1888 on "Recent Investigations of Smut Fungi and Smut Diseases."²⁶

Brefeld [he later wrote] has shown for many of the smuts that they can vegetate for long periods in forms resembling yeasts. In a magnificent paper on corn smut the same author has shown clearly that, unlike most smuts, the pustules appear in about fourteen days from the time of infection, and that only young, actively growing tissues can be infected.

What seems to have interested Smith in the work of Brefeld as much as anything, however, were his methods of culture study and the comparison of his classification of genera, subgenera, and species of fungi with those of DeBary.

In 1885 Jens Ludwig Jensen²⁷ had begun in Denmark to investigate smuts in cereals, and his blossom infection experiments were of immediate value to workers in experiment stations of the grain-growing states of the central west. In 1887 he treated oats experimentally with hot water for five minutes at a temperature of around 55° C. and discovered that, without damaging the oats' germinating power, the smut was killed. This, Ravn has said,²⁸ was "the starting point for the development of the 'Jensen hot-water treatment,'" and soon the remedy was extended against smuts in barley, *Bromus*, and other plants, and to prevent types of damping off in sugar beets and mangels. In 1883-1884 he had published his method for disinfecting seed potatoes by heat, but the hot air, effective against the potato fungus, proved less so than the hot water against the grain maladies. Furthermore, the heat potato-treatment was superseded by Bordeaux mixture. It had been known for a century that copper sulphate could be used to free seed wheat from the germs of hard smut; but, when lime was added, the danger of injuring the wheat foliage was reduced and Bordeaux mixture became important in this connection, also.²⁹

Jensen's paper, "The propagation and prevention of smut in oats and barley," was made available to English-reading scientists

²⁶ *Jour. Myc.* 6 (1): 1-8; (2): 59-71, 1890; 6 (4): 153-164, 1891.

²⁷ F. Kølpin Ravn, Jens Ludwig Jensen, *Phytopathology* 7(1): 1 ff., 1917; E. C. Large, Danish hot water, *The advance of the fungi, op. cit.*, 240-247; H. H. Whetzel, *op. cit.*, 81-85 (including an account of the life and work of the Danish botanist Frederick George Emil Rostrup).

²⁸ F. K. Ravn, *op. cit.*

²⁹ J. C. Arthur, Hist. and scope of plant path., *op. cit.*, 160-161.

by the *Journal of the Royal Agricultural Society* of England, and a reprint was reviewed in America in 1889 by the *Journal of Mycology*.³⁰ By 1890, at experiment stations in the important grain-growing regions, workers were publishing studies on rusts and smuts of cereals, and the Jensen hot-water treatment for smut in oats and other grains was found preferable to other fungicides for many purposes.³¹ Smith saw this demonstrated when he examined the experimental work against smut in wheat done at the Kansas station³² and he believed³³ that Jensen's "experiments were subsequently repeated, expanded and confirmed in this country by [W. A.] Kellerman and [W. T.] Swingle" there.

Within the next decade, new discoveries of chemotherapeutic remedies would be made. Formaldehyde, for instance, recommended by Professor H. L. Bolley of the North Dakota Agricultural College, would be used to treat several diseases of grains, notably in wheat, oats, and flax.³⁴

Such remedies were not found, however, for every malady of cultivated crops, and, occasionally, other procedures either supplemented or were the more important part of the work of prevention or control. During the 1880's, when a leaf disease of cherry trees menaced the German nation's sweet cherry industry, A. B. Frank's discovery that an overwintering ascomycete was its chief cause made possible the invention of a practical control technique of stripping the leaves from affected trees during two seasons.³⁵ Or, in the case of black stem rust of wheat, when it was established that alternate hosts, the common barberry and the wheat plant, harbored the fungus, *Puccinia graminis*, at different stages in its development, control-efforts were made by removing bar-

³⁰ 5 (1): 42-43; (3) 164-165 (by B. T. Galloway).

³¹ See, W. A. Kellerman and W. T. Swingle, Prevention of smut in oats and other cereals, *Jour. Myc.* 6(1): 26-29, 1890; also, Fifty years of pathology, *op. cit.*, 21.

³² See Chapter V. There was also work on diseases of cereals at the North Dakota station under H. L. Bolley, at the University of Nebraska under Bessey, at the Indiana station under Arthur, and elsewhere, much of it reviewed in the *Journal of Mycology*: (Kellerman's, Swingle's preliminary report) 5(4): 218-219, 1889; (Arthur's "Smut of Wheat and Oats") 5(3): 165; (Bolley's "The Heteroecismal Pucciniae") 5 (3): 167; as examples.

³³ Plant pathology: a retrospect and prospect, *op. cit.*, 608.

³⁴ J. C. Arthur, *op. cit.*, 161; H. L. Bolley, *Bull.* 27, N. D. Agric. Exp't Sta.

³⁵ H. H. Whetzel, *op. cit.*, 76.

berry bushes or forbidding by law their being planted near wheat fields.³⁶

Erwin Smith followed with interest the study made by Harry Marshall Ward³⁷ of a leaf disease of coffee trees on the island of Ceylon. Ward was an honor graduate of Christ College, Cambridge University, England, a science student of the Science and Art Department at South Kensington, and a former student of Sachs at Würzburg and of DeBary at Strassburg. When he began his investigations of the coffee disease, the losses were already estimated in millions of pounds sterling.³⁸ Year after year, this ravage destroyed foliage on whole areas of coffee plantations, discouraged planting, and threatened to change, as it finally did, the center of the coffee growing industry from Java and the English Indian Ocean Empire to Brazil and South America.³⁹ Ward proved that a fungus, *Hemileia vastatrix*, was the cause, and that fungicides had to be applied to the foliage at a precise time anticipatory of the fungus and its recondite development, to wit,

after the spore, germinating on the leaf-surface, had put out its delicate germ-tube but *before* this germ-tube had time to penetrate through a breathing-pore into the leaf-interior. "The life of the parasite," [Ward said] "is so arranged that as short a time as possible shall intervene between the well-protected spore condition and the safely ensconced mycelium."⁴⁰

Fungicidal treatments—lime and sulphur among them proved efficacious—were applied so as to be "already on the leaves when the spores were germinating." The next best method practically available when the malady occurred was to plant the areas affected with crops not subject to attacks from the fungus. Some control was achieved by protecting cultivated species of coffee by natural or artificial windbreaks, wild species being noticed to be comparatively unharmed and could form a barrier against "spore-laden winds." Still another method used in Africa was to restrict coffee

³⁶ B. T. Galloway, *Progress in treatment of plant diseases, op. cit.*, 192; J. C. Arthur, *op. cit.*, 153-155; E. C. Large, *op. cit.*, 131-135.

³⁷ H. H. Whetzel, *op. cit.*, 100-102; E. M. Freeman, Harry Marshall Ward, *Phytopathology* 3 (1): 1 ff., 1913.

³⁸ Fifty years of pathology, *op. cit.*, 19; also, E. F. Smith, The coffee disease of Ceylon, *Science* 23 (575): 80, 1894.

³⁹ E. C. Large, *The advance of the fungi, op. cit.*, chap. XV, 196-208.

⁴⁰ E. C. Large, *op. cit.*, 205-206.

cultivation to particular altitudes and districts. But the remedies were tardy and the losses enormous.

Ward's abilities as a pure and practical scientist were recognized. In 1889-1890 appeared the first edition of his text *Diseases of Plants*. Dean Emeritus E. M. Freeman of the College of Agriculture of the University of Minnesota, one of Ward's students at Cambridge and for several years with the United States Department of Agriculture in cereals investigation, has written of his work: ⁴¹

From 1881, the time of his return from Ceylon, to 1888, Ward's contributions dealt with a large range of mycological subjects, including important results on the tubercle organisms of leguminous plants . . . In 1888 he published his paper On a Lily Disease ⁴² in which he seems to have become fairly launched in what was probably the main problem of his life work, the problem of parasitism. His attention was naturally attracted to any association of plants in intimate relations of nutrition, and his paper on the Ginger Beer Plant opened the way to new conceptions of fungous nutrition.

Between 1886 and 1888, at the seventh, eighth, and ninth meetings of the Society for the Promotion of Agricultural Science, three papers were read in which directly or indirectly the root nodules of leguminous plants and the atmospheric fixation of nitrogen by certain bacteria were discussed. In 1886 Dr. R. C. Kedzie in his address "Humus as a Source of Nitrogen for Plants" ⁴³ excluded considering "the recent announcement" by Wilbur Olin Atwater of the Connecticut Agricultural Experiment Station that "under certain circumstances the free nitrogen of the air is assimilable by plants." But, from the standpoint of agricultural chemistry, he discussed other phases of his subject, and in 1887 Dr. B. D. Halsted, in his paper "A Hint as to Nitrogen Appropriation in Clovers," ⁴⁴ alluded to Kedzie's summary of his experiments on the "different capacities of cereal and leguminous

⁴¹ *Phytopathology* 3 (1): 2, 1913.

⁴² See Erwin Smith's review, A lily disease, *Jour. Myc.* 5 (1): 46-48, 1889. Ward's paper appeared in the *Annals of Botany* 2 (7): 319-382. Smith later (Plant pathology: a retrospect and prospect, *op. cit.*, 607) called this "a remarkably fine paper," ranking in importance with papers by DeBary, Woronin, Farlow, etc. on the etiology of various plant diseases. The biology of the form-genus considered was "a *Botrytis* of the *Polyactis* type."

⁴³ *Proc. 7th Ann. Meet. Soc. Prom. Agric. Sci.* 37-41, 1886.

⁴⁴ *Proc. 8th Ann. Meet. Soc. Prom. Agric. Sci.* 41-44, 1887.

crops for appropriating soil-nitrogen." The next year, Dr. G. C. Caldwell of Cornell University, reading a paper on "The Present Aspects of the Question of the Direct Utility of the Free Nitrogen of the Atmosphere for Plant-Food,"⁴⁵ reviewed the results of study on the subject in England and continental Europe and said of Halsted's paper:

There appears to be no doubt that there is a close connection between these tubercles and bacterial life. Hellriegel says they are filled with bacteria. Dr. Halsted, in his excellent paper on this subject, read at the last meeting of this Society, says that there is no question but that they become centres of development of bacteria in the soil and that there are, moreover, motile bodies within their tissues which are probably bacteria; but as to their function he suggests only a possible participation in the ordinary nitrification in the soil. That there is anything more than this, or that these tubercles have anything to do with the fixation of free nitrogen in connection with the growing legumes must as yet be regarded only as an inference waiting for substantial proof.⁴⁶

H. T. Pledge, in his recent book *Science Since 1500*,⁴⁷ says that

In 1866 certain nodules seen by Malpighi (1686) on the roots of leguminous plants [had been] shown to contain bacteria. Ten years later Berthelot took the subject up, and after a further decade showed that certain bacteria present in the soil can fix nitrogen from the air. In 1887 it was shown that leguminous plants fertilise the soil by increasing its nitrogen content, and in 1888 the Germans Hellriegel and Wilfarth showed that the bacteria in the nodules transform atmospheric nitrogen into the nitrate form in which most plants take it up.

Strictly speaking, these phenomena are not parasitic.

The root-tubercle bacilli of the Leguminosae were believed, during Smith's years of study, by some botanists to be parasites.⁴⁸ His bibliography on the subject⁴⁹ included for the year 1888 also a paper by M. W. Beijerinck entitled "Die Bakterien der Papiionaceenknöllchen,"⁵⁰ and of this paper H. A. Harding, American plant pathologist, in 1912 said:⁵¹ "We have added little to our knowledge of the histology of legume nodules since Beijer-

⁴⁵ *Proc. 9th Ann. Meet. Soc. Prom. Agric. Sci.* 21-24, 1888.

⁴⁶ *Idem*, 23-24.

⁴⁷ Ministry of Education, Science Museum, Philosophical Library, 256, 1947.

⁴⁸ *Bacteria in relation to plant diseases, op. cit.*, 1: 64.

⁴⁹ *Idem*, 239-240.

⁵⁰ *Botanische Zeitung* 46: col. 725-735, 741-750, 757-771, 781-790, 797-804, 1888.

⁵¹ The trend of investigation in plant pathology, *Phytopathology* 2 (4): 161 f.

inck's paper of 1888." During the 1880's botanists were still arguing the improbability of bacterial diseases of plants. A part of their theory was that "the reaction of plant juices is acid and bacteria grow only on alkaline media." Susceptible host plants as well as parasites, however, were being investigated; and some of the younger botanists were beginning to challenge points of view of their elders.

In 1889 Smith reviewed in the *Journal of Mycology*⁵² Dr. Robert Hartig's *Lehrbuch der Baumkrankheiten* and observed that the book went further than its title in that diseases and parasitic fungi which attack grains, vegetables, and other herbaceous plants were described along with diseases of forest trees. Occasionally the reviewer met "a questionable statement" and his criticism read:

Under *Gymnosporangium* four species are mentioned—*G. conicum* (*juniperinum*), *clavariaeforme*, *Sabinae* (*fuscum*), and *tremelloides*. The author thinks a further investigation of the forms thus far known and described is desirable, as the results of some experiments instituted by him do not agree with those commonly accepted. No mention is made of the labors of Dr. Farlow or of Dr. Thaxter.

Under bacteria Dr. Hartig urges the commonly accepted view that the acid reaction of most plants is unfavorable to their growth and development, and evidently thinks they play a very unimportant role in the production of plant diseases. They have been found as parasites, he says, only in thin-walled, soft parenchymatous tissue, such as bulbs and tubers, and here are often preceded by fungi. Even in Waacker's hyacinth disease (the yellow, slimy bacteriosis) "the bacteria do not attack entirely sound, well-ripened bulbs under normal conditions," but only those that have been wounded or previously attacked by fungi; especially by a hyphomycetous fungus, which is almost always associated with this bacteriosis. In damp places the bacteria enter the wounds and cause the rot. The following paragraph on pear blight will hardly pass muster, and was certainly not to be expected in a handbook published in 1889. All the recent American publications on this subject, especially the papers by Dr. Arthur, appear to have escaped the author's attention.

"Recently a disease of pear and apple trees, called blight, has been described by [T.] J. Burrill in Urbana, Ill., the cause of which this investigator ascribes to the invasion of a bacterium. The disease appears to bear a resemblance to the tree canker (*Baumkrebs*) caused by *Nectria ditissima*, and since in this fungus small bacteria-like gonidia are produced in great numbers in the bark, it becomes necessary to inquire first of all whether this disease has not been wrongly ascribed to a schizomycete."

⁵² 5 (3): 174-177, 1889.

Mention is made of fifteen species of *Exoascus*, all of which produce characteristic hypertrophies. Seven of these species also cause *hexenbesen* or witchbrooms, and these peculiar growths are also induced by various *Uredineae*, notably by the accidium (*Peridermium pini*) of *Coleosporium senecionis*, and by *Aecidium* (*Peridermium*) *elatimum*.

The black-knot of the plum and cherry, *Plourightia morbosus*, is said to occur only in North America, but the author thinks it may be introduced into Europe at any time. This is quite likely and the wonder is that it should not have occurred before, owing to the fact that it is found in all our species of *Prunus* and is very destructive in many parts of the eastern United States. . . .

In this review Smith did not mention Luigi Savastano's recent assertion that *Bacillus oleae* had been found in Italy to cause the disease known as tuberculosis of the olive. Preliminary pronouncements on the organism's pathogenic nature had been made in 1886 and 1887.⁵³ Either complete and positive proof, tantamount to that available in pear and apple twig blight, had not come to Smith's attention or he still indulged some skepticism. News of new scientific discoveries in Europe were still slow in reaching America. Smith's growing interest in plant bacteriology was still mainly a reading interest. His account of this discovery was later as follows:⁵⁴

In 1886 Archangeli described the olive tubercle, giving the name *Bacterium oleae* to something observed in it but without what we should now [1920] consider to be a proper description, i. e., it was named from the microscope, without cultures or proofs of its infectiousness by inoculation, and with the statement that it probably had nothing to do with the disease, which was ascribed by him to other causes. Savastano's inoculation experiments (1887-1889), repeated and confirmed by [Fridiano] Cavara, first proved the olive tubercle to be due to bacteria, but neither of these men described the organism sufficiently. Savastano called his cultures *Bacillus oleae-tuberculosis*, but, following Trevisan, systematic writers generally have spoken of *Bacillus oleae* (Arch.) Trevisan as the cause of the olive tubercle.

Smith, during the 1880's, like most other American botanists,

⁵³ Nosologie végétale—Les maladies de l'olivier et la tuberculose en particulier, *Comptes rendus* 103: 1144; Tuberculosi, iperplasia e tumori dell' olivo, *Ann. R. Sc. sup. Portici* 5, 4th fasc, 1887.

Proof of the bacillus was presented in, Il bacillo della tuberculosi dell' olivo, *Rend. della R. Accad. dei Lincei.*, 92, 1889.

⁵⁴ *An introduction to bacterial diseases of plants*, 389-411, at p. 394, Phila. and London, W. B. Saunders Co., 1920. A whole chapter of this book is given to "The Olive Tubercle" or olive knot.

was more absorbed in the study of fungous, than bacterial, diseases of plants. In 1889 Dr. A. Heinz, director of the Botanical-Physiological Institut in Agram (Zagreb), Croatia, described the bacteriosis of hyacinths (*Bacillus hyacinthi septicus*),⁵⁵ and his paper, "Zur Kenntniss der Rotzkrankheiten der Pflanzen," was published by the *Centralblatt für Bakteriologie und Parasitenkunde* in April of that year.⁵⁶ In December Smith reviewed in the *Journal of Mycology*⁵⁷ J. H. Wakker's "Contributions à la Pathologie Végétale" and discussed his "additional light on the gummosis of the hyacinth," his study of the fungus *Peziza tuberosa* and its relation to *P. bulborum* which attacks hyacinths and related Liliaceae, and other matters. But Heinz's paper was not reviewed, although in 1896-1897 when Smith would begin a series of articles on "The Bacterial Diseases of Plants: A Critical Review of the Present State of our Knowledge,"⁵⁸ he would follow his examination of Wakker's study of the yellow disease of hyacinths (*Bacillus hyacinthi*, 1883) by an elaboration of Heinz's *Bacillus hyacinthi septicus*. Smith then said that Dr. Heinz did not regard his organism "as in any sense a variety of *B. hyacinthi* Wakker."

In 1886 there was published in Germany by Die Landwirthschaftlichen Versuchs-Stationem of Berlin Dr. Adolf Mayer's account "Ueber die Mosaikkkrankheit des Tabaks."⁵⁹ Owing to the remoteness of its publishing medium, many botanists in the United States, including Smith for several years, did not read this important paper. In 1894, however, the subject having become "one of much interest," he reviewed its contents in the *Journal of Mycology*,⁶⁰ and in 1892 wrote Mayer evidently to inquire whether as yet he had isolated a pathogenic microorganism from plants affected by the disease. A reply, dated July 23, 1892, was written from the Proefstation der Rijkslandbouwschool at Wageningen, Holland, where Mayer was the director.

In 1879 he had there begun his study of the "variegated leaf disease" which had been troubling tobacco growers of the Nether-

⁵⁵ *American Naturalist* 31: 34-41, 1897.

⁵⁶ 5 (16): 535-539.

⁵⁷ V, 4 (Dec., 1889), pp. 224-226.

⁵⁸ *Amer. Nat.* 30: 797-804; 912-924, 1896.

⁵⁹ *Land. Ver. Stat.* 32: 451-467.

⁶⁰ 7 (4): 382-385, 1894.

lands for two decades. Extensive investigation, Harry Ardell Allard has since written,⁶¹ proved the disease to be

communicable by artificial inoculation; that the sap of healthy plants was not infectious; that an incubation period of ten or twelve days preceded the first observable symptoms; that the disease was persistent, appearing ultimately in all immature growing parts of affected plants; that diseased sap filtered once through filter paper still retained its virulence; that sap sufficiently heated lost its virulence; and that the seed of affected plants did not carry the disease to the next generation. Mayer failed to produce the disease in other solanaceous plants. Further, he found that liming, crowding the plants, sudden atmospheric changes, unfavorable conditions of whatever sort affecting the roots, i. e., mechanical injury, nematodes, parasitic fungi, etc., were not in any way responsible for the origin of the disease. . . . He recognized the sporadic occurrence of the disease in the field, and finally concluded that its spread must be through the soil, both in the field and in the seed bed. He recognized the fact, however, that a transfer of the disease by means of the soil had never been proved.

During the 1880's the study of fungus diseases had dominated research in plant pathology, and not infrequently maladies, later shown to be caused by bacteria, were attributed to fungi. Slowly now the study of bacterial diseases was beginning to gain the ascendancy it attained during the next decade, and the basis for a third major classification of plant diseases—those caused by viruses—was beginning to acquire outlines. For years the failures by research students to isolate specific pathogenic organisms from virus plant diseases (that is, by pure culture, reinoculation into healthy plants, and reproduction of the disease) had the effect of discouraging study of these maladies: or, if studies were made and no bacteria or fungi were found, of too often causing them to attribute the disorders to physiological disturbances. Occasionally, furthermore, these maladies were ascribed to a bacterial origin, and this notwithstanding no parasite had as yet been isolated.

Tobacco mosaic is today believed to have been the first disease in plants (or in animals for that matter) to be positively proved to be caused by a virus. Mayer's conclusion, however, was that it is of bacterial origin, and Smith's "rather literal rendering" of the European's first points read as follows:

The "mosaic disease" of tobacco is a bacterial disease, the infective

⁶¹ A review of investigations of the mosaic disease of tobacco, together with a bibliography of the more important contributions, *Bull. Torrey Botanical Club* 41 (9): 435-458, 1914.

organism of which has not yet been isolated so as to know its form and mode of life. The infective power of the disease from plant to plant under the artificial conditions of sap mixture has been established with certainty. Under natural conditions there is no plain infection from plant to plant. The seeds of diseased plants can produce sound plants.

From 1887 until 1894 Smith had been investigating peach yellows, another disease believed today quite positively to be due to a virus, and, although no parasitic bacterium had been isolated, a theory of a bacterial origin had been studied. This will be elaborated further. His interest in Mayer's study centered about the latter's efforts to demonstrate bacteria in accordance with Koch's canons. None of the bacteria which Mayer found, when inoculated into healthy plants, produced the disease. But the juice of diseased plants, when inoculated, did so, and by

rubbing up a plainly diseased leaf in a few drops of water, taking up a little of this thick, green emulsion in a glass tube drawn out to capillary size, and sticking it into the thick midrib of an old leaf so that it remained without reaching through to the back side, sound plants became badly diseased in nine cases out of ten.

Filtration experiments indicated as possible causes of the disease either chemical substances or "organized bodies small enough to pass through the pores of the paper. A clear filtrate was finally obtained by using a double filter," but, wrote Smith,⁶² "fluid passed through this possessed no infective power. Evidently the cause of the disease was filtered out and could not be a chemical ferment, for it is opposed to all known peculiarities of enzymes to be filtered out of solutions. The common method for the concentration of an enzyme, i. e., precipitation with not too strong alcohol from the crude juice and re-solution in water, was tried. This led to no substance which had infective power." Besides, that "an enzyme multiplies from itself" was unheard of. Nor could fungi be the cause since "they must assume at some stage some more easily visible form" and are too large to pass through filter paper.

In 1892 D. Iwanowski of Russia demonstrated that the virus of tobacco mosaic may be passed through a Chamberland filter without losing its power to infect.⁶³ By 1893 he had published at least twice on his researches on this malady, but Smith mentioned neither publication in his review of Mayer's work. By 1898

⁶² *Op. cit.*, 384-385.

⁶³ E. F. Smith, Fifty years of pathology, *op. cit.*, 21.

another plant pathologist of the Netherlands, M. W. Beijerinck, confirmed Iwanowski's filtration experiments and added knowledge. He, Smith believed,⁶⁴ "showed that the virus of tobacco mosaic without developing any visible growth would pass in ten days from the upper to the lower layers of an agar plate. He ascribed the disease, therefore, to a '*Contagium vivum fluidum*.'" Beijerinck, Allard⁶⁵ said further.

found that diseased sap so filtered as to be entirely free from bacteria still retained its power to infect healthy plants, in this respect confirming Iwanowski. He showed that a very minute quantity of this filtered juice produced the disease in immature, growing tissue. He held that dried mosaic material retained its infectious properties for some time, and likewise, that it was not rendered innocuous by remaining in the soil throughout the winter. Like Mayer and Iwanowski, he found that heating mosaic virus to the boiling point rendered it harmless. He proved that the virus traveled considerable distances in plants but produced obvious symptoms only in immature tissues. Beijerinck claimed that the soil around diseased roots may infect healthy roots and that plants in some instances apparently recovered from the disease temporarily. Previous to the work of Beijerinck all investigators were strongly inclined to establish a bacterial origin for it, although at that time no direct proof had been obtained.

It must be remembered that as late as 1882 "most of the very numerous papers on bacteria [had] treated the subject from a purely medical point of view," and only incidentally had any bearing botanically.⁶⁶ In America not until 1888 was the movement to establish agricultural experiment stations brought to full fruition. Adequate experimental laboratories and thoroughly trained students for the study of bacterial diseases of plants remained few until well into the 1890's, and few were the specialists and number of facilities in Europe.

During the summer of 1887 Scribner, accompanied by Professor Pierre Viala of Montpellier, France, author in 1885 of a special treatise on grape diseases,⁶⁷ visited the principal vineyard regions of the nation and investigated various grape-vine maladies. In 1887 Scribner, by a paper, "On a new fungus disease of the vine,"⁶⁸ announced to the Society for the Promotion of Agri-

⁶⁴ *Idem*, 27.

⁶⁵ *Op. cit.*, 438 f.

⁶⁶ W. G. Farlow, Accounts of the progress of botany for the years 1879-1883, *Smithsonian Reports*, 313, 1880; 391, 1881; 551, 1882; 681, 1883.

⁶⁷ E. F. Smith, Fifty years of pathology, *op. cit.*, 21.

⁶⁸ *Proc. Soc. Prom. Agric. Sci.* 8th meet., 74-76, 1887.

cultural Science their discovery of bitter rot of grapes. Their field study was followed by laboratory investigations, by Viala in France and by Scribner at Washington. In France for the first time the mature stage of the fungus causing black rot of grapes was found. Scribner obtained specimens from Vineland, New Jersey, and Neosho, Missouri, and in 1888 in two more papers, "New observations on the fungus of black rot of grapes"⁶⁹ and "Successful treatment of black rot,"⁷⁰ he reviewed the results of their studies, genetically connecting the "three stages or forms" of the fungus of black rot, described the past efforts in placing the forms in correct mycological systemization, and called attention to the disease's geographic distribution "from Canada to Florida, and west to Texas." He warned that the recent introductions of vine stocks from the east to California to "avoid the ravages of the Phylloxera" might take the fungus to the Pacific coast. The important point was that in New Jersey a successful treatment for the disease had been experimentally demonstrated by Colonel A. W. Pearson, a special agent of the Department of Agriculture. That confidence in the treatment was shared was shown in a letter of December 14, 1887 to Charles Sprague Sargent from W. A. Stiles who said: "The experiments of Scribner and Viala have been pretty well ventilated. I mean the study of the downy mildew and another kind [of grape disease] and its treatment with Copper Sulphate, and also the trial of the Copper Sulphate as preventive of black rot." During this year the control of black rot of the grape and some "decisive results . . . in controlling several potato diseases"⁷¹ strengthened the position of the federal government's work in the study of diseases of agricultural crops.

In 1888 Viala and Ferrouillat prepared a manual on the treatment of vine diseases.⁷² Sulphur and copper compounds and their uses, together with various types of apparatus for spraying and dusting, were described. Scribner told the Society for the Promotion of Agricultural Science that year that "proper applications of compounds having sulphate of copper for their base" would combat the black rot of grapes. But he also advised that Bordeaux

⁶⁹ *Idem*, 9th meet., 68-72, 1888.

⁷⁰ *Idem*, 72-73.

⁷¹ B. T. Galloway, Progress in the treatment of plant diseases in the United States, *Yearbook of the U. S. Dep't of Agric.* for 1899: 195.

⁷² Erwin F. Smith, Fifty years of pathology, *op. cit.*, 21.

mixture had given the best results, better than *Eau celeste* and other compounds. Bordeaux mixture was found a dependable remedy for potato diseases.

Throughout the first half of the year 1887 Scribner's assistant, Erwin Smith, contemplated enrolling at either Harvard University for further study of fungi or at the Johns Hopkins University in Baltimore for special biological study under Martin or Welch. In fact, on April 5, 1887, he had gone to Dr. John S. Billings of the Surgeon General's Office at Washington and obtained a letter of introduction to President Daniel Coit Gilman of the Hopkins University. In June, however, Smith was ordered by Scribner to complete his report on the potato rot, and prepare to take up a commission as of July 1 to investigate a serious disease threatening the American peach-growing industry—the yellows. Peach yellows was a disease which had baffled the scientific ingenuity of even Thomas Jonathan Burrill and Joseph Charles Arthur.

Burrill's and Arthur's studies of peach yellows had been incomplete. Burrill in 1881, when he announced with "much confidence" his belief that peach yellows, like pear and apple twig blight, was of bacterial origin, cautioned his readers that he had not given the "full investigation" to the peach malady as to the pear disease.⁷³ Arthur, whose budding and grafting experiments were made to reproduce the yellows in healthy trees and by several years preceded the work of Smith, failed through no fault of a proper scientific procedure but because, as he explained to Smith by a letter of December 4, 1890, "the bud or graft died before becoming fairly started." Arthur by 1891 regarded his work at the Indiana Agricultural Experiment Station as "so diverted into the domain of vegetable physiology that pathological subjects," he said,⁷⁴ "have fared poorly for some time." He, however, told Smith in his letter of December, 1890: "I am delighted to hear that you are progressing so favorably with your work on peach yellows. If you complete a full explanation of the cause and conditions of the disease it will be a notable achievement and a brilliant triumph for science."

In July, 1887, Smith went into the field to investigate peach yellows. On August 3, C. W. Garfield wrote to him:

⁷³ *American Naturalist* 15: 527, 1881.

⁷⁴ Letter, Arthur to Smith, May 14, 1891.

I am glad you are hard at it and I am delighted to learn of your progress. At the peach meeting of our local society here [Michigan] the other day one man reported several serious cases of yellows in his orchard. We have but one form of the disease in our State and this is the virulent type. Your description of tufts of wiry growth indicates that the trees were in what we denominate the second stage of the disease, the first being that of premature ripening. I am glad you are interesting Bailey and Troop. I would be glad to assist you in any way I could but I have no peach trees or seedlings on my place and could do nothing in the way of budding on that account. I shall be greatly interested in your research in New Jersey—where they have “resolved that the disease is not contagious.” That is like the resolution passed by the Farmers Club at the close of a discussion on the Hessian Fly, “that the best way to manage was to sow wheat early and have a frost occur early.” Just how to manipulate the frost king the record saith not.

I enclose you the announcement of our American [Pomological] Society meeting in Boston. If in your investigation you run across some testimony that can be concisely stated, will you not favor us with it. . . . We had one man in west Michigan that did not believe the disease was contagious and was so certain that he drew a tree condemned and hauled out, all around among his trees. The next season a streak of yellows marked “the path in which he trod.” If the trees from which you take diseased pits for planting have the genuine yellows as we know it the chances are that not one in a hundred will grow. I wish you would crack open a lot of the pits of diseased fruit and note the appearance of the seed and germ. We find it true that the disease is worst on heavy strong soils. Be sure and note the character of soil with reference to fertility, and especially whether barnyard manures have been freely used.

We can control the disease perfectly among thrifty orchardists because they will dig out every infected tree as soon as the disease develops. The malady is not now spreading in our State on this account.

Smith began his studies of peach yellows in Michigan. While there, he arranged with Bailey, who was still a professor at Michigan Agricultural College, to bud trees with scion stock to be sent him. Trelease, professor in charge of the Shaw School of Botany at St. Louis, regretted that he could not undertake at that time a series of experiments there. In August, Bailey suggested that Smith enlist the research facilities of Professor William Rane Lazenby of the Ohio State University at Columbus and Professor L. R. Taft of the University of Missouri at Columbia.

Smith left Michigan and began to study the peach disease in eastern Pennsylvania, New Jersey, Delaware, and Maryland. In September, Scribner was commissioned by the government to go by way of Texas to southern California to study a new vine disease

which was doing serious damage to the vineyards there. Before he left, he advised Smith that he and Beal had strongly recommended him for membership in the Society for the Promotion of Agricultural Science. Since the start of his field studies, Smith had been sending to the Department at Washington specimens of fungi, especially those concerned in diseases of cultivated plants. In turn, he was furnished drying paper, alcohol, twine, and other accessories to preserve his materials for future laboratory examinations. Some of his fungous collections may have been made for purposes of completing his report on the distribution and severity of the potato rot. He was still working under an order to place in manuscript his deductions as to the history and development of this malady and the results of his laboratory "observations on the growth and development of the fungi upon the tubers." During the previous spring, winter, and autumn, he had used a Zentmeyer microscope, and now Scribner wanted to exchange this for a Zeiss instrument which Smith had with him in the field.

Scribner did not always agree with Smith. But the chief allowed his assistant freedom of choice in research. Scribner was skeptical of whether Smith would find "a specific origin or cause for 'yellows,'" and Smith himself was not altogether convinced that he would locate a parasite. He collected soil samples to study peach fruit deterioration on the basis of a lack of proper nutrition. Chemists of the Department were few. But Scribner promised to secure, when possible, their services to analyze the soil and wood samples which Smith submitted. Scribner approved Smith's plan of "treating diseased trees with special fertilizers."

That autumn the chief ordered from him the completion of his report on the potato rot and a report on the progress made in his peach yellows investigations. Smith protested and won the privilege of completing his yellows research as he had planned. Scribner warned that funds might not allow their completion. The determined young research scientist, nevertheless, began systematically to examine at the microscope the interior structures of healthy and diseased peach twigs. Memoranda of November, 1887, indicate that he stained cross sections of materials and examined carefully their tissue and cellular structures, especially those of the cambium. All this was done, presumably, to locate a parasite or explain the conversion from healthy into diseased activity, once the malady had taken hold. That month, when Smith was in

the midst of this work, Scribner notified him that by March 1, he would be expected to present

a full report on the subject of your present investigations—Peach Yellows. The length of time you were in the field will enable you to minutely, or, at least sufficiently, characterize the disease upon all the organs of the tree, and your microscopical examinations of healthy and diseased tissue will enable you to describe the essential characters or changes which take place in the latter. The characters indicating the *effects* of the disease must be familiar to you now, and if you have thus far failed to discover anything of a definite nature (or fail soon to do so) which may afford a basis for an investigation, or a *clue* to the cause of the disease, continued examinations of tissues appear to me to be a needless waste of time.

Such chemical analyses as will show the difference in composition between the healthy and diseased tissues, will be made and the results placed at your disposal.

In your report you will observe the following sequence of topics: (1) Historical. (2) Characters. (3) Losses-distribution and effects. (4) Conditions favoring the disease. (5) Conclusions.

It is hoped that your studies will enable you to suggest some rational mode of treatment, or, at least indicate what experiments may or may not be tried with the view of preventing or curing the disease.

Smith agreed to prepare this report with the understanding that he be permitted to "make necessary additions or corrections at any time during the remainder of the fiscal year." August 24, 1887, Garfield had offered to write to Commissioner of Agriculture Colman and commend Smith's plan of work, arguing, if need be, that Smith "be continued long enough to carry th[rough his] investigation to a finality." He told of a Michigan nurseryman who had performed some "careful experiments," growing trees from yellows pits and "budded diseased buds into healthy seedlings." Trees, he said, may develop "unmistakable signs of yellows before arriving at puberty." T. T. Lyon, the nation-wide known Michigan horticulturist, a scholar whose acquaintance Smith had enjoyed before leaving Ionia, had imported trees from Georgia that had developed yellows the same season they were transplanted.

September 5, 1887, Smith obtained from Dr. W. S. Maxwell of Still Pond, Kent County, Maryland, permission to establish an experimental orchard at the Point, a locality of rare beauty near Chesapeake Bay. Two weeks later he received a letter from David Pearce Penhallow, who, when botanist at Houghton Farm, New

York, and while at the Massachusetts Agricultural College and experiment station at Amherst, claimed, with Dr. Goessmann, to have discovered a cure for peach yellows. In fact, he told Smith that at Amherst he would find "the original trees experimented upon and cured."

Goessmann, it will be remembered, had read a paper in 1882 before the third meeting of the Society for the Promotion of Agricultural Science, "Observations regarding the Yellows of the Peach," in which was projected a theory of a physiological disorder, a nutritional disturbance, induced by poverty of soil, some lack of essential elements of plant food, or some injurious soil substance. Samuel T. Maynard, Goessmann, and Penhallow had participated in the research. The last named was a graduate of Massachusetts Agricultural College. For a while after his graduation in 1875, he had studied in Japan, returned from there in 1880, worked for a brief time at the Gray Herbarium of Harvard, was employed as botanist and chemist at Houghton Farm, and then in 1883 went to McGill University where during many years as professor of botany he took a prominent part in the scientific research leadership of Canada, achieving renown as a paleobotanist and plant physiologist.

During the summer of 1882 Goessmann had recalled him to Amherst "to study the condition of the cellular tissues in branches collected on the 11th of November, 1881, from trees thoroughly diseased, and also from trees which were once diseased," but by the time of Goessmann's address in August, 1882, were "in good healthy condition." Goessmann himself chemically analyzed mineral constituents of both branches and healthy and prematurely ripened peach fruit. The theory of elaborating first causes prior to fungous infestation attracted Smith's interest. To procure pamphlets published by Penhallow while at Houghton Farm and the results of his and Goessmann's analyses, Smith, on August 16, 1887, wrote to Penhallow who replied six days later:

The methods of analysis employed by us were the same as would be employed by any good chemist and vegetable histologist. If you instruct the Department Chemist that you want certain elements determined in the wood, he will be sure to give you an analysis that will compare with those published by us and by others. The elements you enumerate are those which should be determined; others would be of little or no value. I would suggest that you will find a soil analysis, however accurately made,

by no means a correct indication of the capacity of the soil in meeting the requirements of nutrition. If you are to get exact data in this direction, you will probably find it necessary to institute a careful series of feeding experiments to compare with a parallel series of analyses of the wood and fruit.

You are probably aware of the fact that our mode of treatment has been tried in New Jersey for several years past, with a considerable degree of success.

Penhallow accredited Goessmann with first advancing "the idea upon which [their] final success was based." He later advised Smith to prune his orchard in the autumn, being certain to cut off all conspicuously diseased wood; then, with the special treatments, new and healthy wood might be formed. During both spring and autumn, before and after leafing, treatments of Dissolved Bone-black, Muriate Potash, and Keiserite were to be applied at least a foot from the trunk, spread as far as the large branches extended, and worked into the soil surface. The treatments were to be mulched with sod and straw. Rain would bring the mixture into direct contact with the roots. "Some effect may be noted the first season," wrote Penhallow on September 19, "but more marked the second." If the disease had not progressed very far, that is, if the trees were not very far gone, two or three years would probably complete the cure. But, to prevent recurrences, careful treatments later would be necessary. Penhallow told Smith to consult Dr. George H. Cook, director of the New Jersey State Agricultural Experiment Station, located at Rutgers College, New Brunswick, Mr. H. H. Appleton of Odessa, Delaware, and Dr. Goessmann.

Smith, however, did far more. He sent out hundreds of letters of inquiry, information blanks, and specially prepared forms. In November at Washington he discussed the disease and its manifestations with Professor Viala. He gathered together every available book and report on the subject he could find. He studied the history of the peach-growing industry in the United States, regionally ascertaining as best he could when the disease had first appeared. He tabulated data on the effect of early autumn frosts, of cold winters, excessive rain-fall, unfavorable moisture and temperature conditions, and he analyzed in relation to the disease such phases of cultural practice as neglect of cultivation, pruning, and proper fertilizing. He did not constitute either Dr. Goessmann

or Dr. Penhallow his final authorities on the matter of soil exhaustion or infertility, although he spent much time studying their conclusions and information sources. In the *Annual Report of the Secretary of the Michigan State Pomological Society* for 1871, he found that Dr. Kedzie and Dr. Beal had made and published some chemical and microscopical examinations of peach yellows. Work also had been done at the Connecticut Agricultural Experiment Station. He went among orchardists and nurserymen and discussed such questions as to what influence, if any, excessive use of nitrogenous manure had on producing or spreading the infection, what degeneration of stock could be attributed to continued propagation by budding, and again and again he verified his belief that yellows attacked seedling trees no less than budded ones even as it attacked recently originated varieties and the older or oldest tree stock. The disease attacked trees grown on land nearly exhausted by cropping, and it was known to appear on trees grown on virgin soil, that is, land cleared of its original forest within less than a decade.

Smith admitted some validity to the claim that the disease was propagated from imperfect or infected pits, but little exact knowledge on this point was available because of a dearth of carefully planned experiments. Arthur's experiment at Geneva, New York, and those of Michigan nurserymen about whom Garfield had written, and experiments in Pennsylvania and Maryland, had yielded inconclusive results. So Smith had arranged with Bailey, Troop, Taft, and other trustworthy horticulturists, to grow, under controlled conditions, hundreds of budded and healthy trees to study the origin and communicability of the infection, if found contagious. Care, of course, was required to preclude the possibility of introducing the disease in orchard areas not yet affected. Smith, at his father's place at Hubbardston, Michigan, set out several hundred trees. The experiments again proved inconclusive, but years were required to settle the many points under investigation. Bailey secured a field north of the Agricultural College grounds and, with the help of C. S. Crandall, set out an experimental orchard. He in 1888 offered Smith laboratory facilities for his work with the microscope. Smith, aware that much further field and laboratory research was necessary, advanced few definite theories this year. But on this point he seemed rather sure:

There is undoubtedly some reason for believing that the disease is propagated by diseased pits. I can not state positively that trees grown from premature peaches will develop yellows, but I think it likely. There can be no doubt that such seeds have an enfeebled vitality, and it is not likely that they will give rise to robust trees. How great the danger may be from this source I am unable to say. Some experiments of my own lead me to think it is overestimated. Exact experiments to determine this point have not been very numerous.

"When I began my field-work, in July, 1887," Smith confessed, "I had no favorite theory to advance, but gave very careful attention to [Goessmann-Penhallow's theory], among others, hoping, for the sake of the fruit-growers, to be able to confirm it."⁷⁵ Peach yellows, with plenty of justification, was exciting alarm throughout eastern United States. The public press and magazines of nation-wide circulation, aware that whole areas of the peach-growing industry already had been destroyed and more were threatened, gave space to accredited treatments and supposed cures. Smith personally visited the orchards where treatments or control methods were believed to have been effectual. September 3, 1887, he wrote to Scribner:

To settle more definitely pro or con the effect of mineral manures on peach yellows, I would like to have carried on, at suitable places in Maryland, and Delaware or New Jersey, as may seem most advisable, a series of *four* feeding experiments of one acre each (100 trees), reserving in each case another acre in alternate strips for control experiments, making in all 800 trees (8 acres) in localities, 400 being under treatment and 400, equally sick over, being reserved untreated for comparison, the feeding to continue long enough (two years at least) to reach well-defined results. The remedy I wish to try is Penhallow's peach mixture, as recommended in the Houghton Farm Bulletin. . . . The greatest uncertainty exists in the minds of peach growers and such a series of experiments would be watched with very great interest by peach-men from Connecticut to Georgia and from Michigan to Texas. Peach growing on [the Chesapeake peninsula] is in a bad way and unless something can be done to arrest "Yellows" the orchards will be swept out, as they have been largely already in Cecil County, Maryland, and New Castle County, Delaware, and are now being destroyed in [the Dover section] and north Kent County, Maryland. The peach is the great money crop here, and the loss of the orchards not only produces "hard times" but leads to a very general depreciation of real estate. A farm on which peaches can no longer be grown is not worth nearly as much as it was before.

⁷⁵ Peach yellows: a preliminary report, Bulletin 9, Sect. Veg. Path., U. S. Dep't of Agric., 126, 143.

Thus were begun Smith's "Experiments with fertilizers for the prevention and cure of peach yellows, 1889-1892," the results of which formed the materials of Bulletin 4 of the Division of Vegetable Pathology of the same title, published by the Department in 1893. In 1888, when he published his preliminary report on peach yellows, Bulletin 9 of the Section of Vegetable Pathology of the Botanical Division, Smith admitted he had not been able to confirm the results of Goessmann's and Penhallow's experiments and analyses. Formal authority to conduct the experiments was not obtained until the autumn of 1887. February 17, 1888, T. T. Lyon, president of the Michigan State Horticultural Society, advised Smith that he felt "very sure that [he would] reach the conclusion, at least so far as Western Michigan [was] concerned, that soil starvation has little to do with the development of Yellows. . . ." Confidence in legislation which required the removal and eradication of infected trees as soon as discovered was being built up, since more and more this appeared to be the inevitable and only solution of the peach yellows problem. Not even the fact that the law would be enforced under official government supervision minimized its importance to orchard owners. January 13, 1888, Lyon described the history of the Michigan law to Smith:

I drew the original "Yellows" Bill, which was enacted as drawn although subsequently twice amended, and each time made less effective by interference with its capacity for prompt execution. The scope however is not changed. I (without consultation with others on this point,) included the nectarine because it is in fact a peach; the only difference being its smooth skin; and for the reason that it is always budded upon peach stocks. The almond was included not because I had known it to become affected; but mainly because it also is grown on peach stocks, which would be liable to become diseased. The Almond also is in fact a peach, with the seed developed instead of the pulp. I had some years since procured the crop of an Almond growing at South Haven—yielding nearly a bushel of seed, and planted this seed intending to bud the young trees with peaches, as an experiment. During the spring these seedlings gave decided indications of disease, a great many failing to grow; while during that summer, the parent tree developed the yellows. . . . I have no knowledge respecting the existence of yellows in California nor yet in the peach growing regions of the eastern Continent. The occasion or cause must be more or less climatic, even if the primary cause is in fact bacterial. . . .

Wesley Webb, editor of the *Delaware Farm and Home*.

requested the Department of Agriculture to send H. E. Van Deman, noted horticulturist, or Smith, and both men if possible, to attend a meeting of the Peninsula Horticultural Society to be held at Dover on January 11, 1888. In a letter to Smith of December 26, 1887, he asked:

How are you coming on with microscopic investigations of Peach Yellows? Have you yet seen the supposed bacterium that by excessive multiplication turns this delicate tree to such a sickly hue, and fills our Delaware peach growers with consternation dire? . . . If you have come to any definite conclusion and we can have the full benefit of your work so far we shall be very glad to pay your expenses. . . . Dr. Maxwell will be here with a long paper on Pears. . . .

On January 22, 1891, at Easton, Maryland, Smith would address the Society on "Peach Yellows,"⁷⁰ present his conclusions as to the contagious nature of the disease, and announce that that year Bulletin 1 of the Division of Vegetable Pathology would fully treat the subject of its communicability. He described his inoculation and excision experiments 1887-1888 and his reasons for recommending legislation and "the ax and the fire-band" to check the disease's spread. "In answer to a question," he said, "I have isolated several organisms from the diseased tissues, but I am not yet able to say whether any one of these is the cause of the disease. This part of my investigation is not yet completed. . . ." Webb invited Smith to attend the Chestertown meeting of the Society January 28-30, 1890, and tell his "story about the fungus that causes the [peach] rot."

In 1887 Smith had recommended that "the leaf curl, the rot, and some other known parasitic and seriously injurious diseases of the peach should receive attention" as a part of his investigation. Since 1888, however, many growers on the peninsula voluntarily had begun digging up their orchards on account of the yellows, and at a meeting of the horticultural society a committee had been appointed to secure passage of a law in Delaware and Maryland to compel all orchard owners to destroy diseased trees. In January, 1888, Webb again asked Smith whether he had found bacteria in the diseased wood. On February 12 Dr. Maxwell told him that he believed that the disease was caused by bacteria.

Smith refused to "make statements, until [he had] verified them

⁷⁰ *Trans. Peninsula Hort. Soc.* 4: 55-61, and reprint.

beyond the possibility of a doubt." At some occasion between the years 1888 and 1890, presumably in the state of Delaware, he spoke his belief that the malady is contagious because, said he,⁷⁷

It does not appear to be due to poverty of soil. Chemical analyses are contradictory. They do not agree in showing a deficiency of lime, potash, or phosphoric acid. Copious applications of these and other chemicals have not cured or arrested the spread of the disease. Trees set in place of those dug out on account of yellows are not more likely to contract disease than others in the same orchard. *It does appear to be a contagious disease.* My inoculations show that it can be conveyed from tree to tree by budding, and it is therefore a foregone conclusion that part of it must come from the nursery. The disease also appears to spread in the orchard from tree to tree but in what way I do not know. Not all of it comes from the nurseries, because old trees are not exempt from its ravages.

His efforts to establish the nature of the communicability of the ravage were interrupted for a time. On February 20, 1888, Commissioner Colman wrote: "your services will be dispensed with after February 29th instant." He asked that his report be completed immediately that he might "present to Congress additional evidence of the importance of continuing the investigation."

Four days later there followed a letter from Scribner:

The circumstances which have necessitated my discontinuing the work on peach yellows I deem most unfortunate for the Section, and I think I can appreciate your personal feelings in being obliged to give up now a work which has so completely occupied your mind for the past 8 months. You must certainly understand that this work would not be interrupted at this stage in its progress were it possible to prevent it. When I proposed to the Commissioner that this work be taken up, I recommended you as being the man whom I thought best qualified to carry it on, and I look to your preliminary report to convince him that I was correct in my choice. *It is imperative that this report be sent in on the first of March.* The Commissioner will doubtless continue the work with the beginning of another year, when I trust we may be able to secure your services.

Charles W. Dabney Junior, President of the State Agricultural and Mechanical College of the University of Tennessee and Director of the state agricultural experiment station located at Knoxville, had written Scribner on February 3 and asked him to recommend "a worker in the botanical field for our Experiment Station," who could also do some teaching in botany or horticul-

⁷⁷ Memorandum undated entitled "Synopsis of Address by Erwin F. Smith on Peach Yellows" and found among his collected papers.

ture. Scribner recommended Smith; and on February 10 Dabney invited Smith to apply for the position of botanist at the experiment station. While research was to be foremost, the offer called for Smith to teach a class in elementary botany and an advanced class in either botany or agriculture.

Dr. Dabney had discussed this offer with Professor Spalding, and it is likely that to obtain Spalding's opinion Smith went to Ann Arbor. "Want of funds" was the only reason ascribed by Scribner for discontinuing Smith's services. On October 31, 1887, Dr. Beal, "as a friend of progressive agriculture [and] on [his] own hook," had asked Smith whether he would accept an offer "to work on fungi" at the Michigan Agricultural Experiment Station when, under the Hatch Act, that station should be fully organized by February 21 with President Edwin Willits of the College as director. L. H. Bailey also urged his friend and fellow-botanist to investigate this position.

Moreover, an interesting communication from the state of Delaware was received. On February 25, 1888, Wesley Webb at Dover advised Smith: "Our college directors are looking for a man to take charge of the Experiment Station. I wish you would apply." This was from the peach yellows region where the youthful investigator had done much of his field research. He took this suggestion seriously, and again letters recommending him for a position with another college went forward, this time to J. Alexander Fulton who was chairman of the committee on selection.

Professor A. J. Cook of Michigan Agricultural College wrote:

I learn that there is some thought of appointing Mr. Erwin F. Smith to the Directorship of your Station under the Hatch Bill. Mr. Smith is one of my students. I regard him as a very superior student, and as a scientist of great merit. As a fungologist I doubt if he has a superior in some respects in the Country. He could do exceptionally good work in Delaware where the Yellows prevail. We tried to secure Mr. Smith for our Station but failed.

Beal, Garfield, Barnes, Coulter, and others, sent letters which Chairman Fulton found "highly creditable to [Smith] as a scientist and gentleman." Arthur of Purdue University wrote his recommendation direct to Smith:

Your practical knowledge of agriculture and horticulture, and your

training as a student and investigator of the sciences which underlie these pursuits peculiarly fit you for the position. I shall take pleasure in lending my influence to secure your appointment, not only because I think you will find it a suitable place in which to exercise your powers, but because I believe the interests of the state, particularly the fruit interests, will be promoted by your appointment. Consider me at your service.

In the spring of 1888 Bailey, at first refusing the directorship of the agricultural experiment station at Cornell University, accepted an offer from there of the first American professorship of experimental and practical horticulture. At Michigan Agricultural College he had built and equipped, if not the first, one of the first American experimental laboratories devoted exclusively to work in horticulture. His ambition was to remain in research, and on May 16 he wrote Smith his belief that any directorship was not desirable for the specialist because of the excessive amount of time required for "purely executive matters."

Nevertheless, Smith was interested in the Delaware position because in March he had been advised by Fulton that the state might appropriate special funds for an investigation of peach yellows. As late as May he was not definitely sure that the federal investigation would be resumed. Early in March he submitted to Scribner a copy of his report "so far as written" and received his general criticism that what was wanted was "an account of the investigations [he] had made in the field and laboratory together with a brief history of the disease." He learned from several sources that Congress was being asked for funds to renew the investigation sometime in the spring or at the beginning of the next fiscal year. In fact, on March 30, Scribner requested from him a special statement for Congress "as to what [he had] accomplished and the probability of reaching definite results within a reasonable period." He returned to the Section of Vegetable Pathology his microscope, note books, and a package containing forceps, shears, and other equipment. But he retained his "dried and alcoholic specimens, negatives and photos," probably to prepare slides to illustrate his account of his histological examinations.

Since the spring of 1887 he had had in mind taking advanced study in biology at Johns Hopkins or in fungi at Harvard. He had done neither, and now with an opportunity before him he had chosen to continue his orchard studies at his home at Hubbard-

ston and to complete his histological examinations at the botanical laboratory of the University of Michigan.

On May 8 the Board of Trustees of Delaware College met and elected a man other than Smith as director of their experiment station. Frederick D. Chester, professor of agriculture, besought Bailey to recommend "a good man to act as Horticulturist to our new Experiment Station to be organized under the provisions of the Hatch Act. The horticultural interests of Delaware," he wrote, "are very important, and this position is in my mind the most important one on the station." Bailey forwarded his letter to Smith. Wesley Webb urged Smith to seek "an appointment as special agent to investigate the yellows," and assured Smith that "the fruit growers [would] raise money to pay [his] expenses." But no final arrangements were ever made.

In June Smith received an inquiry of interest from Acting Commissioner of Agriculture F. C. Nesbit: "Will you," he demanded, "please inform this Department whether, from your investigations of Peach yellows, you can assert that the disease *is* or is *not* caused by a parasitic fungus."

On November 19, Burrill confided to Smith that he had "found a living organism in the diseased bark but [was] sure of nothing further yet." Smith had sent him some trees for planting and observation but, despite good care, the young ones died, and those of a shipment received in the fall grew, "showing characteristic 'yellows.'" He also sent a box of peach fruit, "exactly" what Burrill wanted. Cultures were made at once. But on July 26, 1889, he wrote: "I am well convinced that there is no parasite unless it is of the bacteria—or other micro-forms, but am not certain that it will prove anything." On account of other work his peach yellows research had been "comparatively resting awhile." Smith answered immediately and on August 5 the Illinoisan explained that all he had found was "the organism which [he had] supposed the thing before." He had uncovered no "positive information of [the organism] in the fruit," was not confident of the bacterial theory, and wanted specimens of inoculated trees, promising in return a culture.

Where in the United States was there a reputable scientist who could have answered positively the inquiry of Acting Commissioner Nesbit? Smith and Burrill, with their characteristic honesty and candor in science, could not. The most that they could have said

was that each believed the disease to be of parasitic origin and caused, as Burrill said, by some "micro-form."

Some other scientists were doing some field study of peach yellows. In April 1888, for instance, W. R. Lazenby of Ohio had agreed to take "about three hundred trees . . . 250 inoculated and 100 unbudded," if desired. He was one of many American scientists who protested the discontinuance of Smith's peach yellows investigation. He wrote that there was "a demand for a mycologist" at the Ohio experiment station and he was hoping that within a few years funds would be provided. J. Troop at the Indiana experiment station agreed to take seventy-five to one hundred trees. L. R. Taft of the University of Missouri agreed to take as many as Smith sent. But Smith and Burrill were the acknowledged students of the malady and their research included both field and laboratory examinations.

On March 7, 1888, A. N. Prentiss of Cornell University deplored the suspension of Smith's investigation and promised that he would "take pleasure" in endorsing him for any position. "It is uncertain as yet," he wrote, "as to what will be done here in the way of Bot[anical] work for the Ex[periment] Station. It may possibly run in the line of mycology—but the salary thus far contemplated—\$750—is quite too insignificant for any one who has already done any work in the subject. I hope the right thing will turn up for you speedily." Garfield advised: "I should not sacrifice myself on the altar of Science were I in your place. If a good place were offered in any other direction, I would drop yellows or blues and delve in a field where payments would be made for services rendered."

At the University of Michigan Spalding had placed Smith during the second semester in charge of his department's "Microscopical Laboratory." A class of twenty-four students met on Mondays and Fridays for laboratory or quiz and other days of the week for regular laboratory work. He lived at Ann Arbor until June and possibly until August. During May and June he lectured on histological subjects, and his note book reveals a wealth of learning in morphology and physiology, and some attention given to the histology of the peach. Most of the year 1888 was devoted to completing his preliminary report on peach yellows, which was submitted on November 10 to Beverly Thomas Galloway, the newly appointed chief of the Section of Vegetable Pathology at

the end of October displacing Scribner who went to the University and experiment station of Tennessee.

September 29, 1888, Smith jubilantly wrote to Farlow that "Mr. Galloway talks as though he desires me to continue the investigation." Farlow was then at work on his North American bibliographies of mycological, and later phycological literature, and Smith desired to obtain a copy of part I, Polypetalae, of his and Seymour's *Provisional Host-index of the Fungi of the United States*, part II, Gamopetalae-Apetalae, of which was to appear in September, 1890, and part III in June, 1891. Sixteen months of continuous research had gone into his preliminary report by the time Smith completed it. Even then, he pointed out as many matters which needed further study as those on which his mind was fairly well made up. He adhered to his theory of a disease caused by a microorganism. He believed he had clearly shown that the disease was communicable and that other prevalently held theories were at least in part discredited.

The spread of yellows from diseased buds to healthy stocks, which I have carefully verified, points strongly to some *contagium vivum* as the cause. . . . Among supposed causes deserving further inquiry I should place root-aphides and root-fungi. I am inclined to believe that neither one is at the bottom of the trouble. . . . I can think of nothing else but micro-organisms. . . . I write this paragraph with ease, but the work itself is full of difficulties. Nature does not yield her secrets upon the mere asking. . . .⁷⁸

February 1, 1889, Commissioner Colman commissioned him at a salary of \$1,800 a year "to continue his field and laboratory investigation of peach yellows, and to prepare a special report on which particular attention shall be given to the microscopic appearances of healthy and diseased tissues, with a view to settle, if possible, the parasitic nature of yellows."

Smith accepted this commission and early in August had returned to Washington. He began to reside at 229 Eighth Street Northeast and soon was busy at field examinations. Prentiss and other botanists expressed their pleasure on learning of his reappointment "to go on with the important work so promisingly begun—on the peach yellows. I hope," wrote the Cornell pro-

⁷⁸ Bulletin 9, Botanical Division, Section of Vegetable Pathology, U. S. Dep't of Agric., 179, 1888; see also, Report on peach yellows, by special agent Smith, *Report of the Comm. of Agric.* for 1888: 393-398 at pp. 395-396.

fessor, "you will find no further interruptions, but that your efforts to get at the bottom facts of this mysterious scourge may meet with complete success."

On October 15 Spalding wrote: "I hope you will keep on with the 'yellows' though it would be a gratification to have you working here again." He was proud of his class of four students studying fungi and twenty-six students in biology and advanced work. Smith and he exchanged some letters concerning a spot disease of quince leaves. But the most important question in need of immediate settlement was: how was Smith to complete his work, already started, toward getting his doctorate in science at the University of Michigan? In his preliminary report on peach yellows he had described what investigations remained uncompleted. He applied to the university faculty for permission to combine the work toward his doctorate with this which was now tantamount to an outline of his plans for the work of his new commission. On November 9, 1888, Alexander Winchell, professor of geology and palaeontology, notified him that the faculty unanimously had granted him "permission to study in absentia." Spalding's letter of three days before explained more fully. It read:

Your letter came yesterday, just before faculty meeting. Dr. Winchell presented your case and after some explanation on my part as to the necessity of your having an opportunity to conduct the investigation in the field and to do a portion at least of the work in Washington, the faculty voted to permit you to go on with the work *in absentia* and to present yourself for examination in June next.

At the same time the feeling was expressed that it was desirable for you to do a portion of the work if possible here, and from what you had written I took the responsibility of saying that you would work here a part of the year if you could. So the matter is left with you, but if things shape so that you can be here for a semester, or for some time, it will be agreeable to the wishes of the faculty. They are disposed to give you every chance, but at the same time to hold to the principle that it is best in almost all cases for the candidate to be *on the ground*.

Now about the book. We *must* go through with it and we shall find no better time if we wait ten years. Let us make a break and *do it* even if we come far short of our ideal. Even such a little book as Winter's, if illustrated and brought down to the times and with the ideas of *Bekämpfung* worked in, would be far better than to leave the thing any longer. Now I propose this—My class will be working (I think) a good part of the year on *Peronosporae*, *Uredineae*, and *Ustilagineae*. I will write up those

groups and have them done by July 1st, if you will take the Ascomycetes with the imperfect forms and get them done by the same time.

Spalding with the aid of his class already had commenced his work on the book. He had urged that, unless a tempting offer from the United States Department of Agriculture hove into sight, Smith should return to the University of Michigan that winter, work with the students on fungi, finish the book, obtain his degree in June, and be prepared to accept a position as professor of botany and horticulture for which he had been recommended by President Angell and Dr. Spalding to Professor Scott of Rutgers College. The university's botanical laboratory had undergone some repairs, and a few improvements had been added. Spalding had been bringing together a herbarium for mycology. Specimens had been obtained from Smith and other botanists of the nation. With Sanford's aid, drawings of species in *Cystopus*, *Peronospora*, *Puccinia*, *Ustilago*, *Ascomyces*, *Podosphaera*, *Uncinula*, *Sphaerotherca*, *Fusicladium*, *Oidium*, and other genera, had been made. Materials had been photographed, notes taken on distribution. Spalding valued his correspondence with Smith, since the latter kept him abreast of the times by writing to him what diseases were being given attention at Washington. "What you say about the foot rot in Florida and the vine disease in California," said Spalding on November 27, 1888,

makes me ache to have some one ready to take it up and if Galloway is not in too much of a hurry I have little doubt that in due time he can be provided with a trained man from our laboratory. . . . One thing we can do—wonder if it will not be worth while—we can put together our pieces of special study some time during the year and let Mr. Galloway have them for the next annual (or bulletin).

He made a specific offer:

Towards the end of the year I can give each of the class a species to investigate and report on, or we may, perhaps, if we do not find it too much of an undertaking prepare a joint paper on the *Pathological Changes induced by Parasitic Fungi*. This you know is what is coming "bye and bye." We have done enough of it here already to see a little of its importance and what an immense amount of work it is. . . . Do you know what a pest the tomato rot has been this year? I have alcoholic material and want to get to work at it. Is there a lot of material at the dep[artmen]t, or has no one sent it in? Then there is the melon disease. A student from Ohio brought me a lot of material and reported badly

damaged crops of melons last summer. Want to know all about it. I should feel happier if I had just the class in fungi and no other teaching to do. . . .

Smith supplied Spalding with many specimens of diseased plants for study. Desiderata from the professor sometimes listed as many as a dozen and a half special requests, and the young Department scientist advised him of newly discovered ailments. He sent to the laboratory grape-vine stems and leaves from California showing an "obscure" malady which Newton B. Pierce was to study. A. C. Eycleshymer was to begin his important special study of *Plasmodiophora*.⁷⁹ By the end of the year Spalding had prepared a general account of *Plasmodiophora*, had finished numerous special investigations, written up the *Peronosporae*, and was starting the *Uredineae*. He wrote Smith December 27th, 1888:

The class in fungi are *workers* to the last degree. [Frederick Charles] Newcombe will take up the Black Knot and carry on its study through the winter, reviewing all of Farlow's statements and extending our knowledge of the subject if possible. [N. B.] Pierce is working on germination of *Phytophthora* and will continue for a time. There is reason to believe that no one has ever given a full account of the extraordinary changes that precede the formation of zoospores—and afterwards will probably study the changes that pine wood undergoes in becoming "punky." Waples is to study the germination of *Puccinia graminis* and the relation of this fungus to its host, particularly the penetration of the wheat, the time of germination, the capability of the uredo spores of functioning as resting spores, whether the mycelium is perennial etc.

Already Spalding had seen one laboratory research produce still further promising results. Physiological and anatomical study of pine woods begun under his direction by Filibert Roth so interested Bernhard Eduard Fernow, chief of the Division of Forestry of the United States Department of Agriculture, that Fernow visited Ann Arbor and authorized Roth to go on with the investigations. "[W]hat I did in the study of the quality of coniferous woods," wrote Spalding to Smith, "resulted in Mr. Roth's taking up a most important investigation." Spalding had begun the study of the mechanical and physiological properties of economically valuable American timbers. Roth continued the work which formed a basis for his later important timber physics research in the Department of Agriculture.

⁷⁹ *Journal of Mycology* 7 (2): 79 et seq., 1892.

Spalding wanted Smith to remain with the United States Department of Agriculture. He was aware of the gains to be realized there. Yet, he informed Smith of opportunities in the state agricultural colleges and experiment stations. In December, 1888, B. D. Halsted was chosen, considerably because Farlow had recommended him, for the position at Rutgers College and the New Jersey Agricultural Experiment Station. This left a vacancy open at Iowa Agricultural College, to which institution Halsted had gone when a state experiment station had been established at Ames under provisions of the Hatch Act. Spalding by letter of January 11, 1889, told Smith: "Mr. Reighard spoke to me some days ago about Dr. Halsted's place at Ames which it seems is vacant. I thought you might like to try for it if there is nothing definite yet at the Dep[artmen]t. If they do what is right for you there, though, I hope you can stay and help bring up the work as it ought to be." With the beginning of the second semester at the University of Michigan, a place was open on the faculty of Dr. Spalding's department. Promptly he offered it to Smith. He could offer no more than a temporary appointment to "take charge of two classes, the class of literary students corresponding," Spalding told Smith,

to the one you had last year, and the Junior pharmacy class (beginning the last Monday in March). The assistant will by this arrangement be obliged to give instruction to two pretty large classes. All the work, however, can be brought into the forenoons of the week, as it will not be expected that any advanced students will receive instruction in that laboratory. This is, from my point of view, a just and reasonable arrangement, and I intend hereafter to have the elementary work in botany in both departments—literary and pharmacy—done by the assistant, reserving for myself the biological work and the special courses for advanced students. . . . You are my first choice, and while there are a number who have either personally or through their friends signified their willingness to succeed to Mrs. Stowell's position, you will have the preference. We can work with mutual confidence and together do a good thing in building up the botanical department. I should *very much* like to have your help.

Smith was to be privileged to build up a "special course" such as they might agree upon. But since the Board of Regents would not meet before the time when Smith would begin, and since President Angell likely would not take the responsibility of assuring a permanent position, while the offer appealed as a "chance of a lifetime to take a position in the University of Michigan,"

Spalding could not promise it would be a paying one and he admitted the prospects were uncertain. Smith refused the offer, and by letter of January 31 Spalding agreed Smith had "done wisely. . . . Your estimate of the chances is correct," Spalding said, "I should not come if I were in your place." Then, when Commissioner Colman commissioned Smith two days later to "continue his field and laboratory investigation of peach yellows," with provision made for special microscopic analyses, which, most of all, Smith had wanted, Spalding wrote: "I was heartily glad that you were at last in a position to do a good piece of scientific work in your own way and can hardly imagine anything better than the way you are situated now."

March 7, 1889, J. M. Rusk assumed the duties of the newly created office of Secretary of Agriculture. Early in February, the federal Congress had approved legislation which raised agriculture to department status, with a place in the cabinet, in the executive branch of the government. Norman J. Colman served an interim period as Secretary between the time his commissionership ended and Rusk was appointed and qualified as Secretary when Benjamin Harrison took office as twenty-third President of the United States. Rusk immediately asked for "a laboratory to be erected on the Department grounds, suitable for the purposes of important investigations which [could] not now be undertaken." Chief Galloway had prepared the report for the year 1888 of the Section of Vegetable Pathology. Since his appointment had not become effective until November 1 of that year, the work reported had really been that of Scribner. Galloway had been brought to the Department as an assistant pathologist in 1887 by Commissioner Colman, both men being Missourians. He had been born at Millersburg, Missouri, on October 16, 1863, and had graduated in agricultural science in 1884 from the University of Missouri. Immediately he had been constituted an assistant in the university's horticultural department, a position which, because he showed special aptitude as a viticulturist, he retained until Colman, in search of a man of unusual capabilities to help Scribner in his work on fungous diseases of grapes, induced him to accept a similar position at Washington. Not only had Galloway's father been a specialist in this field but also his university instructor had been trained abroad, and Millardet's discovery of Bordeaux mixture brought the new treatment of viticultural diseases into the fore-

ground of practical scientific work. When Galloway was promoted to chief of the Section, he was also elevated to the rank of pathologist.

In 1888 Merton Benway Waite, Burrill's student and assistant at the University of Illinois, was brought into the employe of the Section. On July 29, 1889, Galloway wrote to Smith, who was then working at St. Joseph, Michigan: "Mr. [David Grandison] Fairchild of Kansas was appointed an assistant in the Section on the 25th inst[ant]. He took the examination on the 23rd." Fairchild that year had obtained his second degree, in science, from Kansas State Agricultural College. He was the son of the college's president, George T. Fairchild, who formerly had taught at Michigan Agricultural College, in fact, young Fairchild had been born while his father was professor of English literature and librarian there and his birth had taken place in a home on the campus. A student of William Ashbrook Kellerman in botany and a nephew of Byron David Halsted, Fairchild had been urged to accept employment at Washington by his uncle and by Galloway who was visiting Halsted at New Brunswick when he met Fairchild. By the summer of the year 1889, the Section of Vegetable Pathology, under Galloway's direction, had four assistants: Miss Southworth, Waite, Fairchild, and Smith.

In March the Section's bulletin no. 9, Smith's preliminary report on peach yellows, was published, and the entire edition of 5,000 copies was almost used up within a month. Galloway reported at the end of the year:⁸⁰

There have been nearly two thousand five hundred applications for this bulletin in excess of the edition, and the plates used in illustrating it have been purchased by a number of horticultural societies, their plan being to publish extracts from it in their annual reports.

In the same annual report, Galloway included a summary of the results of Smith's continued field examinations in Michigan, Maryland, and Delaware, and what disclosures Burrill could add July 1, 1889, to the special investigations he had undertaken in Illinois since 1888 to ascertain the malady's cause.⁸¹

Many plant scientists wrote Smith and praised his report.

⁸⁰ Report of the Section of Vegetable Pathology, *First Report of the Secretary of Agriculture*, 397-432 at p. 397, Washington, Gov't Print. Off., 1889.

⁸¹ *Ibid*, 421-423. See Burrill's letter, pp. 422-423.

Lieutenant John P. Finley, Professor Harrington, G. H. Failyer, and others not directly connected with plant work, wrote also appreciatively. During the spring of 1889, Smith began new experiments to test the validity of the theory that peach yellows is a nutritional disturbance resulting from soil exhaustion or starvation. He selected an orchard in the north part of Kent County, Maryland, and, utilizing ten representative plats which were subdivided and treated with Kieserite, muriate of potash and dissolved bone black, and ten equally representative plats for comparison he launched the experiments which lasted over three years and in 1892 were reported in detail to the American Association for the Advancement of Science.⁸² These were not the most important peach yellows investigations made by Smith. But these, together with other studies of the disease, would have been sufficient to make his reputation as a research scientist memorable in plant pathology.⁸³ On May 7, 1889, L. H. Bailey, professor of horticulture at Cornell University, advised Smith that his preliminary report on peach yellows would be reviewed in *Garden and Forest*, probably that summer. He wrote: "I am much interested in your proposed experiments. You are doing the best, most practical work on a plant disease of anyone in this country."

Smith was not made a member of the Society for the Promotion of Agricultural Science until the year 1893. Although already a member of the American Association for the Advancement of Science, his appearance at Ocala, Florida, before the American Pomological Society February 20-23, 1889, to present the first of his elaborate study of "The chemistry of peach yellows"⁸⁴ was an event of real significance. He had spoken before scientific societies, but never with the same role of prominence as now. Part II of his paper was presented two years later, September 22-24, 1891, at the twenty-third session of the Society, meeting at Washington, D. C. By the time this study was completed, Smith was maintaining that "we are to look for the cause of Peach Yellows and the means of prevention in an entirely different direction [from that of] the Goessmann-Penhallow method of

⁸² E. F. Smith, On the value of superphosphates and muriate of potash in the treatment of peach yellows, *Proc. Amer. Assoc. Adv. Sci.* 41: 226, 1892.

⁸³ G. P. Clinton, Erwin Frink Smith, *Proc. Amer. Acad. Arts and Sci.* 70 (10) 577, 1936.

⁸⁴ *Proc. Amer. Pomol. Soc.* 22: 38-41, 1889.

treatment.”⁸⁵ Professor Penhallow and Professor Maynard had neither searched for, nor found, proof of contagion in this disease.⁸⁶ On the other hand, Smith’s bulletin I of the Division of Vegetable Pathology, published in 1891,⁸⁷ “Additional evidence on the communicability of peach yellows and peach rosette,” had “well-established [the] contagious nature of” peach yellows. There remained the “middle ground” which Smith claimed was now Goessmann’s: “believing that whatever be the cause of Yellows, [they could] enable Peach trees to resist it by giving them a sufficient quantity of [plant] food.” This Smith dealt with in several papers: “On the value of wood ashes in the treatment of peach yellows,”⁸⁸ presented at the forty-first meeting of the American Association for the Advancement of Science; several articles of lesser value;⁸⁹ and his important Bulletin 4, “Experiments with fertilizers for the prevention and cure of peach yellows, 1889-1892.”

Apparently by 1888 he was convinced that his inoculations had demonstrated the communicability of peach yellows. He seems to have been convinced that the disease is of parasitic origin. But his utterances on these and other points were made with scientific caution, and he urged the necessity of further research. Months had been spent reading the best available literature, sending out inquiries, and visiting peach orchard regions to consult with owners and nurserymen to get their opinions as to the nature of the malady, its behavior, methods of control, et cetera. Often he travelled from orchard to orchard in a buggy or by wagon. His letters of inquiry went as far west as Kansas and as far south as Georgia.

During the summer of 1887, he began to correspond with J. D. Husted, president of the Middle Georgia Horticultural Society and owner of the Midland Fruit Farm at Vineyard, Spalding County. Their letters dealt mostly with peach diseases—leaf

⁸⁵ *Proc. Amer. Pomol. Soc.* 23: 21-26, 1891. Also a reprint, from which the quotations have been taken, 16 pages, at pp. 14 for Smith’s conclusions and 4 for Smith’s consideration of Penhallow’s, Maynard’s, and Goessmann’s theories with reference to contagion and the remedy therefor.

⁸⁶ *Ibid.*, 4.

⁸⁷ 65 pp., 38 plates, U. S. Dep’t Agric.

⁸⁸ *Proc. Amer. Assoc. Adv. Sci.* 41: 224-225, 1892. Abstract of paper presented at Rochester, New York, August, 1892.

⁸⁹ For instance, What to do for peach yellows, *Jour. of Mycology* 6 (1): 15-16.

curl, rot, and rosette—but diseases of grape and plum were also considered. Peach rosette, a disease similar to, yet distinguishable from, yellows, was of more than usual interest.

In June 1889 Smith took his doctoral examination at the University of Michigan on his thesis subject, the peach yellows. Dr. Spalding invited a pre-medical student, Lewis Ralph Jones, to attend the examination and so inspired was he by "the significance of and opportunity for research in the field of plant pathology"⁹⁰ that, after further conferences with Spalding, Jones decided to make plant pathology his life work. Some of his undergraduate years had been spent at Ripon College in Wisconsin, his native state, but an interest in biological sciences had caused him to transfer to Michigan. Smith's examination resulted, therefore, not only in his receiving the degree of Doctor of Science but also in attracting to the science Jones, another future leader.

Smith enjoyed more than one honor this year. The alumni of Ionia High School invited him to present their annual address, a distinction which, when he accepted it, was planned as the "one great event" of the Commencement exercises.

While in Michigan, he may have consulted with F. G. Novy. After securing a degree of Master of Science from the University of Michigan, Novy had gone to Chicago to teach. August 16, 1887, while still there and just before he went abroad with Dr. Vaughan to study with Koch and others, he informed Smith: "I have resigned my western professorship and accepted, instead, a position as instructor in Hygiene and physiological chemistry in my own Alma Mater." Two years later he wrote again:

A copy of your report on Peach Yellows has been recently received. I congratulate you, as a friend and a classmate, upon this highly creditable production and feel confident that no matter how obscure the cause of the disease may be at present, you are the most competent person to carry out the investigation to its fullest extent. I am surprised to see that so little work has been done in regard to the possibility of the bacterial origin of the Yellows. If I am not mistaken you, yourself, have not approached that side of the question.

Mr. [Newton B.] Pierce has been recently appointed by the Department [of Agriculture] to study the Vine disease of California. [He] has taken a course in bacteriology in our laboratory and is enthusiastic on applying that study in the investigation of vegetable diseases.

⁹⁰ G. W. Keitt and F. V. Rand, Lewis Ralph Jones, *Phytopathology* 36 (1): 1-17, at p. 2, 1946.

March 20, 1889, Pierce had telegraphed to Smith the news that "Mr. Galloway wishe[d] [him] to undertake [the] California Vine investigation." Smith replied and urged him to visit him in Washington before he left for California. "I am anxious to get into the field," wrote Pierce on April 7, 1889, "for I believe every week of growing weather will count with me in the study of the disease." There would be investigations now in plant bacteriology in the Section of Vegetable Pathology. Waite was beginning his studies. Smith had been reading and learning on the subject. Pierce would bring the latest knowledge from a university classroom.

CHAPTER V

INVESTIGATIONS IN PLANT PATHOLOGY PLACED ON A NATION-WIDE BASIS

DURING THE YEAR 1889 agents of the Section of Vegetable Pathology were located in New Jersey, Delaware, Maryland, Virginia, South Carolina, Mississippi, Missouri, Wisconsin, Michigan, and California. Already several accomplishments were justifying the work. A few years earlier, the nation-wide grape-growing industry was believed threatened by abandonment of whole areas. But the situation was more hopeful now. In 1887 the successful treatment of black rot and by 1889 promising results from experimental treatments of anthracnose and downy and powdery mildew had greatly encouraged growers. Several potato diseases had been brought under control. Before 1887 few, if any, efforts had been made to control some serious nursery stock diseases which damaged foliage and interfered with the proper budding of fruit in apple, pear, plum, cherry, etc.; and large amounts were rendered valueless. Experimental treatments were started in 1888 and within several years it would be demonstrated that some of the more destructive of the maladies could be controlled.¹

In 1889 Galloway said: ²

It must be remembered that the work we are engaged in is entirely new; there is no beaten path of precedent to follow, consequently it is to be expected that failure will sometimes follow our efforts. Heretofore the work of this nature has been confined almost entirely to grape diseases, since this year we have broadened our field, increasing thereby, as we have good grounds for believing, the usefulness of our labors. . . . Plans have been made for an extended series of experiments next season in the treatment of peach-rot and blight, a disease which ranks next to yellows in its destructiveness to the peach crop. We also propose to attempt something in the way of combatting pear blight, a disease which has yet baffled every effort at control. The work on apple-scab and other apple and pear diseases will be extended, and special endeavor will be made to discover

¹ B. T. Galloway, Report of the Section of Vegetable Pathology, for 1889, *op. cit.*, 399, 405; Progress in the treatment of plant diseases in the United States, *Yearbook of the U. S. Dep't of Agric.* for 1899: 195, 197-199.

² Report for 1889, *op. cit.*, 419-420.

a cheaper and more practical means of applying the remedies. The results of the potato and tomato-rot experiments have encouraged us to give the copper remedies a thorough trial for these diseases, and special attention will be given to the comparative cost of the different preparations, the best time to apply them, and the number of applications necessary. The grape work will be continued, as there are yet many points in the treatment of black-rot and anthracnose which need further elucidation. We have now under way some experiments in the treatment of the diseases of cereals, this work being chiefly confined to the smuts.

Aside from the grape maladies, the "principal diseases under treatment [were] scab, rust, bitter-rot, and powdery mildew of the apple; leaf-blight of the pear and quince; potato-rot and blight; melon blight, strawberry leaf-blight . . . and peach yellows. In two cases, namely, Michigan and Wisconsin," Galloway reported,³ "we co-operated with the [state] experiment stations." In the *Journal of Mycology*, "some of the more practical subjects discussed [had been]: (1) Treatment of apple scab, by Prof. E. S. Goff, of the Wisconsin Experiment Station; (2) treatment of potato rot, by Clarence M. Weed, of the Ohio station; (3) peach rot and blight, by Dr. Erwin F. Smith," due to *Monilia fructigena*, Persoon.⁴

The "first systematic attempt on the part of any station or organized body in the United States"⁵ to make "a thorough study" of plant diseases had taken place at the New York experiment station under Arthur. There, in four of its annual reports, he had published on "a great number of important diseases," and while the "keynote to the practical work of treatment had not yet been struck" this was to follow "as a natural result of the studies" made during these pioneering years. Even as late as the year 1885, Galloway pointed out,⁶ "there were only three institutions besides the Department of Agriculture making an organized effort in the way of teaching or in experimental work of this character." Burrill's work in Illinois and Farlow's work at Harvard were the two other principal research centers until the provisions of the Hatch Act made possible the establishing of other centers in

³ *Idem*, 399.

⁴ *Idem*, 397. See, E. F. Smith, Peach-rot and peach-blight, *Jour. Myc.* 5 (3): 123-134, Sept. 1889; Peach blight, *idem* 7 (1): 36, 1891; also, Spotting of peaches, *idem* 5 (1): 32-33, March, 1889.

⁵ Progress in the treatment of plant diseases, *op. cit.*, 193-194.

⁶ *Idem*, 199.

land-grant colleges where agricultural science was taught. In 1888 Arthur, America's first station botanist, went to Purdue University to accept a, and probably the first, professorship of vegetable physiology and pathology in an American university. Emmet Stull Goff, the Geneva station's official horticulturist, transferred to the University of Wisconsin.

Goff in Wisconsin and L. R. Taft in Michigan made important studies for the Department in, among other matters, fungicidal treatment for apple scab (*Fusicladium dendriticum*, Fckl).⁷ F. S. Earle, a Department agent located at Ocean Springs, Mississippi, investigated treatments for blackberry rust, plum and peach rust, strawberry leaf-blight, and grape powdery mildew. The work of other agents was reported by Galloway in 1889. Elaborate space was given to a first report on the work of Newton B. Pierce at Santa Ana California, on the vine disease⁸ which was destroying vineyards and beginning seriously to affect the raisin industry of the state.

In May 1889 he had started his investigations, mapping the disease's distribution and studying many environmental factors of climate and soil: effect of pruning, irrigation, drainage, atmospheric humidity, shading, land elevation, and other circumstances. In trying to determine whether the malady is communicable, he gave special attention to the point of temperature and the disease's virulence, and to the malady's similarity to the Italian disease, *Mal nero*. Scribner and Viala had arrived at no definite conclusions as to its cause, and, by December 6, Pierce had "carefully considered" most of the "non-parasitic agencies" which might be responsible. He wrote Galloway:⁹

When considering the disease as due to parasitic or pathogenic organisms, three lines of investigation have been pursued, viz: Entomological, mycological, and bacteriological—the last as distinct from mycological work mainly in the method of treatment. The work in these branches of the investigation is in no sense matured. It should be followed by much careful laboratory work.

During the late summer of 1889 Director W. A. Henry of the Wisconsin Agricultural Experiment Station, while visiting in California, was requested by Galloway to call on Pierce and inform

⁷ Rep't of Sect. of Veg. Path., *op. cit.*, 405-412.

⁸ *Idem*, 423-429.

⁹ *Idem*, 426.

the Department at Washington what he believed of the progress of the work. Henry was surprised that the disease had spread over so large an area in so short a space of time. Vine growers in Los Angeles and Orange counties were desolate, and discouragement pervaded all viticultural regions. He found Pierce devoting himself most enthusiastically to his task and possessed of "a great deal of ability for this line of investigation." "As before noted, we found signs of the disease in every vineyard, though the attack is very recent," Henry reported.

Wherever we went, as soon as the people learned the objects of our visit they gathered about us and asked many questions, all parties showing deep interest and great anxiety. In cases as soon as they were informed that the disease seemed to be present, the next question was if the agent had found a remedy. Naturally enough they care little for the name or history of the disease; all they want to know is how to cure it.

In the light of modern knowledge, this disease, like peach yellows, has been shown to be caused by a virus which is transmitted by a leaf-hopper. The importance of Pierce's investigations lies more in the evolution of field and laboratory technological research than in the immediate consequence of his discoveries. In his study of this disease, his exclusion of parasitic fungi as causes, his examination of the possible connection of insects and worms to the malady, his find of "bacteria-like bodies (*Micrococi?*) in large numbers within the chlorophyllose cells of the spongy parenchyma immediately surrounding the spiral vessels supplying that region," and other of his proofs and procedures were in keeping with the best laboratory practice, and reveal the quality of his preparation at the University of Michigan. While most of his study was performed at Santa Ana, some of the work was sent to Washington.

That year work facilities of the Section of Vegetable Pathology in the large red brick building of the Department of Agriculture at Washington had been expanded. More room, some of it attic or garret space, had been made over into small laboratories, and improved apparatus had been procured. Galloway announced in his 1889 report:

A bacteriological laboratory with all the modern improvements has been fitted up, thus enabling us to greatly extend our field of research. A small green-house has also been purchased and has proved very useful. For the

work we are engaged in a collection of fungi is absolutely necessary; realizing this, we have spared no effort to make the herbarium what it should be. Three years ago the number of specimens in the collection did not exceed three thousand; now there are something over fourteen thousand named, labeled, and mounted on seven thousand herbarium sheets. During the year a large number of economic fungi have been collected, one assistant spending a month in the field engaged in this work.¹⁰

Pierce explained why he sent materials to Washington:

After a long series of observations, made on material from various portions of the diseased district, which in no case failed to disclose the diseased vines as swarming with these bodies in all portions where sap had a ready flow, I believed it proper to undertake a series of experiments to determine if these bodies, always present, bore any relation to the disease as a whole. I had little doubt that they were microorganisms and gave to the local spotting of the leaves their characteristically sharp outline. Cultures from various parts of the vine were made in agar-agar and other media. Three sorts of bacteria were found with enough constancy to warrant further study, but I have not so far been able to determine whether any of these are the cause of the disease. Healthy vines were procured, set, and inoculated; but in due time I found both inoculated and control plants showing signs of disease. Owing to our inability, thus demonstrated, to make a fair test of the action of the germs in the infected district, these and analogous experiments—such as grafting, the testing of hardy stocks, etc.—have been inaugurated at Washington, outside infection being carefully guarded against.¹¹

Before Pierce concluded his first period of investigation in California, he secured translations of foreign literature and studied the similarity of other vine diseases: *Mal nero*, *Folletage* or *Apoplexie*, and *Oidium* especially. He soon established that *Phylloxera* did not cause the disease he was studying. His study of "every order of insect," several saprophytic and some parasitic fungi, root fungi, termites, nematodes, and the many physical factors investigated, were to be continued, including study of the effect of long continued propagation of vines from cuttings. He found "no good evidence that seedlings [would] exist longer [against] this disease than vines long propagated from cuttings." He appears to have early contemplated two possible solutions: first, to import healthy cuttings from disease-free areas, and, second, to attempt grafts of resistant grape species onto susceptible varieties. On Muscat as well as Mission vines Bordeaux mixture,

¹⁰ *Op. cit.*, 420-421.

¹¹ *Op. cit.*, 427.

and other fungicides, germicides, a powder recommended by local individuals, and special substances were tried. Oidium, anthracnose, and peronospora of the vine had been checked in Europe. He saw no reason why with time and careful work this disease also could not be understood as to cause and methods of prevention and cure. He told Galloway:¹²

Observations of value have been made relating to resistant stocks, and this feature of the work will be continued. Yet from what is known it is probable that Vinifera tops can not be maintained on native roots in this region in the face of the present prevalence of the trouble. The variation in the hardiness of varieties is evident and many notes are in hand on the subject. The effects of grafting on stocks of perhaps twenty different varieties have been recorded. I have noted the effects upon the raisin when considered from a market stand-point, the loss in productiveness of vines, etc.

When M. B. Waite had arrived at Washington in the autumn of 1888, he had found no laboratory equipment for research in bacteriology in the quarters of the Section of Vegetable Pathology. Further study of pear blight was contemplated. He gathered together some equipment which could be spared from the laboratories of the Bureau of Animal Industry, and set up what he has since described as "the first bacteriological laboratory for the study of plant diseases at Washington or east of the Alleghany Mountains."

"When Smith came in from field work in the fall of 1889," Waite has told,¹³

he at once became interested in this bacteriological work and equipment. He had read about these things, but had never had an opportunity to come in contact with them, that is, with such problems as the making of cultures, pouring of plates, isolation of organisms, sterilization and preparation of culture media . . . Smith . . . was already becoming somewhat discouraged by his inability to find any causal organism [of peach yellows]. He said repeatedly that he envied me in that I had a specific disease with a disease-producing organism to work on—one that I could see with the microscope, and cultivate, propagate, and inoculate and study. He stated several times publicly that he watched the work with intense interest. What started him was the desire to hunt for bacteria as the cause of peach yellows, so he made up some culture media, including some neutral peach-twig infusions.

¹² *Op. cit.*, 427.

¹³ Memorandum approved also by A. F. Woods of the Department.

In 1889 the Bureau of Animal Industry published its historically famous bulletin, "prepared," Smith said,¹⁴ "in great part by Dr. Theobald Smith," on "Hog cholera. Its history, nature, and treatment." For several years, bacteriological investigations had been in progress with reference to this disease.¹⁵ Years later, in 1903, DeSchweinitz and Marion Dorset of the Bureau proved that this animal disease, hitherto ascribed to bacteria, was, in fact, due to a filterable virus.¹⁶

In the middle 1880's swine plague had been differentiated from hog cholera, and its specific organism discovered and described. Proof of the infectiousness of these maladies and knowledge of the conditions of their transmissions helped to bring about important preventive measures.¹⁷ During the next decade, serum treatment would indicate as successful a "stamping out" of these diseases as the eradication of contagious pleuropneumonia, one of the work's great achievements. Before the end of the century, the efficacy of a vaccine against "blackleg" or symptomatic anthrax would promise to reduce to a minimum cattle losses from this infection. Sheep suffering from scabies would have to be dipped in prescribed mixtures and cured before or during shipment to market.¹⁸ Vigorous inspection, quarantine, disinfection, and slaughter regulations were enforced against several diseases. For instance, animal pens and freight cars were isolated, cleaned, and disinfected to halt the spread of southern cattle fever. Subsequent immunizing methods, moreover, proved effective.¹⁹ Antitoxin, serum, and vaccine therapy became a final goal in the experimental study of animal diseases.

Pierre Paul Émile Roux, French physician, bacteriologist, and pathologist, began to publish in 1887 on immunity against symp-

¹⁴ *Bacteria in relation to plant diseases, op. cit.* 1: bibliog., 212.

¹⁵ See *Reports of the Comm'r of Agric.* for several previous years; *Report for 1887*: 481-491.

¹⁶ E. F. Smith, Fifty years of pathology, *op. cit.*, 31; T. Swann Harding, *Two blades of grass, A Hist. of Sci. Develop. in the U. S. Dep't of Agric.*, 153, Univ. of Oklahoma Press, 1947; Gustav Seiffert (tr. by Marion Lee Taylor), *Virus diseases in man, animal and plant*, 180, N. Y., Philosophical Library, 1944.

¹⁷ V. A. Moore, The influence of animal experimentation upon agriculture, *Proc. Soc. Prom. Agric. Sci.*, 22-34, 1896.

¹⁸ G. F. Thompson, Administrative work of the Federal Government in relation to the animal industry, *Yearbook U. S. Dep't of Agric. for 1899*: 452-455.

¹⁹ D. E. Salmon, Some examples of the development of knowledge concerning animal diseases, *Yearbook U. S. Dep't of Agric. for 1899*: 111-113, 124-134.

tomatic anthrax. Two years later he and Alexandre Yersin established the existence of a filterable toxine in diphtheria and furnished the basis for Emil von Behring's antitoxin, demonstrated in 1891-4 and reported to the Budapest Congress. Following Schütz's discovery in 1887 of a coccus believed the cause of contagious pleuropneumonia in horses, Roux and Nocard isolated in 1898 a very minute coccus-like organism from contagious pleuropneumonia of cattle. In 1898 Shiga found the first known strain of the dysentery bacillus, and Theobald Smith comparatively studied bovine and human types of tubercle bacilli. How one find in pathology led to another always interested Erwin Smith. "Following Koch's work," he observed,

tuberculosis was discovered in birds and in various quadrupeds, and following Laveran's work malarial parasites were found in the blood of quadrupeds, birds, and reptiles. Coccidial diseases also were discovered in man and in various animals. In 1890 Koch discovered tuberculin by means of which early diagnoses of tuberculosis can be made [although] the great hope of a cure from its use, which filled the public mind for a time, was doomed to bitter disappointment. In 1890 tetanus antitoxin was developed by Behring and Kitasato. In 1891 Councilman and Lefleur, at Johns Hopkins in Baltimore, following earlier studies of Lösck in Russia (1875) and Kartulis in Egypt (1885), showed clearly that one form of dysentery is due to an amoeba. In 1893 the plague appeared in Hong Kong and in 1894 Kitasato and Yersin, independently, announced the discovery of the plague bacillus. In 1892 Theobald Smith discovered serum anaphylaxis and in 1894 the protozoan cause of infectious enterohepatitis in turkeys and the following year published a full account of it. . . . In 1895 V. A. Moore differentiated infectious leukaemia of fowls from chicken cholera. During this decade Pfeiffer (1892) isolated and described the influenza bacillus. In this decade (1896) it was discovered that immunity against typhoid fever could be induced by the inoculation of sterilized cultures (Pfeiffer and Kolle).²⁰

Great names in plant pathology were still few. Great names in plant bacteriology were even fewer. Secretary Rusk regarded the work of the Section of Vegetable Pathology as "very important." The fact that no successful treatment for the blight of the LeConte pear, studied in southern Georgia by Waite during 1889, had been found did not discourage him any more than that the cause of peach yellows was not yet known. Peach yellows and the California vine disease were believed caused by bacteria. The best knowledge

²⁰ Fifty years of pathology, *op. cit.*, 25, 26, 30, 31.

today points to a virus origin of both diseases, and that insect vectors transmit each disease is now known. The early workers, however, were confident, and rightly, that each disease is caused by a *parasitic agency*. Root-rot and rust of cotton, as well as a number of diseases of grapes, pear, peach, and apples, had been shown to be due to parasites. That remedies for each and every one had not been discovered was disheartening only to the impatient. Experience showed many times that once the cause was known, the remedy was soon found.²¹ At least, a control measure was soon devised. The important first conquest was to establish the cause of each disease.

Smith, by his peach yellows researches, was showing that the disease was not due to winter injuries. His field experiments were proving that the malady was not due to soil exhaustion, and that the disease was not transmitted through the soil. Budding and grafting experiments were establishing that the disease is transmissible. Tabular data collected between 1887-1890 indicated the disease occurred more frequently in dry than in wet summers.²² Some truth seemed to lie in each element and in the older belief that overbearing of the tree was connected. But the disease attacked young and older, unfruitful as well as fruitful, trees. Smith was seeking the real, as distinguished from interrelated, causes. In Kent County, Maryland, and Kent County, Delaware, he saw the disease

destroy many large orchards in seven to ten years from planting, whereas 50 miles away (Caroline County, Maryland) there were peach orchards 40 years old still entirely free from the disease but which subsequently became infected. [He] also got, when set into badly diseased orchards, what [he] considered to be a few undoubted cases in peaches worked on plum roots. The tops came from trees that were outside of the diseased area and that remained healthy.²³

He had carried his peach stones to his doctoral examination at the University of Michigan. One of the points proved was that the disease was not carried on seed from diseased trees. Root aphides had been a suspected cause of transmission. His notes of January 7, 1890, show that on this day he received a specimen

²¹ *First report etc., op. cit.*, 22.

²² *Journal of Mycology* 6(3): 110, January 3, 1891.

²³ "Synopsis of Researches," prepared by Smith, and published in the Nat'l Acad. of Sci. biog. memoir of Smith prepared by Jones and Rand, *op. cit.*, 18-19.

of a "black peach aphid from Dr. W. S. Maxwell, Still Pond, Md., taken from [the] nursery of John Stokes." In *Entomologica Americana*²⁴ he published his description of "The Black Peach Aphid. A new species of the genus Aphid." His first account, appearing in his preliminary report on peach yellows, had been prepared before he had seen the winged form. If Smith did not investigate the insect as an agent of transmission, he did study it as a possible cause of the disease. In the continuation²⁵ of his article, he said:

I first collected the winged viviparous form at Still Pond, Md., in April, 1889. . . . This aphid has been called "The peach phylloxera," and the injuries due to it are very considerable. The "Yellows" itself has been ascribed to it, but on insufficient evidence. On the whole, it is more to be dreaded than the borer or the curculio.

Insecticides and spraying could rid the insects when above ground. When, however, the insect was underground, unless some preventive remedy applied to the roots before planting could be devised, no certain method to destroy the insect was known.

During these years, furthermore, Smith was devoting much time to studying *Monilia fructigena*, the causal fungus of peach rot and blossom blight and canker of the shoots and stems. His field notes were filled with observations and experimental data on many peach diseases: among them, leaf curl, brown rot, black spot, mildew, rust, yellows, and rosette.²⁶ West, north, south, and east throughout the United States, these notes and data were taken. The *Monilia*²⁷ research, however, was important in that in considerable part, both as to time and place, Smith's work of describing the diseases and life history of the causal fungus paralleled his experimental researches in peach yellows. Besides showing that the fungus overwintered in mummied fruit and that orchard hygiene was of more value than the few fungicidal treatments known, he demonstrated his ability to conduct, side by side,

²⁴ 6 (6): 101-103, June 1890.

²⁵ The black peach Aphid (*Aphis persicaeniger* n.sp.), *Ent. Amer.* 6 (11): 201-207, 1890, quotation at pp. 204-205.

²⁶ Field notes, 1890, *Jour. Myc., op. cit.*, 6: 107-110; Field notes, 1891, *Jour. Myc.* 7 (2): 88-94.

²⁷ Peach rot and peach blight, *Jour. Myc.* 5 (3): 123-134; Peach blight, *Jour. Myc.* 7 (1): 36-38, Sept. 10, 1891. The first, Sept. 1889; the second, Sept. 1891.

studies of a fungous disease and one of supposed bacterial origin. Several years later J. B. S. Norton, searching the second rather than first season of the fungus, found the Sclerotinial or perfect form of the *Monilia*, something for which Smith searched "in vain."²⁸

During the spring of 1889, Smith gathered bark specimens showing a brown spot, of a tree badly diseased with yellows, from an orchard in Delaware. Preserving the material in picric acid, he determined to infiltrate and imbed it in paraffin for section cutting and examination according to a method of Dr. J. W. Moll about which he had read in the *Botanical Gazette*.²⁹ Five and a half pages of quoted notes had been culled from this article and copied into his "Record Book of Culture Media."

The superior staining procedures applied in animal pathology were making possible searching inquiries into the smaller bacteria and higher fungi. But the technological advancements in the study of plants were still slow. A dozen or more years had passed since Weigert, Paul Ehrlich, and others had begun to utilize anilin stains to demonstrate bacteria in animal tissues. Plant scientists, however, were slower to ascertain the "best methods" to stain bacteria in vegetable tissues. In 1905³⁰ Smith suggested as a reason the difficulty inherent in "the fact that the tissues of the higher plants often take the basic anilin stains as readily as the bacteria and retain them even more tenaciously."

Several hundred examinations of stained and unstained materials, made by Smith between November 15, 1889 and February 15, 1890, resulted in "only negative results. No fungi and no bacteria could be discovered. Part of the sections," Smith explained, "were infiltrated, embedded, and cut on the microtome. The microscope used in all these examinations was Zeiss stature I, Oculars 2, 4, and 8 (22.5 mm) and 18. (10 mm). Objectives AA. D. F. and Achromatic 3 mm. Homag. in Apert. 1.30. Stand with Abbe condenser." It was plainly disappointing that sections cut through and in the vicinity of the small brown spot of the bark, though infiltrated and paraffin-embedded according to Dr. Moll's method, revealed no parasite.

February 17, Smith began another month of experiments. A section, stained three days in Haematoxylin according to a formula

²⁸ Fifty years of pathology, *op. cit.*, 28. The year of Norton's discovery was 1902.

²⁹ 13: 5, Jan. 1888.

³⁰ *Bacteria in relation to plant diseases, op. cit.*, 1: 29.

of Ferdinand Hueppe, was washed out in 60 per cent alcohol and mounted in glycerine. Smith had noted that Hueppe had said that glycerine could be used only with the brown dyes, since it extracts other anilin colors rather quickly. Five differing sets of stained sections were examined that first day, but still no trace of mycelium was found. While certain darkly stained rod-shaped particles in the nuclei resembled bacilli, Smith believed these were only parts of nuclear filament. A section stained five days in a watery solution of Eosine, washed in distilled water, and examined, again showed "no mycelium and nothing suggestive of bacteria." Then were tried three sections stained five days in a concentrated alcoholic solution of methyl violet diluted with distilled water.

Sections through the black pith of a yearling branch affected by yellows were prepared, and stained, five sections with fuchsin and six sections with iodine diluted; then tangential sections through the inner bark of the same branch with fuchsin and iodine, gentian violet, magdala, and safranin. March 7 found Smith studying the gum pockets in diseased wood. J. H. Wakker had published a method by which he had demonstrated gum in living cells, and this Smith had copied into his notes. In an 1882 edition of a book, *Practical Microscopy*, published in England, Smith found a formula for picro-carmin staining of vegetable tissue. With slight modifications the formula could be applied to wood sections. He tried staining the gum pockets of diseased peach wood with picro-carmin. Section were stained also with fuchsin, methyl violet, gentian violet, iodine, and bismarck brown. March 10 he thought he had a discovery. Cross sections showed "many small dark bodies in interstices of tissue, but I," Smith wrote, "cannot determine them to be bacteria. They disappeared after washing in acid alcohol, though much color still remained. These bodies are smaller than pear blight, longer than broad, and more like bacteria than any bodies yet seen." He manipulated the microscopes with all the skill at his command, repeated the experiment, but his examination ended again with "no trace of the bacteria-like bodies." When he cut through the brown spot of the bark, some mycelial fragments seemed to be in evidence, "some distinct from tissue, others between cell walls." So he put these aside for further study. March 11, however, his entry was concluded with "Uniform transparent brown stain. No trace of bacteria and mycelium indefinite." March 14, he concluded that certain of the

cells contained granules which had the appearance of bacteria. Owing to their size, these could be mistaken for such, "but," wrote he, "they are not grouped in such a way as to make me think they are. Moreover, I have never seen any breaking down of cell walls or other tissues such as we might expect from their presence." Full strength of Hueppe's haematoxylin formula disclosed: "No mycelial threads. Some clusters of granules scattered about deep stained and resembling bacilli held together by some intermediate substance. A very few of the cells show similar bodies in place, but nothing distinct." Serial sections cut from pit to skin of a premature peach were last studied. But on the final day, March 17, Smith wrote, "Examined for bacteria. No trace. Protoplasm especially of the nucleus is very granular—granules are like bacteria, but are not."

Galloway reported that year:

Two suspected organisms have been isolated from the diseased tissues grown on and in various nutrient media, and studied as carefully as time permitted. Both are short rods (*Bacilli*). Both were found in nearly every diseased tree, but they appeared so rarely that grave doubts have arisen as to their disease-producing nature. If the disease is due to a microorganism it must be rather abundant, judging from the results of bud inoculations. To complicate matters, three yeasts were also isolated under conditions which render it almost certain that they came only from the inner bark. These also were rare. At that time no peach trees suitable for inoculation were at hand. These have since been grown from seed procured in three localities free from this disease, and are now ready for inoculation.³¹

Necessary field work, Galloway further said, interrupted the progress of research in peach yellows. Smith, however, had found time to consult some authorities with a view to perfecting his technical proficiency, once his indoor laboratory researches were resumed. Up to this time, he could not have had much experiential knowledge of culture media and their preparation. In a small note book, containing entries of both the years 1889 and 1890, appears a reference to "Culture Media. 1% agar agar or 10% gelatin. Boil disc. 3-2-3 minutes each. 100°C." References to paraffin in turpentine, to anilin stains, to sticking slides on covers, to oil of cloves, to useful formulae for methyl blue stain and its preparation, to methyl violet stain and its utilization with

³¹ Report of the Chief of the Division of Vegetable Pathology, *Report of the Secretary of Agriculture for 1890, op. cit.*, 393-408, at p. 404.

anilin by Koch when isolating the tubercle bacillus, to the fact that fuchsin might be substituted for methyl violet, to projection eye pieces and their most effective use, how to make holes through glass, and such, appeared in his "Record Book of Culture Media." By far the most important references were those to a formula for preparing an "Agar Medium with Beef Broth," supplied to him on February 26, 1890, by Moore of the Bureau of Animal Industry, and another agar formula supplied by Dr. W. M. Gray, microscopist of the Army Medical Museum at Washington, on March 25 of the same year. Smith chose his *magnum opus*, his monumental three-volume work, *Bacteria in Relation to Plant Diseases* (1905), in which to acknowledge his debt to Dr. Theobald Smith and Dr. Moore. In its preface, he thanked each of these distinguished men for "friendly advice and helpful suggestions [given] at a time when the writer was beginning his bacteriological studies and was perplexed in many ways."³²

Dr. Moore's formula for nutrient media read:

To each gram of ground or minced meat add 2 cc of distilled water. Set away 18 hours, i.e. overnight, in ice box. Strain through a towel. If necessary add enough water to bring the fluid up to the original amount of water added. Add $\frac{1}{2}\%$ sodium chloride, and 1% of peptone. Add of normal sol[ution] of sodium carbonate (53 grams flame dried salt per litre of distilled water). Enough to render the acid liquid slightly alkaline. Boil 30-35 minutes on water bath. Filter.

The stock flasks not needed for immediate use may now be boiled $1\frac{1}{2}$ hours on each of three consecutive days, and then set away.

To the flasks wanted for immediate use add 1% of agar, dry and cut very fine. Boil briskly 2 hours on the covered water bath in Erlenmeyer flask, keeping the water up to level of the fluid in the flask. Cool down to 40° or 45°C and add white of egg at rate of one white to each 200 cc of the liquid. Mix thoroughly by beating with a spoon, or by pouring from one dish to another. Again boil 2 hours briskly on the water bath. Filter on hot water filter through 2 layers of Japanese filter paper (it will not pass in any quantity through ordinary filter paper). Then fill tubes or plates.

If rolls are made, the tubes must lie *flat* and must be examined in that position, as where the agar cools some water always exudes.

On July 8, 1885, Dr. William M. Gray had been appointed by Dr. John S. Billings to perform some bacteriological analyses incident to the work of the Army Medical Museum. A skillful

³² Page v.

microscopist and a photomicrographer of note, he had distinguished himself as a preparer of microscopic specimens, "the best," Dr. Billings had said, "in this country."³³ Smith surely had used the splendid library of the Museum and perhaps before this time had visited the microscopical room located next to the Museum on the third floor of the building completed on February 15, 1888. Pathologists occasionally gathered at meetings of, or held in, the Museum. On at least one occasion Dr. W. H. Welch of Johns Hopkins had attended. Men of the Bureau of Animal Industry were interested in at least one of his investigations. A preliminary account of his studies on hog cholera was published in the first *Johns Hopkins Bulletin* of December 1889.³⁴

In 1889 Smith may have visited, among other laboratories, those of Dr. Prudden and Dr. Hermann Biggs in New York. Dr. Biggs was a friend of Theobald Smith, and from him Smith may have obtained his list of laboratories which included the Hoagland Laboratory in Brooklyn where Dr. B. Meade Bolton was in charge and where Dr. Sternberg in 1888 had given a lecture on the "Bacteria."³⁵ In his "Record Book," Smith wrote at some time: "The first filtering of meat infusion, agar, etc., may be through cotton. The results are very satisfactory. This method saves much time and is in constant use in Dr. Prudden's laboratory." He, furthermore, in April 1891, wrote Galloway of a "very readable article on bacteria" by Prudden in *Harpers Monthly Magazine*.³⁶

Dr. Bolton in 1887 had been among those seventeen physicians who had taken work as students or special investigators in Welch's laboratory at Johns Hopkins. Councilman was then Welch's associate and, among others enrolled, were such doctors of future distinction as Franklin P. Mall, fellow, Alexander C. Abbott, W. D. Booker, W. S. Halsted, C. A. Herter, and Sternberg.³⁷ That year Bolton assisted Welch in the bacteriological instruction which included "methods of isolating and of cultivating micro-organisms and the morphological and the biological

³³ *A history of the United States Army Medical Museum 1862-1917*, compiled from official records, by Dr. D. S. Lamb, pathologist of the Museum. A mimeographed original may be found at the Museum's library. See pp. 88, 95, 96, 103, 104, 107.

³⁴ Dr. Harvey Cushing, *The life of Sir William Osler, op. cit.*, 1: 322.

³⁵ *Johns Hopkins Circular* 69: 26, 1889. There the lecture was noticed.

³⁶ *Glimpses of the Bacteria*, 82: 706-718, Apr. 1891.

³⁷ *Johns Hopkins Circular* 55: 64 f., Feb. 1887.

properties of the most important species, particularly of the pathogenic forms." ³⁸ By 1890 ³⁹ among twenty-eight medical students, eleven were studying bacteriology under Welch and Abbott, and Dr. William Osler, later a friend of Smith, had charge of the medical department of the hospital.

Smith obtained from Dr. Gray in 1890 a formula for the preparation of nutrient agar. His entry in his "Record Book of Culture Media" read as follows:

Add to one litre of distilled water 1-2% of Agar and boil *two* days (not necessary to boil at night). Add 1% of peptone; $\frac{1}{4}$ % of Liebig's Fleisch extract and whites of two eggs. $\frac{1}{2}$ % of grape sugar may also be added, if desired. Neutralize with carbonate of soda solution and filter. By boiling two days the agar will readily pass through one thickness of filter paper, and does not even require to be put on the hot water filter.

As a second paragraph, Smith added: "Dr. Gray showed me many tubes prepared in this way and they were very clear and satisfactory. He much prefers Liebig's extract to meat infusion and says it gives just as good results." The work of preparing nutrient media to cultivate and examine the growth microorganisms make when infecting plants must have been thought of by Smith as part of his regular duties. A list of subjects for special study, prepared February 27, 1890, must have been planned for hours outside of his laboratory. These were (1) photography including microphotography, (2) drawing, (3) a review of German grammar, (4) a review of botanical Latin (5), study of the Italian language, and (6) razor honing.

During February, March, and April of that year, Waite, as a part of field work in pear blight, went to Georgia to test a series of fungicidal treatments in connection with this disease caused by bacteria.⁴⁰ This was the year when Galloway and Fairchild at Vienna, Virginia, were to demonstrate that 93 to 99 per cent of the grape crop could be saved from black rot by the use of copper fungicides at a cost of 1 to 2 cents per pound of fruit.⁴¹ The

³⁸ *Idem* 51: 123 f., July 1886;; also, *idem* 55: 64. Concerning Bolton's association with Welch in bacteriological instruction, *idem* 60, Nov. 1887; 64: 54, Mar. 1888.

³⁹ *Johns Hopkins Bulletin* 1 (8): 99, Nov. 1890.

⁴⁰ Report of the Chief of the Division of Vegetable Pathology for 1890: 394.

⁴¹ Erwin F. Smith, Fifty years of pathology, *op. cit.*, 21. See also Galloway's discussion, Treatment of black rot of the grape, Division's report of 1890, *op. cit.*, 394-396. Also *Jour. Mycology* 6 (3): 89-95, Jan. 6, 1891.

historic orchard of Dr. Maxwell near Still Pond, Maryland, moreover, became the scene of experimental treatments for pear leaf-blight and scab.⁴² August 6, Fairchild told Smith by letter, "The treatments were too late for a good result to be obtained with the scab, and the Leaf-blight has not attacked the leaves this season like it did last." Nevertheless, the results of the experiment, he believed, would show "a good many things," and these were incorporated in Galloway's official report for that year, as well as made the second part of a series of three articles prepared by Galloway and Fairchild, entitled, "Experiments in the Treatment of Plant Diseases."

These dealt with grape rot, pear leaf-blight and scab, and the results of other experimental work performed by field agents at Greenville, South Carolina; Vineland, New Jersey; Neosho, Missouri; Charlottesville, Virginia, and other special localities. In the main, the program had to do with testing the efficacy of fungicidal treatments. Each year official bulletins and circulars had been issued by the Department, prepared by officials, assistants, or agents of the Section (or Division as it became known in 1890) of Vegetable Pathology, and, while grape diseases—black rot, downy mildew, etc.—were most frequently published on, the work of each year in diagnosis and treatment of fungous and other classes of disease was annually summarized not only in these publishing media but also in the chief's regular report made part of the secretary's official report. Peach, pear, and apple diseases received appropriate consideration. In 1889 circular 9 dealt with "Root Rot of Cotton"; and in 1891 circular 10 elaborated results of the important studies of "Treatment of Nursery Stock for Leaf Blight and Powdery Mildew." Several subjects were estimated in special articles appearing in quarterly issues of the *Journal of Mycology*. Furthermore, wherever results of an especially valuable piece of research were announced by officials or investigators of state agricultural experiment stations, these, as far as possible, were coordinated with the work of the Department. In other words, a great service organization at Washington, though still small in personnel and with a none too abundant annual appro-

⁴² Report of the Chief of the Division of Vegetable Pathology for 1890: 397-398. Also, B. T. Galloway and D. G. Fairchild, Experiments in the treatments of plant diseases, part II, Treatment of pear leaf-blight and scab in the orchard, *Jour. Mycology* 6 (4): 137-142, Apr. 30, 1891.

priation, was seeking to be of use to the farmers and agricultural communities of every section of the nation.

Circular 4, a three-page pamphlet (1886), had made available to farmers the best known, prescribed "Treatment of the Potato for Blight and Rot." The several diseases to which tomatoes, another basic crop of the agricultural economy, were found subject were being investigated. Various state experiment stations, ambitious to solve the problems of crop diseases within their jurisdiction began to shape research programs of considerable magnitude, workers of various stations collaborating in studying various widespread diseases. By 1890⁴³ B. D. Halsted of the New Jersey station, whose work served as a model on several subjects, was calling attention at the Indianapolis meeting of the Society for the Promotion of Agricultural Science to the need for study in all affected regions of the sweet potato rots—ground rot, soft rot, black rot, yellow or stem rot, and dry rot—diseases of real interest to his state because of the industrial impairment being suffered there. He was preparing his New Jersey station bulletin 76, "Some fungous diseases of the sweet potato," similar to his bulletin 64 and 70 on fungous diseases of the cranberry and spinach. Collaborating with Fairchild, the next year Halsted would publish in the *Journal of Mycology*⁴⁴ a quite important study of the "Sweet-Potato Black Rot."

Since the start of its publication under government auspices, the *Journal of Mycology* had been presenting reviews, written by the chief and assistants of the Section of Vegetable Pathology, of especially valuable articles published both in the United States and abroad. In 1889 Fairchild added to this important service by inaugurating in the *Journal's* first issue of 1890 a prepared "Index to North American Mycological Literature,"⁴⁵ a stupendous task when one considers that it was done in spare hours from his regular duties and gathered from station bulletin and annual reports, from botanical publications, and other sources of information, eventually from publications the world over.

On October 18, 1890, Fairchild was directed by Galloway to go to Lockport and other points in New York State to examine "a

⁴³ Report of the 11th annual meeting of the Society, August, 1890: 27-28.

⁴⁴ 7 (1): 1-11, Sept. 10, 1891.

⁴⁵ The first series of abstracts was published in *Jour. Mycology* 6 (1): 42-44, Mar. 1890, and were continued with quarterly issues of the publication.

new disease of the grape, which [was] generally referred to under the name of 'blight' or 'rust.' " In the *Journal of Mycology*⁴⁶ of January 6, 1891, was published Fairchild's first report on this and other grape diseases which he found there. Western New York was a center of pomological industry. Scribner and Viala had made some of their most important studies there. Arthur, Goff, and now William R. Dudley of the agricultural experiment stations at Geneva and Cornell University had established investigations in plant pathology on a state-wide basis. Soon the celebrated work of M. B. Waite on the life-history of the pear blight organism, and its transmission, would culminate at Brockport. From experiments on flower buds of the pear, he would prove that the germs, or bacterial infection, are distributed by insects.⁴⁷

On April 12, 1891, Smith, at Dover, Delaware, reported to Galloway that he was "in good season to study the effect of yellows on the blossoming," and that he should make good progress studying his mapped peach orchards. Again he was delivering lectures before farmers' institutes and horticultural societies. Scientific information concerning the disease, projected remedies, and control by legislation were much under discussion. Delaware had a law which applied to the lower half of the state. In the January 1891 issue of *American Gardening*, the editor, Liberty Hyde Bailey, had written an article, "Peaches and Yellows in the Chesapeake Country," in which he commended and described Smith's work. Bailey reminded his readers that Michigan, New York, and Virginia had definite yellows laws. In Michigan the value of required eradication, enforced by law, had been demonstrated, whereas the absence of such legislation was painfully evident when the havoc wrought by the disease in this beautiful Chesapeake country was examined. The more important was this since the region was regarded as part of one of the world's most productive peach-growing centers.

Soon after receiving Smith's letter of April 12, Galloway went over to Dr. Maxwell's orchard at Still Pond, Maryland, and there found Fairchild and P. H. Dorsett applying new fungicidal treatments to pear diseases in the experimental area. Fairchild soon left for Geneva, New York, and Galloway told Smith to send to

⁴⁶ 6 (3): 95-99.

⁴⁷ Report of the Chief of the Division of Vegetable Pathology for 1891: 372-373.

Fairchild diseased pear flowers and leaves that he might make some cultures of a fungus and inoculate pear flowers there when they opened. May 7, Fairchild, at Geneva, wrote to Smith, still at Dover:

I have not yet had time to examine the pear blossoms, but will, of course, attend to them this morning the first thing. I am very much interested indeed in the disease, and have spent some time in growing it in culture media. It seems to me possible that we can follow this fungus through its whole life history, and perhaps find a number of very interesting points in connection with it. I have just been looking at a culture which I made of a fungus I found very abundantly, in fact almost universally, growing within the hairs situated just below the buds of apples, especially the fruit buds. If possible, I wish to establish the connection between this fungus and the scab beyond a doubt.

Director Peter Collier of the Geneva station had placed at Fairchild's disposal the station's facilities, and he was inaugurating some far-reaching experiments. The substance of these—treatments of pear leaf-blight, cracking, and scab, treatments of nursery-stock diseases of cherry, plum, quince, pears, apple, and peach—would be reported, together with work on grape black rot at Sterling, Virginia, some nursery work examinations at Mullikan, Maryland, and results of E. S. Goff's experimental studies of treatments of apple scab in Wisconsin, in bulletin 3 of the Division, prepared by Galloway and styled, "Report on the experiments made in 1891 in the Treatment of Plant Diseases."⁴⁸

One Saturday night, on June 27, 1891, Fairchild, in a mood to put aside his work for a while and chat with Smith, exchanged congratulations. Some inoculation experiments which Smith had started in Georgia the year before to study the new disease often confused with peach yellows, peach rosette, had exceeded even the fondest hopes. "Now for the specific germ of the trouble," Fairchild suggested.

When is it to be found and will it grow upon anything but living peach tissue? It seems to me you are gradually nearing your goal, Smith, and must corner the cause soon.

My indoor work has been spent mostly in studying upon the philosophy of the fungicides. I find little has been done in the matter and find also that the investigation of biological questions is no easy task.

Last week I spent several days at Cornell looking through the library

⁴⁸ Washington, Gov't Print. Office, 1892.

there for material that other people have done upon fungicides but find little. I played while there a game of tennis with Prof[essor] Bailey one evening and almost miraculously beat him.

The nursery experiment of 10,000 stocks is growing well and has already received three sprayings. I hope notwithstanding the dry season to show that spraying will keep the leaf blight off of pear, quince, cherry, and plum stocks. I have also an additional experiment upon large plum trees, one on orchard quinces and another upon a different kind of plum. I am trying the following mixture in a comparative way, all being made up to a common strength of solution.

Very weak Bordeaux mixture

Copper acetate (two different strengths)

Potassium sulphide

Chloride of lime (three different strengths)

Glue mixture of CuSo_4 & Na_2Co_3 & Glue

I also started in the green house a series of experiments to see if the potting bed botrytis can be controlled. I put in eight different substances into the soil, but so far they have turned out entirely negative.

But the hardest work I have to do, Smith, is to keep the nurserymen and fruit growers alive to the importance of spraying and make friends of them for the Division. So far I have not seen a single man who seemed disappointed in the work, or who was really inclined to run down the Agricultural Department. I have stirred up a few of the men to the importance of a state law against black knot and would not be greatly surprised if a law was passed next winter prohibiting the maintenance of such a disease in the region. . . . Thaxter I see is studying the means of combatting the trouble and I judge he intends to do it by some kind of spraying.

December 1888, Smith, then not acquainted with Thaxter, had written Farlow for a copy of the latter's recent monograph on the Entomophthoreae, and received a reply that Thaxter was "now connected with Connecticut Agr[icultural] Ex[periment] Station." Farlow's assistant in biology at Harvard since 1886 and the possessor of three degrees from that university, Roland Thaxter, had been honored by being chosen the Connecticut station's first official botanist, more precisely, its first mycologist. He remained there but a short while, returning to Harvard in 1891 in the capacity of assistant professor of cryptogamic botany, and became a full professor in 1901 and professor emeritus in 1919. Although his stay at the Connecticut station was brief, the breadth and influence of his work were permanent, primarily because of his skill as a research technician and the superiority of his exact methods of studying, under controlled conditions, the crop diseases he elaborated. In 1889-1890 Smith profoundly admired his de-

scriptive work of downy mildew of lima beans, in 1890-1891 his discovery and description of the cause of potato scab,⁴⁹ and, even more, his splendid work on the smut of onions, first written up by him in the station's report for 1889 and of which Fairchild in his first "Index to North American Mycological Literature" said,⁵⁰ "A most admirable treatment of the disease in which the botanical history and origin as well as the practical points of inquiry are well worked out." Farlow had commenced the earliest researches in America on a number of plant diseases which Thaxter further studied. William Codman Sturgis, also the holder of three degrees from Harvard University, who became an assistant in the Harvard Cryptogamic Laboratory during 1888-1889 and served as vegetable pathologist of the Connecticut station from 1891 until 1901, collaborated with Thaxter in studying onion smut, and of their work, Smith said:⁵¹

Thaxter and Sturgis demonstrated that onion smut was only communicable during the seedling stage of growth and that, if plants were grown for a few weeks in healthy soil, they might be transplanted to fields badly infested with this smut without danger of infection.

Thaxter's work on onion smut was notable for another reason. At that time the disease was destroying nearly fifty per cent of the crop in Connecticut. Director J. G. Horsfall of the Connecticut experiment station wrote in 1941 of Thaxter's work: "Deciding that the organism was soil-borne he designed a chemical treatment in which he applied sulfur in the row with the seed and obtained striking reduction in disease. This was a pioneer test of soil treatment."

Henry Luke Bolley, a graduate and advanced student in science under Arthur at Purdue University, also studied the potato scab disease. In 1890 he joined the faculty of North Dakota Agricultural College at Fargo as professor of botany and zoology, and was also botanist and plant pathologist of the government experiment station located there. G. H. Coons, in an article, "Seed

⁴⁹ Fifty years of pathology, *op. cit.*, 21. As to potato scab, see discussion hereafter.

⁵⁰ *Jour. Mycology* 6 (1): 44, Mar. 1890. Notice also the references to *Phytophthora phaseoli*, Thaxter, downy mildew of lima beans, and other onion diseases.

⁵¹ Plant pathology: a retrospect and prospect, *op. cit.*, 608, 1902.

Tuber Treatments for Potatoes,"⁵² has compared the contributions of Bolley and Thaxter thus:

When the potato scab was assigned to a bacterial cause by Bolley in 1890, this concept cleared away much of the confusion of the old literature and pointed out a rational plan of treatment. In his first paper Bolley suggested as a control measure the use of disinfectants such as used in medical practice. Following the work of Thaxter ["The potato scab," *Conn. Agr. Exp't Sta. Rep't 14: 81-95 (1891)*] in which the causal organism of scab was isolated, there developed an extensive literature consisting for the most part of detailed tests of various germicides, and as a result of the great body of work, there crystallized out the definite program of tuber treatments which has been recognized by the Department of Agriculture and all the Experiment Stations.

In 1890 Burrill associated Irish potato tuber rot with a specific bacterium, which he claimed to have isolated in pure culture.⁵³ Bacterial origins for several diseases in other crops were being urged that year, among them, Burrill's bacterial disease of corn.⁵⁴ Smith that year was searching for a bacterial explanation of peach yellows. But during the spring he had to leave these studies for orchard work on the Chesapeake peninsula, and in the summer he had to go to Georgia and Kansas for other field research. Some time was spent also in Michigan and Illinois, and, for purposes of preparing an exhibit, he may have gone in August to Minneapolis, Minnesota. His most important work was in Georgia where, during June and July, at Vineyard, on the plantation of J. D. Husted, he began experiments to bud and inoculate 125 peach seedlings and 56 cuttings of Marianna plum. He was to demonstrate for the first time the "dangerous, contagious nature" of peach rosette, prevalent there and in Kansas. Dr. Kellerman and his aide, Swingle, of Kansas State Agricultural College welcomed Smith when he arrived at Manhattan. Kellerman wrote Smith at Hubbardston and urged the latter to schedule his arrival in Kansas when he would be there. Smith visited for a time Viola Holmes Greene of Stevensville, Kansas, the younger sister

⁵² *Phytopathology* 8 (9): 457, 1918; see also *Rep't Conn. Exp't Sta.*, pp. 153-159, 1891.

⁵³ Preliminary notes upon the rotting of potatoes, *Proc. 11th Ann. Meet. Soc. for Prom. Agric. Sci.* (August, 1890, Indianapolis), 21-22. The other diseases of believed bacterial origins will be discussed at other points in this book.

⁵⁴ *3rd Ann. Rep't Ill. Agric. Exp't Sta.*, 1890; *Bulletin* 6; 165-175, August 1889.

of his adored Ida Holmes of his boyhood years at Gilbert Mills, New York.

Possibly when first studying peach rosette, Smith visited Kansas during the year 1889. Later when recommending Swingle for a Harvard fellowship, he spoke in a letter of May 26, 1895 to Farlow of first becoming acquainted with Swingle that year. But also he refers to Swingle as having "recently graduated" and being Kellerman's assistant. Swingle obtained his bachelor of science degree from Kansas State Agricultural College in 1890. Whether in 1889 or 1890, Smith thought well of Swingle's abilities as a research worker. He told Farlow:

I saw more or less of him for a week or two, and set him down as one of the brightest young men I ever met, and felt that we must certainly have him in our Division if possible. Galloway met him the same summer and was so favorably impressed that an appointment followed. Since then, as you know, he has been connected with our Division. During this time he has not published much but has been laying a broad and deep foundation for the best work. I have come to know him intimately and during all these years have seen no reason to change my first judgment. He is in many ways a remarkable man,—energetic, enthusiastic, critical, sympathetic, the hardest kind of a student both in literature and in nature. He is sure to achieve more than ordinary distinction. There is no man in our Division, so thoroughly familiar with botanical literature Phanerogamic and Cryptogamic, no one who has such keen insight and goes so surely and quickly to the heart of a question. His mind works so quickly and he has so many bright ideas and fertile suggestions that to a plodder like myself, it is often positively painful to be much with him, and yet I am never with him long without receiving some helpful suggestion. Of course, much of what he knows about plants has been learned since leaving school. During this time, however, he has also found leisure to *master* French and German, and he also reads Italian and Russian so as to be able to use both languages with aid of a lexicon. He has also done a good deal of miscellaneous reading in philosophy, history, and general literature, and is a well informed man on all general topics of conversation. More than this, he has been a diligent buyer of the best botanical books and has a library unusually rich in pamphlets and treatises on this special line of study.

Swingle, together with Herbert John Webber, a graduate in science under Bessey at the University of Nebraska, for a while a graduate student under Trelease and an instructor at the Shaw School of Botany at St. Louis, was to have charge of the Department's sub-tropical laboratory founded in 1893 at Eustis, Florida.

Kellerman's work in Kansas "upon smuts and breeding of corn" was regarded in 1891⁵⁵ "as fine examples of his many important investigations beyond the Missouri." Smith's visit to the Kansas State College and experiment station enabled him to examine some of Kellerman's and Swingle's fungicidal treatments for stinking smut of wheat. In the *Journal of Mycology*⁵⁶ he reviewed their bulletin 12 on their preliminary experiments and said that in the main these were

a repetition and confirmation of those made in Europe by Jensen, Kühn, and others. . . . The wisdom of the recent establishment of State experiment stations by the General Government, has been called in question in certain quarters. Nevertheless, the stations are here to stay, and their public usefulness becomes more and more apparent, especially after reading such a paper as this from the Kansas station. The results are striking and conclusive; and worth more to the wheatgrowers of this country than the cost of all the stations.

Kansas, however plenteous in some pomological crops, was not regarded as a "' first class ' peach country though," Kellerman told Smith,⁵⁷ "some peaches [were] raised [there] and very many [were] produced in the southern part of the state." That year one T. C. Wells, a Kansan, had sent Smith specimens of peach rosette; and Kellerman and Swingle had carefully examined the diseased orchard materials.⁵⁸ In reply to a letter from Smith, Kellerman said:

I cannot think that the peach trees here were affected with "Yellows." Mr. Wells is of the same opinion, though Prof[essor] Burrill, to whom specimens were sent by Secretary Brackett of Lawrence (who received them from Mr. Wells) says the trouble was "Peach Yellows." Whether he made a careful examination or not I do not know. I had no correspondence with him about it. I could not find any "prematurely ripened and spoiled peaches" though search and inquiry were repeatedly made.

No root lice had been seen, and Kellerman did not believe that excessively hot weather of itself could account for the trouble. This collaboration might have continued for some time had not,

⁵⁵ B. D. Halsted, What the station botanists are doing, *Botanical Gazette* 16: 288 f., Oct. 1891.

⁵⁶ 6 (3): 117-118, Jan. 6, 1891.

⁵⁷ Letter, September 30, 1889.

⁵⁸ E. F. Smith, The peach rosette, *Jour. Mycology* 6 (4): 143-148, Apr. 30, 1891 (results of his 1890 investigations in Georgia and Kansas).

on September 1, 1891, Dr. Kellerman resigned his position to accept the chair of botany at the Ohio State University. On May 1, 1891, Swingle had accepted a call to the Division of Vegetable Pathology of the United States Department of Agriculture. Earlier, when he was informed by Galloway of Swingle's appointment, Smith rejoiced that "Swingle [was] now one of us." A. S. Hitchcock, a graduate of Iowa Agricultural College and an assistant in the Missouri School of Botany, took Kellerman's place at Kansas. His interest was more in taxonomy and agrostology than mycology and pathology. However, a very able graduate of the class of 1887, Mark Alfred Carleton, was recalled by his alma mater, Kansas State Agricultural College, from his position as an instructor in natural sciences at Garfield University. His appointment as assistant in botany was to take effect for the year 1892. At this time, Carleton was showing promising astuteness as an investigator of the cereal rusts and smuts, at least, an enthusiastic interest in their study, and, not many years hence, he, too, would be summoned by Galloway to the Department's service.

Throughout the year 1890, Smith, as he had promised himself to do, did much with photography. In the autumn, he journeyed to Griffin, Georgia, and photographed his inoculated trees, together with the controls, of the experiment. Twice he travelled to Amherst, Massachusetts, discussed with Dr. Goessmann, and Maynard, the results of his orchard experiments in peach yellows, and, while there, became acquainted with James Ellis Humphrey, vegetable physiologist of the Massachusetts Agricultural Experiment Station. "Greatly" interested in Humphrey's work on black knot, and knowing him to be a student of Farlow, Smith wrote to Farlow to explain he regretted he had been unable to get to Cambridge also and to praise Humphrey's work. As soon as he returned to Washington, Smith sent Humphrey a specimen of the black knot from Maryland. Harvard's Cryptogamic Laboratory was now in its new building, and Farlow, replying there were a "good many things to be seen," regretted, too, that Smith had not found it possible to visit the Harvard campus. Smith had visited the campus of Yale University, but never Harvard's. Smith informed Farlow concerning the return from studies abroad of Newton B. Pierce,⁵⁹ who, before studying plant pathology with

⁵⁹ Beverly T. Galloway, Newton B. Pierce, *Phytopathology* 7 (2): 143, Apr. 1917.

Dr. Spalding at the University of Michigan, had pursued special work in entomology at Harvard. Smith wrote:

Pierce, our special agent at work on the California vine disease, has come back from six months in the vineyards of Mediterranean Europe and Africa—full of new facts and interesting observations. There was no money available for investigation in foreign countries. So he paid his own expenses. He is now writing a preliminary report upon the California disease. Until conversation with him this winter I had really a very inadequate notion of its extent and destructiveness. It is worse even than peach yellows in Delaware.

He, while at Naples, Italy, and Palermo, Sicily, had written to Smith. Much he told of the agricultural practices of the regions he had visited, even the practices reminiscent of those of the ancients and still in vogue. Probably Pierce remembered Smith's review⁶⁰ of Felix von Thümen's monograph, "Die Pilze des Aprikosenbaumes (*Armeniaca vulgaris*, Lam.)," recollecting that Smith had pointed out the treatise's importance to the apricot industry of California. For, wrote Pierce:

Peach trees look well in Italy but, so far as my observations have extended, are badly blighted by leaf-curl in France. The fruit is just now coming into market here—probably from regions to the south of Naples. Apricots are in the market constantly, but thus far will not average up with southern California apricots. No peach yellows has been seen, and I have been constantly on the lookout for it on your account. To my mind, peach-curl does much more harm to the tree than people are in the habit of thinking(?) Does not gummosis of the peach follow this trouble?

I have had some decided compliments for your work on peach yellows—please accept.

Cavara and Briosi are the best men met thus far in Italy—they are at Pavia—and I spoke to them of our work. . . .

Thus far I have covered the leading vine regions of France and Italy, have the situation very well in hand as regards distribution of vines and methods of culture, as well as much information relative to diseases. I am not ready to speak yet relative to Mal Nero and our disease. First the work in this Province and then of Catania in Sicily—the latter place being where the disease did its worst work years since. . . .

A letter, written by Pierce at Rome, Italy, evidently has been lost. After he had been at Catania, however, he sent his letter from Palermo:

⁶⁰ *Jour. Mycology* 5 (4): 222-223, Dec. 1889.

I was glad to see that you had had a chance to go to the western and southern lines of the field of your labor. As you justly remark, it pays to study these diseases and all the conditions possible. I am overjoyed at my opportunity to study my matter in Europe, and I feel that I will lose nothing in the end even if it will cost me about a thousand dollars besides losing several months salary.

It gives one confidence in his opinion to know that he has seen and studied his subject over as much or more of a field than have those who have propounded their theories. I assure you this is a rich field for study in our line. Everything in the Vegetable Kingdom is diseased here, and many new forms of disease—so far as we are concerned. I have been obliged to stay here much longer than I expected, but I have felt as you wrote that I would regret it if I spared either time or money while here. I have placed some material in alcohol for you from Syracuse and I believe I have some dry material from one or two other points—I mean of the peach aphid. I have some facts, material and photos, relative to peach diseases here which will interest you, I am sure. No. "yellows" has been seen thus far in any part of Europe. . . . Peach leaf-curl is very bad in France but not so bad in Italy as Sicily.

Tuberculosis of the olive has given me a study of deep interest as it is one of the few diseases which has been proven as caused by bacteria. I have collected a considerable amount of good material. Have formed the friendship of Dr. Savastano who did the work on this disease at the Zoological Laboratory at Naples where there is a bacteriological outfit. Savastano is a fine fellow and is located at the Agricultural School at Portici, where prof[essor] Comes is, and where Dr. Giglioli is Director.

Pierce also used photography in aid of his studies in southern France, Italy, and the eastern half of Sicily. Returning to America, he immediately began to prepare his report on the California vine disease,⁶¹ and, since the olive industry was "becoming one of importance on the Pacific coast," he prepared an article on "Tuberculosis of the Olive" which was published April 30, 1891 by the *Journal of Mycology*.⁶² Bacteriological investigations of the California vine disease⁶³—those performed by Pierce in California and those done by Galloway at Washington—had yielded only negative results; at least, thus far, no causal connection between the isolated bacteria of the vine inner tissue and the disease had been established beyond questioning.

One night, while at Washington during the period Pierce

⁶¹ Bulletin No. 2, Division of Vegetable Pathology, U. S. Dep't of Agric., Washington, Gov't Print. Office, 1892, entitled, The California vine disease. A preliminary report of investigations.

⁶² 6 (4): 148-153.

⁶³ The California vine disease, preliminary report, 208.

worked on his report and made use of the Department's laboratory facilities, he again wrote to Smith:

"Tis Tuesday evening and I am at my room. Tired as usual at night. Have been hard at the subject of *Dematophora* etc. at the Dep[artmen]t today. Thank goodness I am now beginning to see daylight through the other side of my report. If my nerves hold out I expect to complete the ms. in two to three weeks. Have to consider *non-parasitic diseases*, the *nature of disease*, and *methods of treatment* tried. I have also to write the opening chapter on the "situation." Probably may have to speak of the hot house experiments also though that depends on Prof[essor] Galloway's ideas. I have said nothing of my experiments in California along that line and don't care to until the work is more complete—hope Mr. Galloway will think the same about the hot-house work as its results were all negative. I hope you are doing nicely in your peach orchards nowadays. I would like to drop in on you. . . . Swingle appears to be a good fellow and smart as you said—good addition to the strength of the Division. Waite is north spraying trees. He has made some good points this spring I think. . . . Has demonstrated that crabs and pears may be blighted through the flower without puncture, that the other flowers on the same tree will be blighted without further inoculation—probably by transfer of germs by insects, has captured insects and grown germs from them, has shown on a small scale that the Bordeaux mixture will kill germs in inoculated flowers and also prevent the blighting the same. Good strong points and valuable in my estimation.

Smith had already begun to gather literature on plant disease dissemination. Pierce's letter was written on May 5, 1891, and two months before, on March 5, Trelease of the Shaw School of Botany had responded to a request from Smith for his published writings, if any, on the subject, saying: "My lectures on dissemination were never published. All I had was a synopsis of topics, distributed at the lectures, and no copies were saved."

Smith believed that the "greatest advance of this decade, probably, was the discovery that diseases of animals and plants may be disseminated by insects and arachnids. This idea," he said,⁶⁴

did not come wholly out of the blue. Nothing ever does. Various persons had ascribed Texas fever to cattle ticks. Arthur had suspected insects of carrying pear blight and Carlos Findlay, of Havana, as early as 1881 had suspected the mosquito of being the transmitter of yellow fever, and had actually pointed out the particular one that does it (*Aedes aegypti*). But these were happy guesses unlike a thousand others. There have been innumerable hypotheses on all sorts of subjects, some very clever ones, from

⁶⁴ Fifty years of pathology, *op. cit.*, 24-25.

the time of the Greeks, but we must carefully distinguish between hypothesis and proof. What Arthur and Findlay and others suspected detracts in no way from the honor due to the men who first experimentally demonstrated the existence of carriers of diseases. . . . Merton B. Waite, working on pear blight, proved conclusively the transmission of the disease by bees, from whose mouth parts he again cultivated the parasite (1889-1891). . . . Theobald Smith, working on Texas fever of cattle, discovered the parasite to be a protozoan inhabiting the red blood corpuscles and he and Kilbourn demonstrated that the carrier of the disease from infected to healthy cattle was a blood sucking tick (1889-1893).

The search for intermediary hosts in the transmission of plant diseases was not new. Recollect Farlow's work on the cedar ball fungus which begins its life on cedars, spends its first year there, in the spring emits gelatinous projections which when rain falls becomes a mass, and from which spores, windborne, infect fruit trees miles away with orchard rust, then in the late summer to begin another life cycle. Recall the work of DeBary, Ward, and others, tracing fungus life histories through developmental stages and more than one host.

Proof of disease transmission by insects, however, was new. Before the end of the century, an Englishman, Sir Ronald Ross, would demonstrate that the mosquito, as an intermediary host, transmits malaria. Others earlier had suggested this. But Ross's work, supplying knowledge of the method of action of the transmitting agency as well as the cause of the disease, made possible a period of practical prevention and extermination of the malady in seriously infected regions.⁶⁵ He, for the value of his proof, received one of the first Nobel prizes for medicine.

During the summer of 1889, to test the prevailing theory that ticks caused southern cattle fever, Dr. F. L. Kilborne of the Bureau of Animal Industry arranged at the Bureau's Washington Experiment Station some preliminary experiments. In one enclosure he placed North Carolina cattle with native cattle, and picked off the ticks of all southern stock. This was done to prevent any ticks from maturing or depositing eggs which might later infect the animals. In another enclosure the ticks were not removed from the southern cattle. Native cattle, mixed with these, contracted Texas fever and died, whereas the cattle of the first enclosure lived, unharmed by the disease. In September further

⁶⁵ H. Cushing, *The life of Sir William Osler, op. cit.*, 1: 274; 2: 164.

field experiments were arranged: "one with southern cattle and ticks, a second with southern cattle from which the ticks were removed, and a third over which only adult ticks had been scattered. . . . In the first field no natives died," reported Dr. Theobald Smith in his account of "Investigations of the Infectious Diseases of Animals," published in the report of the Chief of the Bureau for the year 1890,⁶⁶ "but careful examination of the blood by the writer showed Texas fever in an unmistakable manner. In the 'tick' field one animal died of Texas fever, and the examination of the blood showed that most other natives in the field were sick. In the third field containing southern cattle without ticks no disease could be detected." Confirmatory and further experiments followed in 1890, during which "ticks were hatched artificially and placed on cattle with the result that Texas fever appeared in every case."⁶⁷

Galloway, in his annual report as Chief of the Division of Vegetable Pathology for the year 1891,⁶⁸ announced that Waite, working mainly that year on the life-history of the pear blight organism, had cultivated in the laboratory "the germ" on sterilized nutrient media, and its life history, through culture study used in field experiments, had been carefully examined, both at Washington and in orchards near Brockport, New York. Waite had found that the

germs normally gain an entrance through the tissues of the nectaries. At other points the germs gain an entrance through a puncture or injury to the epidermis. The germs multiply in the nectar of the flowers, and are carried by insects from one flower to another. Bags of paper, cheese cloth, mosquito netting, or in fact anything keeping the insects out, will preserve the trees from blight. Certain varieties of pear, the Bartlett among them, failed to set fruit when insects were excluded. Others, such as the Dutchess and Seckel, did not need insect aid.

More time and research would be required to transmute from premise to conclusion, from observation to fact, another contribution for which Waite is known—his concept of mixed plantings

⁶⁶ Pp. 105-110, at pp. 107-108.

⁶⁷ T. Smith and F. L. Kilborne, Investigations into the nature, causation, and prevention of Texas or Southern cattle fever, *Bureau of Animal Industry, Bulletin* No. 1, 301 pp., 10 pls., 7 figs., 1893. See also *Experiment Station Record* 4(9): 755-758, Apr. 1893, at p. 756.

⁶⁸ Pp. 372-373.

in horticulture as indicated by the self-fruitfulness of some varieties while others to set fruit require cross-pollination by insects.

Waite's carefully prepared bulletin 5, "The Pollination of Pear Flowers," a scholarly work, replete with the results of careful, scientific, experimental study performed over two years, would appear, published by the Division of Vegetable Pathology, in 1894. Waite admitted that, while the work was not strictly pathological, its origins had sprung from what was at first a pathological investigation. His conclusions, arrived at from extensive experiments conducted at the Old Dominion orchard at Chestnut Farm, Virginia, at the Smith and Slosson orchards at Geneva, New York, in the Ellwanger and Barry orchard at Rochester and at Brockport, New York, offered substantiated facts, valuable to advance pure science in horticulture as well as practical rules which the farmer should follow in planting orchards of mixed varieties.⁶⁹

In 1890 twenty-nine botanists were employed at various agricultural experiment stations over the United States. When Halsted asked them to list what problems appeared most immediate in their states, sixteen replied "fungous diseases of cultivated plants." Studies of grasses and forage crops, weeds and their migration, reforestation and forest trees, waste land planting, cross-fertilization of plants, relations of climate and soil to vegetation, seed testing, and allied subjects, were ranked in order next in importance. One botanist, unnamed, considered bacterial diseases of plants as among the more important research subjects.⁷⁰ By 1893 thirty-two stations employed botanists. Fungous diseases of plants and their treatment was still the favored investigation subject, and, joined to this, was a growing interest in plant bacterial diseases. Many botanists were yet preoccupied with systematizing their native flora. A few were tracing life histories of fungi. Some few had inaugurated physiological experiments.⁷¹

George Francis Atkinson, who on October 1, 1892, had left the Alabama station and been appointed cryptogamic botanist

⁶⁹ See pp. 80-82 of *Bulletin 5*. See also Waite's article, Treatment of pear leaf-blight in the orchard, *Jour. Mycology*, 7(4): 333-338, Aug. 15, 1894, in which are set forth the author's experiments carried on in the orchard of the Old Dominion Fruit Co., on James River, near Scotland, Virginia, and elsewhere. Waite had begun in 1890 at Thomasville, Georgia, to observe the role of insect visitors in pear flowers. See *Bulletin 5*: 17. Fairchild performed the experiments at Geneva.

⁷⁰ *Botanical Gazette* 15: 279, 1890.

⁷¹ *Botanical Gazette* 18: 81, 1893.

in the college of arts and sciences at Cornell, compiled for *Science*⁷² a review of station botanical work during 1892 similar to that which Halsted had assembled for the *Botanical Gazette*.⁷³ the preceding year. Atkinson found "one of the most unique features of the botanist's work at the experiment stations is the study and treatment of plant diseases caused by fungi and bacteria." He, while in the south, almost alone, had placed on an enduring basis the study of plant diseases according to crops. He may have patterned his work after policies of the Division of Vegetable Pathology. But the plan of his work influenced other workers at experiment stations in the south. On January 6, 1891, Miss Southworth published in the *Journal of Mycology*⁷⁴ her study of "Anthracnose of Cotton," and described the new species, *Colletotrichum gossypii*. After her work was completed but before it was published, Atkinson read a paper on the same subject before the American Association of Agricultural College and Experiment Stations. During these early years of the science, more than one study of the same malady was made by different workers. Occasionally, the same disease was studied from different aspects. Miss Southworth "cited Professor Atkinson's authority in regard to the parts of the host plant attacked." Their work, however, was independent.

Atkinson, in his 1892 article on "Botany at the Experiment Stations," commended the use of artificial cultures in studying the life histories of parasitic fungi. "No other feature of botanical work at the experiment stations, in my judgment," he wrote, "is doing so much to lay the permanent foundations for a rational economy in the treatment of plant diseases" as this. He listed Alabama, Connecticut, Delaware, Illinois, Indiana, Iowa, Kansas, Kentucky, Massachusetts, New Jersey, the New York stations at Geneva and Cornell University, North Carolina, and North Dakota, as stations where the cultural apparatus was more or less complete.

At these stations, most of the workers, as Atkinson himself, had been students of Farlow or had studied under someone who had learned culture practice from him. On the point of station equipment, not every botanist agreed with Atkinson. As late as 1894,

⁷² Botany at the experiment stations, *Science* 20(514): 328-330, Dec. 9, 1892.

⁷³ What the station botanists are doing, *Botanical Gazette* 16: 288, 1891.

⁷⁴ 6(3): 100-105, Jan. 6, 1891.

Louis Hermann Pammel of the Iowa station complained to Smith that some of the station work in bacteriology was so careless that he feared that it would discredit all American work. During the school year 1885-1886 Pammel had been Farlow's secretary and from 1886 to 1889 he had been Trelease's assistant at the Shaw School. Before becoming professor of botany at Iowa Agricultural College, he had done special work for the federal department of agriculture. Particularly he criticized "some of the work done at some of our Experiment Stations recently in which some of our bacteriological diseases of plants are described and no attempt made to cultivate the organism." Pammel had written a paper on "Bacteria in relation to Modern Medicine, the Arts and Industries." Smith read it, sent a letter of appreciation, and on October 1, 1894, he replied that he had prepared the article "to correct the numerous statements that appear in our daily press as well as some of our scientific literature." He believed that some of the responsibility for erroneous statements was due to "some of the work done by Americans on this subject."

In January 1889 Michigan's special laboratory devoted to teaching and research in hygiene had been opened at the university at Ann Arbor. Vaughan and Novy were justly proud of this building in which they were to teach the results of their study with Koch and at other research places in their recent visit to Europe. It was announced that all of the improved apparatus employed by Koch was available for a three-months' course in bacteriology.⁷⁵

In 1889 Welch had arranged for a laboratory of hygiene for the faculty of medicine being organized at Johns Hopkins. Modelled after von Pettenkofer's laboratory at Munich, this was to supplement other laboratories and facilities for work in experimental pathology, bacteriology, pathological histology, etc. In 1886 he had sent Alexander Crever Abbott, a graduate in medicine in 1884 from the University of Maryland, to study with von Pettenkofer and Koch at the universities of Munich and Berlin. Abbott had returned and started the hygiene laboratory at the Hopkins. But within a few years, Dr. Billings organized an Institute of Hygiene at the University of Pennsylvania and Abbott went there as professor of hygiene and bacteriology.⁷⁶ Billings had

⁷⁵ Announcement made in the University of Michigan catalogue. See, *William Henry Welch* etc., *op. cit.*, 342; also *A doctor's memories*, *op. cit.*, 146-149.

⁷⁶ *William Henry Welch* etc., pp. 341-341, 506, n. 1.

lectured on hygiene at Johns Hopkins since perhaps 1883. A great School of Hygiene and Public Health at the University of Pennsylvania would grow out of this Institute, later Laboratory, of Hygiene. On University Day, February 22, 1892, appropriate ceremonies were held to dedicate its building on the university campus. That year the first edition of Abbott's *Principles of Bacteriology* appeared. Further, when that year seven ships brought into the port of New York almost a hundred cases of cholera from Hamburg, Germany, where a severe epidemic was in progress, ample justification for equipping the first municipal bacteriological laboratory was seen, and Biggs, Prudden, and Welch made the most of it. Already the distinctively American achievement of establishing laboratories in conjunction with state boards of health was under way. Public support of this first municipal bacteriological laboratory in the United States was enthusiastic when Dr. Edward K. Dunham, under Biggs' direction, made cultures of cholera bacilli which were confirmed both in this country and in Germany, and, with the use of quarantine, preventive, and other sanitary methods, pathologists and hygienists were able to halt the spread of a dangerous disease.⁷⁷

In 1892 Dr. Councilman, also a graduate in medicine from the University of Maryland and since 1886 associated with Welch in pathology at Johns Hopkins, went to Harvard as Shattuck Professor of Pathological Anatomy, and to his place was appointed Simon Flexner, a young druggist from Louisville, Kentucky, who had studied medicine at the university there, taught himself pathology, and since 1890 been in Baltimore eagerly endeavoring to study with Welch.⁷⁸ George H. Falkiner Nuttall, a graduate in 1884 in medicine from the University of California, who had obtained his Master of Arts and Doctor of Philosophy degrees from the University of Göttingen, was Welch's assistant in bacteriology at the Hopkins. In 1893 the Johns Hopkins Medical School officially opened its doors with its "Faculty par excellence,"⁷⁹ comprising, of course, its great four, Welch, Osler, William S. Halsted, and Howard Atwood Kelly, and eight professors. Four of these professors held degrees from the University of Michigan

⁷⁷ *William Henry Welch and the Heroic Age of American Medicine*, *op. cit.*, 343, et seq.

⁷⁸ *Idem*, 160.

⁷⁹ *A doctor's memories*, *op. cit.*, 223, a quotation from Victor C. Vaughan.

—Henry Mills Hurd⁸⁰ in psychiatry, William Henry Howell in physiology, John Jacob Abel in pharmacology, and Franklin Paine Mall in anatomy. Fielding H. Garrison has said of their work:

. . . its faculty soon established a well-deserved reputation, at home and abroad, for original scientific work. In Welch's laboratory, Nuttall, Flexner, Councilman, Mall, Abbott, Wright, Sternberg, Walter Reed, and many others were trained, and out of it came his own original work on the experimental production of diphtheria by its toxins, on the bacteriology of wound infection, on the gas bacillus and the diseases produced by it, as also the work of Walter Reed on the pathology of typhoid fever, of MacCallum and Opie on the malarial parasite, of Opie on pancreatic diabetes, of Thayer and Blumer on gonorrhoeal endocarditis. Reed, Carroll, and Lazear, who discovered the causation and prevention of yellow fever, were all pupils of Welch. From Osler's clinic came the extensive studies of malarial fever by Thayer and others, of amoebic dysentery by Councilman and Lafleur, of eosinophilia by Thayer and Brown, of pneumothorax by Emerson.⁸¹

For several years H. Newell Martin and W. K. Brooks had been giving undergraduate and advanced laboratory and lecture instruction in biology. Research and learning in the phenomena of living things were taught, and organisms from lower to higher plant and animal forms were traced, including such genera as *Torula*, *Protococcus*, *Amoeba*, *Micrococcus*, *Bacterium*, *Spirillum*, *Penicillium*, *Mucor*, *Spirogyra*, *Nitella*, and others.⁸² At Massachusetts Institute of Technology, Dr. Sedgwick had been developing biological instruction, and graduating theses under him during 1887-1892 included examining water supplies, biological and chemical characters of bacteria, life history of amoeba, physiology of the sundew, and such other subjects of recognized value that by 1892 five men in addition to Sedgwick were required as departmental instructors.⁸³

During the 1890's profound specialization began to characterize the building of bacteriology as an exact science. Erwin F. Smith regarded this as a milestone of progress. In various types of investigation, specialists began to come forward. Along with animal and plant bacteriologists emerged specialists in the grow-

⁸⁰ Thomas Stephen Cullen, *Henry Mills Hurd, the first superintendent of the Johns Hopkins Hospital*, 16 et seq. Baltimore, The Johns Hopkins Press, 1920.

⁸¹ John Shaw Billings, *A Memoir, op. cit.*, 210-211.

⁸² *Johns Hopkins Circular* 58: 97, July, 1887.

⁸³ *A pioneer of public health, William Thompson Sedgwick, op. cit.*, 35-37.

ing fields of water bacteriology, milk bacteriology, and soil bacteriology. Not yet had the era of special societies devoted to study of special diseases (for instance, tuberculosis) arrived fully. The establishment of scientific journals, dedicated to advancing special phases of bacteriological research, was not yet fully under way. State and municipal health laboratories naturally were pre-occupied with diseases to which the human body was subject, furthering not only diagnostic aid to physicians but also immunological and serological assistance in the forms of vaccines, anti-toxins, chemotherapeutic remedies, and the like, and securing and enforcing quarantine and sanitary regulations, sometimes with the aid of the law. From an academic point of view, the differentiations that developed may be illustrated by glancing at a course given by Sedgwick at Massachusetts Institute of Technology, which, in 1883 a course on germs and germicides, became by 1888 a course in bacteriology, from which eventually were split off separate courses, sanitary biology in 1889 and industrial biology in 1896.⁸⁴ Prudden's early work in bacteriology of ice and air was pioneering, even as was H. W. Conn's work at Connecticut Wesleyan University in milk bacteriology. Probably no one American institution contributed more in the pioneering period of these subjects and in microbiology of water supplies—water purification and sewage disposal—than the Massachusetts Institute of Technology. Its work, together with that of the Lawrence Experiment Station founded in 1886 and the biological laboratory of Chestnut Hill Reservoir, had begun "an important epoch in American sanitation." Steadily the work had gone forward. Sedgwick had graduated some brilliant students whose work from time to time will appear in this story.

Among them was Edwin Oakes Jordan, '88, who after serving as chief assistant biologist of the Massachusetts State Board of Health 1888-1890 and lecturer at Massachusetts Institute of Technology during his last years there, and after two years as fellow in morphology at Clark University, went as an associate in anatomy and instructor to the University of Chicago. In 1895 at this institution he became an assistant professor of bacteriology. Allen Hazen, '88, George Chandler Whipple, '89, and George Warren Fuller, '90, all from the Massachusetts Institute of Technology,

⁸⁴ *A pioneer of public health William Thompson Sedgwick, op. cit.*, 40, 59-60, 64.

became members of a celebrated firm of consulting engineers of New York City. Whipple later was a professor of sanitary engineering at Harvard University and his alma mater. Fuller, after a postgraduate course at the University of Berlin and a period with the Massachusetts Board and in charge of the Lawrence Experiment Station, performed elaborate tests for the cities of Louisville, Kentucky, and Cincinnati, Ohio, to determine the most feasible method of purifying water, and in 1899 engaged in practice privately as a hydraulic and sanitary engineer. Gary Nathan Calkins, '90, was later a professor of protozoology at Columbia University. The graduate who was to succeed Sedgwick as professor and head of the department of biology and public health at Massachusetts Institute of Technology was Samuel Cate Prescott, '94. In 1895 he became as assistant in biology, in 1896 an instructor, in 1903 assistant professor of industrial biology and bacteriology, in 1909 associate professor, in 1914 professor of industrial microbiology, and in 1922 was placed in charge of the department.

Plant bacteriology would not figure much in this work for many years. By 1911, however, the research work of Erwin F. Smith in the United States Department of Agriculture had so stimulated Sedgwick's interest that he addressed a letter to Smith, as follows:

Plant pathology nowadays is a mighty interesting thing, and those of us who twenty or thirty years ago undertook to say that there is really such a subject as Biology, because of the close relationship and the analogous behavior of plants and animals under various conditions, rejoice to find in your work any justification that may be needed for their attitude.

In 1891, just two decades before, Harry Luman Russell, having found the study of bacterial diseases of plants still conspicuously lacking in Germany, had returned to the United States and registered as a graduate student in pathology and biology at Johns Hopkins University. Even in Koch's laboratory he had found the research interest almost entirely confined to infectious diseases of animals and humans. Carl Fraenkel had been then in immediate charge. Koch directed, supervised, and often worked in the laboratory. Engaged there in study and research had been such workers of distinction as Shibasaburo Kitasato, not yet forty years of age, and Emil von Behring, who together had elaborated an antitoxin for tetanus.⁸⁵ In 1890 von Behring, from the blood of

⁸⁵ Douglas James Guthrie, *A history of medicine, op. cit.*, 287.

horses immunized against diphtheria, had developed a diphtheria antitoxin or serum by which the human death rate from this disease was definitely lowered. In 1894 Kitasato would make his discovery of the bacillus of plague. By 1891 he had founded, and become director of, Japan's famous Institute for Infectious Diseases. Six years he had studied under Koch.

In October 1890, while Russell was in Koch's laboratory, the great master had startled and thrilled the world with the announcement of his treatment, tuberculin, for human tuberculosis, a remedy, which, while it proved disappointing as a "cure" for pulmonary tuberculosis, was made use of later by Russell and others as a diagnostic method of much value in detecting tuberculosis in cattle.⁸⁹

Charles Wardell Stiles, later an American authority on hook-worm diseases, had been educated at Wesleyan University, Connecticut. While Russell was in Europe, Stiles was there, too, studying at the universities of Berlin and Leipzig. In 1892 he would become professor of medical zoology at Georgetown University, and in 1894 a special lecturer on the same subject at the Army Medical School and later Johns Hopkins.

Plant students had gone to Europe for advanced study. But, during the early 1890's, their number was still few, and doubtless far less than those interested in the animal sciences.

During the summer of 1889, Volney Morgan Spalding had studied at Jena and been given such a welcome that he wrote of it to Smith. Professor Detmer took Spalding into his home and his private laboratory and permitted him every facility for some physiological experiments, mostly on plant assimilation. Spalding stopped at Bonn and heard the great Eduard Strasburger lecture, enjoyed a pleasant chat with Schimper, examined the laboratory and appliances there, learned more of microscopes and the camera, visited Leipzig and its botanical institute, and on his journey homeward spent some while at Oxford. At the University of Michigan eight students by 1890 were in his class in plant physiology. Among them and graduating that year, Frederick Charles Newcombe, after doing some teaching at his alma mater, went to the University of Leipzig and obtained in 1893 his doctorate

⁸⁹ Unpublished manuscript, In Koch's Laboratory when his "Tuberculin Cure" was announced, prepared by Dean-Emeritus Russell.

in philosophy. But before leaving Michigan he and Spalding had collected and studied many parasitic species. During the first semester 1889-1890 the class in plant physiology under Spalding had translated Zimmermann's *Pflanzenzelle* and the second semester read Sachs's treatise on physiology. Considerable time was spent also with laboratory experiments and special reports. Later, on an extended leave of absence, Spalding was to return to Europe to study at Leipzig and other universities and botanical institutes.

What physiology and pathology Russell learned while in Europe was more on the animal than plant side of biological investigation. This was true of his friend Stiles who happened also to be in Berlin. Stiles persuaded Russell to visit the famous Marine Zoological Station at Naples where a research table for American scientists interested in research was maintained. Russell travelled in Germany and the Tyrol Mountains, stopped at Venice and Rome, and, arriving at Naples, was invited by Dr. Anton Dohrn, officer in charge, to stay. Russell's suggestion that he study bacteria of the deep sea met with Dr. Dohrn's approval and he was given laboratory quarters with two or three scientists who were studying tropical diseases. Russell remained at Naples seven months and, after further study at L'Institut Pasteur at Paris, he had to choose whether to stay in Europe, or return to the United States, to complete his work for the degree Doctor of Philosophy.

He chose America. Bacteriologists, even in Europe, were still preoccupied with studying bacteria as an adjunct or "anhang" to a medical course. Russell was desirous of studying the non-parasitic, more normal functions, and, in this, anticipated his future leading role as a milk bacteriologist and student of fermentation. Many European scholars had not begun to study bacteria in the light of their productive functions in foods. Only one or two, Russell found, viewed them as living organisms, and neither the Naples institution nor the Pasteur Institute granted degrees. At the least, he believed more was to be gained by taking advantage of "the broad educational outlook that was coming to be widely recognized in the work of the Johns Hopkins University," which esteemed research and graduate work as of special significance. From boyhood, plant life had always fascinated Russell. But how to study plants and yet take work in Welch's laboratory seemed to present

a problem until Russell conferred with Welch after some preliminary correspondence. Russell has told what he told Welch:⁸⁷

I proposed to undertake a study of the bacterial diseases of plants, laying down as my basic thesis, the idea that here was a relatively new field, one which was then not recognized as of any importance.

Bacteria in general live preferably on neutral or slightly alkaline food; fungi, on the other hand, prefer acid substrata. It is for this fundamental reason, that we find many more diseases of man and animals caused by bacterial parasites, while plant diseases are generally caused by fungi. At this time there were scarcely any workers in bacteriology who had paid attention as to whether plants could become infected with bacteria. I hoped that this point of view would appeal to the broad-minded Welch who readily saw the relationship which existed between healthy and diseased cells in the animal body. Much to my gratification Welch instantly grasped the situation, saying he knew nothing about the pathology of plants, but he saw no good reason why the same general basic principles would not obtain with plant life as obtained with animal life. If I could paddle my own canoe, i. e., develop the necessary technique that applied to plant diseases, he would give me all the aid he could, which however he said would not be much. . . .

No greenhouses were available in which plant experiments could be carried on. The laboratory was reeking with the fumes of burning gas making it impossible even to grow plants. Obviously I was handicapped from the outset through lack of proper facilities. My laboratory colleagues, all headed for M. D. degrees, wondered why I should hang around a place like this. They thought I was wasting my time, after having the experience I had had in the leading European medical laboratories. They advised me to go into medicine and specialize if I wanted to on the side of animal diseases. But it was worth much to me to have the opportunity of contacting "Popsy," as he was lovingly called by all of his student worshippers.

Something, however, had to be done to make good the lack of reasonable facilities where plants could be grown. Doubtless I could have turned to the Botany department of the University, but even here the facilities for greenhouse work were most meager, and the department was located on the other side of Baltimore in the downtown district.

Russell turned to Secretary Rusk of the United States Department of Agriculture and was allotted space for his plants in the Department's greenhouses. Commuting between Baltimore and Washington, he began work on his thesis subject, "Bacteria in their Relation to Vegetable Tissue," and thus became acquainted with Galloway, Waite, Smith, Swingle, Fairchild, and others of the Department. For the first time in American literature, all of

⁸⁷ Getting started in Bacteriology, *op. cit.*, and another unpublished manuscript by Dean Russell telling of his work at the Naples Marine Biological Station.

the known bacterial plant diseases were treated in a single document. The work was divided into three classes: those diseases so established that a causal organism had been isolated from the diseased tissue, cultivated in pure cultures, inoculated into a healthy susceptible host plant, and the disease reproduced and an identical organism taken from inoculated vegetable tissue, in other words, those diseases in which the celebrated canons of Koch had been thoroughly satisfied; second, those diseases in which specific organisms had been found, but the evidence was not fully conclusive that Koch's canons had been strictly complied with; and, third, Russell listed those plant diseases which probably were of bacterial origin but not definitely proven.

Smith later praised Dr. Russell's experiments, especially their "merit of being properly performed, i. e. with sterile juices and pure cultures so that the conditions under which the experiments were made [could] be reproduced by other investigators." His praise was not unqualified,⁸⁸ and he found that Professor W. Migula had reviewed the subject in 1892 "briefly but somewhat carefully." Others⁸⁹ had written on bacterial diseases of plants. Julius Wiesner, a year after Russell's experiments were performed, made some experiments as part of an attempt to classify plants, but cultures were not used and the juices not sterilized by filtration. During the spring of 1892, Charles O. Whitman, then recently appointed professor of zoology at the new University of Chicago, offered Russell the chance to continue his researches in bacteria of the deep sea at the Marine Biological Laboratory at Woods Hole, Massachusetts, and in 1893 he was appointed fellow in bacteriology at Chicago University. The environment was biological rather than medical, and his thesis, published by Johns Hopkins University in 1892, possessed the added distinction of being the first submitted for the degree of Doctor of Philosophy under Welch. Professors Martin, Brooks, and Welch conducted Russell's doctoral examination, and Welch acquired a knowledge of plants sufficient for the purpose from Dr. John P. Lotsy, a Dutch botanist, who became a university associate in biology and was then doing some studying in his laboratory. February 28, 1893, Russell sub-

⁸⁸ The bacterial diseases of plants: a critical review of the present state of our knowledge, *Amer. Naturalist* 30(356): 629-632, Aug. 1896.

⁸⁹ Erwin F. Smith has reviewed the early works of writers on this subject, *Bacteria in relation to plant diseases* 2: 9-12 ff., Carnegie Inst. Washington, 1911.

mitted his list of bacterial plant diseases to Smith for any corrections or ideas he might wish to offer and to secure the benefit of his experience and knowledge of the subject and its literature.

Smith for a number of years had served as secretary of the Washington alumni association of the University of Michigan. Whether in this capacity or while in Michigan he became acquainted with Dr. Henry M. Hurd, superintendent of Johns Hopkins Hospital and professor of psychiatry in the medical school, is not known. However, Dr. Hurd, no later than January 30, 1892, had become acquainted with Smith, for, on that date Hurd wrote him thanking him for his recent monograph on peach yellows and peach rosette and for an "enjoyable evening in Washington" when he had met many old, and some new, friends "with a tie of common interest," which must have referred to an alumni interest in the University. Dr. Hurd added to his letter: "By the way John P. Lotsy a Fellow-by-Courtesy, a Ph.D. of Göttingen, is to give some lectures in Dr. Welch's Pathological Laboratory on Plant Diseases at an early day. If you come over I shall be glad to send you a special invitation."

Lotsy published in June, 1893, "On the Toxic Substance of the *Bacillus Amylovorus*, the Cause of Pearblight."⁹⁰ Certainly, therefore, his lecture could have considered bacterial, as well as fungous, diseases of plants and, if so, Smith must have attended, since by the years 1892-1893 his research interests embraced every plant disease including those caused by bacteria and fungi.

Field research in crop diseases of the southern states and around Washington, including subsisting and new experimental work in Maryland, absorbed his attention. During all seasons of the year Smith's indoor laboratory work had been almost completely interrupted. In 1891 he and Swingle had been sent to Georgia and Florida. Before they went, however, Galloway had appointed Lucien M. Underwood, (Professor of Botany at Syracuse University, and during other periods of time at schools in Alabama and Indiana and at Columbia University, and then on leave of absence and taking graduate work with Farlow at Harvard), a special agent for the Division of Vegetable Pathology, to visit Florida and collect information concerning orange and other plant diseases threatening the state's agricultural economy. During

⁹⁰ *Johns Hopkins Circular* 106: 105, June 1893. The thesis was dated April 18, 1893.

February, March, and April 1891, Underwood had visited orange groves and studied the methods of cultivation and treatment in vogue, in the counties of Brevard, Citrus, Hernando, Lake, Manatee, Marion, Orange, Pasco, Polk, St. John's and Volusia, traversing, also, nine other counties during the winter. He purposely did not report on all the diseases noticed. For instance, he excluded diseases produced by insect pests, although he referred to the presence of the long scale, red spider, rust mite, etc., and such counteracting agents as lady bugs and uses of spraying and proper cultivation. Underwood styled his report, "Diseases of the Orange in Florida,"⁹¹ and considered die-back and foot-rot, attributing these probably to improper cultivation or fertilization; blight, possibly caused by bacteria; scab and leaf spot, believed caused by parasitic fungi; sooty mold, a malady reported on by Farlow in 1876; and leaf glaze, attributed to a leaf lichen.

In May 1891 Smith was at Dover, Delaware, planning and carrying out several series of peach yellows infection experiments, to see what, if any, new knowledge concerning the disease's transmission could be gleaned. He had his microscope with him, and, along with his peach yellows infection experiments, he was studying and making good drawings of the *Monilia*, evidently aware of his lack of complete information concerning the life history of the fungus. That spring he had arrived in the field early enough to study the twig blight from its beginning and experiment with treatments of Bordeaux mixture. Thus far, fungicides had neither prevented nor lessened the blight, so far as Smith could discern. He was trying to coordinate his studies of peach yellows on the Chesapeake peninsula with his work on peach rosette in Georgia which he had started the year before at Griffin. "With a view to clearing up some mooted points in connection with the Georgia Rosette," he told Galloway by letter of May 10th, "I've also budded a good many plums and cherries of different sorts, with buds taken from peach trees having yellows. Many buds were put into each tree and if it is possible to induce the Northern yellows in plums I shall do it."

Soon thereafter he went to Georgia to examine and, to his delight, find that the results of his inoculation experiments of the year previous were "astonishing and important beyond all expecta-

⁹¹ *Jour. Mycology* 7(1): 27-34, Sept. 10, 1891.

tion," so valuable, in fact, that he sent for Swingle or Galloway, both if possible, to confirm his observations. Smith sent for the noted horticulturist and nurseryman Berckmans of Augusta, Georgia. Discovering a "new fact"—gummosis of the fruit—Smith decided to make some sections immediately, and sent for agar dishes for plate cultures with sterilized media, requested Galloway or Swingle to bring a flask of sterile gelatine, twenty-four empty plugged and sterilized tubes, a platinum wire, and agar plates ready for use. Later he sent for a microscope, cedar oil, and other equipment. Swingle arrived and the two men, with high hopes and abundant enthusiasm, went to work, studying not only peach rosette but also other plant diseases, including garden, field, and forest crops. From Canada to Florida and from New England to California, the Department had special agents in the field or cooperative arrangements with experiment station workers studying the more serious of the known diseases of agricultural crops. Galloway in his annual report for that year described with justifiable pride many of the results thus far achieved. The public response to the work was uniformly one of approval.

Smith's and Swingle's experiments awakened a wide interest among the growers and many visited the nursery at Mr. Husted's place. Smith delivered an address before members of the Middle Georgia Horticultural Society. They recognized that the symptoms of rosette were in points dissimilar from those of yellows; and that eradication of affected trees, enforced by law if need be, might prove to be the only solution. They believed that bacteria might cause the malady. This, like peach yellows and another disease which Smith studied later, "little peach," is today believed to be caused by a virus. Scientific knowledge of parasitic organisms in plant diseases, however, had then not progressed much beyond a considerable knowledge of fungi and fungous diseases, and an initial acquaintance with bacteria and bacterial diseases of plants. Many letters which passed between Smith, Swingle, Galloway, and Assistant Secretary of Agriculture Willits have been preserved. These do not say as much as Smith's own final estimate of his accomplishments. He said of his work: ⁹²

In case of peach rosette, which is more southern in its distribution and swifter in its action and was then a new disease, I showed how it differs

⁹² Synopsis of researches, *op. cit.*, 19.

from yellows, proved that it could be spread in the same way by grafting or budding, established that sometimes it did not occur in one side of a tree when it was present in the other side although the following year the entire tree became diseased, showed that when root grafts were made the disease developed later on the top of the plant than when parts above ground were budded with the diseased buds, found numerous gum-pockets in the wood of the diseased roots (wood only of the season in which the disease developed) and in the shriveling fruits, and showed that mere contact of diseased tissues with wounds would not induce the disease but that some fragment of the grafted wood or bark must heal on in order to transmit the disease, although the tiniest bit was sufficient. There were 124 trees in my first budding experiment all but four of which contracted the disease. The signs of disease appeared first around the inserted diseased buds and a few months later the whole top became diseased. These trees stood in two nursery rows and none of the several thousand other nursery trees developed the disease. The cause of the disease was not determined.

Smith may not have found the cause. But he established several fundamental conclusions necessary to a later more complete study of this disease. In America his was the pioneering study and helped to prepare the way for the career in science which was to bring him eminence. June 11, 1891, he advised Galloway:

Today I also discovered that certain cells of the phloem rays (in the roots) are packed full of actively moving bodies 1 to $\frac{1}{2}\mu$ in length. These strikingly resemble bacteria and call to mind what we saw in the cells of the carnation, but whether they are really germs remains to be seen. We made cultures from the bark.

We have made 25 cultures from various parts of diseased trees, but shall have to wait some days for definite results. Some of the agar proved to be cloudy and has behaved very badly. We have two or three sorts of colonies and apparently one germ which liquifies agar.

On June 13 Smith and Swingle notified Galloway that as soon as their cultures would permit, they would go on, as instructed, to Florida. They wished to complete, before leaving Georgia, some inoculation experiments with bacteria they had found. They had also some joint studies with the horticulturist of the "Georgia Experiment Station." Swingle had become much interested in the region's "vinifera" possibilities, and in certain fungicidal treatments being tried against garden crop diseases, tomato, potato, egg plant, sweet potato, in addition to other work. Early in the spring Dr. J. C. Neal of Florida had sent the Department specimens of the southern cabbage-worm (*Plusia brassicae*) affected with a

disease. No fungus being found associated with the trouble, bacteriological methods had been put to use in Washington. Germs were grown on beef peptone, glycerine, potato agar, and nutrient gelatine and broth preparations. But 200 inoculations from pure cultures failed to yield any definite result.⁹³ Swingle, therefore, began to study this subject soon after arriving in Georgia. By letter of June 12 Galloway directed Swingle to gather all the facts he could concerning the disease in Florida. On June 23 Swingle advised Galloway, "We start for Florida tonight. Send plusia disease cultures to Lake City."

Smith and Swingle⁹⁴ went via Jacksonville to Lake City where, after an afternoon at the Florida Experiment Station examining a peach orchard of two to three hundred young trees and coming across a disease like yellows or rosette but believed neither, they spent the evening with Dr. Neal. From Lake City they proceeded to Palatka where more groves were studied, and then took the boat on the St. Johns River to San Mateo where more disease specimens were collected and preserved, particularly of scab and Mal di gomma. By June 30 they were in Boardman where they came in contact with the foot rot disease on lemons and orange blight. As to the blight, Smith wrote in his note book, "This is the first place where this disease has given us any satisfaction." Dr. G. C. Lamar took them southwest to the hills, or high hammock land, and they examined examples of a disease known as "Frenching." The next day they were in Citra, a large shipping point of the Florida citrous industry and near where orange groves sold at one thousand dollars per acre, and there encountered the first undoubted cases of the die-back disease. At this point, lemon scab was so serious that growers were considering digging up whole groves. They were now in the heart of the grove areas of the state, since, excepting along the ocean and gulf fronts, the entire state land area south of Lake Okeechobee was still almost completely uninhabited. Smith wrote in his notes: "Florida is a flat land full of malaria, mosquitoes, flies, and jiggers. It is inhabited principally by Northern people who have gone there to grow oranges or escape consumption. Agriculturally it is poor but there are great possibilities." Southwest from Citra they went to Ocala, and thence to Eldorado, a way station on the north shore of Lake Harris. Lemon

⁹³ Report of the Chief of the Division of Vegetable Pathology for 1891: 361.

⁹⁴ This account is based on Smith's diary or book of notes kept on the journey.

scab, orange blight and die-back for years had been doing damage there.

July 4 they began to examine the important Platt grove whose owner for some while had been among those who had urged the Division to work on citrous diseases. Since the Florida citrous industry represented an investment of more than one hundred million dollars and possessed 2,800,000 bearing orange and lemon trees, according to the latest census, the Division had regarded with favor the many petitions and requests received, particularly since by 1893 one disease—the so-called wilt or blight—during six years would have destroyed more than a million and a quarter dollars worth of trees. On July 7 Smith and Swingle found in this sixteen-acre grove 100 cases of "wilt" already causing losses which Smith estimated at \$1,500. He was told that others had lost more than this sum. Indeed, since the disease was spreading and becoming more serious, threatened losses were large. Carefully he listed and described every symptom, because this new and destructive disease was the real reason why Smith and Swingle had been sent to Florida.⁹⁵ They came to no conclusion as to its cause other than that the disease appeared to be underground, and merited further study.

Smith observed "no signs of insects and no fungi on parts above ground. Symptoms are such as one might expect if tree were deprived of its water supply but no lack of moisture in the soil" could be seen. "Heavy rains," he further noted, "do not revive such trees." Further notes of this and another disease "not in the books" were taken, and on July 8, they left by wagon for Leesburg whence Smith left for Palatka, from there planning to go to Como to visit his high school principal of Ionia, Michigan, Anson P. DeWolf, and another old friend. Swingle remained in Florida and "completed," as Galloway reported that year, "the circuit of the more important orange-growing regions. Groves were carefully examined and the orange growers interviewed at twenty-six towns in fourteen counties."⁹⁶ Smith continued to examine plant diseases, finding on July 9 much genuine "die back" and at least

⁹⁵ Synopsis of researches, *op. cit.*, 20. The statistical data are taken from a letter from Galloway to Secretary Morton, April 1, 1893. Smith's studies during this period of "gummosis of the peach" and of "foot rot of the orange" made with "Comes' findings in mind" ended with the belief that neither disease was due to bacteria.

⁹⁶ P. 373.

one new symptom of one of the diseases studied. DeWolf's animated interest in the work must have been a happy source of satisfaction to Smith. All along the way Smith had enjoyed the Florida flora and animal life. He thrilled to see an alligator, whose bellowings he had heard at night and which was killed when attacking a dog. The number of introduced plants intrigued him, particularly the bamboos, some three to five inches in diameter and sixty feet high. Everywhere the scenery was attractive and beautiful and the visit with DeWolf provided a happy climax. Work awaited him at Washington, however. He little realized he had participated in the preliminary reconnaissance which would finally culminate, largely through the efforts of Swingle and Galloway, in securing the establishment of the Department's sub-tropical laboratory at Eustis, Florida. Pierce may not have been altogether conscious on his first journey to California that his reconnaissance would lead to the establishment of the Pacific coast laboratory of the Department at Santa Ana, California. Only a few years would pass and these two laboratories would be performing some of the Department's most important work. Smith arrived in Washington on July 11, but within a few days was again at work on the Chesapeake peninsula.

CHAPTER VI

EARLY STUDIES IN BACTERIAL PLANT DISEASES. FUSARIUM DISEASES OF PLANTS. FLORIDA AND CALIFORNIA LABORATORIES

ON MAY 7, 1892, James W. Toumey, botanist and entomologist of the Arizona Agricultural College and Experiment Station at Tucson, sent Smith "a small box containing cherry and peach leaves which show[ed] peculiar circular holes and indentations. Rose, peach, apricot, cherry and some of our native shrubs and trees," said Toumey in his accompanying letter,

are affected in a similar manner. On some trees, this disease, if it may be called such, is so bad that nothing is left of the leaves but midrib. The tissue in affected leaves withers and dies in narrow circular streaks and soon the intervening tissue or part of leaf drops out. The remainder of leaf in all cases apparently is perfectly healthy. Whether this condition is brought about by unfavorable conditions of growth, I do not know. I would be pleased for any information in regard to it that you can give.

Toumey had graduated in science from Michigan Agricultural College in 1889, and before obtaining his master's degree from the same institution in 1895, spent part of the year 1893 as a special student at Harvard. In his later prepared bulletin 33 of the Arizona Agricultural Experiment Station, "An Inquiry into the Cause and Nature of Crown-Gall,"¹ he stated that his attention was first called to another disease variously known as crown-gall, crown-knot, root knot, root galls, etc., in 1893 from "many observations . . . made in infested orchards in the Salt River Valley." He examined the disease, its development, and possible corrective measures. The next year was published his preliminary report,² the same year Smith's general account of "Stem and Root Tumors"³ from his "Field Notes, 1892" reached mycologists and pathologists. Smith said in part:

¹ April 13, 1900, Published accounts relating to crown-gall, 8.

² Bull. 12 Ariz. Agric. Exp't Sta., 1894.

³ *Jour. Myc.* 7(4): 376-377, Aug. 15, 1894. See also *Bulletin of the Torrey Botanical Club* 20: 363 (proc. Botanical Club, American Assoc. Adv. Sci., Madison meet., August 18-22, 1893, abstract or statement by Smith concerning his preliminary microscopic examinations).

Tumors on the roots of peach trees have been found by the writer in several localities during the past few years, and have been reported from many parts of the United States. They occur on roots of all ages, and vary greatly in size, the largest ones being several inches in diameter. Usually these tumors are several to many times the diameter of the root, and are entirely unlike the small galls produced by nematodes. They also occur on stems above ground, peach trees thus affected having been received from Texas and Florida.

This disease occurs from New Jersey to Florida and westward to the Pacific, but at present it is most prevalent in Texas and California, where it is causing much anxiety. In California it attacks orchard trees as well as nursery stock, and seriously injures both. . . .

It has been observed by the writer on the peach, plum, almond, pear, and poplar, and it has been reported as occurring on the roots of other trees and shrubs, e. g., apricot, apple, fig, walnut, raspberry, blackberry, and vine, and root tumors of some sort certainly occur on these plants.

The inner tissues in young specimens of the peach and almond tumors appear to be entirely free from nematodes and fungi, bacteria, and phytomyxineae, and their cause is involved in uncertainty. The most probable hypothesis is that they are due to some external irritant. Those who have the opportunity to examine early stages of this disease should certainly look for external parasites, especially animal organisms. . . .

Since December 1892, crown gall as a fleshy outgrowth on roots of deciduous fruit trees had been described in the literature of the state experiment stations. That year C. M. Woodworth, in a bulletin of the California station, had called attention to the disease. Edward J. Wickson, Professor of Horticulture of the university at Berkeley, had written on it. Liberty Hyde Bailey, in his *Annals of Horticulture* for North America during the year 1892,⁴ had referred to it in an article on "Plant Diseases and Insects," and, with every year the literature grew until such leading authorities as Atkinson and Bailey in New York, Halsted in New Jersey, Taft in Michigan, Augustus Dawson Selby, botanist of the Ohio Agricultural Experiment Station, and others, had dealt with it.⁵ During the year 1893 Smith corresponded on the subject with John Scott, horticultural commissioner of the county of Los Angeles, California, and received several lots of specimens of the disease, representing different phases. March 3, 1894, Toumey wrote again to Smith:

⁴ New York, 1893, Rural Publishing Co., pp. 94-124. See also the *Annals* for 1891, p. 109.

⁵ Published accounts relating to crown-gall, *op. cit.*, 9.

More than a year ago I sent the Department of Agriculture specimens of grape roots showing peculiar bunches or galls. This disease is known throughout the Southwest as "crown-root gall" or root-knot. It is not only found on the grape, but affects nearly every variety of fruit tree grown in Arizona. It is especially injurious to the peach. The knots most frequently form just below the surface and not infrequently grow to be two or three times as great in diameter as the tree itself. They sometimes appear, however, on all parts of the root, and even along the trunk a foot or more from the ground. I have found them beginning to develop on the roots of seedlings only two or three weeks from the pit. In a Nursery in Phoenix only a few weeks ago, several thousand one year old peach trees were condemned and destroyed, all being affected with the root-knot. After reaching a certain size, the knots seem to cut off the flow of sap to the parts above, causing the side of the tree on which the knots develop, to die. Even in the later condition, in walking through a peach orchard, those affected by the root-knot are easily recognized by the dark, dull, appearance of the bark, and the distorted buds. Six or seven-year old orchards last year lost from ten to twenty per cent of the entire number of trees, and from all appearances as many more will die during the coming season. The old knots are brownish black and easily crumble in the fingers; the young knots are white, soft and full of sap.

Knowing that you have been working on the peach for the last three years, I thought likely your attention had already been called to this disease, and that you could give me information regarding investigations that have been made in the study of this knot. I enclose with this letter a few specimens cut from peach roots.

Smith had wanted to renew his indoor laboratory investigations of peach yellows and other diseases of agricultural crops. On October 9, 1892, while at Still Pond, Maryland, he had written to Galloway:

Am looking over the orchards once more, and for the last time I trust to get the ups and downs of the disease by years, in varying seasons.

As soon as this year's field work is wound up, I want to begin in the same thorough way on laboratory work, and as a preliminary, I made out a list of lenses and other accessories which will be necessary as soon, almost, as I start in on it, and which I wish you would order for me as soon as possible, so that the whole winter may not be lost in waiting for them. I want to study the diseased protoplasm and can't do much without the best appliances. The mechanical stage is for photographing. With it I can use my stand for that purpose.

Will try and get along with VanDieman's microtome, which I suppose we can continue to use, but it will be necessary to have one or two extra knives. We ought to have *one at least* which shall always be in good order and *never* used on anything but *soft* tissues.

He furnished Galloway with a list of needed apparatus, some, if not most, of which was to be ordered direct from Zeiss, or Zeiss' agent, Eimer & Amend. Galloway had replied that it seemed to him "highly important to push the laboratory work as rapidly as possible. The investigations already made," he added, "have paved the way for a very tempting field for research." At this time, Swingle, who had completed his and Smith's 1891 Florida reconnaissance and in 1892 from March until late summer reconnoitered the state again, studying orange blight during its period of greatest prevalence and other diseases of agricultural crops, had returned to Washington and was outlining with Galloway a plan of work for the Department's new sub-tropical laboratory at Eustis. His list of desiderata for the Eustis laboratory was combined with Smith's list.

On July 1, 1892, the annual appropriation for the Division of Vegetable Pathology had been increased by five thousand dollars. This, together with the generous offers of Florida growers to place at the Department's disposal several large and small groves in different parts of the state for investigational purposes, and, more specifically, the offer of Eustis citizens to donate sufficient grounds for experimental plats and to build under Departmental supervision though at their expense the laboratory, had enabled the Division's officials to grant the importunities of more than one hundred petitions and resolutions of the Orange Growers Convention and the state Horticultural Society. Swingle went to Leesburg, Sanford, Altoona, and other important industrial centers, became acquainted with many of the more progressive grove owners, secured support for the appropriation from legislators, returned to Washington when the appropriation was approved, and, knowing that the laboratory would be authorized, went again to Florida to select the site and put to good use in his orange blight investigation his knowledge of plant physiology and pathology. Botanists had recognized the significance of Kellerman's and Swingle's corn breeding work at Kansas Agricultural College.⁶ Frequently Swingle wrote to Smith and a letter of June 19, 1892, written from Sanford, was of especial interest:

⁶ B. D. Halsted, What the station botanists are doing, *Botanical Gazette* 16: 288, Oct. 1891.

I have been stopping with . . . Lyman Phelps over a week. He knows a *great deal* about Citrus and has many exceedingly valuable crosses. . . . Probably has \$15,000 worth of crosses. Has discovered that the bud taken from *below* a crossed flower may show influence of pollen ! ! ! Showed me influence of stock on scion and has often seen it. Will send you and me boxes of crosses and teratological and pathological growths for study next winter.

July 21 he told Smith by letter, "I have been offered trees to bud (and have budded firm large ones with blight) . . .". Swingle continued his studies of orange pollination, began budding and grafting experiments, and now must have anticipated his famous work in cross breeding and hybridizing of citrus. He chose H. J. Webber to be his co-worker at Eustis. Webber, skillful in plant physiology and pathology, was to become one of America's outstanding authorities on plant breeding. Appointed to his position on September 15, he was soon in Washington to receive his instructions. During the first weeks of October, they were "busy looking up books, apparatus, and making plans"⁷ for their laboratory. Late that month they journeyed to Jacksonville, and thence went by boat on the St. Johns River to Sanford, described by Webber in a letter to Bessey as "the early seat of American Citrus culture and at present probably the home of the greatest practical orange grower in America." They arrived at Eustis on October 25, and by November the citizens there were building their six-room laboratory consisting of a library, culture room, dark room, and three work rooms.

In May, Fairchild had written Smith and told how much he was enjoying working at the Division's laboratory at Geneva, New York, the "best laboratory room," he said, "I have worked in by all means."

On July 31, Newton B. Pierce, again at Santa Ana, had thanked Smith for a copy of his paper on the "Chemistry of Peach Yellows." He had just completed typing "a translation of a valuable paper on the hybridization of the vine, by Millardet. Will probably present the same with my own observations on Coulure," he wrote. "The almond disease which I did some work on last fall has been conquered and I am now of the opinion that the souring of the figs may in part at least be likewise mastered.

⁷ Letter from May Varney to Smith, October 17.

Can I but master that disease I shall feel as if I had done one of the finest pieces of work done by the Department for some time. It is a matter of vast importance to this section of the Union. There are several other diseases which are receiving attention," but his main work then was on the fig malady. Pierce had counted on his "many and long letters" to Galloway to keep Smith advised of his activities. Much interested in Smith's researches on peach yellows and peach rosette, he had written from Santa Ana on January 24:

I would like to learn of the situation in Georgia respecting the disease there. Smith, can it be possible that a ferment in a localized position in a plant may set up a fermentation which may persist and become cumulative? I look for some facts from your Georgia disease which will throw new light on many plant diseases. Hope you may find them. It would please me much to hear from you and learn what are the prospects along the line of bacteriology in your two diseases. The fig disease I am working on is a mighty interesting one, but I shall now have to drop it till another season for want of fruit on trees to experiment on. If you know of any first class works on fermentation and ferments don't be bashful in telling me about them. My chances to get hold of works out here are altogether too slim.

Pierce had returned to California in the summer of 1891 and immediately taken up again his work on the California vine disease. En route to Los Angeles and Santa Ana, he visited northern and central portions of the state: the Sacramento valley, the Napa valley, the Sonoma valley, the Santa Clara valley, the San Joaquin valley, and, in the course of his journey, had called on Hilgard and Harkness at Berkeley. "The leading troubles to fruit in this state," he reported to Galloway on July 30, 1891, "are scale insects on the various trees, Phylloxera in the Napa and Sonoma valleys as well as near Niles, and the disease I am at work upon." On August 4, he reported from Orange:

I came down to Orange and through the Santa Ana valley Monday the 3rd. The old vines throughout this valley and in the neighborhood are now nearly all removed. A great abundance for scientific work remain, of course, but the majority of them are gone.

There are about Orange and Santa Ana—within a few miles of these places—perhaps 25-30 small test vineyards now already started. These comprise vines grown from cuttings and rooted plants procured from unaffected districts and from other new vines of this district, set previously. I have already visited seven of these new vineyards. The outlook is good. . . .

I have run across an important disease of the almond and another of the soft shelled walnut, and as time permits I hope to work them both up for our *Journal*.⁸ . . . I have already obtained the right to treat the affected almond trees. They are losing their leaves now in midsummer from the effects of the fungus. The black knot of the soft shelled walnut is now becoming a serious matter, and I intend to start cultures in tubes at once and see if the trouble be due to any fungus growth—for it certainly looks much as if it were caused in that way. . . .

If the raisin grape is again grown with success, the country will be made. . . .

Pierce's main address was still Los Angeles. He had not yet located his laboratory at Santa Ana. Department officials were experiencing such difficulty in supplying funds for his work that for a while at least vouchers for his accounts were transferred to the Botanical Division. On August 24, 1891, however, he informed Galloway,

I have again located permanently at Santa Ana. I have looked over the field with care and consider this best. Good quarters have been obtained and I am fitting up a laboratory for immediate and future use. Bacteriological supplies must come from the East I find, and I may order them at Santa Ana as well as at Los Angeles. . . . [T]he former place is better situated on account of the young vineyards and the field work.

Faces in the valley have shortened considerably since my last sojourn here—even a smile may now and then be noticed. The healthful appearance of young vines is the cause.

Residents of Orange sought to have him locate his laboratory there. But he chose Santa Ana and by August 27 was ordering peptone, agar agar, and gelatin, as well as test tubes, glass rods, and other supplies. By October 11, he reported to Galloway that he had occupied himself "mostly of late with culture work and a disease of the almond (*Cercospora circumscissa* Sacc.) very injurious to foliage in this region. The germination and morphology of this form are of interest and its effects of importance to be considered. The work has been largely laboratory work. . . ."

Pierce was pleased at the progress being made at Washington. He learned that the Division of Vegetable Pathology was now quartered in "rooms formerly occupied by the B[ureau of] A[nimal] Industry. That must be much pleasanter for you in

⁸ *Jour. Mycology* 7(2): 66-76, March 10, 1892; 7(3): 232-239, May 15, 1893 (concerning the almond disease and remedies therefor).

many ways," he commented. "I have often wondered how you could work under the conditions of the past to any degree of satisfaction to yourself."

In 1892 there was published an article on the "Mysterious vine disease,"⁹ which advanced Pierce's solution of using cuttings from healthy vines and from regions where the disease did not exist. This article gave wide currency in California to the practical results of his investigations, and evidently was appreciated, since his recommendation to start from perfectly healthy cuttings had given the vineyardists their first real basis for hope of saving their industry. At Washington, on June 15, 1891, Pierce had submitted his preliminary report on the California vine disease, and in it included a chapter on treatment. In this he had discussed matters of cultivation, pruning, cutting back, seedlings, and the very important topics of the hardiness of varieties and grafting resistant stock. Yet, publication of the report was delayed, and, the vineyardists prodding, he, on October 26, 1892, addressed a letter to Galloway to learn if possible when the report would be ready for distribution. Said Pierce:

The people among whom I am working very naturally are surprised at the delay in a report of the first moment to all this part of California. It is a piece of work which will certainly give us strength with the vine and fruit growers of this State, but this long delay in publishing is destroying, beforehand, the interest which naturally attaches to the work, and will detract materially from the credit which the Department should reap from having conducted the work.

No one knows half as well as myself the intrinsic value to the vineyardists of this State of that report. The facts brought out in relation to the transmission of the disease by means of cuttings alone are of sufficient value to warrant as early publication of the report as possible. . . . The season for making cuttings for the coming winter's setting is now almost at hand, and I should expect that a loss of many thousands of dollars more would result from the delay in the publication of that report if it is not distributed here in proper season for its facts to be taken advantage of for the coming season's work. There are also hundreds of facts there which would help to guide in the selection of location for vines and the care of the same which are of thousands of dollars value to the young vine growers now about to start vineyards.

In his report he had stated:¹⁰ "The facts point to the present

⁹ *Rural Californian* 15: 129, May 1892; see also *California Fruit Grower* 10: 154, March 5, 1892.

¹⁰ Bulletin No. 2, The California vine disease. A preliminary report of investigations, chap XI, p. 206, Washington, Gov't Print. Office, 1892.

existence of the inciting cause of the disease as either an external parasite or as internal and cumulative." Therefore, how to eliminate the parasitic cause? During the summer of 1892, Pierce prepared a paper in the belief that the disease known as coulure eventually would have to be conquered in the light of Millardet's work on hybridization of the vine. He wrote Galloway to ask whether he should not append a copy of the translation to his paper. "As you of course know," he told Galloway, "Millardet's work has been with the object of obtaining a vine or vines which shall be resistant to the more important diseases and yet retain good fruit-producing qualities. It is not at all unlikely that it will be along this line that the trouble of coulure will eventually have to be conquered. . . ." This letter was dated July 18, 1892. Galloway read Millardet's paper, approved its translation, suggested Millardet might revise and add to the text, sent a few references on fertilization of grapes, explained that other Departmental publications than Pierce's vine disease report had been similarly delayed, and that he, as much as anyone, awaited with pleasure the printed volume. That summer Pierce gave much time to studying the souring-of-figs disease. Alike for this and the grape trouble he planned to go to Fresno. He was sure that in 1888 Emerich Rathay had published in Vienna a paper, "Die Geschlechtsverhaeltnisse der Reben," which contained much valuable information on vine fertilization and the vitality of pollen which might prove helpful in his coulure investigation. He inquired of Galloway whether there was a copy of it in the Department's library or whether he would have to order it from Berlin. Obviously, he was working on his valuable achievement of 1892—the production by hybridization of disease-resistant vines which bore "superb bunches of fruit of excellent quality. Mr. Pierce's first crosses," later¹¹ wrote Smith, "were made according to my recollection in 1892. Mr. Pierce also successfully crossed raisin grapes to resist *coulure*, a disease which renders the bunches ragged and worthless for market by causing the abortion of the whole or a large part of the berries when they are very small."

Galloway reported in 1893 that Pierce had shown that coulure

¹¹ Plant breeding in the United States Department of Agriculture, *Royal Horticultural Society's Report of the Conference on Genetics*—1906, reprint, 303, 1907. See also Fifty years of pathology, *op. cit.*, 22. In that address, Smith, without qualification, gives the year as 1892.

may be "largely prevented by grafting . . . susceptible varieties upon more hardy roots." He also reported results on the crossing of the Muscat of Alexandria with a good variety of raisin grape not subject to coulure, and combining disease resistance with desirable qualities of fruit. He revealed:

From the crossing of stocks the best results are expected. A vine known in California as the Malaga, and usually grown as a raisin grape, has been selected to cross with the Muscat of Alexandria. It embodies hardness of bloom, top, and root, and the qualities which appear desirable. A suitable location for conducting the work was found in the Lucerne Vineyard at Hanford, Cal[ifornia]. During the blooming season the crossing was successfully accomplished, the Malaga being used as the pollinating variety. As a result of this cross several hundred berries set and arrived at maturity. It is hoped that plants grown from the seed of these berries will possess most of the hardy qualities of flower, top, and root, so marked in the Malaga. This view is based on the results obtained by Prof[essor] Millardet in the production of over 10,000 hybrid vines.¹²

Furthermore, it was announced that "A bulletin on coulure of raisin grapes [was then] being prepared, embodying the results to date. It will include a translation of a paper by Prof[essor] Millardet on the hybridization of the vine." This work, begun in 1892, began to attain flourishing promise by 1894. Pierce gave some time in 1892 to completing a paper on "Prune Rust,"¹³ and by letter of November 20 Galloway praised his presentation of this disease and its treatment. "It is an interesting paper," he wrote, "and will, I am sure, attract widespread attention. The photographs are very striking. They were shown Mr. Willits, and he seemed very much pleased with them. The results are especially interesting to me because it is the first rust disease that has really been presented."

Coulure claimed in 1892 losses of more than six hundred thousand dollars¹⁴ in the San Joaquin Valley alone. Pierce realized that, to preserve the industry, he could not abandon his efforts to solve this disease. Since his first years in California, he had kept Smith advised of his progress. July 27, 1894, at Chico,

¹² Report of the Chief of the Division of Vegetable Pathology for the year 1893, *op. cit.*, 273. At p. 273-274 appeared a statement of the work done thus far on the California vine disease.

¹³ *Jour. Mycology* 7(4): 354-362, Aug. 15, 1894.

¹⁴ 1893 report, *supra*, 273.

he wrote of his work in the San Joaquin and Sacramento valleys, and referred to his "nearly 1000 acres of raisin grapes in the Lucerne Vineyard" at Hanford where he had spent some seven weeks in very warm weather. "I made 10 new crosses among the raisin grapes during my stay there," he reported,

which involved the emasculation of something like 16,000 to 20,000 grape flowers. . . . But if we can only grow the vines from them I shall feel abundantly repaid for this hard work. . . . The crossing of the raisin grapes is a line of work very important to this portion of the world and to all vine growing or rather raisin producing countries. . . . The whole world does not contain more than 12 to 15 good raisin varieties, while the good wine grapes may be numbered by the hundreds if not thousands—and the table varieties are not far behind in number. This shows you the newness of the field so far as raisin grapes are concerned, and I know of no good reason why California should not be the birthplace of hundreds of good raisin grapes especially adapted to its climate so as to hold their fruit and be comparatively free from other troubles. As I have started this line of work I think I will keep it up each spring. The field is certainly a most fertile one and wholly unoccupied so far as I am aware. The chances are all favorable to the production of most valuable grapes for this region. The work has in direct view the prevention of Coulure by the production of a hardy variety which will resist the unfavorable climatic conditions as the Malaga now does.

Pierce had not conquered the California vine disease, although Galloway reported in 1893 that during the "past season . . . the disease [had] lost much of its virulence."¹⁵ He took its study into his indoor laboratory, and, along with microscopical examinations of *Plasmiodiophora brassicae*, persisted with this research which he recognized to be on "the very frontier of vegetable pathology." He had no doubt that what he characterized as "constitutional effects were physiological effects." He asked Galloway in a letter of October 17, 1893,

Why are not the peculiar characters of this trouble showing in that valley of 40,000 to 50,000 acres of Muscats and a large extent of wine vineyards? Why does a perfectly ripened cutting from a diseased vine produce a diseased vine in a region where this disease was never seen before? 160 acres of Muscat cuttings died with this disease in Arizona, and still Arizona vines do not die with this disease so far as I am able to learn.

If the disease was wholly a physiological trouble, he could not

¹⁵ 1893 report, *supra*, 274.

understand "why thousands of vines of the most susceptible of all the varieties, the old Mission, should prosper for a century over all this vast region, and why at present perfectly healthy cuttings set out [there] grew with perfect health for one or two years and then [contracted] this disease in all its strong characteristics."

Like Smith in peach yellows, while he suspected a parasite, cellular examinations made with the most advanced learning in section cutting, staining, indeed, working up some new methods of his own in studying the "living plasmodium," still failed to establish any organism positively proven to be the cause of the disease. In his monograph, "Additional Evidence on the Communicability of Peach Yellows and Peach Rosette,"¹⁶ Smith had spoken of a "germ or virus of some sort" transmitted from diseased to healthy parts of a tree. This was a conclusion as to peach rosette. As to peach yellows he had concluded, "Only a very small amount of infective material is necessary, provided it be in the forms of living cells, which can be induced to unite the actively growing tissues of the tree." The problem of how the infectious matter makes its entrance into healthy trees was still unsettled, as were the disease's incubation period, the exact nature of the contagium, and its method of spread other than by bud inoculation.¹⁷ Late in the year 1893, Smith, thanking F. H. Harper of Still Pond, Maryland, for another prescription and claimed cure for peach yellows, expressed the opinion that the disease, "not being due to a root aphid, or, so far as I can make out, to any root parasite," seemed best explained as "a something in all the juices and tissues of the diseased tree, as shown very clearly by bud inoculations." This was not his final writing upon the subject. He had observed the seemingly varying degrees of resistance shown by various trees and had not been unmindful of the possibilities of searching out the bearing which immunity might play. But he had decided to put his "time on lines of work which seem[ed] to promise better results."

On March 27, 1893, Acting Regent Thomas J. Burrill of the University of Illinois had advised Smith that the large peach trees

¹⁶ Bulletin 1: 54, U. S. Dep't of Agric., Div. of Veg. Path., Washington, Gov't Print. Office, 1891.

¹⁷ *Idem*, 44-45.

sent him were dead—two destroyed by careless workmen, and one had died of the disease, its death hastened by freezing during the winter of 1891-1892. Scions of the last tree had been saved, buds taken and inserted in healthy stock, and the disease had been communicated. Burrill regarded this as of sufficient significance to report. By 1893 Michigan, New York, Connecticut, Pennsylvania, Delaware, Maryland, Virginia, and California had passed yellows legislation applicable to parts or the whole of each state.¹⁸ Smith had continued his search for a parasite. But in 1893 the most which Galloway could say¹⁹ in his annual report was that microscopic examinations of healthy and diseased tissues were still being made.

Other workers were finding difficulty in solving fundamentals of their problems. Some experiments to determine the efficacy of methods to prevent wheat rust²⁰ had proved disappointing. In 1894 M. A. Carleton of Kansas Agricultural College would be placed in charge of cereal crop investigations, including rust and smut diseases. January 30, 1893, Pierce confided to Smith:

My work on the vine is not as satisfactory as I wish it could be. I find it exceedingly hard to get down to it on account of the heavy load of other work that I must do as I go along. There is hardly a day that I am not called upon to work on some other disease—material coming to me from all parts of the Pacific Coast. It is all work that should be done, but it almost prevents me from working on my special subject. I have of late found it necessary to add another room to my laboratory. I now have 4 rooms. An office, library and writing room combined facing the east; a bed room back of the office; a large room for a laboratory facing the west; and a room back of this for collections and a general storeroom. There is a grate in the office, and the laboratory can be warmed with a gasoline stove. This gives me east and west light and good rooms to work in. I am getting a pretty solid hold in southern Calif[ornia], and in fact all along the coast, as is evinced by the letters of inquiry being received.

Swingle and Webber at the Subtropical Laboratory at Eustis, Florida, had not yet solved, of course, the difficult orange blight problem, although each was finding working with citrus delightful. Morphological and histological investigations were in progress. They were studying the transpiration of healthy and diseased

¹⁸ 1893 report, *supra*, 275.

¹⁹ *Idem*.

²⁰ Additional experiments in the treatment of wheat rust, *Report* for 1893: 252-258.

plants, and examining leaves and conducting tissue of healthy and diseased limbs. On March 24, 1893, Swingle had written to Smith and told him of his orange grove pollination experiments. "Have made a good number of cross pollination experiments," he said, "and have studied the means by which it is accomplished in nature." Swingle had closely observed the pollinating roles of the honey and humble bees, the humming bird, and he was interested in the warbler and what it, as well as other birds, accomplished in the process. Swingle was a man of far-sighted vision. Webber believed that he possessed a true Darwinian zest for scientific inquiry. Early he envisioned the possibilities of large scale plant introductions, an "acclimatizing scheme" he called it in a letter of May 5, 1893, to Smith, as a principal way of advancing the state's industrial economy. Plant disease study was the other principal investigational branch. By 1894 Galloway was reporting that,

Sooty mold, a destructive disease of the orange and other citrous fruits, has been successfully treated. Scab of lemon, another very troublesome disease, has also been held in check by the application of fungicides. It is believed now that with the increased knowledge as to the cause of these diseases they may be held in check at comparatively small expense. The more obscure diseases of the orange, such as blight, foot rot, and die back, have been further studied and much additional information has been obtained in regard to them.

Swingle and Webber did not confine themselves to citrous diseases, but, like Pierce at the Pacific Coast laboratory, diligently pursued laboratory and field examinations in a wide variety of agricultural crops. An early prominent phase of inquiry was in what Swingle described to Smith in 1893 as "fertilizer-in-relation-to-disease." On behalf of this and the so-called acclimatization work Swingle appeared before the state horticultural society meeting at Pensacola to inculcate further interest and secure special appropriations in aid of the work. Both men became American plant breeders of eminence. Webber, with special abilities as a cytologist and a special interest in plant fecundation, extended at Eustis his knowledge of the art and science of plant breeding. The brilliant achievements of Swingle and Webber in citrous hybridization will be considered at a point later. Now both men were doing important work in plant pathology.

Late in the year 1892 Dr. H. L. Russell was offered a special assignment at the Eustis laboratory to investigate the bacteriology of citrous fruits. He, however, declined owing to the fact that during the school year 1892-1893, as a part of the "Programme of Courses in Biology" at the University of Chicago, he was presenting a lecture course on "Special Problems in Bacteriology." In September 1892 the *Experiment Station Record*²¹ had published a strong editorial on the need of station investigations of plant bacterial diseases made in strict compliance with the canons of Koch. Russell had furnished suggested materials for the editorial. "Four or more Lectures" were all that was promised in his course at the University of Chicago. Knowledge of the bacteria which cause diseases of plants was still immature. In 1892 William Wood and Company of New York had published George M. Sternberg's *Manual of Bacteriology*, a volume of 886 pages. Not until the second revised edition issued in 1901, however, would a chapter on plant parasites be included, and even then consideration of the saprophytes would be altogether omitted. Sternberg requested Smith to submit his publications for this chapter of his volume which in 1896 became *Text-book of bacteriology*, a volume of 693 pages.²² Observers, the editorial of 1892 said, had recognized bacteria, and in instances the bacterial nature, of certain plant diseases. Some students had transferred "bits of diseased tissue containing bacteria from affected to healthy plants," and if the disease was seen to "spread to any extent this ha[d] been regarded as sufficient evidence that the malady was of a bacterial nature." Elaborate equipment was neither the test nor the prerequisite of good work in plant bacteriology since "pathological inquiry from a medical standpoint call[ed] for more accurately adjusted conditions than [was] necessary for the study of many problems from an agricultural standpoint. Good work has and can yet be done with but little other than the simplest appliance." The editorial urged that "the general principles of the subject in relation to agriculture need[ed] to be more thoroughly developed. . . . Medicine ha[d] taken up the subject largely from the hygienic standpoint and outside of pathogenic forms little ha[d] been done on the general biology of microörganisms since the death

²¹ 4(2): 111-113.

²² *Bacteria in relation to plant diseases, op. cit., bibliog., 1: 204-205.*

of DeBary. The whole domain of the physiology of bacteria offer[ed] numerous problems of which " as yet little was known.

During the dry summer of 1892 a destructive disease of cucumber, melon, and squash appeared near Washington. Pre-occupied with the larger investigations of the California vine disease, orange diseases in Florida, and fruit diseases in New York, the half dozen workers of the Division of Vegetable Pathology had not been able to give immediate close study to each newly reported crop disease. Smith, while going to and from Maryland in peach yellows work, may have discovered the wilt disease of cucurbits. His attention may have been called to it, while studying another malady. In the autumn of 1892, a destructive muskmelon disease, not before known in this country, was found in southwest Michigan. Most of his study of this was done in his Washington laboratory. After he had started his culture examinations, he found early in 1893 a published article on it by Dr. Victor A. Peglion of the Laboratorio di Botanica e Patologia Vegetale of the Royal School of Viticulture and Enology at Avellino, Italy. He had another Italian correspondent. In 1888 he had written to Fridiano Cavara of the Laboratorio Crittogamico of the Royal Botanical Institute of the University of Pavia and procured his publications on plant diseases, mostly diseases of grapes. May 18, 1893, Smith told Peglion that, from his reading, the disease he described was identical " with one which did considerable damage in parts of the United States last season," and he asked Peglion to send " specimens of the leaves showing spore tufts," so that he might compare. June 28, he wrote again to Peglion

Your kind letter, together with the specimens and pamphlets, came soon after my second letter to you. I am greatly obliged, and when I have studied your fungus a little more thoroughly I will write you again concerning it. At the present writing I have a number of plate cultures going and it appears to be identical with the one which I have from this country, but perhaps it is too early to speak positively.

Smith isolated the fungus, an *Alternaria*, in pure culture, made many beautiful drawings and obtained " numerous beautiful infections by spraying pure sporulating cultures on the foliage." ²³ He

²³ Synopsis of researches, *op. cit.*, 20; Fifty years of pathology, *op. cit.*, 22.

believed that an ascospore form of the fungus should be present. But this was not found at that time.

In August 1893, when Smith submitted at the Madison meeting of Section G²⁴ a paper on "The Muskmelon *Alternaria*," his work on that new and destructive disease of cucurbits was fairly well completed. Infections with pure cultures, however, had not as yet been secured with reference to the disease of cucumbers, cantaloupes, and squashes. Histological studies made in connection with its symptoms indicated its bacterial origin "almost beyond doubt" and "almost certainly" its transmission by insects. Some anatomical and physiological examinations had been made. But the entire malady was to be restudied and the promised printing of his paper by the Department of Agriculture never appeared. It was at best a preliminary account; with improvements made in his laboratory technique, his study of the bacterial wilt of cucurbits would last many years. This subject will soon be considered again more fully. An International Botanical Congress was being held at Madison at the same meetings of the American Association. Other important papers on diseases of plants were presented before Section G. Swingle described briefly the "principal diseases of citrous fruits" being studied at Eustis: blight, mal di gomma, die-back, etc. P. H. Rolfs, a former student of Pammel and now botanist and entomologist of the Florida station at Lake City, presented a paper on "A sclerotium disease of plants."²⁵

The World's Columbian Exposition was then being held at Chicago, and the Department of Agriculture had an important exhibit there. The Microscopical, Entomological, Pomological, Vegetable Pathological, and other Divisions of the Department had prepared elaborate displays befitting the Exposition's magnitude, and these, housed in the Government Buildings at Jackson Park, required one or more attendants from each Division familiar with the work. A strong interest in accomplishments from the study of plant diseases was especially noticeable. By 1893²⁶ Bordeaux mixture had been successfully used against apple scab,

²⁴ *Proc. Amer. Assoc. Adv. of Sci.*, 42nd ann. meet., 258-259.

²⁵ *Idem*, 260.

²⁶ Report of the Chief of the Division of Vegetable Pathology for 1893, *op. cit.*, 262-265. Fungicides, 262-263; Bordeaux mixture, 263-265.

bean anthracnose, beet downy mildew, cherry leaf-blight, currant septoria, grape black rot, grape downy mildew, mignonette leaf-blight, pear leaf-blight, pear scab, plum leaf-blight, plum leaf rust, potato blight or rot, quince leaf spot or blight, raspberry anthracnose, and strawberry leaf-blight.

In this list appeared also potato leaf-blight or *Macrosporium* disease. On October 19, 1892, Galloway had written to Pammel,

We have obtained excellent results in the treatment of the potato macrosporium. Our crop has just been harvested and the results are very striking. The yield of the treated would have been much greater if the tops had not been cut down by frost. On Oct[ober] 3rd we had the temperature down to 26° F. and of course everything was frozen. The untreated tops at the time of the frost were completely dead. I have been growing the *Macrosporium* for three or four months in various culture media and have obtained some interesting facts in this way.

Before the fourteenth annual meeting, also held at Madison in 1893, of the Society for the Promotion of Agricultural Science, Galloway presented a paper on "The *Macrosporium* potato disease,"²⁷ which he had been studying, along with other potato diseases, for, perhaps, eight years. At this meeting Smith, Pammel, Jones, Goff, Bolley, Chester, and one or two others, were made members of the Society. Each had promoted agricultural science, and their work entirely or in part had been in plant pathology.

Pammel had been doing some especially important work. In July 1891, Galloway had received from him a report of the Iowa Agricultural Experiment Station on fungous diseases of plants and told him that he was glad that Pammel was making mycology such a prominent part of his botanical work. March 10, 1892, *Journal of Mycology*²⁸ had published a paper by Pammel on "New Fungous Diseases of Iowa" which divided the subject according to forage plants and cereals, fruits and fruit trees, and forest trees. Regarded as especially valuable were Pammel's studies of root diseases. The life histories of *Peziza sclerotiorum*, *Rhizoctonia betae*, and *Cercospora beticola* were being traced by him in 1892,²⁹ along with some fungicidal experimental work, some experiments in seed germination and crossing of cucurbits,

²⁷ *Proceedings of the Society*, 46-58, 1893.

²⁸ 7(2): 95-103.

²⁹ George F. Atkinson, Botany at the experiment stations, *Science, op. cit.*, 329.

and of much interest were original studies of chromogenic bacteria of the Ames flora.³⁰

G. F. Atkinson also had done some especially valuable work. The scientific and practical worth of his bulletin 41, issued in December 1892, from the Alabama College Station on cotton diseases—yellow leaf blight or mosaic disease, frenching, damping off, anthracnose, shedding of bolls, angular spot, areolate mildew, leaf blight, and root gall—made it probably the most important plant pathological work thus far from the southern states. Immediately on going to Cornell University he had begun to study serious tomato diseases, and his bulletin, or part of bulletin, 53 of the New York Agricultural Experiment Station there on "Oedema of the tomato," published in 1893, remained for years a leading investigation on the subject. Smith later said of this:³¹

[In 1892-1893 Atkinson] experimented with the oedema of the tomato, which is an intumescence and reached the conclusion that on susceptible varieties oedema may be induced by insufficient light and bad ventilation coupled with too much water in the soil, and a soil temperature too near that of the air, leading to the accumulation of acids in the plant and to weak cell-walls, easily stretched as water is imbibed. He says: "When there is an abundance of water in the plant these acids draw large quantities into the cells, causing the cells to swell, resulting many times in oedema." . . . "Ordinarily there is no increase in the number of cells."

Smith believed that his experiments should be repeated because, first, those were few by which he claimed to have produced oedema by forcing an excess of water into the plants, and, second, he described no experiments which sought to determine the increased acidity. Atkinson also in 1893 elaborated some pioneering studies of carnation diseases, a subject in which others, prominently Arthur and Bolley, had figured.³² Whetzel has classed two others of his early studies at Cornell, "Leaf curl and plum pockets" (1894) and "Damping off" (1895) as among Atkinson's more important works.

Atkinson, while at Alabama, secured pure cultures of the causal organism of Pammel's root-rot of cotton. Plant pathologists of

³⁰ Pammel presented several papers at the Madison meeting of the American Association for the Advancement of Science, see *Proceedings, op. cit.*, 263-264.

³¹ *Introduction to bacterial diseases of plants, op. cit.*, 489.

³² H. H. Whetzel, *An outline of the history of phytopathology, op. cit.*, 104, n. 3. See also, Bacteriosis of carnations, by Arthur and Bolley, Bulletin 59, 7 (March, 1896), Indiana Agric. Exp't Sta., and authorities cited.

the state experiment stations confined their work to state boundaries less and less and collaborated in more than one plant disease research. This was noticeable in the work of Halsted who, invited to Mississippi, published at the agricultural college and experiment station there his important bulletin 19 (1892) on tomato and melon blight. In August 1891 he had read before Section F a paper on a disease of melon, squash, and cucumber, believed caused by bacteria, and a notice of this was published in the *Botanical Gazette*.³³

Smith, during the years 1895-1896, would re-study this work for reasons to be explained more fully in Chapter VII. Suffice it to say now that, notwithstanding the excellent work of Waite, plant bacteriology had not gained a strong foothold in the Division of Vegetable Pathology. As late as March, 1893, Galloway told Burrill that they had "very few specimens of bacterial plant diseases on hand . . . pear blight, olive tuberculosis and perhaps one or two other things."

In January 1893 Pammel published in the *Botanical Gazette* what Smith later characterized as the "first important paper"³⁴ on the bacteriosis of rutabaga. This malady, in time, became known as the "Black Rot of Crucifers." The causal organism was not named by Pammel *Bacillus campestris* until the year 1895 when he issued as part of bulletin 27 from the Iowa experiment station a further consideration of this disease which, with Pammel's knowledge and approval, other plant pathologists, including Smith and Russell, restudied.

Whether workers in all quarters realized it or not, plant bacteriology as an exact laboratory science, requiring the services of a leading American specialist and authority, was widening in scope each year. In the September 10, 1890 issue of *Journal of Mycology*³⁵ Galloway and Miss Southworth had presented "Preliminary Notes on a New Destructive Oat Disease," possibly attributable to bacteria, and in August 1891, before Section F of the American Association for the Advancement of Science, Galloway read a paper which incorporated "Further observations on a

³³ 16: 257-258, Sept. 15, 1891.

³⁴ *Introduction to bacterial diseases of plants, op. cit.*, 159.

³⁵ 6(2): 72-73; also *Botanical Gazette* 16(9): 257, Sept. 15, 1891. Report of the Chief of the Division of Vegetable Pathology, for 1891: 361.

bacterial disease of oats." Galloway believed in plant pathology and its future, combining exact laboratory research in bacteriology.

For adequate work in bacteriology, the Division's workers could not depend on cooperative arrangements with the state experiment stations. Duties in university administration often took Burrill away from research. During 1891 and 1892³⁶ he had continued to study several diseases of plants believed to be of bacterial origin. Students interested in the plant bacterial diseases, however, were still very few, and even Burrill studied other subjects. Excellent results were reported from the use of copper compounds against grape rot, apple scab, and potato blight. Like many other workers in experiment stations of the central west, he was investigating diseases of grains. *Puccinia rubigovera* was found to winter over in wheat leaves and in the spring produce rust spores which grow on fresh foliage. He was studying also the economic smuts, raspberry rust, and a disease of plum.

Early in 1893 Fairchild announced plans to spend three years in study in Europe and in October he was appointed a special agent of the Division to investigate plant diseases and scientific agriculture there.

Smith and Waite were the two men of the Division now qualified for the needed work in plant bacteriology. Waite was studying pear blight and Smith realized that a study of the bacterial wilt of cucurbits would yield more definite results than further investigation of peach yellows. He did not relinquish entirely his field and laboratory studies of peach yellows. For nearly two years, however, he had been gathering together and reading everything he could find concerning laboratory apparatus and methods of work. He had excellent advisers in Theobald Smith and V. A. Moore of the Bureau of Animal Industry. He read the Johns Hopkins bulletins. He wrote to Drs. Vaughan and Novy and others familiar with the latest in research methods. He read many new books on plant physiology and other subjects of importance.

Erwin Smith was a far happier man than he had been for many years. He was now married, and was beginning the new work which was to test his high calibre as a student.

³⁶ G. F. Atkinson, Botany at the experiment stations, 1892; B. D. Halsted, What the station botanists are doing, 1891, works cited.

Near Easton, Maryland, at "Sunset Farm" on Miles River, he had met Charlotte May Buffett, daughter of Dr. Lewis and Anna Virginia Perry Buffett. She had been born at Cleveland, Ohio, and about 1887, when sixteen years of age, moved to Maryland. Gifted with a love of nature and the beautiful, of literature, music, and languages, interested in the sciences and poetry, she and he were well-mated, and with her he found "deep sweet peace." "There was nothing selfish or petty in this woman's soul," he later wrote,³⁷ "and to share her life as I did for thirteen years, was to dwell continuously in the temple of God." Together they studied nature and books, and lived the philosophy of life so well expressed by him as follows:

An intimate acquaintance with any branch of nature naturally begets a philosophy of life. We come to feel instinctively that certain concepts of the Universe must be so, and that certain others cannot possibly be true. To my mind this is one of the chief individual benefits arising from the pursuit of science. The intellect is clarified; reason supplants impulse; and superstition and dogma no longer sway feeling and mold action.

April 13, 1893, at "Sunset Farm," Reverend J. T. Sunderland, editor of *The Unitarian* and pastor of the church at Ann Arbor which Smith attended while a university student, performed their marriage ceremony. Allen L. Colton, a college and fraternity brother in Phi Delta Theta, friend at Ionia, and astronomer of considerable prominence, served as best man.

Spalding, in the midst of preparing to sail for months of study in Europe, sent them a letter of felicitation. He noticed that the Department of Agriculture now had a new Secretary, the Honorable J. Sterling Morton, creator of the Arbor Day idea promulgated in his home state, and later adopted by practically every state of the Union.

For several years Smith had been helping his teacher with literary botanical work, and Spalding accepted his offer to read proofs on a "little book" he was preparing. Already Smith had submitted criticisms and suggested revision. D. C. Heath and

³⁷ *For Her Friends and Mine: A Book of Aspirations, Dreams and Memories*, privately printed, Washington, D. C., 1915, introductory note, wherein appears a biographical sketch and intimate appreciation of Charlotte May Buffett sometime wife of Erwin F. Smith, a volume of poems and commemorative tribute to her and their years of happiness.

Company that year published Spalding's *Guide to the Study of Common Plants, An Introduction to Botany*, which, as a laboratory guide rather than general text-book, was well received, particularly for instruction in high schools for which it was planned. This book, Smith believed, exercised a strong influence in changing the type of botanical teaching in our common schools.³⁸ It was in harmony with his own concepts of what modern university botanical instruction should stress. He believed that "floristic botany [had] dominated too long in this country," that emphasis should be placed "on the more important parts of Botany where it belongs, on anatomy, morphology, and physiology, until," he said, "we have gone a little below the surface of things and have the real basis for comparison of genera and species and the difficult problems of geographical distribution and origin of species."³⁹ But never once did he urge a departure in taxonomy from the great botanical tradition represented in America in part by Torrey and Gray. He knew that changed conditions and new or better knowledge would make revisions necessary. But in the main he was a conservative in this, and in Chapter VII we shall see that he no longer regarded himself as a systematic botanist.

Part of Smith's literary work during 1892 and 1893 consisted in preparing botanical definitions for Funk and Wagnalls' *Standard Dictionary of the English Language* published from 1894 to 1897. Frank Hall Knowlton of the Smithsonian Institution had the entire work in charge, and Smith's task, one of "midnight hours,"⁴⁰ included definitions of mycology and plant pathology. Some of the work was divided with W. T. Swingle.

Twice during the late summer and early autumn of 1893 Smith had to go to southern Michigan to study diseases of plants: in August to continue his investigations of peach yellows, and in October to begin study of a new peach disease reported from Douglas, Michigan, by William Alton Taylor, assistant pomologist of the Department and a graduate in science from Michigan Agricultural College. These journeys were important since for the first time careful microscopic examinations and a field study of

³⁸ Dedication, *Introduction to bacterial diseases of plants, op. cit.*; also, see *Botanical Gazette* 18: 430, a review, 1893.

³⁹ Memorandum found among Smith's papers.

⁴⁰ Synopsis of researches, *op. cit.*, 22.

symptoms were made of the new disease known as "little peach." At his home at Hubbardston he continued his study of the bacterial disease of cucumbers, cantaloupes, and squashes on which he had read a paper before the recent botanical congress at Madison. On September 3 he wrote Galloway:

The cucumber blight is here in plenty on my father's squashes and I am learning all I can about it. The squashes are much more resistant than the cucumbers and melons and hold out for several weeks—one of the symptoms reminds one of the orange blight, viz. the development of an enormous number of flower buds on short branches. In a few cases the vines seem to have partly recovered, having sent out very leafy side branches which again reminds one of the orange blight.

He had been ordered to Michigan both times from Chicago where, at the World's Fair, he was one of several workers who at various times had charge of the Division's exhibit on plant diseases and their treatment. The farmers and fruit growers showed a "lively interest in the problems [the Division was] studying,"⁴¹ particularly in fungicidal treatments for disease-prevention. So worn became the plates of "sprayed and unsprayed pears"⁴² that new plates or additional specimens of pear varieties had to be sent for; and to this was added a request for demonstration materials of "sprayed and unsprayed" grapes. Smith wrote a special memorandum on potato disease treatments. This read:

Within the last two years a distinct advance has been made in this country in economic phytopathology by the discovery of methods for preventing two widespread and destructive potato diseases—the blight and the scab, both due to fungi. This was strikingly illustrated by two exhibits in the united experiment stations' exhibit at the World's fair. Mr. L. R. Jones of the Vermont station, who dealt with the blight, exhibited photographs of sprayed and unsprayed parts of fields, the vines covering the ground [where] spray[ed] and almost totally destroyed [where not sprayed]. Photos of the yield of tubers on sprayed and unsprayed parts showed equally gratifying results. . . . The results were obtained by the use of copper fungicides.

The other exhibit was from Professor H. L. Bolley of the North Dakota experiment station and had to do with a remedy which Smith described and of which he wrote later:⁴³ "Bolley showed

⁴¹ Letter, Smith to Galloway, September 18, 1893.

⁴² Letter, Smith to Galloway, September 25, 1893.

⁴³ Plant pathology: a retrospect and prospect, *op. cit.*, 608.

that the potato scab was frequently disseminated by seed potatoes, and in such cases could be controlled very satisfactorily by soaking the infected seed potatoes in a solution of corrosive sublimate. This treatment is, however, not successful in case the fungus is already present in the soil." By 1897 Arthur announced formalin as a remedy against potato scab. Soon thereafter Bolley discovered the fungicidal value of formaldehyde and advocated its use against smut in oats.⁴⁴

During the early 1890's so many studies of various plant diseases were being made at agricultural experiment stations over the nation that workers began to specialize in different types of investigation. The bacterial disease of cucumbers, cantaloupes, and squashes provided Smith with his speciality. In July 1893 he had photographed the disease through four stages in cucumber plants located on a hill in Anacostia in the District of Columbia. In August, when reading his paper before Section G, he had given the results of his histological and anatomical examinations: the disease's beginning in the leaf blade; the bacillus' entrance into the stem through the spiral vessels; the organism's destructive action believed to be mostly confined to the phloem part of the bundle, the spiral and larger netted and pitted vessels filling with bacilli, and the formation of cavities filled with bacilli in the tissue around the spiral vessels. He had announced these and his major conclusion that the sudden wilting is due to the filling of the vessels and stopping of the plant's water current. Pure culture infections as yet had not been secured, and positive proof that the disease is transmitted by insects had not yet been established.

He had brought the "cucumber germ" on potato from Washington to test in his father's garden whether the germ would blight squash. A card-board memorandum indicates that several hypotheses were in his mind. Some were "not tried," and some yielded "doubtful" conclusions, while to others the answers was a final "no." His purpose seems to have been to test the range of susceptibility among various crops and further to study the areas of infection and the time required and mode of action

⁴⁴ See, Fifty years of pathology, *op. cit.*, 27. Also, *Bull. Ind. Exp't Sta.* 65, and unpublished memorandum by Arthur, already cited. See also, H. H. Whetzel, *Outl. Hist. of Phytopath.*, *op. cit.*, 105-106.

of the germ within different parts of the plant. He obtained what he thought were infections, but the "work of the first few months was thrown away, principally because," he later said,⁴⁵ "I did not know how to proceed, my technique being defective. Up to November 24, 1893, I had isolated five or six organisms . . . but had not obtained infections with any, and was very much at sea after a great deal of hard work." On September 23, and perhaps before, he had begun to keep carefully prepared memoranda of every investigational procedure. These show that he was determined to study the disease, if at all, in accordance with the canons of Koch.⁴

In December, at the regular meeting of the Biological Society of Washington in Assembly Hall of the Cosmos Club, he presented a thirty-minute paper "On a Bacterial Disease of Cucumbers, etc., working through the Fibrovascular Bundles; Probably Transmitted by Insects," Immediately F. M. Webster, entomologist of the Ohio Agricultural Experiment Station, inquired whether he planned to publish it since he planned to begin work the next spring on the entomological aspect of the problem. Two years before, Waite had presented "good evidence"⁴⁶ of the insect-transmission of pear blight. The completed demonstration that pear blight bacteria are carried from flower to flower by insects, furthermore, nearly coincided in point of time with the confirmatory experiments which established that southern or Texas cattle fever is transmitted by a cattle tick (*Ixodes bovis* Ry.). Smith admitted that these discoveries and their proof led him to search for proof of his hypothesis that the plant bacterial disease he was studying is insect-transmitted. We shall meet this point again later in this book.

On January 6, 1894, before the Botanical Seminar, he presented a "Synopsis of [J. R.] Green's paper on Vegetable Ferments," published in the March, 1893, issue of *Annals of Botany* and a review of which article, also prepared by Smith, was to appear in *Science*⁴⁷ on March 2, 1894. With much reason it may be questioned whether, in fact, Smith during these years could be regarded as a deep student of vegetable ferments. That he was

⁴⁵ *Bacteria in relation to plant diseases op. cit.*, 2: 217.

⁴⁶ E. F. Smith, *Intro. to bacterial diseases of plants, op. cit.*, 25, 30, and authority cited.

⁴⁷ 23(578): 116-117, March 2, 1894.

zealous to learn of them, both from the available scientific literature and his own laboratory research, cannot be doubted. Every bio-chemical laboratory technician and ardent follower of Louis Pasteur watched in America with eager restlessness the accumulating scientific progress being achieved by his disciples in the Pasteur Institute and other of the great laboratories of Europe. In 1877 Émile Duclaux had begun to assemble within the compass of some pages for Dr. Dechambre's *Dictionnaire des sciences médicales* a coordinated body of learning from scattered facts concerning the enzymes.⁴⁸ In 1882 had appeared Duclaux's *Ferments et Maladies*, and in 1886 his *Le Microbe et la Maladie*. We cannot affirm with certainty that Smith by the years 1893-1894 had studied these volumes. Most assuredly within a few years he was acquainted with the work of Duclaux, and by the year 1894 had familiarized himself with the views of the German Oskar Loew.

Among scientists, the view then, as now, was that certain of the ferments of fungi and bacteria are enzymes. Throughout the rest of the century, Smith, when describing a bacterial disease of plants, listed the factor of enzymes among the fermentation products of the causal microorganism. Enzymes were believed to have been isolated from a number of bacteria, and Smith now so stated in his review of Green's paper. "Vegetable ferments," he said,

are readily destroyed by boiling, and are for the most part very sensitive to acids and alkalies, a slight excess destroying them or stopping all action. They are not readily identified in tissues by use of stains. Some are very unstable. Enzymes have very slight power of diffusion. They can make their way through cell walls, but not through the parchment walls of dialyzers. They appear to act in an ordinary chemical way, causing hydration (myrosin excepted) and subsequent decomposition. Most of the changes brought about by enzymes can be effected in the laboratory by ordinary chemical processes. They are extracted for experimental purposes by water, salt water, or glycerine, and are quickly precipitated by excess of alcohol. One of their most striking peculiarities is the enormous power of conversion they possess, a sample of invertase (which occurs in a variety of vegetable substances,—yeast, bacteria, fungi, malt, buds and leaves, pollen, grains, etc.) being capable of inverting 100,000 times its own weight of cane sugar without injury to itself. The ferments of the fungi and bacteria are also enzymes, and the old view of Naegeli, that there are

⁴⁸ Erwin Frink Smith, Introduction, *Pasteur, The History of a Mind*, by Duclaux, and translated by Smith and Florence Hedges, pp. xvii, xv and xvi.

two distinct classes of ferments, organized and unorganized, is no longer tenable. Enzymes have been isolated from a number of bacteria, and even several from the same organism,—in case of the potato bacillus, *B. mesentericus vulgatus*, no less than five, viz., diastase, invertase, rennet, a proteohydrolytic enzyme and one destroying the middle lamella of vegetable cells.

Smith acknowledged that the "constitution of enzymes [was] still in dispute. Loew," he continued, "as the result of analyses, considered them to be proteids closely allied to the peptones, but spectrum analysis and other evidence has now made this doubtful." Recently in America, Loew's natural system of the actions of poisons, establishing a physiological system and arranged according to prevailing views and knowledge of elementary units of the animal and vegetable body, had found favor.⁴⁹ This work, organized on the basis of extended, positive knowledge of the cell, the constituents of protoplasm, and structural and physiological properties of living matter, accomplished more than the older studies, either in medicine or the pathology and physiology of mammals, in that more known poisonous actions were included. General and special poisons were described and classified. Loew's "Ein natürliches System der Giftwirkungen," dedicated to von Pettenkofer on the occasion of his fifty-years' doctor-jubilee, had resulted in part from the author's own research. Reviewer J. Christian Bay found more reason than ever to believe that time had arrived to establish "a special general physiology of animals and plants."

A year and a half later *Science*⁵⁰ published Loew's study of "The Synthetical Powers of Microorganisms." He, offering numerous equations and formulas, analyzed not only organic and inorganic chemical substances involved in microorganic activity but also the synthetic products and by-products which the lower organisms can form for life and death purposes. His interests were in nutrients and the nutritive qualities of acids and other by-products as much as in poisons and noxious elements, and he examined the chemical activities of non-pathogenic as well as pathogenic organisms. It was a work done principally from the

⁴⁹ J. Christian Bay, Missouri Botanical Garden, Loew's natural system of the actions of poisons, *Science* 22(550): 93-94, Aug. 18, 1893.

⁵⁰ I, 23: 35-36, Jan. 19, 1894; II, 23: 144, Mar. 16, 1894.

standpoint of physiological chemistry but the definition, for purposes of experimental study, of the sources of such basic elements as carbon, nitrogen, sulfur, etc., and the various possible processes of change and combinations of elements resulting from synthesizing activities of such organisms had important bearings in laboratory work of pathology and to some extent in field studies.

Early in 1893 Pierce agreed with Smith that they could not expect to "master all plant diseases the first time trying." He wrote:

I feel well satisfied if one-fourth or one-third of those undertaken by myself are handled satisfactorily. I much regretted that the fermentation of figs could not be prevented by the use of sprays. The experiments conducted in that disease the past summer were laborious and extensive, but with no marked results so far as prevention is concerned. It is evident that insects take the yeast to the center of the ripening figs, and all the sprays in Christendom applied to the exterior won't save a fig. I was greatly disappointed when I found this out, but what can a man do about it except *catch the insects or stop up the figs?*

The souring ferment of figs was a disease of the internal parts of the plant and, to this extent, similar to Smith's bacterial disease of cucumber, cantaloupe, and squash. From his field research Smith had soon learned that the wilting, the cucurbit disease's most important symptom, was an internal phenomenon set up after the germ had secured entrance, and this though the external parts of affected stems appeared to remain uninjured. Quite properly, therefore, he had not given his first attention to prophylactic spraying remedies but studied the insects which possibly transmitted the disease and examined the plant's physiology to understand its pathology and the workings of the bacterium.

The significance of a prescriptive remedy, measured in dollars and cents to growers, was not always proportionate to the complexity of the achievement. Years of discouraging effort sometimes were spent by pathologists to arrive at a simple remedy, which, however, when found, proved of great economic value. From Chico, California, on July 27, 1894, Pierce wrote to Smith of his "largest and most complete experiment" he had thus far undertaken—namely that of finding a fungicide by which to control curl leaf of the peach:

. . . as "bad luck" would have it, the disease did not develop in my sprayed orchard this year, although last year it stripped 40 acres of these trees of all the leaves and fruit. Many of my experiments in other parts—where the curl developed—are coming out all right, but none of them will compare with what my own would have been had the disease developed. I have concluded not to publish but to give the work another year, and within the past week I have made arrangements to repeat the spray work on the same trees I had last winter—the Hatch and Rock orchard at Biggs, California, where there are 1600 acres of trees and where trees five years old are so large that you cannot see between the rows although the trees are set 25 feet apart! The orchard is by far the most beautiful sight I ever saw, of its kind.

There are three other fungous diseases in this orchard—a root fungus; a shot hole fungus of leaf, stem and fruit; and a mildew of stem, leaf and fruit. They are all serious diseases when developing under favorable conditions. I have material for future study as well as good illustrations of the work done by them. The root fungus is very common in California and is causing much loss. The mycelium is like that of *Agaricus melleus*, but as yet I have been unable to determine if it be of that parasite, and there are some good reasons for thinking, in fact, that it is of another species and perhaps genus. It is destroying acres of fruit trees of various kinds, and I am now trying to learn if there are any kinds which are exempt—any root which may be grafted upon and prove resistant. The other diseases will probably have to be handled with sulphur and copper.

I have seen an apple disease from Oregon which I believe does not exist in the east. It is a scab quite unlike that we are acquainted with. Then there is a disease of peach and apple trees which induces a sore-like appearance, and the cause of which is not well understood if known at all. Another disease affects the cherry when packed, and one in eastern Washington is said to be bacterial in nature. This I hope to see.

On November 8 of that year, Galloway reported to Secretary Morton,

It has been found that peach leaf curl, a disease which annually causes many thousands of dollars damage to the entire country, may be almost completely held in check by a simple and inexpensive winter or early spring treatment. To test this remedy experiments were made at a number of points on the Pacific Coast, as well as in the eastern part of the United States, during the year. In all sections where the curl prevailed, the treatment was highly successful.

Pierce was not without a bacterial disease of plants to study. From Santa Ana, California, on January 19, 1896, he told Smith:

For spring work I have to plan and conduct a series of spraying experiments on a disease of walnuts which is beginning to do considerable injury

to the industry here. Thus far it appears to be bacterial in its nature, but as pure culture inoculation experiments have not been completed as yet it is not settled beyond question. Inoculations from impure culture have reproduced typical cases of the disease. It is a disease both of the nut and tree—and the organism which seems to be the cause is a bacillus of medium length. As it is an important matter I hope to give it special and personal attention this spring.

Reports from Washington are favorable respecting the treatment of the serious apple disease I worked on while north, and which was so beautifully reproduced after my arrival at this laboratory a year ago. In due time I will work this disease up for a special bulletin as it is of the deepest interest both from a horticultural and a scientific standpoint.

The matter of olive culture has occupied my time of late—several diseases are getting a foothold which may in time require more attention. You will see what I have done on the olive after a time!

As the walnut trouble is a serious one and as it may be, and quite likely is, a bacterial one I am interested to know what means Mr. Waite suggests for the prevention of pear blight. I suppose it is the spraying of flowers much as with codling moth, but do not know. What do you think of the use of Lysol—the new French treatment? Some time when you can, write me about Mr. Waite's treatment. I would like to try Lysol and Corrosive sublimate in some form—though from the way the disease works I question our power to control.

With a view to developing disease-resistant varieties, later on, both pear and walnut varieties were sought to be introduced into this country, and attempts, which met with partial success, were made by hybridization to solve the economic problems of Pierce's bacterial walnut disease, and pear blight.⁵¹ In California, Pierce obtained walnut hybrids with a fair degree of resistance to disease, and in Oregon, F. C. Reimer, utilizing Asiatic species of *Pyrus*, got good resistant pear stocks. Moreover, Waite, over a period of years, presented knowledge which showed that, on trunks and limbs of occasional trees, "hold over" patches, which ooze living bacteria, last over winters in pear orchards. By thoroughly eradicating these "hold over" spots so that bees cannot have access to them and transmit the bacteria to new blossoms and shoots of the following spring, a control method, tried both in Georgia and California, accomplished real results.⁵²

⁵¹ Fifty years of pathology, *op. cit.*, 30, 39. Also, David Fairchild, Plant introduction for the plant breeder, *Yearbook of the U. S. Dept. of Agriculture* for 1911, *op. cit.*, 411-422, at p. 421. Also, V. B. Stewart, Notes on the fire blight disease, *Phytopathology* 5(6): 327, Dec. 1915, and cited references to articles on the subject by Reimer and N. E. Hansen.

⁵² Erwin F. Smith, *Introduction to bacterial diseases of plants, op. cit.*, 68.

Smith, together with Secretary Morton, Galloway, and Entomologist C. V. Riley, had been invited to California in 1893 to attend the State Fruit Growers' Meeting held at Los Angeles November 21-24 of that year. Resolutions of the State Pomological Society and Farmers' Institute had been received which protested the withdrawal by the federal Department of Agriculture from California of D. W. Coquillett and Albert Koebele and requested that Coquillett be reestablished as field agent in economic entomology in Southern California. Coordinate vegetable pathological and entomological services maintained by the federal government in conjunction with state agricultural agencies had been much appreciated by farmers and fruit growers there in "their warfare with insect pests." Certain scales infesting orange trees, particularly San José scale, loomed as one of the most difficult problems of California agriculture, and under Coquillett's direction hydrocyanic acid gas, very effective when applied under tents in orchards of oranges, was discovered to be an efficient remedy, more so than lime-sulphur spray which was also used but which injured the foliage. San José scale had been introduced into California from eastern Asia and during the first half decade of the new century became alarmingly prevalent in eastern United States, too, its spread taking place on infested nursery stock through carelessness or lack of scruples on the part of two eastern dealers.⁵³ Smith, however, never visited California.

For months he had tried to secure pure culture infections and isolate the bacillus of his bacterial disease of cucumbers, melons, and squash. His career in plant bacteriology was definitely started.

January 16, 1894, his laboratory memorandum read:

In early stages of the disease the bacteria are confined almost exclusively to the spiral vessels of the inner xylem. These seem to be the first attacked. Bundles seen today in all stages of fullness. Some just commencing to have vessels gorged, others in which every vessel is full but not yet broken down; others full, structure not distinct as if vessels broken down. My sections too thick to settle this point. This disease serves as additional proof

⁵³ Plant pathology: a retrospect and prospect, *op. cit.*, 608; Fifty years of pathology, *op. cit.*, 24; in the autumn of 1894 Messrs C. L. Marlatt and Coquillett extensively tested washes in a scale-infested orchard in Maryland, among which was lime-sulphur salt wash as used in California, and the Oregon wash, both used at ordinary and double strengths. See an article by A. L. Quaintance, Lime sulphur washes for the San Jose scale, *Yearbook of U. S. Dep't of Agric.* for 1906: 429. This gives a brief history of the various formulas used in the eastern states.

that the water passes through the bundle and to be more specific the xylem part. . . . It is important to make out clearly to what extent the parenchyma is involved where the general wilt begins.

[April 14] This much seems clear, when the constitution[al] symptoms first begin, i. e. the general wilt of the foliage, the clogging and destruction of tissues is still confined to the inner non-lignified part of the xylem portion of the bundle.

On February 11, after examining fourteen slides, Smith prepared a memorandum "On Question of Infection of Phloem in Cucumber." "Even where nearly all of the vessels in a bundle are full," he noted, "and large cavities have formed in the primary vessel parenchyma, the phloem is uninjured. Slides from two stems. One bundle found with germs in one sieve tube. Six longitudinal radial sections stained in haematoxylin were also examined. No breakdown in phloem."

April 1, 1894, he spoke before the Biological Society of Washington on "The Length of Vessels in Higher Plants," the same organization before which he had spoken on May 6, 1893, "On the Symbiosis of Stock and Graft: A Digest of Recent Experiments by Strassburger and [Hermann] Voechting." Smith's research memoranda of these years reveal that on points of final study of peach yellows and laboratory examinations of the cucumber germ he reviewed his knowledge and the latest learning of vegetable physiology. Four or five of Strasburger's writings published 1888-1893 were probably acquired at this time. "Books to buy," a small note book, lists several books by Europeans and at least one work on the microtome.

During April 1894, Smith outlined the following research on the cucumber study:

Notes (and drawings) on all my cucumber germ slides (parafine sections) for answer to the following questions: (1) Are there any cavities in the phloem? (2) Any bacteria in the phloem? (3) What proportion of the xylem vessels are full? or contain some bacteria? (4) Which vessels fill first after the spirals, i. e. where situated? (5) Where are the cavities in the xylem? (6) Does the primary vessel-parenchyma stain differently from phloem and rest of xylem?

By May 5 he had evolved⁵⁴ a formula which would not only

⁵⁴ *Introduction to bacterial diseases of plants, op. cit.*, 117, where it is said, "Long since (1894) the writer resorted to methyl violet, preceded by a bath in tannin water to reduce the excessive stain of the host tissues in stems of cucumber

stain the germs in cucumber but also leave the tissues faintly stained, too. The more important extensions and improvements of plant bacteriological technique made in connection with research on this disease were commenced, however, the following autumn and winter, after Smith had spent a summer studying, and had presented a paper before the August meeting at Brooklyn, New York, of Section G of the American Association for the Advancement of Science on, "The Watermelon Disease of the South,"⁵⁵ caused by a fungus which he provisionally named "Fusarium niveum."

Smith himself can best describe his accomplishments in this, another and next line of researches, which culminated in a still more important address, "The Fungous Infestation of Agricultural Soils in the United States,"⁵⁶ read August 25, 1899, at the Ohio State University before the Botanical Section of the Association. During a period of sixteen years (1894-1910), together with his epoch-making laboratory investigations of bacterial diseases of plants, Smith devoted himself "to the study of the Fusarium diseases of plants, a subject which was then very new. There were," he has written,⁵⁷

at this time in the United States a number of destructive diseases of unknown origin, particularly on staple crops in the Southern States, in which I found Fusaria constantly and suspiciously present. I studied and made experiments with the Fusaria present in diseased melons, cotton, cowpeas, potatoes, tomatoes, and cabbage. I isolated the fungus from the interior of these plants in pure cultures derived usually from single conidia and with it produced the disease abundantly in case of several of them, thus showing it to be the parasite. I proved infections from the soil; inability of the various isolations to cross-inoculate, *e. g.*, on soil infected with pure cultures of the melon fungus I grew, from seed, rows of watermelons containing many plants, every one of which contracted the disease, alternating with rows of tomatoes, none of which contracted the disease although they were in the infected soil and only a few inches away from the dying melons; showed that the melon Fusarium was still infectious after being held dry in culture tubes for three years, and in case of the cabbage disease that the organism causing it remained alive and able to infect in soil from a diseased field which had been kept dry in the laboratory for two years.

attacked by *Bacillus tracheiphilus*. . . ." Smith's memorandum shows the date of this as May 5, 1894.

⁵⁵ *Proceedings*, 43rd Meeting, 289-290, pub. March 1895.

⁵⁶ *Scientific American* Supplement, no. 1246: 19981-19982, Nov. 18, 1899.

⁵⁷ Synopsis of researches of Erwin F. Smith, *op. cit.*, 20-21.

Of Smith's work on fungous diseases of plants caused by parasitic *Fusaria*, H. W. Wollenweber has said: ⁵⁸

The opening up of the *Verticillium* problem we owe to Reinke and Berthold (1879), that of the whole *Fusarium* problem to Erwin F. Smith (1899). The latter investigator obtained the first conclusive results in the etiology of the watermelon wilt by securing 500 successful infections by simply inoculating the soil with pure cultures of the vascular parasite. He considered the organism a variety of the same species as causes cotton and cowpea wilt. The parasite of watermelon wilt was not infectious to cototn or cowpea, nor to tomato or potato, through soil inoculation.

Dr. Smith also discovered a very peculiar fact. A perithecial stage (*Neocosmospora*) was frequently associated with the parasite on cowpea, especially on parts already killed. In pure cultures derived from a single ascospore, all stages (mycelium, conidia, ascospores) of *Neocosmospora* redeveloped the ascus stage, and the conidia in subcultures of the fungus were similar to the small conidia of the parasite. Cowpea inoculations with the fungus derived from ascospores of *Neocosmospora* failed, however. Since this failure might be due to the fact that the natural method of infection had not been discovered, the author did not feel justified in regarding it as proof of saprophytism. Therefore, he chose the hypothesis that the vascular parasite and the ascomycete *Neocosmospora* were identical.

Not until 1899 did Smith propose his new genus *Neocosmospora*, and then he was unsure whether it was "one fungus, or three." ⁵⁹ Nevertheless, he had "opened up a new field of plant parasitism," had been, he believed, the first to publish on "soil infections due to *Fusariums*, some of which are as destructive as *Peronosporas*," ⁶⁰ and through his work "special attention [had been drawn] to the fact that this form-genus, hitherto generally supposed to be saprophytic, contained a number of very destructive soil parasites." ⁶¹

On March 26, 1892, Smith had read before the Botanical Seminar of Washington a paper on "Relations of the Soil to Plant Nutrition." That year Milton Whitney had outlined an elaborate plan of soils investigation in the belief that some clue would be found which might help to solve the problem of peach yellows. Whitney was in charge of a division of soils investigation at the Maryland Agricultural Experiment Station and later became chief

⁵⁸ Studies on the fusarium problem, *Phytopath.* 3(1): 24-50, Feb. 1913, at 36.

⁵⁹ Bull. 17, U. S. Dep't Agric., Div. Veg. Phys. and Path., 39, 46, 1899.

⁶⁰ Synopsis of researches, *op. cit.*, 22.

⁶¹ Fifty years of pathology, *op. cit.*, 22.

of a Division of Soils in the Department of Agriculture. He had studied in South Carolina diseases and insect ravages of the cotton plant in relation to soil conditions, and his knowledge of agricultural chemistry further qualified him for such an investigation. Soils research was then emerging into more definite scientific prominence. During 1892 the *Experiment Station Record* by editorials⁶² had recommended to students more research in plant diseases caused by bacteria and also cooperative soils investigations of the type begun by the Department of Agriculture and the Maryland station and Johns Hopkins University under Whitney's leadership.

Whitney offered to study with Smith a few Maryland localities where healthy and diseased peach trees were in a close range of proximity. Top-and-subsoils bearing diseased trees were to be examined both in the orchard and from soil samples in the laboratory. The theory was that changes in the "texture of the soil around a peach-tree might change the physical conditions of growth sufficient to lower the vitality of the tree and render it more liable to take the disease." Conversely, factors leading to increased disease-resisting vitality were to be studied. Smith believed that the contagious nature of the disease was now "well established,"⁶³ and the results of his experiments with chemical fertilizers were known to him, though not fully published. He had not solved the problem and much work had been done. It is likely that no collaboration ever took place. But, if it did, Smith learned more of soil chemistry and soil physics and related this knowledge to his interest in the nutritional requirements of plants. In his study of the South Carolina watermelon disease, furthermore, he might have made some use of the knowledge.

On July 1, 1894, from Monetta, South Carolina, he advised Galloway:

The watermelon fungus turns out to be one of the most interesting things imaginable. . . . It is no germ but a fungus! Of this I am now certain, having examined 21 vines microscopically in a good many parts of root and stem and having found this abundant in every case and in the early stages of the disease, i. e., as soon as the leaves begin to wilt and before stems or roots show any external signs of injury. The symptoms

⁶² 3(10): 665-670, May 1892; 4(2): 111-113, Sept. 1892.

⁶³ The chemistry of peach yellows, pt. 1, *op. cit.*, reprint, 14.

are much the same as in the germ disease, [cucurbit wilt] i. e., sudden wilt and drying up of long shoots or whole vines. This comes about in the same way too, i. e., by the mechanical clogging of the vessels. This I have found most abundant in the stem near the root but also abundant in the root itself and in many stem vessels 2 to 5 feet away from the root. At this stage the injurious action seems to be chiefly mechanical but as the vine begins to die the fundamental tissues are invaded. The fungus produces colorless conidia in the vessels, and there is a pycnidial fruit occurring on stem and petiole but I have not yet examined this closely enough to be certain whether it is part of the life cycle. My belief is that the fungus winters over in the dead vines. I am now looking for other spore forms in trying to determine how it enters the plant. Think I have found the place of entrance but am not sure and will not say anymore about it for the present.

Two days later, believing that while there he could isolate the fungus, he sent for litmus paper, more agar tubes, and sterile petri dishes. Galloway remembered a bulletin by Atkinson on a cotton disease which seemed similar and sent a copy. But before the materials arrived, Smith had succeeded in growing the fungus in agar and had pure cultures from single spores three days old; he also had six tube cultures made by cutting out small segments of infected stems.

For years, whenever he encountered a multiplicity of organisms in his cultures, Smith had been skeptical of the value of his results. In 1891 especially, when in Georgia preparing field cultures of peach rosette in a make-shift laboratory, he had been conscious of possible air contaminations and the wisdom of scrupulous care and cleanly work. He, therefore, would not assure Galloway that each of his field cultures made in South Carolina were "entirely pure." Nor would he assure Galloway that in details his watermelon *Fusarium* agreed with Atkinson's cotton disease organism but admitted it might be the same. "Spores are larger in this form and symptoms different," Smith wrote. But with the cultures he possessed he "studied the growth from single spores (in agar) round to the time when they have fruited again (less than twenty four hours), and have some good camera drawings," he informed on July 8.

This week I hope to start infection experiments with pure cultures but there is so much of the disease here that these will have to be made on a large scale to establish anything. Can you not have some watermelons started at once in one of the Department greenhouses, preferably the

Pineapple house and at the same time in the open air, so that when I return they will be big enough for inoculation? . . .

Smith sent for the experimental and systematic literature on *Fusarium*, including that of Saccardo. He sterilized his old agar dishes in the kitchen oven of the home in which he was living. Days he spent in the fields studying the vines and evenings he spent with his cultures and drawings. July 15 he announced in a letter to Galloway he had "made some discoveries which throw a flood of light on the nature of the fungus. It is an active parasite when the vines are young, damping them off in large numbers as readily as a *Pythium*. This was a very unexpected result."

He explained a long, complicated series of investigation. He was aware of the error of his first belief that "the fungus got in above ground." He now believed that the *Fusarium* threads grow through the tissues in every direction boring through cells, even the parenchyma cells deep in the tissue, and fruit abundantly in all parts and inside the cells, a dozen spores in a cell sometimes. This discovery, he realized, made the possibilities of treatment complicated. Spraying would not prevent the disease, even if any vines were left for such trial experiments. He became satisfied that the disease gels underground, and that the fungus entered the plant through the root. Bordeaux mixture experiments, later tried, further confirmed this. So, after following up several other hypotheses and gathering together his carefully prepared and saved materials for further work in his laboratory in Washington, he, having spent almost two months in field work, returned to his home. It is not impossible that he had made a hurried trip to South Carolina in 1893. It is certain, moreover, he was to return there in 1895 for extended researches in *Fusaria*-caused diseases of cotton, cowpea, tomato, and other crops. His visits were memorialized in a poem entitled "A Sunset in South Carolina" and written in 1926:

As plain as yester-eve, burns through my head
The flaming glory of a sunset old,
Nor will it ever fade till I am cold,
For thirty years and more since then have fled.
At first the whole blue dome was overspread
With small torn clouds of silver and of gold
And then with crimson clouds 'twas flecked and rolled
In one vast canopy of blue and red.

Canaletto never saw diviner hues
Of silver, crimson, gold upon his blues,
Nor any other painter after storm!
Singing and shouting, beside myself with joy,
I walked the fields alone in the twilight warm,
All else forgot, and was again a boy.

Sometime during 1894 Smith obtained diseased cotton samples identical with those described by Atkinson and determined to investigate still further the question of soil parasitism. In 1895 a similarly caused cabbage disease reported from New York, and found also in Maryland, stimulated him to renewed effort. Sweet potatoes sent from Iowa to Washington disclosed a *Fusarium* "plugging the vascular system," and Halsted worked on the same or a similar disease in New Jersey.⁶⁴ Smith learned, likely from Swingle, Rolfs, or Webber, of a tomato disease in Florida, also due to a *Fusarium*, which practically put an end to tomato growing in some places for early northern markets. During the period 1894-1899, Smith obtained more than 500 melon plant infections, and in more than 400 of these demonstrated the presence of the fungus within the vessels. More will be told of these researches later. Years were spent in examining the action of three parasites (or one) to solve the extreme puzzle how to place them in a proper order of systemization. For, said he, "when I make cross-inoculations, I cannot get any cross infections that would indicate them to be the same fungus."⁶⁵ Some practical rules for the farmer were early deduced. The prime rule was to keep out the fungus and to this end care was recommended in the use of fungus-free tools and appliances. Cattle should be kept from pasturing in fields where soil was infested, thereby preventing spread of the fungi and diseases on the cattle's feet. Good farming practices were many which would aid in eliminating either origin or spread of the infectious agency. Where soils became infested, either complete abandonment of the field for a period of years or crop rotations utilizing plant varieties not susceptible to the diseases were indicated alternatives. It had long been good farming and scientific practice to pull diseased plants and burn them. The method of greatest scientific interest, however, lay in the later

⁶⁴ The fungus infestation of agricultural soils in the United States, *op. cit.*, 19981-19982.

⁶⁵ *Idem*, 19981.

development of technical procedures to produce disease-resistant varieties by seed selection experiments, and by hybridization.

Smith, while in the south during these years 1894-1899, must have become acquainted with cotton breeding there. Atkinson, as early as 1892,⁶⁶ referred to crossing varieties of cotton at the Alabama experiment station by Mell, botanist and meteorologist in charge of phanerogenic botany. Whether such was aimed at accomplishing directly or indirectly the object of disease resistance cannot be stated with certainty. Galloway urged Smith to visit the Alabama station and Secretary Morton directed Smith on July 31, 1895, to visit points in Georgia, Alabama, and Mississippi as part of his study of cotton and melon diseases. Smith's diary to be quoted later shows definitely that on July 24, 1895, at James Island, opposite Charleston, South Carolina, he observed closely cotton seed-selection work practiced over a period of three years by plantation owners to prevent crop deterioration.

In November 1894 V. M. Spalding, home from study at Leipzig with Pfeffer and Detmer and planning to return to Europe soon for further work, regaled him with a letter from Cambridge, Massachusetts: "I feel like shouting vigorously with you," he wrote, "over your discoveries in the case of the watermelon disease. As a matter of fact I might just as well come down to Washington to study fungi as to put in the time with Brefeld, but I want to see how he does it, and then perhaps we can compare notes later on." Smith recently had given much of his spare time to reading and criticising Spalding's book then being published. In reply to some criticisms on matters of plant physiology, Spalding asked: "Is there anything fundamental in plant physiology that you can think of that is really settled?" He invited Smith to visit him in Cambridge and together they might examine the laboratories at Harvard and consult some of the distinguished men of the university.

Smith must have immediately written him of his new investigations of the "germ" of the bacterial disease of cucumber, melon, and squash. In September he had artificially infected squash vines and three weeks later made cultures and found that the organism grew readily in broths of beef, peptonized beef,

⁶⁶ Botany at the experiment stations, *op. cit.*, *Science* 20: 329, Dec. 9, 1892.

cucumber, or potato. He may have told him of other experiments. There were many during that month and October.⁶⁷ He had found that the germ is acid-producing, grows in alkaline media, and he wanted to know whether it could change the cucumber juice from an acid to an alkaline content. He was trying to explain the pathological condition once the germ had entered, and he was experimenting further to see whether insects were the means of the germ's entering. Whatever he wrote Spalding, his former professor replied: "Now you *have* made a discovery. . . . I venture to say that you have made a most important contribution to plant physiology and pathology. . . . I congratulate you most heartily on getting hold of a new thing under the sun, as it seems to me it must be."

On November 3, Smith had planned a series of experiments to determine the acid produced by the germ, its reaction on growth media, and his first notation indicated a comparative study between the anatomy and physiology of the cucumber plant in health and the pathological changes which take place when the infective action begins. In 1893 he had mapped the geographic distribution of the disease. He had studied all the extrinsic aspects of the malady he could think of; now he was carrying deeper his detailed examination of intrinsic factors. He sketched the distribution of the germ in various parts of the plant. He traced the spread of the infection from organ to organ. His ultimate aim seems to have been to examine the aspect of acidity under conditions of disease, and to test the organism's growth on various culture media, as well as in the plant, was part of this aim.

In September he had begun to distinguish between a primary and a secondary wilt. By October his broth cultures had been increased from four to nine. He found that the germ could live on meat extract peptone agar twenty days. Later he discovered that, under favorable conditions, the organism would live and multiply for almost four months with the use of suitable media. He next experimented with acidified broth cultures. At the same time he maintained some thermal death point experiments. He uncovered facts of especial interest while testing the germ's sensitivity to organic acids—acetic, malic, citric, oxalic, and tartaric.

⁶⁷ *Bacteria in relation to plant diseases, op. cit., 2: 219-233.*

Every experiment yielded up data of the organism's biology of development and action. Culture media had four-fold uses: ⁶⁸ to isolate organisms from mixtures or directly from diseased plant tissues, to grow organisms without loss of virulence over long periods, to serve the purposes of differential description, and to aid in chemical analysis. Smith, in "the first treatise of its kind in the world," an introductory text book, ⁶⁹ not a monograph, on bacterial diseases of plants, stated in 1915-1920 some objectives he had maintained in differential descriptive cultural study for more than a quarter of a century. Said he:

What we seek here are media that will bring out not necessarily good growth, or any growth at all, for that matter, but *differences in behavior* when a variety of bacteria are tested in it, that is, changes in gross appearance, morphology, pigmentation, precipitates, pellicles, crystals, weak vs. dense clouding, medium reactions (acid, neutral, alkaline), using neutral litmus and phenolphthalein, etc.—and here a medium generally neglected by bacteriologists may be just the one needed. . . . Often, when the colonies of two organisms look exactly alike on plus 15 peptone-beef agar, we try potato agar, prune agar, string-bean agar, starch agar, whey agar, or some kind of gelatine medium, and find a difference.

By 1895 Smith found himself able to improve his technical proficiency in the study of plant bacteria. It was perhaps as late as November 1894 before he began to refer in his research memoranda to the cucumber "germ" as *Bacillus tracheiphilus*. During this month he increased his acidified broths to at least five or six. Really not until this month did he invent his own formulas for beef and potato broth cultures. It was during this month that he concluded that *B. tracheiphilus* "splits up glucose forming an acid." As well as studying the organism's growth and behavior, he was exploring its nature and needs, its range of tolerance to acids, alkalies, salts, sugars, N-compounds, alcohol, et cetera. In the year 1894 he completed his first technical description of the plant pathogen *B. tracheiphilus*. At this time, he must have believed that he had satisfied the requirements of Koch's canons. But not until 1895-1896 did he prepare his last elaborate outline of "Things which must be done before I can close out my experiments on *Bacillus tracheiphilus*." One of the points for

⁶⁸ Uses of culture-media, *Intro. to Bact. Dis. of Plants, op. cit.*, 99-100.

⁶⁹ *Ibid.*, v-vi, 99-100.

verification and rechecking was then: "Determine what acid is produced by the breaking up of the sugars." Second, "Repeat experiments, growing bacillus in juice of parenchyma, to determine if the acid actually retards growth, comparing with cultures in potato broth and beef broth. . . ." When repeating his thermal death point experiments, he wanted to "determine whether heat just below thermal death point only paralyzes or actually destroys the majority of the bacilli."

From experiments and culture study made to see whether the germ would grow only, or at all, on alkaline media, Smith later evolved a theory of alkalinity of vessels. A memorandum read:

The fluid in each one of these six tubes is plainly alkaline to neutral litmus although the media was plainly acid on the start. It is probable that an acid is also present for on warming the blued litmus paper in the bottom of a deep beaker over a gas flame the blue color quickly disappears and the spots are then redder than the rest of paper as if from presence of some acid. If this is true then this germ breaks up grape sugar (muscle sugar) and will probably grow in closed end of fermentation tube. Anyway the germ is a plain alkali producer and this alkali is readily driven off by heat i. e. it is ammonia.

Dr. Smith has elaborated the vast detail of his many experiments made with this organism.⁷⁰ What is presented here indicates his thoroughness and foresight in such investigations. On November 27 he observed under the microscope in a hanging drop from a broth culture a number of rod-like organisms with rounded ends and the "exact appearance of bacteria, *but highly refractive, like spores.*" He examined them with the polariscope. They were not crystals. Arthrospores? he asked. Settlement of this question was the sixth among the thirty points before concluding his original studies of this disease. He restudied his notes on the durability of the organism on various media. He investigated other species of plants susceptible to the malady. His experiments were made under widely differing sets of environmental conditions, all with a view to establishing the disease's etiology. In 1893 he had noticed on diseased cucumber vines several insects which might be carriers of the infection, and during the next years he proved its transmission by the striped beetle (*Diabrotica vittata*). Many experiments involved also the squash-

⁷⁰ *Bacteria in relation to plant diseases, op. cit., 2: 217-282, etc.*

bug, *Coreus tristis* and the spotted beetle, *Diabrotica 12-punctata*. He repeated, and extended, these. He rechecked his dessication experiments. He retried some experiments to determine why he had failed. The last dozen points of his unfinished work read:

Repeat experiments with acids, determining just the amount of lactic, oxalic, malic, and citric which will inhibit growth when used in potato broth or beef bouillon or Dunham. Filter a flask of carbonate of lime potato broth, beef bouillon, or Savory & Moore's Dunham solution (in which germ has been growing two weeks) through a Chamberland filter. Divide fluid into two parts, boil one and use other unheated to determine if a ferment destroyed by heat is present, using as proof small sterile uncooked pieces of stem or fruit containing vessels, the walls of which should soften or be dissolved in one and not in the other. Isolate this ferment if possible and determine its properties more accurately. How are the sugars broken up? By another ferment. Collect into one place from notes names of all species attacked. Inoculate basal leaf of large vines and determine accurately the rate of progress of the bacillus through the tissues (a) when plant is trailing on ground, (b) when trained upright. It is of special importance to settle two points, viz: (1) Must bacillus be present in clogging numbers in vessels of each petiole before leaf blade will collapse? (2) How long a time after the bacillus has entered the stem (wilt of first leaf up shows entrance beyond doubt) before presence of germs can be detected in vessels in uppermost part of stem either by culture methods or by microscopic examination of stained sections. If transpiration current actually moves in the lumen of the vessels it would seem as if this actively motile organism ought to find its way to extremity of plant, at least in small numbers, in a few hours after it enters the stem. If not, why not? More careful anatomical-pathological studies from paraffin sections. [Secure] microphotographs. Did not the ropy potato broths again become fluid? Look up in notes. Growth in Dunham plus sugar in fermentation tubes. . . . Get together from notes all that relates to growth on alkaline media of various grades of alkalinity. What degree of alkalinity inhibits growth? My four grades of gelatin good for this. Grow in F[ermentation] tubes in peptone water (Witte's plus) 1% C.P.G.

Two other points were included where the omission is noticed. Smith planned to study the behavior of the organism on cooked cocoanut and on stewed pineapple. More important, he planned to repeat what he described as his "hydrogen experiment," using sugar agar this time, and he styled this an experiment in "Facultative anaerobism." On December 4 he had made what he called an anaerobic or buried culture, that is, one flourishing without free oxygen, and begun studying this on January 5, 1895 together with pure cultures of *Bacillus tracheiphilus*.

That same evening, before the Botanical Seminar meeting, Smith presented his first technical description of the organism in a twenty-minute address, entitled, "*Bacillus tracheiphilus*, new species, the cause of blight in cucurbits." April 30, 1895, *Centralblatt für Bakteriologie* published his first real pronouncement in plant bacteriology, "*Bacillus tracheiphilus* sp. nov., die Ursache des Verwelkens verschiedener Cucurbitaceen."⁷¹

This was published in the second, or Zweite, Abteilung. General medical bacteriology had been given the first, or Erste, Abteilung of the *Centralblatt*. Publications on diseases of plants, along with those on bacteriology of the soil, dung, milk, cheese, vine, and chemical technology, were now, so far as this publication was concerned, officially in separate status. Throughout the first twenty-five volumes of the section devoted to plant bacteriology, Smith was to serve as an associate editor of the journal. He had not finished with the researches on *Bacillus tracheiphilus*. Years later, he would devote the first of several chapters on specific bacterial plant diseases in his famous text, *Introduction to Bacterial Diseases of Plants*,⁷² to the cucurbit wilt. In the second volume of his monumental three-volume work, *Bacteria in Relation to Plant Diseases*,⁷³ ninety pages were to be given over to a consideration of the wilt of cucurbits. Furthermore, numerous references and descriptions of the disease were to appear in other less important articles and treatises.

In his investigation of the cucurbit wilt and the next plant bacterial disease which he studied, that of tomato, egg plant, and Irish potato caused by *Bacillus solanacearum* new species, Smith achieved several important extensions of laboratory technique.

After Smith had sufficiently completed his inoculation experiments of pure cultures of *Bacillus tracheiphilus* in acidified and alkaline broths to be satisfied with his conclusion that the germ is sensitive to acid tissues of the plant and thrives on alkaline juices, on January 5, 1895, he took potato broths mixed with organic acids—malic, succinic, citric, tartaric, and oxalic—in proportion of 10cc. broth to 1cc. N/10 acid, which gave, he noted in his memoranda, about the same acidity as unboiled juice of the cucumber plant. These were steamed and sterilized, and a precipitate formed in the oxalic filtered out. The results of these

⁷¹ 2(1): 364-373, 1895.

⁷² *Op. cit.*, 132-145.

⁷³ *Op. cit.*, 2: 209-299.

were compared with the results of experiments which determined the acidity of the cucumber juice

by titrating against N/10 caustic soda recently set from N/10 H[ydrogen] C[h]l[oride], using Phenolphthalin as an indicator after compar[ing] this with anthocyan, methyl orange, and various other indicators. The juice was obtained in quantity from the parenchyma of green fruits from the market, as many as possible of the large vessels being cut away, [the] rest graded, juice pressed out and at once titrated. Samples, [he found, from the] two different sources gave [the] same results.

Smith wrote into his *Centralblatt* article his observations on the alkalinity of vessels. He hesitated to publish more until he had thoroughly checked the literature to see what already had been written, and to test the matter of alkalinity through more species of the cucurbits. In field research that year, using neutral litmus paper from the Division of Chemistry, he began testing a wide variety of plants to determine their alkalinity and acidity. August 12, in a manuscript of about fourteen pages of rather closely written matter, he announced, "This is the 6th genus of cucurbits tested and the fluid in the vessels of each is strongly alkaline." The point, he believed, was bound up with practical methods of preventing the disease. A quotation will explain:⁷⁴

The practical method of preventing the disease is to fight the insects that feed upon the vines. Destroy them and you wipe out the disease. At the same time all diseased plants should be pulled and burned, so that the insect shall find nothing but healthy plants to feed upon.

I seem to hear some one ask: "If these things are true, why does not every vine become diseased and so it become impossible to grow cucumbers or other cucurbits where the disease exists?" It also happens that I can answer that question. The germ is peculiarly sensitive in certain ways, for example, to dry air. Exposure to direct sunlight for some hours also kills it. Moreover it is sensitive to the acid tissues of the plant and thrives only on alkaline juices. In cucurbitaceous plants the green tissues are acid, while the fluid in the water ducts is alkaline, hence we find it thriving in the water ducts and choking them up, but if it were thrust by an insect bite or otherwise into the parenchyma of the plant it would make a feeble growth and if only a very few germs were inserted they might die. In spite of these circumstances enough plants become infected to carry the disease along and to do great damage to muskmelon, cucumber, and squash fields.

⁷⁴ Erwin F. Smith, Some bacterial diseases of truck crops, *Trans. Peninsula Horticultural Society*. Meeting at Snow Hill, Maryland, January 11-12, 1898, pp. 142-147, at pp. 143-144.

In this address, the brown rot of potato and the black rot of cabbage were also described. This was not a technical discussion addressed to an audience of scientists. In 1893 Smith had prepared for the *Agriculture of Pennsylvania* two reports made to the state horticultural association on "Diseases of fruits and the use of fungicides" and on "Peach yellows."⁷⁵

His bulletins and technical papers addressed to scientists usually preceded discussions of a semi-popular nature. Those contained "critical and detailed descriptions of the organisms respectively concerned" and became "exacting models"⁷⁶ for members of the profession of plant pathology, present and future.

About the same year that Smith began to study specific bacterial diseases in plants, another able scientist had been added to the working force of the Division of Vegetable Pathology. On December 1, 1893, Galloway had recommended that Albert Fred Woods be appointed to the position formerly held by Fairchild.

Mr. Woods [Galloway wrote] is about twenty-five years old, graduated from the University of Nebraska in 1890, and was immediately appointed professor in the department of plant physiology and pathology, a position which he now holds. From all accounts [he] is thoroughly qualified for the place, being a man of pleasing address, good executive ability, and thoroughly trained in our special field of work.

He held a graduate degree from his alma mater, and had established there the university's first separate laboratory of plant physiology. In plant pathology he had specialized in a study of mosaic disease of tobacco. At the University of Nebraska, under the leadership of C. E. Bessey a botanical seminar had greatly contributed to the advancement of scientific botany, especially in meritorious work in plant physiology and ecology. H. J. Webber, F. E. Clements, Conway MacMillan, Roscoe Pound, later Jared Gage Smith, and other well known botanists of the period, had been members. Woods brought to the Division of Vegetable Pathology the high calibre of studious accomplishment of this organization and the strong education in scientific agriculture provided by Bessey.

Thus, with Waite as an authority on pear blight, Smith an

⁷⁵ *Rep't. of the State Hort. Assoc. of Penn'a for 1893: 42-49; 54-58.*

⁷⁶ L. R. Jones, *Jour. Bacteriology* 15(1): 2, Jan. 1928.

authority on peach yellows and bacterial diseases of truck crops, Swingle and Webber authorities on orange blight and other diseases of citrous fruits, Pierce an authority on California vine disease and other maladies more or less confined to the agriculture of the Pacific Coast, Galloway an authority on all of the work, and Woods his new assistant and an authority on mosaic tobacco disease, the Division deserved its quickly won reputation of being one of the foremost organizations devoted to the study of plant diseases in the world. The work was producing not only valuable results for science but also farming practice.

Smith immediately recognized Woods's abilities as a scholarly plant scientist. His fifteen-minute address of April 21, 1894, before the Biological Society of Washington on "The Calorific Effect of Light on Plants" so stimulated Smith that he wrote of it to Spalding who found Woods's work "most interesting and suggestive," and added at the end of his criticism, "Mr. Woods has done an admirable service in showing the insufficiency of the traditional 'simple experiment.' The next thing is to turn on the electric light and see what light *without heat* will do. Perhaps it has been done. I do not recall a report of it." Mention has been made of the use of hydrocyanic acid gas as a fumigant against certain scales of orange trees in California. Woods, collaborating with P. H. Dorsett, later would extend this treatment against scale insects and aphides infesting hot-house plants, and the treatment was to become standard practice in the control of nursery stock diseases.⁷⁷ In 1894⁷⁸ a preliminary notice was published by Webber of the discovery in Florida of a fungus parasitic on the white fly, an insect which infested orange trees and excreted a honeydew which nourished the fungus of "sooty mold." This was an important finding in line with a principle of investigation being applied by entomologists and pathologists, and which Smith in part described by saying:

Riley and others conceived the idea that the best method of controlling certain scales would be by multiplying their insect parasites, and the threatened destruction of the orange orchards of California by the cottony cushion scale was avoided in this way, viz., by the introduction of a

⁷⁷ Plant pathology: a retrospect and prospect, 608, 1902.

⁷⁸ H. J. Webber, Preliminary notice of a fungous parasite on *Aleyrodes Citri* R. & H., *Jour. Mycology* 7(4): 363, Aug. 15, 1894.

lady-beetle from Australia. Giard, Snow, Forbes and others have experimented with certain fungous parasites of crop-destroying insects, hoping to spread epidemics among them, but thus far (1902) with only partial success. The dreaded San José scale can now be held in check in this country by insecticidal sprays.

Many disease-prevention principles were being invented or developed. Smith mentioned others: one was the later discovery by Halsted, and others, that heavy liming of the soil may partly prevent club root of cabbage. Another was a hybridization technique. After the manner that Pierce had partly solved the California vine disease problem by bringing in healthy cuttings from disease-free regions, the Dutch in Java some years later considerably checked the spread of Sereh disease of sugar cane by importing healthy cuttings from the hills.

During Smith's lifetime, the cause of the Sereh disease remained in doubt. But a control method was evolved which he regarded as a "great triumph!" He said⁷⁹ of this: "In 1923 Miss G. Wilbrink in Java demonstrated that sugar-cane cuttings could be freed from Sereh by exposure to warm water (30 minutes at 45°C, followed by 30 minutes at 50° to 53°C). From Sereh-diseased canes treated in this manner healthy plants can be grown."

One of Smith's main contributions had been his development of a laboratory of plant pathology. His own work exemplified a high level of skill and efficiency and in it he perfected many important techniques of investigation. In 1895 a convention of American bacteriologists was held in New York City and, in the course of its proceedings, a committee was named to promulgate a program for the proper study of bacteria. In 1891 Paul Sorauer, as editor, had begun to publish his *Zeitschrift für Pflanzenkrankheiten* and, among its pages, occasionally appeared articles on bacteria. In 1892, in Edinburgh and London, *The Journal of Pathology and Bacteriology* had been started. America in 1896, through the editorship of Dr. W. H. Welch, was soon to have *The Journal of Experimental Medicine*. Furthermore, in October 1895, the *Journal of the American Public Health Association* published several valuable papers on bacteriology. An opportunity loomed to fit plant bacteriology into the advancements being made in animal pathology, and of this Smith was not unaware.

⁷⁹ Fifty years of pathology, *op. cit.*, 37.

CHAPTER VII

RECOGNITION OF THE SCIENCE OF PLANT BACTERIOLOGY IN AMERICA

ON JUNE 22, 1895, Smith sent to Farlow a reprint of his recently published paper on *Bacillus tracheiphilus*. "It is the first plant disease I've ever been able to work out satisfactorily," he commented, "the yellows having thus far baffled me, although I am not yet ready to give up on that, but only resting long enough to get a fresh start."

During this year he began to consult Farlow on the proper classification, and points of his culture study, of the watermelon fungus, *Neocosmospora*. Almost two years of "very hard work unravelling its life history" had not solved all of his problems, although by January 6, 1896, he told Farlow in another letter:

I have now worked out the whole life history, and a good deal of general interest concerning the physiology of the fungus, and if I could only settle one way or other whether the three forms (on cotton, cowpeas, and watermelon) are identical or different, I should be ready to throw my two years notes into shape for publication. Here the investigation lags.

Smith continued his cross inoculation experiments during 1896 and 1897; and even in 1899 when his bulletin¹ on the "Wilt Disease of Cotton, Watermelon, and Cowpea" was published, he did not regard his investigation as completed. He published his "main facts," and expected to extend his ascospore inoculations. But his last publishing on this subject took place in 1907² and represented not his own work but that of a colleague, William A. Orton.

Early in 1896 Farlow congratulated Smith for his "excellent reviews" of botanical literature in the *American Naturalist*. In 1895 Dr. Edward D. Cope, science editor of the *Naturalist*, had given Smith "exclusive direction" of a department of vegetable physiology to which he added pathology and morphology.

Smith told Farlow there were "very few men in the country"

¹ 17, Div. Veg. Phys. and Path., U. S. Dept't of Agriculture.

² E. F. Smith, The parasitism of *Neocosmospora*—inference versus fact, *Science*, n. s. 26 (663): 347-349, Sept. 13, 1907.

from whom he would rather have "encouraging words" than from him, since, like his "old teacher and friend Spalding," Farlow knew "good from bad" work and did not "waste breath on mediocrity. Frankly," wrote Smith, "I wish there were more like you. There would be less slipshod, disgraceful work done." Professors of botany were wanting in this country, especially those who possessed "a proper knowledge of the subject" and who "instilled into their students a love of truth for its own sake, and an adequate conception of the dishonesty [in] doing any piece of scientific work in a hasty, crude way, or in any way but thoroughly." In fact, this had motivated his acceptance of Cope's offer to edit a department of vegetable physiology and pathology for the *American Naturalist*:

it was partly to show the young men of the country (such as read only English) how botany [is] studied abroad that I began the series of abstracts in the *Am[erican] Naturalist*. It is my intention to continue these, noticing generally only what I consider the better class of papers, and occasionally mentioning an American publication.

Farlow agreed the present status of botany in America was "unsatisfactory." But he and Smith took pleasure in the promise of American students abroad. Some, like Farlow, D. H. Campbell, B. L. Robinson, W. R. Dudley, W. F. Ganong, Humphrey and perhaps others, had returned from their first studies there. By 1895, according to a letter to Farlow from H. M. Richards studying at Leipzig, there were "at least *nine* Americans studying botany" at Leipzig, Bonn, or some other university research center. Farlow wrote Smith,

The advanced botanical students in German laboratories are nearly all foreigners. The young Americans now abroad will find it hard work to get places when they come back. They will however come back well trained although not always in fields where there is a demand here. They will, at least, be able to appreciate real work and will form a nucleus for a higher group of workers than we now have. In fact, the standard of work is beginning to rise already but for years to come we must expect more of what we are now having.

Smith's important study, "The Path of the Water Current in Cucumber Plants"³ had been undertaken "to verify some of

³ *American Naturalist* 30: 372-378, May 1896; 451-457, June 1896; 554-562, July 1896.

Strasburger's statements in his book *Ueber den Bau und die Verrichtungen der Leitungsbahnen in den Pflanzen*, and partly to determine, as accurately as possible, the path of the water current in Cucurbitaceous stems, subject to the attack of *Bacillus tracheiphilus*." The year before, from work of several Europeans including Gonnerman, Smith had prepared another principal article in the nature of a review, on "Root tubercles of Leguminosae."⁴

Between 1892 and 1895 he had selected several important research studies for reviews: among them, Dr. Alfred Fischer's work on the "Phycomycetes" from Rabenhorst's Kryptogamen flora⁵ and a later paper by the same scholar on the physiology and morphology of the flagella, bacterial classification, and plasmolysis of bacteria.⁶ In 1893 he reviewed Dr. Wilhelm Zopf's *Die Pilze in morphologischer, physiologischer, biologischer und systematischer Beziehung*, for the general student "the best handbook yet published," and Dr. Oscar Kirchner's *Die Krankheiten und Beschädigungen unserer landwirtschaftlichen Kulturpflanzen*, a practical, useful book for farmers and gardeners, "unique in the literature of plant diseases."⁷ H. Marshall Ward's "Diseases of conifers" and his study of the action of light on bacteria were reviewed in 1894-1895.⁸ Experimental researches done in German laboratories by W. Rothert and Rudolph Aderhold were studied and written up in 1895.⁹ The Dutchman Beijerinck's method of detecting the enzym glukase by auxanographic methods and T. W. Engelmann's results as to "Demonstration of photosyntax by bacteria,"¹⁰ were described the same year. June 22, 1895, Smith asked Farlow, "Have you seen Woronin's new paper on *Sclerotinia padi* and *S. aucupariae*? Fine," he observed, "I like all his work," and then followed Smith's review, "Woronin on *Sclerotinia*."¹¹ Smith recommended for reading, among much

⁴ *Amer. Nat.* 29: 893-903, Oct. 1895.

⁵ *Jour. Mycology* 7(2): 135-140, Mar. 1892.

⁶ *Amer. Nat.* 29: 847-851, Sept. 1895. Fischer's name given as "Albert" in the review. Smith, however, corrected this later. See *Bact. in Rel. to Pl. Dis.* 1: 264.

⁷ *Jour. Mycology* 7(3): 282-283, May 1893.

⁸ *Jour. Mycology* 7(4): 396-398, Aug. 1895; *Amer. Nat.* 29: 671-674, July 1895.

⁹ Ueber das Schicksal der Cilien bei den Zoosporen der Phycomyceten (W. Rothert). *Amer. Nat.* 29: 583, June 1895; Rothert on heliotropism, *Amer. Nat.* 29: 585, June 1895; Perithecial stage of the apple-scab fungus (Aderhold) *Amer. Nat.* 29: 583-584, June 1895.

¹⁰ *Amer. Nat.* 29: 750-753, Aug. 1895.

¹¹ *Amer. Nat.* 29: 749-750, Aug. 1895.

else, F. von Tavel's *Vergleichende Morphologie der Pilze*.¹² Other works by Hermann Vöchting, G. Haberlandt, H. Graf zu Solms-Laubach, Louis Mangin, Treub and Nawaschin, E. Kroeber, Bokorny, and other Europeans were included in reviews. A few works by Americans were also chosen, including that of Professor J. M. Macfarlane on paraheliotropism and Thomas B. Osborne's study of proteids of the rye kernel and barley and the chemical nature of diastase.¹³

August 23, 1895, Pammel of Iowa Agricultural College congratulated Smith:

I have just received your interesting paper on *Bacillus tracheiphilus* . . . in Centralbl[att] f. Bakt[eriology] u. Parasitenk[unde]. It is an excellent paper. It seems to me you have adopted the proper way of describing species of bacteria. No one will be able to recognize many species of bacteria described by authors. . . . I have sent a paper on certain gas producing species to [the] St. Louis Academy [of Sciences] which will probably appear sometime this fall. In this paper several species are fully described. . . .

That year, in bulletin 27 of the Iowa experiment station, Pammel described the "Bacteriologists of rutabaga" and its causal organism, *Bacillus campestris*. In May, under this title, he published on it in the *American Monthly Microscopical Journal*.¹⁴ During the summer Pammel had "very little opportunity . . . to do Bacteriological work." He promised to send a copy or abstract of his paper.

Smith in August 1896 began to publish in the *American Naturalist* his critical evaluations of the present state of knowledge of bacterial diseases of plants.¹⁵ Pammel on December 25, after reading the first three or four of these, sent another letter to Smith. First, he explained that he had lacked for many months a place to carry forward his investigations in bacteriology and could not furnish him cultures of *Bacillus campestris*. He thought Smith's "scheme outlined in *American Naturalist* is a most excellent one, and should be followed. This has not been done by many of us," he added, "but what can be expected in

¹² *Jour. Mycology* 7(4): 389-396, Aug. 1894.

¹³ *Amer. Nat.* 29: 1100-1103 (Macfarlane), Dec. 1895; "Separation of enzymes," *Amer. Nat.* 29: 586, June 1895.

¹⁴ 16: 145 ff.

¹⁵ The bacterial diseases of plants: a critical review of the present state of our knowledge, *Amer. Nat.* 30 (356): 627 (first of series), Aug. 1896.

the early days of the science. We should now start out in the right way." He liked Smith's treatment of "physiological characters." The reason why he could not furnish Smith with cultures was that

everything . . . in the way of cultures [had been] lost last winter. I was moving about from place to place last year. First in with the Agr[icultura]l Chemistry dep[artmen]t, then into an old building and finally into my present quarters. I lost my *Bacillus aromaticus* as well. Russell wrote me a few weeks ago for cultures but I was compelled to make the same statement. . . . [N]o one regrets this more than I do. I don't remember just the number of gelatin cultures . . . I made a half dozen or so cultures in gelatin but I cannot give you the exact number. I did note this, a slight liquefaction which I attributed to a foreign organism. I worked with neutral or slightly alkaline media. I am inclined to think that many of the species are variable when we come to consider physiological characters. . . . I have some additional notes on other gas species which I may send you in the course of a few weeks. . . . Are you making a collection of fungi of your own? I shall in the course of a few months distribute a first century of Iowa fungi and shall be glad to send you a set.

The black rot of crucifers was the third bacterial plant disease which Smith studied. His work on this did not get fully under way until 1897, although his preliminary investigations began soon after reading Pammel's first papers. Halsted of Rutgers College and the New Jersey Agricultural Experiment Station had congratulated him in 1895 on his investigations of his first bacterial plant disease studied: "It delights me," he wrote, "to get your paper upon *Bacillus tracheiphilus*. . . . I am glad you have gone to the bottom of the trouble and can show so clearly that the *wilt* is caused by a microorganism." When Pammel wrote on Christmas Day, 1896, he had just received Smith's bulletin 12 on *Bacillus solanacearum*, cause of a bacterial disease of tomato, egg plant, and Irish potato, and the second such disease studied by Smith.

American botanical work, except in phanerogamic taxonomy and to some extent in cryptogamic botany, was not fully recognized in many centers of European scholarship. Inferior quality of some American work partly explained this, but not altogether since some very creditable work had been done here. Highly reputable research in plant physiology and pathology still passed practically unnoticed abroad. So valuable a bulletin as Waite's "The Pol-

lication of Pear Flowers" (1894) was mentioned in an important German journal.¹⁶ But the reviewer had not studied the bulletin; in fact, had learned of it only from a scant notice in *Gardeners' Chronicle*. In 1894 Galloway had tried to bring American work more to the attention of European scholars. He arranged with editors of Just's *Jahresberichte*, *Hedwigia*, and *Botanische Centralblatt* to furnish abstracts of articles by Division members, and placed Joseph F. James in charge of the abstracting, instructing him to prepare his work in conjunction with the authors of the articles. Scientists in America as well as in Europe had complained that they had not received regularly or at all the publications of the Division. Smith told Galloway:

The necessity for a wide distribution of our publications among scientific workers in the same field can not be gainsaid. Science knows no country or nation, but is broad as the world, and if we are to keep abreast of modern scientific work in the line of plant diseases we must be in constant touch and communication with workers in this field in all parts of the world, and the only possible way by which this can be done is to send our own publications promptly to these workers wherever they are in the world so that we may receive in exchange their own publications, many of which are of the utmost importance to our own work, and ultimately to the fruit growing and farming population of the United States. The scientific workers of the world as a rule are very willing to exchange, but they can not be expected to give something for nothing. Neither are we in condition to purchase all of their publications, even if we knew when and where they were published.

Satisfactory intercommunication between research centers of the world was needed in plant pathology and mycology. Fairchild's indices to literature since 1892 had included foreign sources, and, for a few years after 1886, *Annals of Botany*, of which Farlow was American editor, had published valuable botanical bibliographies which included references to American work. Fairchild, before he had gone abroad, had completed extensive work on fungicides including bibliographical references nearly a century old. His total number of references was estimated by Galloway as "something over eighteen hundred."¹⁷ The Botanical Club, or Section G, of the American Association for the Advancement of Science, at its Madison meeting, had appointed a committee

¹⁶ Material taken from a letter by E. F. Smith to Galloway, January 31, 1895.

¹⁷ Letter from Galloway to Halsted, April 6, 1894.

consisting of Halsted, Galloway, Atkinson, and Bessey, to study and report on nomenclature of plant diseases. A preliminary report, with a recommendation that the study be continued, was presented at the Brooklyn meeting of August 1894.¹⁸ Leadership in forwarding this was assumed by Halsted who each year since 1891 had been publishing at the New Jersey experiment station illustrated reports on diseases of plants.¹⁹ The committee continued its work in 1895. But at the Springfield Massachusetts, meeting of Section G, plant physiology was the main topic. Dr. Arthur spoke²⁰ on its status and future in American and European science.

Since the Rochester and Madison meetings, newly promulgated rules of nomenclature in phanerogamic botany and a proposed list of plant names had been under discussion. The favorable action taken toward these rules was regarded by Smith as not final. At his own expense he published a vigorous protest.²¹ It is unnecessary in this book to deal with the major point of this controversy. Suffice it to say that his protest was commended by many of the nation's outstanding botanists. He saw no fundamental need for nomenclatural reforms, however much some changes in rules (priority, for instance) were approvable. In 1894 and 1895 he had discussed questions of nomenclature before the Botanical Seminar and the Biological Society of Washington. In 1894, before the seminar, he had also read a paper on "Brefeld's classification of fungi." He wanted permanence and stability in nomenclature, especially plant names. But, even more, he wanted botanists now to devote their energies to the "thousand and one problems in bio-chemistry, anatomy, morphology, physiology, ecology, cytology and ontology" of the "new botany."²² He knew that to pursue his interest in the "biological side of botany" frequently the correct plant name had to be ascertained. But energies were not conserved for the more important tasks when much time was wasted searching through various classification

¹⁸ *Proc. Amer. Assoc. Adv. Sci.*, 43rd meet., 296-297, 1894.

¹⁹ Fifty years of pathology, *op. cit.*, 21.

²⁰ Development of vegetable physiology, *Proc. Amer. Assoc. Adv. Sci.*, 44th meet., 163-184, 1895.

²¹ The Botanical Club Check List: a protest, privately printed, Washington, D. C., July 22, 1895.

²² *Idem*, 2.

schemes to determine what species was being studied. The "great systematic botanists of the world" had established the current botanical nomenclature. No "new lamps for old ones" were needed. Whatever new interpretations of the existing code were wanted could be made at an International Botanical Congress.

Many letters from leading botanists of various sections of the nation attested to a widespread agreement with the stand upheld by Smith in his protest. Farlow wrote on August 17, 1895, that he had "read with great interest your paper on nomenclature in which you forcibly present the absurdity [of] the claim that the Rochester Code and the List are accepted without question by the majority of American botanists and is approved by European botanists. . . . The policy of ignoring criticism from abroad and opposition at home," he hoped, would "prove a boomerang."

Dr. W. C. Sturgis, mycologist of the Connecticut Agricultural Experiment Station, congratulated Smith upon his "stand upon the nomenclature question. . . . I agree with you thoroughly," he wrote on August 17, "and congratulate practical botanists on at last having the views of the majority on this matter so clearly expressed."

F. C. Newcombe of the University of Michigan read Smith's protest and, for the first time realizing "very clearly that the methods of the [check list] reformers will disturb all of us as well as agriculturists, pharmacists, et al.," hoped that "the battle" would be fairly fought. "That incidental suggestion of yours," he continued,

that another journal might not come amiss, I would like to see pushed. In the whole English language we have no journal like the *Botanische Zeitung* or *B. Centralblatt*. The *Annals* is good for extensive articles, but gives us no reviews. The *Gazette* gives us neither. *Science* does part of the work; but I should like to see a journal combining both extensive articles and numerous reviews. You have observed probably that Americans furnish one-third or more of the copy for the *Annals*. I do not believe in multiplying unnecessarily the journals; but do we not need an American journal of botany? The number of real botanists is increasing in this Country year by year. You can count from 12 to 20 now in this Country who will be sure to publish year after year. I think I said the same to you last summer. I would like to see Farlow take hold of the matter and put some of his cash into it for a few years.

This letter was written on August 18, 1895. Approximately two

decades later, *The American Journal of Botany*, official publication of the Botanical Society of America, and published by the Brooklyn Botanic Garden with Newcombe as its editor-in-chief, appeared on the American scientific scene.

In American botany, the year 1895 was prophetic in another particular. December 30 of that year, J. E. Humphrey, now of Johns Hopkins University, soon to be promoted, and as part of his work to deliver a course of eight public lectures on the history and present outlook of Botany, addressed a communication to Smith:

At a meeting of botanists present at the Philadelphia meetings²³ a committee of five, comprising E. F. Smith, L. H. Bailey, [W. F.] Ganong, Bessey, and myself, was chosen to canvass the botanists of the country, especially of the east, for an expression as to whether they favor and will actively cooperate in the organization of a botanical society to meet with the several other scientific societies which meet during Christmas week. We are also charged with getting suggestions as to the scope of such a society, and, if the response is sufficiently encouraging, to make definite recommendations for organization to the meeting next year, and to prepare a program for that meeting. I hope you are willing to act as a member of the committee and that you will agree to canvass the Washington botanists thoroughly on the question.

At this time neither plant physiology nor plant morphology were mentioned in the wording of this letter or in a printed form, prepared June 1, 1896, by the committee, and circulated among botanists. However, by letter of January 3, 1896, Humphrey advised Smith that if the latter had been present at Philadelphia he "would have seen that the whole sentiment of the botanical meeting was morpho-physiological." The prominent participants in the meeting had been W. P. Wilson, Macfarlane, Ganong, Miss Gregory, and H. W. Conn, who to Humphrey represented bacteriology. Humphrey had begun to attend meetings of the Washington scientific societies. In the autumn of 1896 he told Smith that botanical work at the university had begun "with distinct gains." He and Smith, however, did not agree wholly as to the scope and purposes of the new society, although their differences were chiefly whether the research fields of plant physiology and morphology were sufficiently developed to warrant a new society, and not whether each was needed. Plant physiology,

²³ Of the American Society of Naturalists.

as a fertile developmental branch of botany, was proving its strength more with every year. Prolific in potentialities, objective physiology was to widen the botanical research orbit to advance the plant sciences both purely and practically and create whole new interpretations from the older fabric, whole new branches of science in organized forms—ecology, genetics, a “new physiology,” a “new pathology,” an “experimental taxonomy,” etc. Years would go into the making. But the envisioned future was in the minds of many of the most alert scientists. We do not know what were Smith’s ideas of the new botanical society. But Humphrey’s reply on December 20, 1896, was:

As to your scheme for a society, I think it quite impossible. Very many of the men who have stayed out of the Bot[anical] Soc[iety of] America have done so, on account of the size of the fees; and I would never join a society of that sort, no matter how able I might be to do so, financially. As to organs, we have too many already. I think it will not be many years before we can worthily support a journal of the character of the *Annals of Botany*, without editorials or reviews, filled simply with good papers. But we cannot do it yet, and we want nothing else. I hope to talk it over with you, sometime. But there is no hurry. There will not be more than half a dozen botanists at Boston, and nothing is likely to be done.

December 30, 1896, botanists present at the Boston meeting of the American Society of Naturalists and some residing in Cambridge gathered in the Cryptogamic Laboratory of Harvard University and heard the report of the committee of which Humphrey was chairman. Those present were Farlow, Wilson, Macfarlane, Emily Gregory, Benjamin Lincoln Robinson of the Gray Herbarium, Thaxter, H. M. Richards, J. M. Greenman, later of the Missouri Botanical Garden, and Ganong, appointed secretary of a “Committee on the organization of a Society for Vegetable Morphology and Physiology to meet annually with the American Society of Naturalists.” It was understood and agreed that

The new Society should conflict as little as possible with those already in existence. There is no need for a new Society of general scope. Since the interests of those most concerned in the movement are mainly in Morphology and Physiology, the new Society should be limited in its scope to them and to the broader aspects of Systematic Botany, while Taxonomy and its dependent subjects, so amply provided for in the existent societies, should be excluded.

February 1897, eleven other botanists were invited to join and cooperate with the committee, each of whom accepted: George Lincoln Goodale, Clara E. Cummings, D. P. Penhallow, G. E. Stone, W. C. Sturgis, J. E. Humphery, E. A. Burt, Galloway, Atkinson, Bailey, and Smith. December 27, 1897, the committee met at Ithaca, New York, where was scheduled to take place the sixteenth annual meeting of the American Society of Naturalists, in conjunction with meetings of the Association of American Anatomists, the Association for Botanical Morphology and Physiology, the American Morphological Society, the American Physiological Society, the American Psychological Association, and Section H, or Anthropology, of the American Association for the Advancement of Science. The committee resolved to "constitute itself a society for the promotion of research in Plant Morphology and Physiology, with the general understanding that it shall meet with the American Society of Naturalists," and "that its name shall be *Society for Plant Morphology and Physiology*."

About thirty botanists were present at the Society's first meeting and an equal number of papers was listed on the program.²⁴ Smith published abstracts of several of the papers: his own "Additional Notes on the Bacterial Brown Rot of Cabbages" and "Occurrence of Kramer's Bacterial Disease on Sugar Beets in the United States," Albert F. Woods' "Variable Reaction of Plants and Animals to Hydrocyanic Acid," one by Webber, two by Swingle, one by Benjamin Minge Duggar, and one by Harper, among them. Botanists elected to membership included Spalding, Webber, Swingle, W. W. Rowlee, J. W. Harshberger, Fairchild, Harper, Woods, A. J. Pieters, a graduate of the University of Michigan and whom Newcombe had recommended for employment by the Division of Vegetable Physiology and Pathology, G. H. Hicks, H. C. Porter, Harriet L. Merrow, and Theo. Holm. Farlow was elected the Society's first president; Macfarlane and Atkinson, vice presidents; Ganong, secretary-treasurer; and Smith was named chairman of the committee on membership for a period of three years. Serving on the committee with him were Miss Cummings, Fairchild, Spalding, and Bailey. In the forum discussion of the American Society of Naturalists on "The Biological

²⁴ Erwin F. Smith, The first meeting of the Society for Plant Morphology and Physiology, *Amer. Naturalist* 32 (374): 96-110, Feb. 1898.

Problems of To-day," Dr. Trelease of the Missouri Botanical Garden represented botany.

Noticeable was the number of members in the Society from the Division of Vegetable Physiology and Pathology. For several years research in the physiology, as well as pathology, of plants had been gaining. On June 21, 1895, Halsted told Galloway he was "enjoying very much the growth your Division is making and in particular along the physiological lines. I," Halsted said, "am getting interested in questions of soil water as related to health of plants." When Smith had accepted editorship of a department of vegetable physiology and pathology in the *American Naturalist*, Spalding in London on April 25, 1895, had written, "I think you are right in feeling that one of the greatest services needed at present is faithful and prompt abstracting and reporting of monographs etc. where American readers can get their substance. I have for years thought of this as an important field not properly worked in as yet."

American students were not always pleased with the quality of their European instruction. With no known exceptions, students from the United States held in high esteem the laboratory and classroom instruction at the Botanical Institute in Bonn under Eduard Strasburger. The privilege of study under Wilhelm Pfeffer at Leipzig was also highly valued. But the great German authorities, most of them, adhered tenaciously to their traditional disbelief in bacterial diseases of plants. Oskar Brefeld was one. Denial of this class of diseases tempered considerably the American students' confidence in the masters of fungology. Spalding, while liking Brefeld as an individual, criticised his system as too simple, his scheme of relationships of the fungi as laid in conceptions "purely anatomical, and," wrote Spalding to Smith on June 1, 1895, "from all he has said in his lectures it would be difficult to assume that he really comprehends what phylogenetic relationship means." Fairchild, Harper, and Spalding had

been waiting patiently to see whether Brefeld would construct a genealogical tree and at least put on the terminal branches. This he ought to be able to do if his system is what he believes it to be. But if he tried to, I am afraid he would be sadly puzzled over some of those cross lines. . . . Brefeld evidently knows a great deal less about the descent of fungi than he thinks he does. . . . "Our little systems have their day, They have their day and cease to be."

Spalding also criticised Brefeld's culture methods as "awfully crude in the light of modern methods." Contrariwise, Swingle at Bonn on December 26, 1895, revealed to Smith that "Strasburger wears well—he is the most inspiring man I have ever seen. He doesn't waste his time on methods or material—all he cares about are results."

Spalding returned to the Botanical Laboratory of the University of Michigan and by January 1896 Smith was gratified to learn that the department was functioning "on a solid foundation." He contemplated at this time²⁵ associating himself with some university to give as much of his time as possible to develop research work and study in bacterial diseases of plants. He would like to have gone to Cornell University since there his two close friends, Liberty Hyde Bailey and Veranus A. Moore, were located, the former in plant work and agriculture, and the latter still studying diseases of animals as a member of the faculty of the New York State College of Veterinary Science. Formal negotiations were never commenced, although Smith consulted both Bailey and Moore, and both men wanted Smith at Cornell. Bailey was still professor of horticulture but facilities of his department were not sufficient, especially in view of the fact that the type of work which Smith had in mind was regarded there as botanical, not horticultural. Not yet had the New York State College of Agriculture been founded. It is not likely that Smith turned to any other university, although in January, Spalding had volunteered to say that with about one hundred literary students and twenty-five to forty pharmacy students his department was building up its work in anatomy, physiology, and the laboratory side of the science. The laboratory was better equipped and the teaching force was ample. Newcombe's excellent work in histology and physiology was attracting advanced students. They were "prepared to do good work in histology, physiology, morphology of at least certain groups of cryptogams." Funds were not sufficient. In fact, Spalding was financing some items of expense. An offensive was on with Professor Reighard to secure a new building. Additions were being made to the herbarium. Smith evidently had urged Spalding to employ Charles F. Wheeler. Smith's and Wheeler's friendship had never wavered, and consis-

²⁵ Letters, Smith to Bailey and to Moore, and vice versa, December 1896.

tently each had sought to aid the other. Not in the least had Spalding's admiration for Smith's accomplishments diminished, either. On August 7, 1896, he again praised Smith's work:

I most heartily congratulate you on the actual results you have been able to attain. I set a high value on these accounts of your work, knowing as I do that every bit of work you do is done with downright honesty and done to last. . . . When your bacterial and physiological studies keep coming to me, and [Douglas Houghton] Campbell sends me his work on Archegoniata, and I call to mind the excellent work of still others who used to have a place in the Michigan laboratory, I wonder what I have been doing all these years! I don't utterly give up hope of accomplishing something yet, but you have conducted investigations enough to form some conception of how rapidly such work would progress if you gave three-fourths of your time and nine-tenths of your energy to teaching, organization, university committees, and the administrative duties inseparable from the direction of a laboratory that has had to be made almost *de novo*. At present I must "wait upon teaching". . . .

Never once, however, during this or any year hereafter does it appear certain that Smith consulted Spalding with reference to employment for himself. Smith must have believed that his work could be best furthered in a university where agriculture and veterinary science, and medicine too, were taught, and experimental laboratories and research in each provided.

March 11, 1896, Halsted communicated to Galloway news, recently received from A. B. Seymour of Harvard, that his and Farlow's "Index to the literature of North American Fungi" was complete, containing something like 100,000 references. A movement to have the work printed was now to be set in motion, indicating the progress thus far made in the study of fungi. This year Flügge's *Microorganismen mit besonderen Berücksichtigung der Aetiologie der infektiöskrankheiten* in third edition, a work of several authors, and describing 161 kinds of bacteria, appeared. This showed, Smith later said,²⁶

to what extent pathologists and bacteriologists had already multiplied and become interested in culture methods and classifications. Already 22 groups of the rod-shaped bacteria were recognized, of which at least 16 groups contained parasites; not to mention Coccaceae and Spirillaceae, in which groups also parasites had been found; not to mention also Protozoans. The latter were divided into 4 main groups each containing parasites. Nothing,

²⁶ Fifty years of pathology, *op. cit.*, 26.

perhaps, in brief, gives a better idea of the progress of pathology . . . than an enumeration of the then-recognized classes of the rod-shaped bacteria. Flügge's groups of these bacteria are named from prominent organisms in each group and those containing parasites are as follows: The hay-bacillus group, the anthrax group, the oedema bacillus group, the symptomatic anthrax group, the tetanus group, the proteus group, the fluorescent bacilli, the water bacilli (here plant parasites), the *Bacillus aërogenes* group, the *Bacillus coli* and typhoid group, the hemorrhagic septicaemia group (here mouse typhus), the *Bacillus sputigenes* group (here mouse plague), the influenza bacillus group, the swine measles group, the glanders group, the diphtheria group, and the tubercle group. At the end Flügge has a catch-all for diseases of uncertain origin, variously ascribed to bacteria, but all imperfectly understood and requiring further investigation: such diseases as rabies, smallpox, measles, typhus fever, whooping cough, yellow fever, beri-beri, and trachoma.

As yet, no book had been written on bacteria which affected plants. In a prepared "Memorandum of things to do," of date September 12, 1895, Smith listed as the second item, "Write a book on Bacterial diseases of Plants." He began this by putting in order for the *American Naturalist* his historic critical reviews of the present state of knowledge concerning, first, diseases of clearly established bacterial origin; second, diseases which appeared to be constantly associated with bacteria and which were believed probably due to some specific organism, but full proof of which had not been furnished; third, diseases said to be more or less closely associated with the presence of bacteria and ascribed thereto, but in which little or no proof had been brought forward to establish the causal relation; and, fourth, communicable diseases which had been ascribed to bacteria but associated with which no organism had been found and which were believed probably of non-bacterial nature.²⁷ Arranged according to hosts and based on materials supplied so far as possible by the most reputable authorities who themselves had studied and first published on the diseases—for example, Wakker in yellow disease (1883) of hyacinth—these studies, preceded in America only by Dr. Russell's thesis on bacteria in relation to vegetable tissue, represented the first real effort by an American scientist to evaluate the existing knowledge on plant diseases caused by bacteria.

²⁷ The bacterial diseases of plants: a critical review of the present state of our knowledge, *Amer. Naturalist* 30 (356): 642, Aug. 1896.

"The great development of modern bacteriology," Smith affirmed,²⁸

is attributable largely to the discovery that human diseases are due to these organisms, and to its consequent alliance with medicine, but there is no reason why the same rigid scrutiny of methods and sharp calling in question of statements which have led to such brilliant results in animal pathology in recent years should not be applied in the same way to vegetable pathology. Accurate experimentation and trustworthy results are from a purely scientific standpoint quite as desirable in one field as in the other.

This statement in considerable part became Smith's doctrine. His own work began to prove what he thought possible for a science of bacteriology in plants. L. R. Jones, receiving his account of "A Bacterial Disease of the Tomato, Eggplant, and Irish Potato (*Bacillus solanacearum* new species),"²⁹ wrote:³⁰

It stands unrivalled as a thorough piece of work on bacterial disease of plant. This together with your contributions in Am[erican] Nat[uralist] should serve two purposes—first, to furnish an example and standard for us "yankees" to work to—second, to secure proper recognition of American work from Europeans.

In 1897 Smith read before the Biological Society of Washington a paper on "A Bacterial Disease of Cruciferous Plants."³¹ But his main publications on *Pseudomonas campestris* were in the *Centralblatt für Bakteriologie* II³² that year and, after reexamining and confirming the facts of this paper, in 1898 another paper in the *Zeitschrift für Pflanzenkrankheiten*. Early in 1897 Dr. Paul Sorauer of Berlin wrote for his reprints on plant bacterial diseases. His work was recognized in America also. Among many others, Dr. Henry M. Hurd of Johns Hopkins Hospital praised the depth to which his study had gone.³³

In 1893 Dr. Russell had accepted a call to return to his alma mater, The University of Wisconsin, as assistant professor of bacteriology. By 1897 he had been made full professor of the subject, and was in 1903 to be constituted director of the Wisconsin State Hygienic Laboratory and in 1907 dean of the college

²⁸ *Idem*, 635.

²⁹ Bull. 12, U. S. D. A. Div. Veg. Phys. and Path., Dec. 19, 1896.

³⁰ Letter, Jones to Smith, February 20, 1897.

³¹ *Science* n. s., 5: 963, June 18, 1897.

³² *Bacteria in relation to plant diseases* II: 333, bibliog.

³³ Letter, Hurd to Smith, November 13, 1897.

of agriculture and director of the agricultural experiment station of Wisconsin. *Experiment Station Record*³⁴ in 1894 formally noticed that he had

entered upon his work at the station in September and [would] devote himself almost wholly to the study of the fermentations of milk and its products. By combining his efforts with those of Dr. Babcock the station hope[d] to contribute materially to the knowledge of dairying. With the coming of Dr. Russell the university offer[ed] an advanced course of dairy instruction. . . .

Dairy bacteriology, therefore, became Russell's specialty. In 1894-1905 he published *Outlines of Dairy Bacteriology* and in 1898 *Agricultural Bacteriology*. Some further work in plant diseases, however, was also done, notably that in "A Bacterial Rot of Cabbage and Allied Plants," with Harry Alexis Harding who graduated in science from the university in 1896 and obtained while serving as a fellow 1897-1898 his degree of master of science in 1898. Harding studied for a while in Europe and at Massachusetts Institute of Technology, became in 1899 bacteriologist of the New York Agricultural Experiment Station at Geneva, earned his doctorate of philosophy degree at Cornell University in 1910, and in 1913 was appointed professor of dairy bacteriology at the University of Illinois.

In 1895, before Section G at the Springfield, Massachusetts, meeting of the American Association for the Advancement of Science, Russell's preliminary notice of "A leaf-rot of cabbages"³⁵ was read. Smith was in South Carolina when this meeting took place. His two papers on "The Watermelon Wilt and other Wilt Diseases due to *Fusarium*" and "The Southern Tomato Blight"³⁰ were read for him to Section G by Albert F. Woods. The cabbage leaf-rot had been called to Russell's attention in July 1895. Pammel three years before had discovered the organism which caused this disease. Russell, however, believed that he had established its pathogenicity only as to rutabagas and turnips and had not connected it with other species of Brassica, more specifically, the cabbage and cauliflower rot. Since the disease had been found

³⁴ 5 (4): 443, 1894.

³⁵ *Proc. Am. Assoc. Adv. Sci.* 44: 193, 1895. See also, H. L. Russell, A bacterial disease of cabbage and allied plants, *Proc. 11th Amer. Coll. and Exp't Sta.*, 86, prepared for meeting of July 1897.

³⁰ *Proc. Am. Assoc. Adv. Sci.* 44: 190 f., 1895.

in Wisconsin, Russell began a study of several years which finally culminated in his bulletin 65, "A Bacterial Rot of Cabbage and Allied Plants," published in 1898 by the Wisconsin Agricultural Experimental Station. In this he and Harding described the organism and its morphological, physiological, and cultural characters,³⁷ their experimental research, and their conclusions as to the disease, its sources of infection, treatment, etc.

We shall consider Smith's researches on "*Pseudomonas campestris* (Pammel)" later. He evidently began his work after Russell and Pammel had each started theirs. Samples of diseased leaves were sent at about the same time by the same man to both Smith and Russell;³⁸ and Smith published his paper³⁹ on the subject before Russell's and Harding's bulletin appeared. He believed this his right. As his laboratory investigations of bacterial plant diseases went forward, he studied the new diseases called to his attention and restudied and verified the work of others. For instance, he "verified most of the statements of T. J. Burrill, J. C. Arthur, Merton B. Waite and others, respecting *Bacillus amylovorus* (Burrill) Trevisan, the cause of fire blight of apples, pears, quinces and other pome fruits."⁴⁰ As to his study of *Bacterium campestre* (Pammel) Smith said: "Pammel's work on turnips was extended to cabbages, cauliflowers, kale, rape, mustard, etc., with many additions and much field, laboratory and hothouse work." He proved infection through the plant's water-pores and described the causal organism of black rot of crucifers.⁴¹ Director S. M. Tracy of the Mississippi Agricultural Experiment Station suggested that he work out the life history of the organism, *Bacillus solanacearum*,⁴² the cause of a disease in tomato, eggplant, and Irish potato, and differing from *Bacillus tracheiphilus*.⁴³

³⁷ Wis. Bull. 65: 10 ff., Feb. 1898.

³⁸ *Idem*, 11. Dr. Davis's letter, hereafter quoted, to Smith was dated September 30, 1896. In a research memorandum of September 19, 1896, Smith set forth another and earlier letter from Dr. Davis dated September 16.

³⁹ Erwin F. Smith, *Pseudomonas campestris* (Pammel). The cause of a brown rot in cruciferous plants, *Centralbl. f. Bakt.* II, Abt. 3 (11/12): 284-291, July 7, 1897; (15/16): 408-415, Aug. 8; (17/18): 478-486, Sept. 10.

⁴⁰ Synopsis of researches, *op. cit.*, 41 f.

⁴¹ *Idem*, 27.

⁴² Erwin F. Smith, A bacterial disease of the tomato, eggplant, and Irish potato, U. S. D. A. Div. Veg. Phys. and Path. Bull. 12: 5 (note 1), 6, 9, 22-23, 25, Dec. 19, 1896. At pp. 10-19, description and study of organism.

⁴³ Bull. 12, *op. cit.*, 25. See also *Garden and Forest*, Jan. 1897 (review).

Burrill in 1890-1891 had published "Preliminary Notes upon the Rotting of Potatoes" ⁴⁴ but, since the "origin of the material [was] not given and the micro-organism [was] not described," Smith was not sure that this disease was the same as the one he studied. ⁴⁵ Halsted, too, during the years 1891-1893 had presented various papers on tomato and potato blights. But, since the distinct disease of the bacterial wilt of cucumbers and cantaloupes appeared confused in these writings and since the results were not based on pure culture experiments and not "very conclusive," ⁴⁶ Smith, in 1895, using diseased tomato plants sent from Mississippi, began "an extensive series of microscopic examinations, artificial cultures, and plant inoculations . . . in the laboratories and greenhouses" of the Department. He isolated *Bacillus solanacearum* but he accredited Halsted with being first to draw attention to the disease of tomato and potato; and evidently he believed Halsted's "cucumber and muskmelon disease . . . entirely different" from the cucurbit wilt due to *Bacillus tracheiphilus*.

In Chapter VI we considered Smith's field studies of 1894 in South Carolina. These, continued in 1895 from July until September on James Island and at Monetta, brought forth his discovery that eggplant, as well as tomato and potato, is subject to the bacterial "wilt" or brown rot of Solanaceae. Later, in 1908, ⁴⁷ he described the disease from tobacco, and he and other workers would find the malady present in many other kinds of plants. His studies of the organism and the various plant genera affected by it would last until 1920 and extend to Euphorbiaceae (*Ricinus*), Onagraceae (*Fuchsia*), Leguminosae (beans and peanuts), and Compositae (*Helianthus*), etc. ⁴⁸

In 1896, furthermore, he successfully infected potato by using the Colorado potato beetle (*Doryphora decemlineata*) ⁴⁹ as an agent of disease-transmission. Thus, in a greenhouse under carefully controlled conditions, he arrived for the second time at proof that insects play an important role in spreading maladies.

⁴⁴ *Proc. 11th Ann. Meet. Soc. Prom. Agric. Sci.*, 21, 22, 1890; also, An additional note on the rot of potatoes, *ibid.*, 29, 1891.

⁴⁵ *Bull. 12, op. cit.*, 1.

⁴⁶ *Bull. 12, op. cit.*, 5, 6.

⁴⁷ The Granville Tobacco Wilt, *Bull. 141 (11)*, *Bur. of Pl. Indus., U. S. D. A.*

⁴⁸ Synopsis of researches, *op. cit.*, 26-27.

⁴⁹ *Leptinotarsa decemlineata*. See Smith's *Intro. to bact. dis. of plants, op. cit.*, 199. Also, Fifty years of pathology, *op. cit.*, 25. *Bull. 12, op. cit.*, 22-23 for an account of his investigations.

In South Carolina also took place his discovery that cowpea is affected by the same fusarium as that which causes watermelon "wilt." There originated his belief that the fungus on melon, cotton, and cowpea is "one thing" and there he discovered the already mentioned⁵⁰ perithecial form which he saw "on agar, infected with the mycelium from a single ascospore."⁵¹ Smith also studied there "southern tomato blight," "potato rot," and "Rolfs' sclerotium disease." Selection experiments for disease resistance were not definitely in his mind at this time. But he closely examined cotton seed-selection as practiced by the planters there and wrote the following memorandum on July 24, 1895:

Great care used in selecting seed. One or two plants are picked out as models, seed saved, and planted next year by itself and third year there is enough to plant many acres. This selection is carried on year after year, and when it is not done cotton is said to deteriorate. Cotton does not follow cotton here, but the fields are allowed to rest a year or are planted to other crops.

When he returned to Washington, he planned to "translate and publish Von Tavel's Pilze,"⁵² something which he never did, and to "write a book on Bacterial diseases of Plants."⁵³ Education in agricultural science was giving more attention to instruction on diseases of plants. A "work on Plant Diseases," with Smith as one of the authors, was evidently announced as in preparation. Professor Kellerman on March 4, 1897, inquired of Smith whether this would be a text book. At present he was giving lectures to his students in the department of botany at the Ohio State University. By 1897 the University of Illinois and Purdue University gave courses⁵⁴ in bacteriology. As early as 1893, as part of a special and winter course in agriculture at the University of Illinois, George P. Clinton had presented a course in "Diseases of

⁵⁰ See Chapter VI, p. 262 of this book.

⁵¹ Letters, Smith to Galloway, September 2, 8, 1895. An earlier letter, August 7, told of planned cross-inoculation experiments, his discovery of the parasite on cowpea, and other points. Smith prepared elaborate memoranda, and also wrote letters to Woods concerning some of his findings.

⁵² See Smith's review of F. von Tavel, *Vergleichende Morphologie der Pilze*, *Journal of Mycology* 7(4): 389-396, Aug. 15, 1894. Smith must have started this translation but in July 1896, Spalding, who had used the book in an advanced class, somewhat discouraged its completion since the subject was vast and the book "a work of moderate dimensions."

⁵³ Quotations from a "Memorandum of things to do," September 12, 1895, prepared by Smith. Points 1 and 2.

⁵⁴ *Botanical Gazette* 23: 73, Jan.-June 1897.

crops—rusts, smuts, mildews, and blight," and Burrill a course in "Vegetable physiology—life, nutrition, growth, and products of plants."⁵⁵ These, and other, similar courses added to plant disease instruction, inaugurated some years earlier by Farlow, Burrill, Bessey, Arthur, Spalding, and others.

Instruction in plant bacteriology still was not academically recognized except in courses on plant diseases, in instances such as Russell's lectures, or as a minor part of courses on animal and medical bacteriology. Examples must have been few, since trained bacteriologists seldom knew about plants and their ills. But a rising new generation of students was beginning to create a demand sufficient for Smith to take cognizance of a need for a book on plant bacterial diseases.

Other brackets of Smith's "Memorandum of things to do" are of interest:

Work out Cotton, Watermelon, Okra, and Cowpea wilt and publish an illustrated bulletin. (Publish a short note on the *Nectriella* this fall).

Work out the Tomato blight and Potato blight and rot problems and publish.

Work up a big paper on Parasitic Alternarias—including mine on melon, Galloway's on potato, Waldron's on squash, the one on tomatoes, on cotton, etc. Go over whole subject, make cross-inoculations, and let some light into this subject.⁵⁶

If any vitality is left, take up again the Peach Yellows problem.

Smith might have carried out all of these resolutions had not his other duties exacted so much study and diligent laboratory investigation. To verify and, where necessary, extend the conclusions of authors who had published on bacterial diseases of plants, imposed a heavy responsibility, especially since these works formed an important basis for his critiques of knowledge on the subject for his series of articles in the *American Naturalist*.

An important literature in laboratory technology had to be read, and its methods applied. In 1895 George W. Fuller's report on standard methods⁵⁷ for making and adjusting beef broth, the

⁵⁵ *Exp. Sta. Record* 4 (5): 451, Dec. 1892.

⁵⁶ Plant pathology: a retrospect and prospect, *op. cit.*, 607. Smith mentioned three species of Alternaria: Galloway's early blight of potatoes, Peglion's muskmelon spot disease, and Dorsett's spot disease of violet (overcome later by breeding a disease resistant variety).

⁵⁷ Agnes J. Quirk and Edna H. Fawcett, Hydrogen-ion concentration vs. titratable acidity in culture mediums, *Jour. Infectious Diseases* 33 (1): 1-59, and reprint. "Historical," at pp. 2-3. Intro. by Smith.

fundamental culture medium of work in bacteriology, provided such an instance. Smith estimated the several papers on bacteriology which appeared in October 1895 in the *Journal of the American Public Health Association* as valuable.⁵⁸ In the introductory article of his reviews of bacterial diseases of plants, he cited as authority⁵⁹ Fuller's paper, "On the proper reaction of nutrient media for bacterial cultivation." In this appeared a table which illustrated "the effect on the growth of water bacteria of comparatively slight changes in the reaction of gelatine," a point having to do with the growth of organisms in acid media. This was pertinent in that "Many bacteria prefer alkaline media, and some are very sensitive to the presence of acids, while a variety of bacteria commonly met with in water will not develop at all if the medium is rendered strongly alkaline. Other organisms grow well in acid media."

In 1896 Smith, with more than two years experience studying plant bacterial diseases in his own laboratory, began full-scale investigation of the cultural characters of several bacteria pathogenic to plants: *Bacterium campestre*, *Bacterium hyacinthi* (Wakker's yellow disease of hyacinths), and *Bacterium phaseoli* (bean blight).⁶⁰ He redescribed the morphology and pathogenic properties of Wakker's organism in publications of 1897⁶¹ and 1901.⁶² Securing repeated infection by pure cultures, he described in 1897 *Bacillus phaseoli* as a new species. Beach of the New York Agricultural Experiment Station and Halsted of the New Jersey station, from microscopic examinations, in 1892, had described the disease as due to bacteria.⁶³ But Smith pointed out that the organism itself

⁵⁸ *Bacteria in relation to plant diseases*, *op. cit.*, bibliography, 1: 203. See especially Fuller's report, *Jour. Public Health Assn.* 20: 381, 1895.

⁵⁹ The bacterial diseases of plants: a critical review etc., *op. cit.*, 1: 639.

⁶⁰ Erwin F. Smith, The cultural characters of *Pseudomonas hyacinthi*, *Ps. campestris*, *Ps. phaseoli*, and *Ps. stewarti*—four one-flagellate yellow bacteria parasitic on plants, Bulletin 28, Div. of Veg. Phys. and Path., U. S. Dep't of Agric, Washington, Gov't Print. Office, letter of transmittal by Woods Jan. 15, p. 2. See also, Synopsis of researches, *op. cit.*, 27, 28.

⁶¹ Wakker's Hyacinth Bacterium, *Proc. Am. Ass'n for the Adv. of Sci.* 46: 274, 1897. In this Smith said that he took up the reexamination in 1896.

⁶² The cultural characters of *Pseudomonas hyacinthi* etc., *op. cit.*, *supra*.

⁶³ Erwin F. Smith, Description of *Bacillus phaseoli* n. sp., with some remarks on related species, *Proc. Am. Asso. for Adv. of Sci.* 46: 288, 1897. Organism under pure cultures had been studied for about 13 months. Beach's and Halsted's 1892 studies were: N. Y. Agr. Exp. Sta. Bull. 48 n. s.: 331; Rept. Bot. Dpt. N. J. Exp. Sta. for 1892: 283-285. Smith cited these in his 1901 bulletin.

had not been described, nor had it been shown by means of pure cultures and inoculations to what organism this disease of beans was due.

Smith's research work in botany and bacteriology was regarded with such high favor by authorities of the New York Agricultural Experiment Station at Geneva that in 1897, recommended by Fairchild to whom the place was first offered, Smith, without receiving a definite offer, was invited to submit credentials. Fairchild refused the position, since, after completing studies in Europe, some plant introduction explorations around the Mediterranean for the Department, and his first world tour, he had obligated himself to complete, as a special agent of the Division of Forestry under Bernhard Eduard Fernow, a bulletin on "Systematic Plant Introduction."⁶⁴ Fairchild and Swingle, who had also returned from studies in Europe, were envisioning, and had begun to plan with other officials of the United States Department of Agriculture, the program of world exploration and systematic plant introduction which was to form one of the most valuable adjuncts of an expanded Department of Agriculture. During years at the turn of the century, this expansion characterized nearly every principal departmental division connected with the scientific study of plants. Advancing the work of plant introduction was to lend impetus to plant breeding for disease resistance, a branch of study for which Smith in 1899 helped to secure special appropriations.

Smith was strongly urged by Veranus A. Moore of the Department of Comparative Pathology and Bacteriology of the New York State Veterinary College at Cornell University to accept the position at Geneva. On October 25, 1897, Moore informed that he had written Director Jordan "laying stress," he said, "on your work in bacterial plant diseases. We hope you will get the place." On the same day Bailey also advised Smith:

I have written to Dr. Jordan and have not been sparing in my encomiums because I have written just what I feel. I sincerely hope that you may find a foothold there. It is very probable that you could not get an outfit to your liking at once, but if you once get established there, you could [develop] a laboratory, which, I think, would be eminently worth your while.

Other scientists, including Charles Wright Dodge of the Depart-

⁶⁴ Bulletin 21, Div. of Forestry, U. S. Dep't of Agric., 24, 1898.

ment of Biology of the University of Rochester, endorsed him for the position.

On November 8, 1897, Smith's salary as an expert assistant in the Division of Vegetable Physiology and Pathology was raised from \$1,800 to \$2,000 per annum. He remained with the Department of Agriculture not alone because of the salary-increase but because he believed that by staying he could accomplish "the greatest good to the greatest number."⁶⁵ The laboratory facilities, good comradeship, and scientific inspiration in the Department of Agriculture were still attractive to him.

Director Jordan asked Smith concerning the merits of two other workers in the Department, but finally chose to "select a promising young man" for the work. "What is your opinion of Stewart," he said, "not of what he has done, but what he may be able to do?"

On September 7, 1895, Fred Carlton Stewart, botanist since December 1, 1894, at the Geneva station, and whose work until about 1898 was mainly at Jamaica, New York, had written Smith:

At Springfield I heard your paper on the bacterial disease of tomatoes, potatoes, etc. You were on the program for another paper on *Fusarium* wilts but I did not hear that one. From this I learn that you have been giving a good deal of attention to wilt diseases, hence I write you about a potato wilt.

He described the disease found abundantly in southeast New York, and asked Smith his opinion as to its cause. He was an advanced graduate in science (1892-1894) at Iowa Agricultural College. Early in 1893 Galloway had consulted Pammel to recommend someone to take Fairchild's place when the latter had gone to Europe. President W. M. Beardshear and Pammel recommended Stewart as one of their "very best graduates [and] well informed in regard to fungi, and botany in general as well as horticulture," He wanted "a place where [he could] give [his] entire attention to botany." His "considerable training in physiological botany" made him "a very desirable man" for the position in Washington. But several men were considered for the place, and from them Secretary Morton chose Woods. Stewart, meanwhile, had matriculated at Harvard for advanced study under

⁶⁵ Letter, Jordan to Smith, November 8, but obviously a reference to what Smith himself had said. Other letters from Jordan to Smith were on November 19, 29.

Farlow. Galloway told him to continue with his work at Cambridge and hoped "that nothing [would] interfere with [his] future investigations in plant pathology. The subject," he said, "is a broad and growing one and in my judgment there is abundant opportunity for young men to come to the front in this special field."

Stewart, after accepting a position with the New York station at Geneva, corresponded on plant diseases with Smith, Waite, and others of the Division. On August 14, 1897, he wrote Woods:

I wish to congratulate you on your success in ferreting out the true cause of Dr. Arthur's "Bacteriosis" of Carnations.⁶⁶ . . . I suspected that you had discovered that the trouble is not due to bacteria at all. . . . One will do well to be suspicious of bacterial diseases in which it is difficult to isolate the germ and which do not respond readily to inoculation. It might be well for me to take my own advice because I am working on a bacterial disease of sweet corn which does not respond to inoculation as readily as I would like to have it. I am also glad to know that you have gotten to the bottom of the lily disease.⁶⁷ . . . In a separate package I send you some geranium leaves which are affected with a spot disease.

He wished to know whether the disease was of fungous or bacterial origin.

Three "plant diseases were first defined and brought to the attention of the American scientific public"⁶⁸ by Arthur. Two, "stigmanose of carnation" and "curly top of sugar beet," were admitted by him to have been "erroneously ascribed to bacteria," but the third, carnation rust,⁶⁹ withheld the test of subsequent investigation. The rusts were his specialty. Between the years 1882 and 1916, he published "over eighty papers" on these subjects, and much other important work was done by him on the plant rusts which he regarded as his "notable achievement."

Smith was one who restudied Arthur's "bacterial disease of sugar beet,"⁷⁰ and, although he examined specimens sent by Arthur in 1899, he was unable to obtain any results which established a bacterial origin. Arthur in 1897 told Woods, "I am

⁶⁶ *Bull. Indiana Exp't Sta.* 59, summary at pp. 37-38, Mar. 1896.

⁶⁷ Bermuda lily disease, prelim. rep't, *Bull.* 14, Div. Veg. Phys. and Path., 1897.

⁶⁸ Unpublished memorandum, prepared by Dr. Arthur on February 15, 1916, *op. cit.* Quotations of this paragraph taken from this memorandum.

⁶⁹ *Botanical Gazette* 16: 321, 1891; *American Florist* 7: 587, 1892.

⁷⁰ See, E. F. Smith, A bacterial disease of sugar beets (1892) by Arthur and Golden, *Amer. Nat.* 30: 716-731, Sept. 1896, sub-section 3. Also, *Bull. Indiana Exp't Sta.* 39.

delighted that these subjects [including pear blight] are being studied, if it can only be done thoroughly."

Early in 1897 when C. F. Wheeler, consulting botanist of Michigan Agricultural College and Experiment Station, read a reprint of Smith's "Bacterial Disease of Tomato, Eggplant, and Irish Potato," he wrote the Division of Vegetable Pathology and thanked officials for "the finest bulletin yet issued from your Department." He added, "It ranks with the best foreign work," and in a letter to Smith he called it "*the best slice of work done in this or any other country.*"

On September 19, 1896, three months before his elaboration of the cultural characters of *Bacillus solanacearum* was released, Smith had begun to study Pammel's "bacillus in cabbage leaves." His laboratory memoranda show that he thought that this was "possibly" an organism which Dr. Russell had studied. Cabbage leaves, however, had been received the day before from Dr. J. J. Davis of Racine, Wisconsin, whose accompanying letter of September 16 read as follows:

I send by this mail some cabbage leaves, illustrative of a disease which is doing much damage in this vicinity. I take it to be due to the small oval micrococcus so abundant in the conducting tissue. Have you published anything on this disease? If so I would be glad to receive a number of copies for distribution to the growers here where the cabbage has become an important crop. [On September 30, in another letter, he stressed that] The trouble bids fair to check the cabbage industry here which has been quite profitable through cold storage.

Smith, promptly after getting the materials, described the disease and started cultures. By November 20 his memorandum was entitled, "Bacillus campestris Pammel. First set of Fermentation tubes"; November 24, "First set of inoculations in Division's Greenhouse"; December 8, "Gelatine streak cultures. Made from fluid cultures started December 1 and all but one of which I have already used for Thermal death point experiments of December 3, *B. phaseoli*, *B. campestris*, *B. tracheiphilus*, *B. hyacinthi*." "First experiment with sap from waterpores," dated February 19, 1897, was preceded by a "General note on infections occurring presumably as the result of visitations of slugs, flies, aphids, ants, etc." and a special set of experiments and observations on the "Natural method of infection." During this year he demonstrated

that molluscs sometimes transmit black rot of cabbage.⁷¹ Smith's proof of water-pore infections in cabbage black rot, stomatal infections in half a dozen diseases, and insect transmission of cucurbit wilt, cabbage black rot, and potato brown rot, were among the most noteworthy of his early contributions to plant bacteriology.⁷² That his investigations were nation-wide in scope was illustrated by his study of *Bacillus campestris*.

April 14, Swingle sent a box of green cabbage worms. Smith "set them to feeding on cabbage leaves and stems browned by this germ," and began experiments to obtain "Infections by means of larvae of *Plusia brassicae*."

During the summer Michigan Agricultural College forwarded a request from Saginaw, Michigan, to investigate "on the ground" a cabbage disease and other ills of cultivated crops. Wheeler was appointed to make the study and invited Smith to accompany him. Already Smith had published in the *Centralblatt*, and Pammel had commended him for his "very excellent account so far as it extends" of the brown rot of cruciferous plants. Pammel, in a letter of August 14, had told him he was anxious to see his further studies.

Smith, en route to join Wheeler, attended the meeting at Detroit of Section G of the American Association for the Advancement of Science at which Farlow was elected president and Smith secretary of the Section. There, Smith met, and had an interesting chat with, Dr. H. Marshall Ward, who spoke at the meeting and at the Toronto meeting of the British Association for the Advancement of Science. While on this journey Smith frequently wrote to Woods in whose care at Washington he had left some valuable cultures. From one of these cultures it was found, as expected, that a bacillus would grow in hydrogen, and also in carbon dioxide, a result not anticipated. Presumably the organism was *Bacillus tracheiphilus*. On August 19, 1897, from the Bancroft House at Saginaw, Smith acquainted Woods with the results of his further study of the cabbage disease:

Am up here looking at sick cabbage fields. Mr. Chas. F. Wheeler of Michigan Agric[ultural] College is with me. We saw 200 acres of

⁷¹ Erwin F. Smith, A conspectus of bacterial diseases of plants, *Annals Missouri Botanical Garden* 2: 377-401, at p. 390, Feb.-Apr. 1915.

⁷² Synopses of researches, *op. cit.*, 23.

cabbages today on one farm and much of the bacterial cabbage disease. The first time I have ever seen it in the field. I was more than right in suspecting that the plants are commonly infected through the water-pores. I counted 411 water-pore infections. . . . My bacterial disease of cucumbers is also here—a new locality for it. Saw also *Cercospora* on sugar beets. They are just commencing to grow sugar beets here on a large scale. The cabbage industry here amounts to about 1,000 acres.

From Racine, Wisconsin, on September 1, Smith continued to Woods:

I am still here studying the cabbage disease. It is very interesting and I cannot help mailing you a bundle of leaves to show how well field examinations bear out my hypothesis as to manner of infection. One bundle of the leaves contains only marginal *water pore* infections. At least 90% of the field infections are of this type. Another bundle contains only middle leaf infections due to the gnawings of insects chiefly larvae of *Pieris ropae*. Margins and bases still healthy. Stems and heads of plants from which these two bundles of specimens come were still sound. The third set of leaves represents a much later stage of disease i. e. the disease has gained entrance to the stem and has worked its way out through these leaves, the bundles in the base of the petioles being the first part to be infected. By a little cutting with a sharp knife you will see that I am right. . . . I have heard of this cabbage disease at Clyde, Ohio, and near Rochester, New York, and should like to stop at these places and see it on my way back from Michigan. . . . The disease is likely to attract wide attention in this country before many years and the Division ought to lead the way. . . . Since coming here I have found the disease in the common field mustard *Brassica sinafistrum* which is the showy yellow flowered weed sometimes called charlock. I am now hunting for it on other cruciferous weeds.

Smith exchanged a letter or so with Acting Entomologist F. H. Chittenden of the Department concerning the cabbage weevil. He was gathering material for the herbaria of the Division. Early in September he received his authorization to study the cabbage disease at Clyde and Rochester. On September 9, however, William Alton Taylor, then at his father's home at Douglas, Michigan, requested Smith to "come over to the peach belt for a few days," since he found the "so called 'little peach' disease [was still] the topic of conversation among fruit growers." September 24, Taylor, who had returned to Washington, wrote again:

After one day spent in infected orchards in the vicinity of Douglas I am satisfied that there is necessity for prompt and thorough work in the inves-

tigation of the new peach disease if it is to be prevented from becoming more rapidly destructive than yellows ever has been in that region. One large orchard . . . between Douglas and Fennville has already gone and several others in the vicinity are almost annihilated while orchards nearer the village and farther west show from a half dozen to a hundred cases each and some cases of yellows. It is reported to have been found associated with crown galls in some cases but I did not find any such.

More than this, in October, arrived a request from Halsted of the New Jersey experiment station for Smith to visit "several badly affected orchards" of a leading peach grower in Sussex County to decide whether a disease, bearing the characteristic attributes of yellows but believed not contagious, was in fact yellows. Smith pronounced this genuine peach yellows, and when Halsted thanked him he appended:

It is with pleasure that I find that my little contribution of diseased beans [*Bacterium phaseoli?*] serves to add another locality to your list of places where the bacteriosis is known to exist. The black rot of cabbage is complained of by some growers in New Jersey, and I will, as I go about during the next six weeks from one end of the State to the other, make inquiries regarding this trouble and gladly communicate any information that may be obtained upon it. Please accept my many thanks for the paper upon the subject. . . .

Not until August and September of the next year was Smith able to follow up his field investigations of the "little peach" disease of Michigan. His duties as an expert diagnostician and pathologist of plant diseases exacted much time in his laboratory since specimens of many various maladies were being received from different quarters of the nation. On August 4, the Entomological Division of the Department sent him a few cotton bolls received from Texas, a disease of which was ascribed to "insects, bug or weevil" and "doing the cotton great damage." Two days later Entomologist Leland O. Howard forwarded more specimens, this time green cotton bolls showing the same disease, but from Alabama. Examinations indicated a bacterial infection but since at the time Smith "was too crowded with other work to make cultures," his study of the bacteriosis of cotton and the insect believed involved did not get under way for several days, and then turned out to be a failure. Not until May 23, 1898, did he make a second attempt to infect cotton bolls, and this time he used "Sea Island cotton from James Island, South Carolina."

Not until the first years of the next century, however, was his study effective. Further on, therefore, will appear more on his study of cotton diseases.

Smith's departmental work for the *American Naturalist* had met with high favor, especially his writings on bacterial plant diseases. On January 31, 1896, E. D. Cope had written that his department was a "great success, and botanists [were] finding that they must have it." He was advised that more lengthy technical articles were not to be included, but rather, general reviews, synopses, and important results of scientific work. In June Cope accepted the first of his critical reviews of knowledge concerning bacterial diseases of plants. It was "such as the *Naturalist* desires for a leading article . . . eminently readable." In July he wrote again: "The series you propose will make leading articles, but will not replace the department of Vegetable Physiology, as you have hitherto conducted it." He hoped to "get another of your excellent contributions." Smith for the most part adhered to "summaries of news and discovery." But by August 1897 he thought of his department as in plant physiology and morphology, or plant pathology. He told this to Farlow when he also said that he was "especially anxious to complete [his] series of critical papers on Bacterial Diseases of Plants."

Scientific journals in America thus far had fought bravely to maintain standards of excellence. Experience had demonstrated, however, that to publish articles of real value to science, yet to attract a sufficiently wide reading public to obviate special outside financing, required resourcefulness. In 1894 N. D. C. Hodges, editor of *Science*, had told Smith that his journal, he believed, had attained its maximum circulation, yet still was functioning at a loss financially. He had found that as much or more attention had to be given to financing, than editing, the publication. With increased costs indicated, some gift solicitations had been found necessary. In August 1897 Smith heard of "new plans" for the *American Naturalist*. He wrote Farlow who replied that several contributors, among them himself, Dr. C. S. Minot, and others, had purchased the publication. R. P. Bigelow was to act as general editor, and Farlow understood that Smith was "to be asked to continue to take charge of the same subjects as previously."

Farlow had just learned that he had been chosen to preside

over the botanical section of the American Association for the Advancement of Science during the next year. He was "surprised" since once before he had been presiding officer when it was "the section of biology." He suggested to Smith "to select some vital topic of the day and have it the subject of one or more sessions." "Sexuality of fungi," or any subject important in physiology, was recommended.

Fifty-six papers were listed, and forty-seven read, at the meeting of Section G which took place at Cambridge the following August 22-25. Smith, as secretary, prepared for *Science*⁷³ an important monograph entitled, "Botany at the Anniversary Meeting of the American Association for the Advancement of Science." Such valuable papers as the following were abstracted by the authors and included: Bradley Moore Davis, "The Carposporic Type of Reproduction of the Rhodophyceae"; H. J. Webber, "Origin and Homologies of Blepharoplasts"; H. L. Bolley, "Some Observations bearing upon the Symbiotic Mycoplasma Theory of Grain Rust," including a consideration of Erickson's famous hypothesis; B. D. Halsted, "Starch Distribution as affected by fungi"; Charles Orrin Townsend, "The Effect of an Atmosphere of Ether upon Seeds and Spores"; Rodney Howard True, "The Toxic Action of a Certain Group of Substances"; M. B. Waite, "Life History and Characteristics of the Pear-Blight Bacillus"; S. M. Babcock and H. L. Russell, "The Biology of Cheese Ripening"; and many others. William B. Alwood of the Virginia Agricultural Experiment Station and professor of horticulture and allied subjects at Virginia Polytechnic Institute presented two papers, one on "The Leaf-spot Disease of the Apple, *Phyllosticta pirina*, and Several Unrelated Forms occurring therewith" and one "On the Occurrence of a Yeast Form in the Life Cycle of *Sphaeropsis malorum*." Since forest pathology represented a branch of science inadequately studied thus far in America, Hermann von Schrenk's "Notes on Some Diseases of Southern Pines" signified a formal recognition of a new type of research in the United States Department of Agriculture.

Smith reckoned von Schrenk's beginning in 1894-1895 to study fungous diseases of forest trees in the United States as an event of first rank in this nation's research in pathology.⁷⁴ Bernhard

⁷³ N. s., 8(202 & 203): 651-660, 690-700, Nov. 11, 18, 1898.

⁷⁴ Fifty years of pathology, *op. cit.*, 22. Von Schrenk, in conference with the

Eduard Fernow, Filibert Roth, and others of the Division of Forestry had recognized the need and worth of such study, and had their scientific program of timber physics, wood chemistry, and forest products investigation been brought to full fruition, doubtless forest tree diseases would have been sooner more fully investigated in this country.

Smith always had been interested in reforestation for Michigan; and in 1898 a review in the *American Naturalist*⁷⁵ of Roth's study of "Forestry Conditions and Interests of Wisconsin" was further proof of this interest. Ecological research, coordinated with the study of plant physiology, was only being developed in this country when he reviewed George P. Merrill's "Notes on the Geology and Natural History of the Peninsula of Lower California"⁷⁶ and Milton Whitney's "Preliminary Report on the Soils of Florida."⁷⁷

In 1890 he had been told by Spalding that Roth had done "an admirable piece of work on the Ascent of Water in the Stems of tall trees." Under Spalding, Roth had studied not only plant physiology but also fungous diseases of plants. Later he taught diseases of forest trees at the New York State College of Forestry at Cornell University.

Fernow and Roth encouraged vonSchrenk when he began his studies of diseases of cypress and pine. At first, von Schrenk's examinations were more mycological and descriptive than the results of experimental investigations made in a laboratory especially created for the study of forest tree diseases. In 1893 he had graduated from Cornell University where he had studied under Atkinson; and the next year had secured a master of science degree under Farlow who, it will be recalled, had published as early as 1879 on "Diseases of forest trees."

At Harvard, under the leadership of Charles Sprague Sargent of the Arnold Arboretum, forest investigation attained prominence during the 1880's owing largely to his important study of American forest trees and forest-tree distribution for the Tenth

author, has revealed that he regards the beginning of his career as an American forest pathologist when he published his study, Note on *Tubercularia pezizoidea* Schwein., *Bull. Torrey Botanical Club* 21(9): 385-388, Sept. 20, 1894.

⁷⁵ 32(380): 603-604.

⁷⁶ *Amer. Nat.* 32(380): 601.

⁷⁷ *Idem*, 602-603.

Census. When the first prospectus for the influential periodical, *Garden and Forest*, was sketched, Sargent submitted it to Farlow who was to have charge of cryptogamic botany. "*Garden and Forest*," this read, "will be devoted to Horticulture in all its branches, Garden Botany, Dendrology and Landscape Art. It will present the results of the latest studies on Plant Diseases and Insects Injurious to Vegetation. It will give special attention to the subject of Forestry."

After studying forest tree and other plant diseases with Farlow, von Schrenk in 1895 went to Europe and studied with Robert Hartig, "father of forest pathology."⁷⁸ He returned to the United States and became an instructor in botany at the Manual Training School of Washington University and instructor of cryptogamic botany at the Shaw School in St. Louis. During the last years of the century, when the federal department of agriculture established its Mississippi Valley Laboratory at St. Louis, he was appointed pathologist in charge, and his work included investigating timber diseases and timber preservation. Moreover, in 1898, at the university, he taught a course for civil engineers in bacteriology, particularly as applied to water problems.

Smith's interest in forest pathology appears to have been first expressed in 1892. He had read a paper by Farlow and on January 5 wrote: "Your paper on *Diseases of trees likely to follow Mechanical injuries* is so good that it ought to have a wide circulation. As a lover of trees it interested me greatly." He wanted to arrange to have the Michigan Horticultural Society reprint it.

His papers presented to Section G at the 1898 anniversary meeting of the American Association for the Advancement of Science were three: "Potato as a Culture Medium with some Notes on a Synthesized Substitute"⁷⁹; "Some Little Used Culture Media, which have proved valuable for Differentiation of Species";⁸⁰ and "Notes on Stewart's Sweet Corn Germ, *Pseudomonas Stewarti*, new species."⁸¹

⁷⁸ H. H. Whetzel, *Outl. hist. of phytoph., op. cit.*, 72-75. See also, Fifty years of pathology, *op. cit.*, 22.

⁷⁹ *Proc. Amer. Assoc. Adv. Sci.* 47: 411-412, 1898.

⁸⁰ *Proc. Amer. Assoc. Adv. Sci.* 47: 412-413, 1898.

⁸¹ *Proc. Amer. Assoc. Adv. Sci.* 47: 422, 1898. During this year Smith also addressed the Saugatuck and Ganges Pomological Society on the "Little Peach" disease of Michigan. *Fennville Herald*, Oct. 15, 1898 and reprint.

This was the year when a committee of American bacteriologists composed of J. George Adami, W. T. Sedgwick, George W. Fuller, Charles Smart, A. C. Abbott, T. M. Cheesman, Theobald Smith, and William Henry Welch, finally reported to the American Public Health Association on procedures recommended for the study of bacteria with especial reference to greater uniformity in the description and differentiation of species. Although a report to the Association's committee on the pollution of water supplies, Smith regarded the pamphlet as of exceeding value, a model of condensation, and considered that it "should be in very laboratory, and in the hands of every student."

Laboratories of bacteriology at last had a manual of standard practice. George W. Fuller's method of titration, his expression in regard to acidity, and his 1895 method of preparing meat infusion were merged with the report of the committee of which Fuller was a member.⁸²

In 1899, at the Minneapolis meeting of the American Public Health Association, another committee, consisting of Fuller, G. C. Whipple, H. W. Clark, Edwin O. Jordan, H. L. Russell, J. W. Ellms, and Robert Spurr Weston, was appointed to study further standard procedures in water analysis, including bacteriologic examination. This committee remained at work for five years, issued progress reports occasionally, but its final report was not made available until 1905. Procedures provisionally recommended by the first committee for the study of bacteria were presented in 1897 at the Philadelphia meeting of the Association. Quite likely, however, Smith did not attend this meeting, since, as yet, his papers in plant bacteriology without exception had been given before botanical societies, and, as far as is known, he had attended no meetings of the American Public Health Association and no convention of bacteriologists. He, therefore, quite likely did not come into possession of the knowledge embodied in the recom-

⁸² *Procedures Recommended for the Study of Bacteria*, submitted at the Phila. meeting, Sept. 1897, of Amer. Pub. Health Ass'n, The Rumford Press, 1898, Concord, New Hampshire. Preface by Deputy Surgeon General Charles Smart. Introduction by Wm. H. Welch. See, Hydrogen-ion concentration vs. titratable acidity in culture mediums, *Jour. Infect. Dis.* 33(1): 1-59, July 1923, "Historical," pp. 2-7, concerning 1899 Assoc. meet. at Minneapolis, etc. Introduction by Erwin F. Smith. The 1897-1898 recommendations were provisional. See, also, Public Health Papers and Rep'ts of Amer. Public Health Assoc., 1898, for report of committee of Amer. bact. for this year.

mended procedures until these were published and distributed in the year 1898. Standard bacteriological practice was under discussion at meetings of scientific societies, including Section G and the Society for Plant Morphology and Physiology. Furthermore, the work of preparing reviews for the *American Naturalist* doubtless brought the report to his notice, if, in fact, the pamphlet was not sent to, or obtained by, him directly.

In the January 1, 1896 issue of *American Naturalist*⁸³ Smith reviewed, under the title, "Relation of Sugars to the Growth of Bacteria," the recently published and "most discriminating" paper on this subject by Theobald Smith, "Ueber die Bedeutung des Zuckers in Kulturmedien für Bakterien (*Centrb. f. Bak. u. Par., Med.*, Bd. XVIII, No. 1)." After serving more than a decade as Chief of the Division of Animal Pathology of the United States Department of Agriculture, Theobald Smith had resigned from that position on May 15, 1895,⁸⁴ to become director of the pathological laboratory of the Massachusetts State Board of Health, and in 1896 professor of comparative pathology at Harvard University.

In September 1896 Veranus A. Moore, after taking Smith's place as chief of the laboratory of pathology of the Bureau of Animal Industry, had also resigned his position to become professor of comparative pathology, bacteriology, and meat inspection—later dean—of the New York State Veterinary College at Cornell University. The rapid advances in exact knowledge of animal pathology being achieved in veterinary science were proving in this field the value of laboratories of experimental research. Within six years after Salmon's famous 1884 experiments on Barren Island, for instance, contagious pleuropneumonia in cattle had been practically eliminated from this country, and at a small expense when compared with the monetary savings made possible. Glanders and tuberculosis, furthermore, could now be accurately diagnosed; and the amount of each disease in the United States had been greatly reduced by proper sanitary measures. The terrors once held for anthrax and rabies had been alleviated. Contagious maladies were being differentiated from

⁸³ 30(349): 66-67, Jan. 1896.

⁸⁴ Transcribed from the official documentary sources at the Agricultural Division of the United States National Archives.

those of non-infectious nature. Specific agents of a few well recognized ailments had been found, and in some both cause and method of dissemination were known. Ways to combat serious diseases of fowl, sheep, swine, cattle, and horses were being discovered. Practical study of dietary troubles had been started.⁸⁵ Dr. Moore's department of comparative pathology and bacteriology became at once important, and by 1899 Erwin Smith would say⁸⁶ of his *Laboratory Directions for Beginners in Bacteriology*: "Dr. Moore's courses in bacteriology in the veterinary school . . . are among the very best in the country, and this book is to be welcomed as an extension of the influence of a conscientious teacher who is at the same time a competent investigator."

Both Moore and Theobald Smith supplied Erwin Smith with materials for his laboratory, including cultures and criticisms of work. Occasionally valuable suggestions and points of information were received from others: for example, on January 7, 1895, Dr. Vaughan told him how to prepare and use blue litmus solution diffused through gelatine as an indicator, and why litmus paper should not be used for the purpose in mind. Dr. Welch also supplied him with similar useful laboratory techniques. He built his knowledge of laboratory practice from many sources.

Not the least of his sources was his reading, much of it done for his reviews of scientific literature in the *American Naturalist*. Frequently he detailed the results of carefully wrought experiments, summarized conclusions of constructive importance, added his own criticisms, and sometimes described methods by which to improve laboratory and field study. His review⁸⁷ of the "second improved edition" of "Saccardo's Color Scale" might be so classified. He recommended that this work by the "learned author" of the *Sylloge Fungorum* be possessed by every botanist and especially by every mycologist. If descriptive color terms in natural history were to be used, reference to "some particular scale or standard" was necessary. In this work was provided "a ready means of

⁸⁵ V. A. Moore, The influence of animal experimentation upon agriculture, *op. cit.*; D. E. Salmon, Some examples of the development of knowledge concerning animal diseases, *op. cit.*; T. Smith, Investigation of infectious diseases of domesticated animals, *Rep't of the Sec'y of Agric. for 1893*: 140-160; E. A. de Schweinitz, *idem*, 161-168.

⁸⁶ *Amer. Nat.* 33(385): 80, Jan. 1899.

⁸⁷ *Idem*, 29: 1009-1010, Nov. 1895.

determining in a thousand and one descriptions exactly what color is meant," and for this reason it had a special value to mycologists.

In 1896 Dr. B. M. Watson of the Bussey Institution of Harvard invited Smith to lecture before the Massachusetts Horticultural Society early the following year on the John L. Russell foundation. He was asked to speak "on the latest discoveries of the connection of the fungi and horticulture," but the subject was "open to the broadest interpretation" and his only restriction was that it be "something on the economic side." He chose "The Spread of Plant Diseases a consideration of some of the ways in which parasitic organisms are disseminated."⁸⁸ His discussion included bacterial as well as fungous diseases, and the occasion took place on March 27. He was invited to visit Dr. and Mrs. Theobald Smith at Jamaica Plain, and he arranged an appointment with Farlow to secure advice on a point about which he brought drawings and slides. So well received was his lecture that the next year when, to attend the 50th anniversary meeting of the American Association and fulfill his duties as secretary of Section G, he again visited Boston, Dr. Charles O. Whitman invited him to spend a day or two at Woods Hole and arranged that the next year he should deliver a dozen lectures at the Marine Biological Laboratory. Smith was a member of the Cosmos Club of Washington. In its assembly hall November 20, 1897, he lectured before the Biological Society on "Bacterial Diseases of Plants." January 1898, again he lectured on "Some Bacterial Diseases of Truck Crops"⁸⁹ to the Peninsula Horticultural Society meeting at Snow Hill, Maryland. His career as America's outstanding spokesman on plant bacteriology began to take him away from his laboratory often.

William Henry Welch, editor of *The Journal of Experimental Medicine* and this year president of the Congress of American Physicians and Surgeons as well as author of *The Biology of Bacteria, Infection and Immunity* (1894) and *Bacteriology of Surgical Infections* (1895), addressed an important letter to Smith on November 22, 1897:

Thank you for your interesting letter regarding bacterial diseases of

⁸⁸ Printed by the Society, 1898; also, *Trans. Mass. Hort. Soc.* for 1897, part I, 117-133, 1898.

⁸⁹ *Transactions Penin. Hort. Soc.* 11: 142-147, 1898.

plants. I am glad to be reassured on the subject, although I had no doubt of the accuracy of your own work.

[Alfred] Fischer's ipse dixit on the subject is calculated to exert an unfortunate influence with those who have no special knowledge concerning these investigations, as his book⁹⁰ is well written and on some aspects of bacteriology it speaks authoritatively.

I have recently had occasion to look up the evidence upon the influence of proper disposal of sewage upon public health and I came across your valuable monograph in the Annual Report of the Michigan State Board of Health. I am ashamed to say that I did not know of it before. It is a most admirable and convincing presentation of the evidence up to the day of its publication.

I shall be glad of the opportunity of studying the cultures which you may send.

Twelve days earlier, on November 10, Welch had opened correspondence with Smith:

Thank you for the reprint of your article on *Pseudomonas Campestris*, which I am glad to have. I am much interested in it, as well as in what you told me about bacterial diseases of plants on Monday last. I am sorry that I was so busy with my class on that day, as not to be able to talk more with you.

I do not know what to make of A. Fischer's position in his *Vorlesungen über Bakterien* (pp. 131 and 132) on bacterial diseases of plants. He goes so far as to deny the possibility even of such diseases and expresses utter skepticism as to all observations supposed to demonstrate them, being indeed quite sarcastic on the subject [Welch here quoted a paragraph from Fischer's book]. As Fischer is a botanist, and I suppose of good standing, I do not understand his position. I had supposed that the demonstration of bacteria pathogenic for plants was no less conclusive and generally accepted than that for animals.

Fischer, in his *Vorlesungen über Bakterien* published in Germany that year, not only denied that bacterial diseases exist in plants but also argued their impossibility. The real causes of these diseases, he believed, were of other origin. Bacteria could enter the plant only through wounds. Stomatal infection was regarded impossible. If the bacteria did get into the plant because of animal injuries, frost, and other reasons, their development was stopped by some formed cork substance. The contamination was saprophytic; and genuine fungi accounted for many of the diseases of plants now being attributed to bacteria.⁹¹

⁹⁰ *Vorlesungen über Bakterien*, published in the last part of 1897 at Jena, Germany.

⁹¹ See *Bacteria in relation to plant diseases*, *op. cit.*, 2: 15-16.

Dr. Smith's "interesting letter regarding bacterial diseases of plants" to Dr. Welch has not been found by the author. We must be content, therefore, with the knowledge that his letter completely "reassured [Welch] on the subject." For many years he had recognized the leadership and authority of German botanists, and in his reviews of scientific literature in the *American Naturalist* he had shown no prejudice toward work by the Germans. In August 1896,⁹² in his introduction to his critical reviews of the present state of knowledge concerning bacterial diseases of plants, he had advanced the opinion that "there are in all probability as many bacterial diseases of plants as of animals." In February 1897⁹³ his last critical review had been of Dr. Ernst Kramer's study of the wet-rot of potato. Kramer was privat docent in the technical high school at Graz, and his study of the bacteriosis of fodder beets had been earlier reviewed.⁹⁴ During 1896 he had three times⁹⁵ reviewed the recent work of Dr. Oscar Brefeld on the smut fungi. A recent contribution on "Water Pores"⁹⁶ by Dr. Anton Nestler had also been reviewed as well as a paper by Leopold Kny of Berlin under the title, "Function of Anthocyan."⁹⁷ Under the caption, "Nitrifying Organisms,"⁹⁸ he re-examined the discovery by Messrs. Burri and Stutzer of the agricultural experiment station at Bonn of a bacillus, in many respects like Winogradsky's organism and which was claimed to change nitrites into nitrates and grow readily in bouillon and on gelatine. This was followed in 1898 by a criticism of O. Zinsser's view that "the root tubercle organism occurs outside of the tubercles in various parts of the plant"⁹⁹ in Leguminosae and other important conclusions from work done on this subject at the Botanical Institute of Leipzig. Other reviews during these years of results directly or indirectly attributable to German botanists might be included. For examples, "Spore formation controlled by external conditions,"¹⁰⁰ based on a *Botanische Zeitung* paper by Johann Bachmann, coincided in point of time with another review, "Ger-

⁹² *Amer. Nat.* 30(356): 627.

⁹³ *Idem* 31(362): 123-138.

⁹⁴ *Idem* 30(357): 716 f.

⁹⁵ *Idem* 30(350): 137-132; (351): 224-226; (353): 408-409.

⁹⁶ *Idem* 30(351): 224.

⁹⁹ *Idem* 32(377): 365-369.

⁹⁷ *Idem* 30(351): 226-228.

¹⁰⁰ *Idem* 30(349): 63-64.

⁹⁸ *Idem* 30(349): 65-66.

mination of Refractory Spores,"¹⁰¹ culled from recent studies made by Dr. Jacob Eriksson of Stockholm.

Smith reviewed the work of botanists from other nations. In 1896 he described the experiments of M. Gaston Bonnier of Paris on "Changes in Structure of Plants due to Feeble Light"¹⁰² and on "Changes due to an Alpine Climate"¹⁰³; and that year he told of the find by Dr. F. A. F. C. Went, the Dutch botanist, of an "Algal Parasite on Coffee,"¹⁰⁴ *Cephaleurus coffeae*. These were three among many others.

During this period A. Engler's and K. Prantl's great work, *Die Natürlichen Pflanzenfamilien*, was being compiled, arranging in ordinal sequence the life histories of phanerogams, algae, fungi, bryophytes, pteridophytes, etc. In 1899 Smith would review *lieferung 180*, the first part of Fünfstück's account of the Lichenes,¹⁰⁵ and more than pointing out that of 20,000 described species, varieties or forms only about 4,000 were well known, he would comment on points of classification. "It may be assumed," he wrote, "that nearly every one knows that lichens are symbiotic growths. It is now just thirty years since the publication of Schwendener's memorable paper, and the matter was fought over and settled in the seventies. In different lichens the relation between the fungus and the alga is very different." He thought of himself as a "non-lichenologist" but his interest in protoplasmic chemistry and sexual reproduction among the lower plant orders caused him to study this new work with enthusiasm. "Ripening of Fleshy Fruits"¹⁰⁶ was another review which clearly showed an interest in the chemistry and physiology of his subjects.

In 1896 he had reviewed *lieferung 129* of *Die Natürlichen Pflanzenfamilien*. This was entitled "A New Classification of Bacteria"¹⁰⁷ and had to do with an arrangement by Professor W. Migula of Karlsruhe which was believed "more practical and satisfactory than that of Alfred Fischer." This paper, Smith told his readers, "ought to be in the hands of every working bacteriologist." He believed that the classification, appearing "to have grown out of a long and wide experience in the laboratory,"

¹⁰¹ *Idem* 64-65.

¹⁰² *Idem* 30(353): 405-408.

¹⁰³ *Idem* 30(349): 61-63.

¹⁰⁴ *Idem* 32(375): 208-210. This was based on a study by C. Gerber (1898).

¹⁰⁵ *Idem* 30(354): 490-493.

¹⁰⁶ *Idem* 30(349): 67.

¹⁰⁷ *Idem* 33(390): 540-543.

was usable, "the best yet devised, and [would] probably come into general use," among botanists. Another work which he regarded "so very good that it deserves to find its way speedily into every laboratory" was another German treatise, K. B. Lehmann's and R. Neumann's *Atlas und Grundriss der Bakteriologie und Lehrbuch der speciellen bakteriologischen Diagnostik*. On March 20, 1897, he spoke about this before the Botanical Seminar and in April reviewed it as current work in the *American Naturalist*.¹⁰⁸ This was "a general work on bacteriology," he said, "covering much the same ground as [C.] Flüggé's *Die Mikroorganismen*, but in a different manner. About 60 of the more common animal pathogenic and saprophytic forms have been studied more or less carefully and re-described according to a pre-established scheme, so that their behavior on all the common media may be readily compared."

Smith was still studying the technique of titrating culture media. "Authors," he wrote,¹⁰⁹

have used phenolphthalein for titrating media regularly since 1894 and recommend it for general use. . . . All the bacteria figured in the Atlas were grown on media slightly alkaline to phenolphthalein, and most of the 60 sorts bore the extra 10 cc. of alkali and the 10 and 20 cc. of acid. This seems rather surprising to the writer and certainly cannot be assumed to hold good for all species. My experience would lead me to select for a universal medium a grade of alkalinity considerably less than the zero or neutral point of phenolphthalein, i. e., one nearer the zero of the best neutral litmus paper, as I am satisfied that some species will not grow on media as alkaline as here recommended.

Since reviewing Theobald Smith's paper on the relation of sugars to the growth of bacteria, he, presumably, had "given up . . . the division of bacteria into acid and alkali producers." At least, the distinction was not as hard and fast as it had been and he was striving to improve his technical proficiency to study "the conditions governing the production of acid" by each species. This, in 1896.

When the foregoing of 1897 was written, Smith had published on two parasitic bacteria of plants, *Bacillus tracheiphilus* and *Bacillus solanacearum*. He was completing his work on *Pseudo-*

¹⁰⁸ *Idem* 31(364): 312-314.

¹⁰⁹ *Idem*, 314.

monas campestris for publications that year.¹¹⁰ His study of these organisms was to continue for many years and in many particulars he would perfect his technique. Workers then were often not entirely confident of their conclusions and the correctness of their methods of work. To illustrate: in 1894 he wrote of peach yellows trees as "full of a virus readily communicated to other trees by budding"¹¹¹ and of peach rosette as "a virus [requiring] a period of some months to penetrate into all parts of a tree."¹¹² But he also allied peach yellows to variegation in plants, "a disease manifesting itself in stunted growth, imperfect assimilation, hastened development, and feeble vitality. Moreover, in a number of variegated plants, e. g., jasmines and abutilons, this condition is transmissible to healthy stocks by budding or grafting, in the same way as peach yellows. The difference in these cases appears to be one of degree rather than of kind." Modern research has brought forth much new knowledge concerning these two diseases and little peach, all three having different symptoms and caused by viruses.¹¹³

During the 1890's there was much uncertainty about the knowledge of bacterial diseases of plants. In 1892 W. Migula, writing for the Middle Java Experiment Station, had concluded that five from "about twenty"¹¹⁴ bacterial plant diseases mentioned in literature were "clearly established." Sorauer, Comes, Russell, Ludwig, and others, then recognized that bacterial plant diseases existed, but agreement among them as to how many and what diseases had been definitely proven to be caused by bacteria was lacking. In his list of diseases of established bacterial origin, Russell included pear blight, Wakker's yellows of hyacinths, Heinz's hyacinth rot, tuberculosis of olive, and Kramer's wet-rot of potato, among several others. By 1897 Migula had increased his list from five to eight maladies proved to be caused by bacteria. Nevertheless, leading text-books on plant diseases continued to

¹¹⁰ *Centralbl. f. Bakt., op. cit.*; The black rot of cabbage, U. S. Dep't of Agric. Farmers' Bulletin 68, transmitted Dec. 16, 1897, published in 1898.

¹¹¹ Peach yellows and peach rosette, U. S. Dep't of Agric. Farmers' Bulletin 17: 10, May 1894. In 1895 Smith published also Farmers' Bulletin 33, Peach growing for market.

¹¹² Peach yellows and peach rosette, *op. cit.*, 17.

¹¹³ See, L. O. Kunkel, Immunological studies on the three peach diseases, yellows, rosette, and little peach, *Phytopathology* 26(3): 201-219, Mar. 1936.

¹¹⁴ See, *Bacteria in relation to plant diseases, op. cit.*, 2: 12-14. The facts of this paragraph are taken from the more complete discussion of this subject by Dr. Smith.

be published in England, France, and Germany, the reputable authors of each clinging to the prevailing insistent skepticism and devoting but a comparatively few pages to plant bacteria. Even Burrill's pear blight was seldom accepted as definitely shown to be caused by a bacterium. In 1897 Migula in his *System der Bakterien* included Smith's wilt of cucurbits as a bacterial plant disease. Pear blight had been included in his early, and what Smith believed "the best, paper of its time."

Migula's second volume, *Specielle Systematik der Bakterien*, published in 1900, was still another book which Smith believed "should be in every laboratory. No other general work," he said,¹¹⁵ "deals as carefully with morphology of the bacteria." Designedly less complete in cultural characters, the book was to be regarded as one which described species rather than considered their pathogenicity.¹¹⁶ He chose Migula's scheme for classifying bacteria, and explained thus:¹¹⁷

On the whole, the classification of Migula, which was proposed in October, 1894, and is outlined at length and applied to most of the well-recognized forms, in his beautiful great work, "System der Bakterien," appeals to me most strongly. Up to this time [1905] the writer has followed this system in his own publications and will continue to do so, with certain modifications, until some distinctly better system makes its appearance. This system is based on the flagella and is much more workable than one based on spores, or on a combination of these two characters. The presence or absence of flagella and their position on the body are used by Migula as generic characters. In 1895 Dr. Alfred Fischer also propounded a new system of classification based on spores and flagella. This system was republished in 1897, with material modifications, in his "Vorlesungen über Bakterien," and is modified still further in the second edition of that work.

The second edition of Fischer's work was published in 1903, two years before the second volume of the third edition of Sorauer's *Handbuch der Pflanzenkrankheiten* was made available. Whereas in the first edition of his Handbook (1874) Sorauer had not mentioned bacterial diseases of plants, many pages and seventy such diseases, more or less, were considered in the later work. "This purely didactic review published in 1905," wrote Smith,¹¹⁸

¹¹⁵ *Bact. in rel. to pl. dis., op. cit.*, bibliog., 1: 205.

¹¹⁶ *Bact. in rel. to pl. dis., op. cit.*, 2: 17.

¹¹⁷ *Bact. in rel. to pl. dis., op. cit.*, 1: 157.

¹¹⁸ *Bact. in rel. to pl. dis., op. cit.*, 2: 19.

"contains the best summary in any general treatise on plant diseases. . . . The statements in it, carefully as the literature has been gone over by Dr. Lindau, show, however, perhaps as clearly as anything, the great need for a reexamination of the whole subject by some one experimentally familiar with it." That year, 1905, Smith, after carefully working over "in the laboratory, field, and greenhouse, as opportunity offered, all of the so-called bacterial diseases of plants, submitting each supposed parasite to all of the tests of modern pathology,"¹¹⁹ began to publish his three-volume monograph *Bacteria in Relation to Plant Diseases* in which he expected to treat or touch upon "more than 125 diseases . . . many of which [had] come under [his] own observation." In this he included an "outline of methods of work" started several years previously and evidently first submitted for criticism to Dr. V. A. Moore. He had profited from Moore's booklet of laboratory directions for beginners in bacteriology.

Smith, all the while, studied fungi, as well as bacterial diseases of plants. His review in the *American Naturalist*¹²⁰ of "Chemotropism of Fungi," a study emphasizing the researches of Manabu Miyoshi, a student of Pfeffer at Leipzig, on "the behavior of fungi toward particular substances," was an illustration of what he estimated as of importance. In 1897 he presented before Section G a paper on the "Nature of certain pigments produced by fungi and bacteria, with special reference to that produced by *Bacillus solanacearum*."¹²¹

His tabular account of the "Sensitiveness of Certain Parasites to the Acid Juices of the Host Plants,"¹²² given the next year before the second meeting of the Society for Plant Morphology and Physiology, was not confined to one organism and one disease, but several, and was typical of the work of a comparative pathologist. Inoculating acid nutrient solutions with *Pseudomonas campestris*, *Pseudomonas phaseoli*, *Pseudomonas hyacinthi*, *Pseudomonas stewarti*, *Bacillus amylovorus*, *Bacillus oleae*, and other bacteria parasitic to plants, he demonstrated the wide difference in their susceptibility to plant acids. The three yellow plant para-

¹¹⁹ *Bact. in rel. to pl. dis., op. cit.*, preface, 1: iii and iv.

¹²⁰ 31(368): 717-720, Aug. 1897.

¹²¹ *Proc. Amer. Assoc. Adv. Sci.* 46: 288.

¹²² *Amer. Nat.* 33(387): 208, Mar. 1899.

sites, *Pseudomonas campestris*, *phaseoli*, and *hyacinthi*, were observed to spread more slowly through the parenchymatic tissues of their host plants; and their action was compared with that of parasites of more rapid spread within these tissues, to-wit, those of pear blight, brown rot of potato, or soft white rot of hyacinths. Hyperdermic syringes were sometimes used in parenchymatous injections, and the exact acidity of each nutrient solution was ascertained by titrating it with N/10 Sodium Hydroxide (NaOH) and phenolphthalein. The conclusion was that "Two of these yellow organisms are vessel parasites, their entrance into the plant being favored by the alkaline juice of the ducts. In all three the restraining influence was believed to be, in great part, at least, the acid juice of the parenchyma."¹²³

"Gelatin Culture Media,"¹²⁴ his other paper presented at the 1898 meeting of the Society, offered reasons, illustrated by a chart, photographs, paintings, and an accompanying plate showing the "varying behavior" of *Pseudomonas campestris* in gelatin with different quantities of caustic soda, why individual workers studying the same organism with these media might reach different results and conclusions. It was pointed out that

Working with gelatin of varying grades of acidity and alkalinity, prepared according to Fuller's scale, . . . with interpolations and extension of the scale, if necessary, it is possible to obtain curves of growth decidedly different for different species, even those which are morphologically much alike and which behave the same on nutrient agar.

Gelatin media, he said, should be rendered neutral to phenolphthalein, and he suggested several techniques usable and effective. He again cited as the "best paper in English" on this subject, "On the Proper Reaction of Nutrient Media for Bacterial Cultivation," by George W. Fuller, then a resident engineer of Cincinnati to whom he had sent some of his reprints.

Fuller was particularly interested in Smith's list of "little-used culture media"¹²⁵ found valuable for differentiating species of bacteria. He promised to use some as time allowed. This list,

¹²³ Details furnished by Smith in, Wakker's hyacinth germ, *Pseudomonas hyacinthi*, Bull. 26, Div. Veg. Phys. and Path., 1901. Preface says that paper was prepared in August, 1897. Also, Bull. 28, The cultural characters of *Pseudomonas hyacinthi*, *Ps. campestris*, *Ps. phaseoli*, and *Ps. stewarti* etc., 1901.

¹²⁴ *Amer. Nat.* 33(387): 214-215, Mar. 1899.

¹²⁵ *Proc. Amer. Assoc. Adv. Sci.* 47: 412-413, 1898.

presented in 1898 before Section G as already stated, provided ten formulas, some of them making use of "crude vegetable substances" having "very different quantities and kinds of nutrient" capabilities. At the same meeting, Smith offered another paper, "Potato as a culture medium, with some notes on a synthesized substitute."¹²⁶ In this he also described how to prepare another medium which he called "nutrient starch jelly."

On May 13, 1898, another graduate of Massachusetts Institute of Technology, Samuel C. Prescott, wrote Smith for "cultures of various organisms." He was then an instructor in Sedgwick's department of biology and, while he had not studied "the bacteriology of plant diseases to any considerable extent," he appreciated "how interesting the work must be." They were then moving into their "new laboratory" and he was putting their "cultures in first class condition."

In March 1898 F. C. Stewart, then at Ithaca, had sent a culture of his sweet corn bacillus growing on potato agar and asked Smith to "work it over and give the organism a name." In December, in bulletin 130 of the New York station at Geneva, he had published on the "Bacterial Disease of Sweet Corn." But he wanted Smith to restudy it at the Department of Agriculture, straighten out any possible confusion in synonymy, cultural characters, and other points. Smith carried the organism through many cultures, compared it with other organisms, found it an undescribed form, tabulated the particulars on which he and Stewart were unable to agree, supplied sixteen points of additional data, and a synopsis of characters. This research was the basis of a third paper submitted in 1898 by Smith to Section G, and was entitled, "Notes on Stewart's sweet-corn germ, *Pseudomonas stewarti*, new species."¹²⁷

Smith, in his last evaluation of his own study of this disease, said:¹²⁸

The writer first described the parasite from cultures sent to him by Stewart for that purpose and figured it as polar flagellate, but subsequent studies in his laboratory showed this to be an error and the statements in [*Introduction to Bacterial Diseases of Plants* ¹²⁹] are more dependable as

¹²⁶ *Idem*, 411-412.

¹²⁷ *Idem*, 422-426.

¹²⁸ Synopsis of researches, *op. cit.*, 29-30.

¹²⁹ *Op. cit.*, 160-176.

to morphology and cultural characters than those in Volume III of [*Bacteria in Relation to Plant Diseases* ¹³⁰]. The organism is non-motile (Lucia McCulloch) and only feebly active on potato starch. . . . The writer obtained the first convincing infections with pure cultures in 1902, making his inoculations by placing the organism on the tips of leaves extruding water, i. e., in the seedling stage and transplanting to the field some weeks later.

His "Completed proof that *P. stewarti* is the cause of the sweet corn disease of Long Island" was announced ¹³¹ in 1903. Further research under his supervision by Miss Hedges, Dr. Rand, and other laboratory assistants, work extending over many years, established that the disease is transmitted on the seed, appears conspicuously one to two months after planting, and, among other points, that infections are stomatal and probably also by way of water pores. About 1923, after Dr. Smith's text book and his evaluation of his own work on this plant disease had been published, Dr. Rand investigated the insect-transmission of the malady by two species of beetles. This will be considered in Chapter X.

At the beginning of the century, Dr. Smith's reputation as a scrupulously careful, conscientious worker, one who went about his work with endless enthusiasm, was well established. That he strove tirelessly to verify and re-verify every important conclusion was known in many places.

Scientists at his alma mater were proud of him and his work. In 1897 F. G. Novy thanked him for reprints and added:

I have often thought how you as a botanist and I as a chemist have drifted into the study of the "definitely little." Originally I took up bacteria to aid me in my work in physiological chemistry and now the latter is the handmaid of the former. 125 students in the laboratory in these two subjects at one time strongly handicap individual work.

The next year he wrote Smith to inquire whether the Department of Agriculture had "published a summary of *known bacterial Diseases of plants*" and for estimates of the annual losses to American farmers from each malady.

Smith was also proud of his friendship with Novy. His demonstration in 1903 with McNeal of the University of Michigan that

¹³⁰ *Op. cit.*, 89-147.

¹³¹ *Science* 17(429): 457, Mar. 20, 1903. Vervollstaendigung des Beweises, dass *P. Stewarti* die Ursache der Suesskornkrankheit auf Long Island ist, *Centralblatt f. Bakt.* etc., II Abt., 10(22/23): 745-746, 1903.

Trepanosoma lewisi from the blood of a rat and *T. brucei* from the blood of a dog brought from Africa could be cultivated on blood agar very much advanced the study of protozoan blood parasites. Smith¹³² classed this in point of time and comparable importance with Negri's discovery of "bodies" present in hydrophobia in the ganglion cells, with DeSchweinitz's and Dorset's proof that hog cholera is due to a filterable virus, and he ranked Novy's and McNeal's work next in order, chronologically; to Welch's and Nuttall's description at Johns Hopkins of *Bacterium welchii*, "their anaerobic gas oedema bacillus . . . type of a group causing deadly wound-diseases."

In November 1897 E. O. Jordan, Assistant Professor of Bacteriology at the University of Chicago, T. M. Prudden of the department of pathology at the College of Physicians and Surgeons, and E. C. Jeffrey of the biological department of the University of Toronto, each thanked Smith for a copy of his paper on *Pseudomonas campestris* and each found its contents interesting. On November 18 T. J. Burrill of the University of Illinois congratulated Smith, and from him congratulations were a coveted honor among plant scientists: "Please accept my thanks for your *Pseudomonas campestris* paper. I have read the article with much interest both on acc[oun]t of the subject matter and as an illustration of good work well recorded."

In 1902, after Smith's address, "Plant pathology: a retrospect and prospect," given as president of the Society for Plant Morphology and Physiology, had been published, America's pioneer plant bacteriologist paid Smith a higher tribute:

You have put things together and made a presentation of the subject so that the position of American workers, individually and as a body, is for the first time set forth. The progress which you show to have been made is perhaps not wonderful when the conditions are understood, but it seems to me that it is at least very creditable to those engaged in the work. If this is true, I may add that to no one is the forward movement due, and the presentation of the same in print, more directly traceable than to yourself.

Burrill sent his "best wishes for [Smith's] future endeavors." Roland Thaxter of the Farlow Herbarium of Harvard was "greatly obliged [for Smith's] very interesting paper on *Pseudomonas*."

¹³² Fifty years of pathology, *op. cit.*, 31.

December 19, 1897, Smith addressed a very interesting letter of thanks to Rudolph Aderhold of Proskau, Germany, a letter which evidently he regarded so valuable that he saved a written copy:

I have to thank you for a very excellent review of my Bulletin No. 12 on *Bacillus solanacearum* in a recent number of *Bot[anische] Zeit[ung]*. My paper on *Pseudomonas campestris* was printed in 2 Abt. of *Centralbl[att] f. Bak[teriologie]* etc. this year but inasmuch as it is in English it is still for many German readers only "half published," just as German papers are for most American and Englishmen inaccessible on account of the difficulties of the language. On this account I would be very glad if you could also find time to review this paper for *Bot[anische] Zeit[ung]*. Perhaps you would like to do this in connection with a second paper on the subject which has just gone to press, and a printed copy of which I shall hope to send you early in January. This is a Farmers' Bulletin to be issued by the U[nited] S[tates] Dep[artment] of Agric[ulture]. It is not a digest of the preceding paper but deals principally with methods of prevention and is based on discoveries made this summer while I was studying the disease in the field.

Your paper on *Die Fusicladien unsere Obstbäume* has interested me and I am now writing a review¹³³ of it for one of our journals. Along with the paper on the cabbage disease, I venture to send you copies of several other papers, bulletins, etc., in some of which you may, perhaps, be interested.

Many of Smith's foreign correspondents have been mentioned. In chronological order, some of them were Oskar Brefeld of Munich (1890-1892), F. v. Tavel of Zürich (1894-1895), H. Klebahn of Hamburg (1895-), E. Koehne of Berlin (1895-), Dr. John Bolle of the Imperial and Royal Agricultural and Chemical Experiment Station in Görz, Austro-Hungary (1894 or 1896-) C. A. J. A. Oudemans of Amsterdam, Netherlands (1896-), W. Migula of Karlsruhe, Germany (1896-), Wladislaw Rotherth of Kazan, East Russia (1896-), Dr. A. N. Berlese of Milan, Italy (1897-), and the number increased after this year. It is likely that Smith received letters from other foreign sources. In 1895 he started a "Ledger" containing the names of all his correspondents, both in North America and abroad, and the reprints sent to each. By the end of the century three volumes were required to incorporate the data. Foreign scientists with whom he began to correspond in 1895 were:

¹³³ See, E. F. Smith, Perithecial stage of the apple-scab fungus, *Amer. Nat.* 29 (342): 583-584, June 1895; Fifty years of pathology, *op. cit.*, 27 (in this Smith spoke of Aderhold's work as between 1896 and 1905).

- Th. W. Engelmann, Professor of Histology of the Physiological Laboratory of Utrecht, Netherlands.
 Dr. M. W. Beyerinck of Delft, Netherlands.
 Dr. Alfred Fischer of the University at Leipzig, Germany, later Basel, Switzerland.
 Dr. Alfred Möller of Eberswalde, Germany.
 Dr. Hans Solereder of the University of Munich, Germany.
 Dr. Karl Tubeuf of the University of Munich, Germany.
 Professor Eugene Warming of the University of Copenhagen, Denmark.
 Professor H. Graf zu Solms-Laubach of the University of Strassburg, Germany.
 Dr. Max Gonnermann of Danzig and Rostock, Germany.
 Dr. Martin Möbius of Heidelberg, later Frankfurt, Germany.
 Charles Joly of Paris, France.
 W. T. Thistleton Dyer of Kew, England.
 Dr. Antonio Berlese of Portici, Italy.
 B. L. Ravaz of France.
 Dr. Henry Tryon, Brisbane, Queensland, Australia.
 Dr. George Hieronymus, editor of *Hedwigia*, of Berlin, Germany.
 Professor Gaston Bonnier of the Sorbonne, Paris, France.
 Dr. P. A. Saccardo of the University of Padua, Italy.—the following, 1896 and after.
 E. Krober of Germany. Johann Bachmann of Germany.
 Dr. Jacob Eriksson of Albano, near Stockholm, Sweden.
 Dr. A. Zimmermann of Buitenzorg, Java.
 Dr. W. J. Behrens of Göttingen, Germany.
 Dr. N. A. Cobb, pathologist of the Department of Agriculture of Sydney, New South Wales, Australia.
 Professor M. C. Potter, The Durham College of Science, Newcastle-upon-Tyne, England.
 Maxwell T. Masters, editor *Gardeners Chronicle*, London, England.
 Burri and Stutzer of Germany (Breslau?).
 Dr. Charles Schilberszky of Budapest, Austro-Hungary.
 Dr. G. Lindau of Berlin, Germany.
 Carl Wehmer of Hannover, Germany.
 Dr. F. Ludwig of Greiz, Germany.
 Dr. Oscar Kirchner of Stuttgart, Germany.
 Dr. Karl Goebel of Munich, Germany.

Among the earliest of his foreign correspondents had been the Italians, Peglion, Savastano, and Cavara, and the German, Paul Sorauer. By 1897 the names of most of the important figures in plant pathology were on his lists. The early students of plant bacteriology were included, and many, many American and European botanists, especially those interested in plant physiology. Woronin of Russia was an early correspondent. So was Francis

Darwin, Professor of Physiological Botany at the University of Cambridge, and H. Marshall Ward of the same great place of learning. Each year new names were added. Among the Englishmen by 1898 were J. Bretland Farmer, Professor of Botany at the Royal College of Sciences, South Kensington; Edgar M. Crookshank, Professor of Comparative Pathology and Bacteriology, and fellow of Kings College, London; Alfred J. Ewart and Percy Frankland of Mason College, Birmingham. Among other Germans were Vöchting of Tübingen; W. Busse of Berlin; L. Kny of Wilmersdorf bei Berlin; K. B. Lehmann of Würzburg; A. F. W. Schimper of Bonn and later Basel, Switzerland; Oscar Loew, then of Germany and later Japan; Rudolf Arendt of Leipzig; A. Meyer and F. G. Kohl of Marburg; and O. Uhlworm. L. Errera was from Brussels, Emerich Ráthay from Vienna, and J. Costantin from Paris. F. Noack was a Brazilian. Correspondents extended to India, Java, Australia, Japan, and other countries. These were scientists to whom Smith sent his papers,¹³⁴ and many of them began to write to him.

American teachers of bacteriology more and more requested either his literature on plant bacterial diseases or cultures of the bacteria he had studied. In 1898, for example, Mazýck P. Ravenel of the University of Pennsylvania wrote Smith for literature on this subject and root bacteria and their function in the fixation of nitrogen. Charles Edward Marshall, bacteriologist of Michigan Agricultural College, wrote for cultures. "Some of my students would like to make a study of the bacteria" of plants, he said.

On December 24, 1900, Dr. Welch invited Smith to dine with him three evenings later at the Maryland Club "to meet members of the Bacteriological Society." Just a year past, at the Yale Medical School, the Society of American Bacteriologists¹³⁵ had been organized as an affiliate of the American Society of Naturalists, and its second annual meeting was being held in the pathological laboratory of the Johns Hopkins Hospital. W. T. Sedgwick was president of the society and on the evening of December 27 he gave his address on "The origin, scope and significance of bacteriology." On the same day, at the medical

¹³⁴ Based on Smith's "Ledgers."

¹³⁵ C.-E. A. Winslow, The first forty years of the Society of American Bacteriologists, *Science*, n. s., 91(2354): 125-129, Jan.-June 1940.

school, the fourth meeting of the Society for Plant Morphology and Physiology also took place, and among the important papers were H. J. Webber's on "Loss of vigour in corn from inbreeding," H. von Schrenk's "A disease of the locust," and A. F. Woods's on "The mosaic disease of tobacco." These were followed by papers by George T. Moore of Dartmouth College on "Improved methods for obtaining pure cultures of fresh-water algae," B. M. Duggar and F. C. Stewart on "A second preliminary report upon plant diseases in the United States due to *Rhizoctonia*," and M. A. Carleton on "Notes on the life history of certain Uredineae."

A joint session of the Society for Plant Morphology and the Society of American Bacteriologists was held at noon in the Pharmacological Lecture Room. Erwin F. Smith, member of both societies, presented a lecture, illustrated by stereopticon slides, on "The bacterial diseases of plants." *Science*¹³⁶ noticed the lecture:

Three diseases were described, namely, the wilt of cucurbits due to *Bacillus tracheiphilus*, the brown rot of solanaceous plants due to *Bacillus solanacearum*, and the black rot of cruciferous plants due to *Pseudomonas campestris*. Fifty-eight slides made from the author's clear and beautiful photographs and photomicrographs were exhibited, showing symptoms, location of the bacteria in the tissues, etc. Many of these illustrations will be published in the near future in *Centralblatt für Bakteriologie*.

On December 28, at the business meeting, Smith was elected president of the Society for Plant Morphology and Physiology for the ensuing year. F. C. Newcombe and L. M. Underwood were elected vice-presidents, and W. F. Ganong was continued as secretary.

Woods's paper on tobacco mosaic¹³⁷ was important because of his theory that the disease was due to an inhibitory action upon starch hydrolysis and translocation, induced by an excess of oxidizing enzymes.¹³⁸ Mayer had proven the malady's transmissibility. Iwanowski had transmitted the disease with filtrates of diseased leaves, and thus offered to science the first proof of a filtrable invisible virus,¹³⁹ and Beijerinck had confirmed his filtration experi-

¹³⁶ N. s., 13(320): 249, Feb. 15, 1901.

¹³⁷ *Science*, n. s., 13(320): 247-248, Feb. 15, 1901.

¹³⁸ *Phytopathology* 5(2): 85, Apr. 1915 (statement based on Ralph E. Smith's condensation of Woods's theory).

¹³⁹ Gustav Seiffert, *Virus diseases in man, animal and plants*, translated by Marion Lee Taylor, 263, N. Y., Philosophical Library, 1944; E. F. Smith, Fifty years of pathology, *op. cit.*, 20, 21, and 27.

ments. Woods believed that "under certain conditions" tobacco mosaic was infectious, and he suggested that his explanation might extend to peach yellows, the California vine disease, and die back of the orange. Smith the next year¹⁴⁰ spoke of

a whole group of diseases, the etiology of which mere field study and the ordinary laboratory methods do not appear to be competent to unravel; for example, the California (Anaheim) vine disease, the wilt of the orange, the serch disease of the sugar cane, gum diseases, the yellows and rosette of the peach, the winter blight of the tomato, the internal brown spotting of potato tubers, etc. . . . A good beginning on this class of diseases has been made by Beyerinck and Woods in the study of the Mosaic disease of tobacco.

He was confident that "these obscure diseases will yield up their full etiology to careful study at some time in the future."

At the Baltimore meeting (1900) of the Society of American Bacteriologists, Smith presented experimental data tending to establish that in bacteriological culture work the practice prevalent then of using thymol and chloroform as antiseptics and germicides was of limited value. He called attention to the fact that twelve micro-organisms were known which grew readily in test-tube cultures of milk or beef bouillon to which an equal volume of chloroform had been added. Two organisms grew readily in beef bouillon to which thymol had been added.¹⁴¹ In discussing some years later in his first volume of *Bacteria in Relation to Plant Diseases* the subject, "Reaction to Antiseptics and Germicides," he said:

Some organisms will grow in a solution saturated with thymol (e. g., in bouillon). Others will grow in the presence of chloroform (5 cc. of chloroform in test-tubes with 10 cc. of milk or beef-bouillon). Ten organisms have been found by the writer which, under the conditions named, grew in the presence of chloroform and two which grew vigorously in the presence of thymol. Russell reports one capable of growing in the presence of sulphuric ether. It is, therefore, not always safe to depend on these substances as antiseptics. Newcombe has made the same observation (*Cellulose Enzymes*, *Annals of Botany*, Vol. XIII, 1899, p. 60). In the opinion of the writer the statements of physiologists respecting the

¹⁴⁰ Plant pathology: a retrospect and prospect, *op. cit.*, 611 (Smith's presidential address before the Society for Plant Morphology and Physiology, fifth meeting, January 1, 1902, held at Columbia University).

¹⁴¹ *Experiment Station Record* 13(2): 114; also *Science*, n. s., 13(322): 327, 1901; also, *Jour. Boston Soc. Med. Sci.* 5: 375; also *Centralb. f. Bakt.*, 1901, 1st Abt., 29: 445-446.

existence of enzymes in the tissues and fluids of the higher plants and animals must be taken with much allowance when chloroform, thymol, and similar antiseptics have been depended upon to keep the solutions free from bacteria. This has been the case very frequently, and in several places in Greene's interesting book on Fermentations, published in 1899, it is said or inferred that the addition of chloroform will prevent the growth of bacteria. This might or might not be true; much would depend on the kind of organisms present. The medium to which chloroform or thymol has been added must be shut in and shaken continuously if the full antiseptic value of these substances is to be obtained.¹⁴²

Alexander C. Abbott, director of the Laboratory of Hygiene of the University of Pennsylvania, heard Smith's paper at Baltimore and much interested in the communication placed a student in his laboratory at work on the subject. Since 1897 Abbott had been receiving Smith's publications. Indeed, since 1895, B. Meade Bolton, now bacteriologist of the Philadelphia Board of Health, had also been receiving reprints, and in that year, 1895, in the *American Naturalist*,¹⁴³ Smith, under the title, "Bactericidal Action of Metals," had reviewed Bolton's published study from the *International Medical Magazine* (December 1894), "The effects of various metals on the growth of Bacteria." Some of the metals tested were copper, brass, silver, gold, magnesium, zinc, cadmium, mercury, bismuth, nickel, and platinum. The bacteria were any or all of *Staphylococcus pyogenes aureus* and the colon, typhoid, cholera, and anthrax bacilli.

January 19, 1898, William Royal Stokes of the pioneering municipal health department of Baltimore thanked Smith for a copy of "The Black Rot of Cabbage" and added, "I always read your articles with great interest and envy you as working in a field where the practical results are always of much great importance. I shall send you several reprints of some work of ours as soon as they arrive."

In 1896 Dr. Simon Flexner of Welch's pathological laboratory of Johns Hopkins had answered an inquiry from Smith for galactose, and the next year Smith began sending to Flexner, Dr. W. S. Thayer, House Physician of the Hospital, as well as Drs. Hurd and Welch reprints of his publications. To E. B. Shuttleworth, bacteriologist of Trinity College at Toronto, J. J. Mackensie,

¹⁴² *Op. cit.*, 74-75.

¹⁴³ 29(346): 933-936, Oct. 1, 1895.

bacteriologist of the Provincial Board of Health of Ontario, Dr. Wyatt Johnston, bacteriologist of the Quebec Board of Health, and, very important, to Dr. J. George Adami, professor of pathology at McGill University, Montreal, he also sent reprints; likewise, to Dr. T. M. Cheesman, instructor in bacteriology at the College of Physician and Surgeons, to J. Christian Bay of the Iowa State Board of Health, and in 1898 to Dr. Jacques Loeb of the Physiological Laboratory of the University of Chicago. Among Washington correspondents to whom copies were sent were Dr. Charles Smart, deputy surgeon-general, and Dr. J. J. Kinyoun of the bacteriological laboratory and Marine Hospital service of the government. Many entomologists, horticulturists, and, of course, many, many botanists received Smith's scientific literature. To list the botanists would probably require repeating the entire distinguished membership of the Botanical Society of America. Not unlikely, therefore, many members of the newly formed Society of American Bacteriologists were familiar with the principal intention of Smith's botanical work during the years 1896-1898. As an American leader in plant pathology,¹⁴⁴ he was accepted into membership in the Society. Many of the early members had been his friends for several years: Sedgwick, Abbott, Conn, Fuller, Jordan, Prescott, Adami, Johnston, Prudden, and especially Vaughan, Sternberg, Welch, Moore, and Theobald Smith.

At the dinner of December 27 to which Dr. Welch invited him, Smith saved a memento which indicates that he sat with Theobald Smith, Flexner, Jordan, Harold Clarence Ernst, Edward Kellogg Dunham, and one or two others. Dr. Welch was elected president of the Society and Dr. Jordan vice-president. Little must Smith have then realized how interested many of these men would become in his work when, during the next decade, he would institute elaborate studies of crown gall in plants, and reason, with authentic proof, an analogy to cancer in animals and man.

On December 9, 1897, Dr. Thomas B. Carpenter, assistant bacteriologist of the Department of Health at Buffalo, had thanked Smith for his reprints on bacterial diseases of plants and written for "other studies." He was a doctor of medicine and probably was acquainted with Dr. Roswell Park,¹⁴⁵ Professor of Surgery

¹⁴⁴ C.-E. A. Winslow, The first forty years of the Society of American Bacteriologists, *op. cit.*

¹⁴⁵ Charles G. Stockton, M. D., Roswell Park: a memoir, *Buffalo Hist. Soc. Publ.*

at the University of Buffalo and for many years surgeon-in-chief of the Buffalo General Hospital. On May 1, 1898, Dr. Park also wrote Smith:

I wish to thank you sincerely for your kindness in placing our school on your mailing list, etc. I am happy to be able to state that our School has just received an appropriation of \$10,000 for equipping and maintaining a "Laboratory for the study of Cancer," the first of its kind in the world.

Dr. Park told Smith that he was "anxious not only to study 'cancer' in trees [plants] etc., but [also to] collect the literature of the subject"; and he bespoke their future interest in mutual assistance which would "redound to our common benefit." "One great aim of [Dr. Park's] life," Dr. Stockton has revealed¹⁴⁶ was "to know the nature of cancer. . . This led to the establishment, first in the University of Buffalo, of the Gratwick Laboratory, which became in 1911 the New York State Laboratory and Hospital for the Study of Malignant Disease."

In 1893 Dr. Park had published on "The Parasitic Theory of the Etiology of Carcinoma."¹⁴⁷ In 1889, at the Cleveland meeting, a collection of fifty-four cultures of bacteria and moulds on agar-agar prepared by him had been exhibited to the Botanical Club of the American Association for the Advancement of Science.¹⁴⁸ He must have known of Smith's early studies of stem and root tumors of cultivated tree and vine plants. The two men, interested in comparative pathology, began to exchange letters on occasions. When later Smith's more important studies on the etiology of plant tumors began to acquire significance in working out problems in the etiology of animal tumors, Dr. Park classed his work among those from whom medical science might expect "light upon the general subject of cancer."¹⁴⁹ The story of how and why this became so will form a theme throughout the remainder of this book.

22: 91-123, with bibliography. Paper read at a meeting of the Society on April 13, 1916.

¹⁴⁶ *Op. cit.*, 108.

¹⁴⁷ *New York Med. Jour.*, March 4; *Trans. of Med. Soc. of State of N. Y.*, 1893; *Med. and Surgical Reporter*, Feb. 18.

¹⁴⁸ *Botanical Gazette* 14: 263, Oct. 1889.

¹⁴⁹ Letter from Dr. Park to Smith, July 15, 1912.

CHAPTER VIII

SMITH CHIEF OF A LABORATORY OF PLANT PATHOLOGY. RECOGNITION OF PLANT BACTERIOLOGY IN EUROPE.

DURING the decade 1897-1906 many plant pathologists, entomologists, and animal pathologists were appointed at various experiment stations.¹ One of the strongest centers for the study of plant diseases was that developed by L. R. Jones at the University of Vermont. He was offered a position with the New York Agricultural Experiment Station at Geneva but he chose to remain at Vermont where as botanist of the experiment station and professor of botany in the university he had been contributing since 1890 important studies in plant pathology.² In 1898 he wrote Smith that he believed his opportunity there "unequalled among our Colleges and Stations." He was to have a "leave of absence for study and investigation" of one-half of every other school year. After considering advanced study at Harvard or in Germany, he accepted Smith's offer to spend his first leave in his laboratory. "Plant pathology and plant physiology are what I am most after," he wrote Smith on April 11, 1898, and he asked for "pure cultures of some of your bacteria, pathogenic to plants especially your *Bacillus solanacearum* and *Pseudomonas campestris*, also *Bacillus amylovorus* if at hand. I should be glad of any others which you can send of European or American origin." During September he and his assistant, William A. Orton, sent Smith plant disease specimens: an "aster trouble" suggesting "the malformations you figure as accompanying peach rosette and yellows," and "some corn sheaths showing blotches which are, I judge," he said, "the thing which Burrill attributed to *Bacillus Serghi*. . . . Have you worked over this matter sufficiently so you are satisfied as to the correctness of Burrill's conclusions regarding the bacterial nature of the disease?"

Dr. Smith's "Record Book of Culture Media" shows that Jones

¹ Fifty years of pathology, *op. cit.*, 28.

² G. W. Keitt and F. V. Rand, Lewis Ralph Jones 1864-1945, *Phytopathology* 36(1): 3, 4, and bibliography, 11-17, Jan. 1946.

began to study in his laboratory no later than February 7, 1899, and remained there at least through June 12. Until the first week in March he worked mainly on preparing culture media. He then began a research, evidently his own choice, on the bacterial soft rot of vegetables. On March 9 he began cutting cylinders from roots of turnips, rutabaga, carrots, sweet potato, beet, radishes, later also asparagus and onion. Each material, each staining, addition of materials, filtration, transfer, steaming, testing with litmus and other tests, each result was described with the care and precision which characterized Smith's work. In one or another of the laboratories of the Division, at informal luncheons, and seminar meetings, members of the Division and guests discussed problems of plant physiology and pathology. Dr. Woods has revealed that the "discussions were chiefly along bacteriological and physiological lines, and Jones was one of the leading spirits. He was looked upon by the Department as a prize visitor. He brought to the group the university point of view. Though he came for further bacteriological training and experience as well as research, he gave fully as much as he got. We were proud to have him with us."

Out of these preliminary investigations grew "Jones' classical papers describing *Bacillus carotovorus* and showing the mechanism of its incitation of rotting by production of the enzyme, pectinase, which dissolves the middle lamellae of the cell walls of the host plant.³

Dr. Smith's "Record Book of Culture Media" further shows that by September 21, 1899, William A. Orton, who had graduated in science two years before at the University of Vermont and while earning his degree master of science served as Jones's assistant, was in Smith's laboratory. September 21, the entry which appears to have been written by him was entitled, "Nutrient Agar"; September 25, "Beef Bouillon"; October 2, again, "Beef Bouillon"; evidently, however, he had been employed for a purpose other than laboratory research. Since 1895, when Smith at Charleston, South Carolina, had observed cotton seed selection practiced by plantation owners to prevent crop deterioration, he had visualized this as bound up with possible remedial measures to combat fusarium-caused diseases. He, in his address, "The fungous infes-

³ Keitt and Rand, *op. cit.*, 5.

tation of agricultural soils in the United States,"⁴ read August 25, 1899, before the Botanical Section of the American Association for the Advancement of Science, referred to the careful cotton breeding practice of the Sea Island growers. While he did not recommend that seed selection be experimentally investigated by scientists to determine whether the technique might be applied to breeding for disease-resistance, he later disclosed in an address, "Plant Breeding in the United States Department of Agriculture,"⁵ presented before the third International Conference on Genetics, that he realized that an assistant was needed to find "a satisfactory remedy for the widespread" disease of cotton caused by the "little fungus known as *Fusarium*," and

picked out for this responsible post Mr. William A. Orton, then a recent graduate of the University of Vermont, who subsequently obtained most brilliant results in overcoming the ravages of this disease by means of selection. I might add in passing that Mr. Orton had never seen a cotton field until he went South on this perplexing and seemingly well-nigh hopeless mission. Very little was accomplished the first year. I well remember a notable conference with Mr. Orton at the close of the first season's work, when he was thoroughly discouraged and expressed himself as feeling that the whole year had been wasted. I cheered and encouraged him as best I could and advised him to continue. The key to the situation was obtained the next year. Subsequently the work was carried out as follows, Mr. Orton receiving great assistance from some of the growers, particularly from Mr. Rivers, on whose plantation the very resistant "Rivers Cotton" originated.

In fields much subject to this disease it was observed that here and there a plant came to maturity and bore fruit. The seeds were selected from these unusually resistant plants, and the following spring they were planted on land subject to the disease. Many of the resultant plants contracted the disease, but a considerable proportion remained free from it or practically free. Selections this year were made from the most resistant plants, having an eye to obtaining plants with other good qualities, such as productivity, shape of boll, length of fibre, &c. The same method was pursued the following year. In the course of four years plants were obtained with good productivity, good quality of fibre, and marked resistance to disease. Such plants stood up and bore a good crop on fields where the ordinary cotton made a total failure. . . .

Mr. Orton has also had charge of the work of obtaining resistant varieties of watermelons to replace varieties much subject to a soil disease which

⁴ *Scientific American Supplement*, no. 1246, *op. cit.*, 19981, Nov. 18, 1899.

⁵ Reprint, from *The Report of the Conference on Genetics*, by permission of the Council from the Royal Horticultural Society's Report, 301-303, London, Spottiswoode & Co., Ltd., 1907.

I was able to demonstrate to be similar to the cotton disease, i. e., due to a soil *Fusarium*. There are large areas in the United States (parts of Carolina, Georgia, Florida, and Texas) where this watermelon disease has prevailed to such an extent that the growing of melons on a commercial scale has been abandoned. . . . The extent of infection in the melon rendered it practically impossible to obtain any resistant plants by the process applied to cotton, namely, by simple selection. Mr. Orton found, however, that a plant known as the "citron" in the United States—that is, a vine with deeply lobed leaves and a hard, striped, roundish fruit, not unlike the watermelon, but inedible until cooked, when it is used for sweet pickles and preserves—was quite resistant to the disease. He used this plant for one of the parents and good varieties of watermelons much subject to the disease for the other, making a number of crosses. The seeds from these crosses when planted gave rise to about a thousand varieties of melons. There were all sorts of fruits—long and short, round and crooked, smooth and rough, plain, deep and pale green, and variously mottled and striped. Of the thousand or more varieties which resulted from these crosses, quite a good many proved resistant to the soil fungus, but only about six varieties had other qualities such as to make them worthy of further consideration. The seeds from these six plants were saved and planted the following year on land much subject to the disease in order to test on a large scale the qualities of the melons, and to learn more respecting their resistance to the disease. All continued to be resistant, but only one of the six proved to be a commercially satisfactory melon. The following year, therefore, only this one variety was propagated, but on a large scale and with excellent results. . . .

Orton's employment with the United States Department of Agriculture had begun on June 1, 1899. Arriving early in July at Edisto Island, South Carolina, he soon reported to Galloway that he had been spending his time investigating "the circumstances attending the spread of the cotton wilt, the gathering of information regarding this and other plant diseases, and the inauguration of some preliminary experiments in checking the disease." This letter was written from James Island, and on August 4 Orton thanked Galloway for "information regarding application of lime and sulphur" and announced from Monetta, South Carolina, "We have a field selected for experimental work with lime and will start the work this fall." He, too, found that the cowpea was susceptible to the disease and he was studying other crops. August 22, however, he informed Galloway:

I believe that I shall within the next week have done all the field work which can be profitably done here this season, and shall be ready to return

to Washington before the end of the month unless you have other plans to be carried out.

It would give me great pleasure to have an opportunity to visit the Alabama Experiment Station as you suggested in your letter of the 8th, but in case you should wish me to go there, I would like to know as much as possible about the experiments which are to be carried out there, in order to do the work properly.

By September-October, and probably some while thereafter, Orton was again in Washington and worked at least part of his time in Smith's laboratory. That he advised Smith as well as Galloway of his progress in South Carolina with reference to originating varieties of plants resistant to fusarium-caused disease was shown by his letter to Smith written on April 3, 1900, at Edisto Island, South Carolina:

I have just come here today from James Island where I had good luck in starting my experiments. . . . I find conditions here very interesting. There is a marked increase in the amount of cotton wilt everywhere, but the prospects of securing a resistant variety are far more encouraging than I hoped for, as we have a considerable number of such plants from which to make selections.

In his address, "Fifty Years of Pathology,"⁶ Smith concisely summarized the significance of these and another set of researches having the same fundamental intention:

Bolley, Arthur's student, studied in 1901 the flax *Fusarium* in North Dakota, and afterwards flax diseases in Europe, with money from my fund, and W. A. Orton, working at first in my laboratory and afterwards independently, overcame the *Fusarium* diseases of melons and cotton in our southern states by hybridization and selection (1889-1906).

Already in this book we have referred to Bolley's pioneering plant pathological work in North Dakota. In the autumn of 1890, visiting at the home of Dr. Otto Lugger at the Farm School and Experiment Station, St. Anthony Park, Minnesota, Bolley's attention was called to a destructive "blight" of flax which required careful study. At once he started

structural and mycological and bacteriological studies based upon the flax plant, its seeds and soils upon which infection occurred. But slight progress was made during a number of years until the use of the physician's centrifuge was applied to the sedimentation of washings from flax-seed

⁶ *Op. cit.*, 27-28.

in the spring of 1900. Certain conidial spores were quite uniformly observed in the sediment from samples examined. These were none other than the spores of a fusarium often previously observed upon the dead roots and stems of wilted, dead, or dying flax plants and upon harvested flax lying unprotected in the fields. A few days sufficed to produce distinctive cultures upon agar and later but a few weeks were necessary to procure pure cultures from the interior of the fibrovascular bundles of wilting but living plants, and to prove the pathogenic nature of this fusarium to flax seedlings by pure cultures applied to sterilized and virgin soil (Bulletin 50). In June of 1900, the regular rotation plot 30 of the department of agronomy was assigned to the department of plant pathology. . . .⁷

July, 1900, Bolley took from "Plot 30" his first pure culture of the flax wilt fungus, *Fusarium lini*,⁸ and completed his infection experiments in field and laboratory for purposes of his Bulletin 50 in 1901.

In 1903 he was sent to Europe by the college and the Department of Agriculture. At various regions of the continent he examined flax diseases, studied their origins, and secured new types of flaxseed for cropping purposes in North Dakota and the northwest. Bolley's bulletin 55, "Flax and flax seed selection," was published that year. Wilt-resistant flax varieties, originated on "Plot 30" during the next years, included N.D.R.-52, N.D.R.-114, and Bison, Buda, and N.D.Golden.⁹

By 1904 Smith, with Deane B. Swingle of his laboratory, would publish on "The Dry Rot of Potatoes due to *Fusarium oxysporum*."¹⁰ To his last years the senior author believed this to be the "first good paper on this widely prevalent potato disease." Unfortunately, an old name, *Fusarium oxysporum* Schlechtendal, "practically a *nomen nudum*," Smith said, was applied to this fungus. But by the bulletin attention was called to

this black ring disease of the potato tuber which was then a new disease, at least to scientific men, and while infections were not undertaken the *Fusarium* was demonstrated to be the only organism constantly present

⁷ Foreword, *Fungi of flaxseed and of flax-sick soil*, Bull. 259, N. D. Agric. Exp't Sta., 2, June 1932. See also, *North Dakotan* 19(6): 7, June 1944. Also, H. L. Bolley, Flax wilt and flax sick soil, Bulletin 50, N. D. Agric. Coll. and Exp't Sta., 36.

⁸ H. L. Bolley, Flax wilt and flax sick soil, *op. cit.*, 37 ff. T. F. Manns, Historical sketch, *Fungi of flaxseed and of flax-sick soil*, *op. cit.*, 3 ff.

⁹ H. L. Bolley, Flax cropping, *Bimonthly Bulletin*, N. D. Agr. Exp. Sta. 3(6): 9-12, at p. 11, July 1941; Harry R. O'Brien, Plants that resist disease, *The Country Gentleman*, Feb. 5, 1921; *Yearbook U. S. D. A.* for 1936: 745-784.

¹⁰ Bulletin 55, U. S. Dep't of Agric., Bur. of Pl. Indus., February 16, 1904.

in the diseased blackened vascular bundles and in the light of the many successful inoculations previously obtained on melon it was stated to be the parasite (a conclusion since confirmed by the experiments of others).¹¹

Still later, Smith would establish a causal connection between *Fusarium cubense* EFS and a destructive banana disease of the West Indies.¹²

L. R. Jones, after his half-year of study in Smith's laboratory, had to devote most of the remainder of 1899 to university matters. On July 1 he wrote to Smith, that he was "beginning to think about bacteria and other work again. . . . I am glad that you have decided to print the 'Outline [of Methods.]' Shall try to secure several copies for my students to wear out." During December he wrote that, while he had prepared a paper on *Bacillus carotovorus*, he was withholding publication until after further culture study had been completed. He was moving into his new laboratory which he expected "to use for both plant physiology and bacteriology," three rooms with adequate storage space. During the school vacation, he worked over his cultures and compared organisms obtained since his work on "soft rot of cabbage and soft rot of stored celery. The most active organism I have yet found associated with cabbage rot," he said, "is remarkably similar to the carrot rot organism and may prove to be identical with it." He sent Smith some agar tubes for transfers from some of the carrot rot tubes left at the latter's laboratory.

Jones attributed his election to the Society for Plant Morphology and Physiology in part to Smith, and thanked him for his recommendation. Either then or later Smith verified most of Jones's statements on *Bacillus carotovorus*.¹³ In 1902 he described¹⁴ the Vermont botanist's paper on soft rot of the carrot and other plants¹⁵ as "one of the best" of the recent papers on plant diseases due to specific bacteria. It was this year that Spieckermann

¹¹ Synopsis of Researches, *op. cit.*, 21-22; H. W. Wollenweber, Studies on the fusarium problem, *Phytopathology* 3(1): 24-50, Feb. 1913.

¹² Fifty years of pathology, *op. cit.*, 32. Erwin F. Smith, A Cuban banana disease, *Science* 31(802): 754-755, May 13, 1910.

¹³ Synopsis of researches, *op. cit.*, 41.

¹⁴ Plant pathology: a retrospect and prospect, *op. cit.*, 607; also, Fifty years of pathology, *op. cit.*, 27.

¹⁵ L. R. Jones, *Bacillus carotovorus* n. sp., die Ursache einer weichen Fäulnis der Möhre, *Centralblatt f. Bakt.* etc., II, 7(1): 12-21; (2) 61-68, 1901. See Smith's literature citations and chapter VI, *Intro. to bact. dis. of plants*, *op. cit.*, 223-252.

in Germany also published a paper on the bacterial soft rot of vegetables.¹⁶ Another important event of 1902 was Zimmermann's find of bacteria in the leaf nodules of Rubiaceae in the East Indies.¹⁷

With the advent of the twentieth century, the literature of plant pathology was growing abundant and extensive bibliographies accompanied important articles and publications. Annually the Department of Agriculture in its *Yearbooks* published lists of plant diseases throughout the nation. Not often, at least not with the frequency of later years, did the phrase "bacterial diseases of plants" appear in literature. During 1898-1899 American pathologists and bacteriologists were still discussing details of proof and priorities of discovery of several organisms in plant diseases believed caused by bacteria. Smith encountered many of the rivalries and disagreements while preparing his critical reviews of bacterial diseases of plants for the *American Naturalist*. Such severe charges as that he was attempting to preempt the field, that he regarded his opinions as "apparently final," that his attitude toward other workers was ungenerous, influenced him finally to relinquish these articles in favor of preparing over many years his three volume *Bacteria in Relation to Plant Diseases* (1905-1914). The *Botanical Gazette* in February 1893 had invited "a summarization of the present status of the subject," even without the aid of critical laboratory study. Miquel and Cambier in their large treatise on bacteriology (1902) complained that the monography of the subject as to plant diseases had "regrettable lacunae" and that many of the species would have to be restudied completely.¹⁸ One prominent American plant scientist, encouraging Smith to continue with "what he [wanted] to and not confine his work to some narrow province," urged, "We should all hail with delight good work." He saw no reason why a "board of enquiry" should be necessary in plant pathology and bacteriology. The circumstances argued the need of a great laboratory of plant pathology in the federal Department of Agriculture.

Smith, in his studies, enlisted the aid of plant and animal path-

¹⁶ Beitrag zur Kenntnis der bakteriellen Wundfäulnis der Kulturpflanzen, *Landw. Jabrb.*, 31: 155, Berlin, 1902.

¹⁷ Fifty years of pathology, *op. cit.*, 28.

¹⁸ Preface, *Bacteria in relation to plant diseases*, *op. cit.*, 1: iii.

ologists. In 1898 Veranus A. Moore sent to him the results of his study of the "bacillus of the beet disease (*B. Betae*)" which Smith had left with him. The next year Jones furnished by request an opinion of a paper on "the beet organism . . . emanating from Dr. Arthur's laboratory." Sturgis's recent paper on "his *B[acillus] hortulensis* [appeared to be] interesting and a good painstaking study." During the summer and autumn of 1900 Jones again wrote Smith concerning his researches and his paper on *Bacillus carotovorus*. He completed part of his study at the University of Michigan under Professor Newcombe, and in 1901 the *Centralblatt* published his work. The year previously, H. A. Harding, bacteriologist of the Geneva, New York, agricultural experiment station, published, with bibliography, in the same publication¹⁹ the results of his study of the bacterial cabbage disease, "Die schwarze Fäulnis des Kohls und verwandter Pflanzen, etc."

Smith's bulletin 28, "The Cultural Characters of *Pseudomonas hyacinthi*, *Ps. campestris*, *Ps. phaseoli*, and *Ps. Stewarti* four one-flagellate yellow bacteria parasitic on plants,"²⁰ was published by the Division of Vegetable Physiology and Pathology on August 6, 1901. He enumerated and described "Other Species belonging to this Group": *Ps. juglandis* Pierce, "an economically serious disease in walnuts"; *Ps. vascularum* (N. A. Cobb), parasitic on sugar cane in Australia and elsewhere; *Ps. amaranti*, new species, occurring in Eastern United States on species of *Amarantus*, weeds in fields; and *Ps. malvacearum*, new species, parasitic on cotton (*Gossypium* spp.) Arthur's and Bolley's *Ps. dianthi*, isolated from carnations (*Dianthus* spp.), was listed, but no description offered, since it was "now believed to be purely saprophytic," a conclusion yet to be established by more thorough study of the cultural characters. As to each of this list Smith said that "knowledge of their cultural characters [was] more or less imperfect." Concerning the two new species, he placed most of his confidence on his description of *Pseudomonas malvacearum*. To this publication he later attributed the naming of the organism.²¹

His real study of the angular leaf-spot of cotton due to *Bac-*

¹⁹ *Centr. f. Bakt.* II, 6(10): 305-313.

²⁰ U. S. Dep't of Agric., Gov't Print. Office, *op. cit.*, 153 pp.

²¹ *Intro. to Bact. Dis. of Plants*, *op. cit.*, 339.

terium malvacearum EFS covered twenty years (1900-1920). Although the disease had been first described in 1891-1892 by G. F. Atkinson, Smith believed himself to have been "the first to reproduce the disease on leaves and bolls of healthy cotton plants (1900-1905) with the true parasite isolated from capsule-spots and leaf-spots. He was also the one who proved the stem-blight known as black-arm and the angular leaf-spot to be due to the same organism" (1905),²² and he showed that "the cotton gummosis of Asia Minor and the leafspot of South Africa are due to it."²³ In 1920, in his text-book, *Introduction to Bacterial Diseases of Plants*,²⁴ was published his most complete account of the disease and its organism, and of the literature on the subject.

In 1898 Smith arranged that a request for samples of gummed sugar cane be sent by officials of the Department to Australia. At that time cultural materials were not available to him in this country, and several years, therefore, went by before he was able satisfactorily to restudy Cobb's conclusions as to *Pseudomonas vascularum*, a bacterium parasitic on sugar cane in Australia and elsewhere. In 1903 he obtained his "first clean cut pure culture inoculations . . . and subsequently worked out the cultural characters of the parasite."²⁵ His 1901 bulletin on the cultural characters of the yellow *Pseudomonas* group was a carefully prepared, comparative study and included a designation of this species as well as occasional references to other not closely related species such as *Bacillus amylovorus*, *B. coli*, and *B. carotovorus*. Indeed, his study of Cobb's disease of sugar cane was started in part to determine whether the "germ" (*Pseudomonas stewarti*) of Stewart's disease of maize, which he was studying in 1898, is the same organism.

In 1898, when Smith began to wage his epoch-making controversy with Dr. Alfred Fischer as to whether bacterial diseases of plants actually exist, he selected six "genuine plant parasites," the pathogenic nature of which had been established and reaffirmed by subsequent investigation: (1) *Bacillus amylovorus*, Burrill, Arthur, and Waite; (2) *Bacillus oleae*, Savastano (1886-1889);

²² *Idem*, 314-316.

²³ Synopsis of researches, *op. cit.*, 32.

²⁴ *Op. cit.*, 314-339.

²⁵ Synopsis of researches, *op. cit.*, 28-29. Smith's study of Cobb's disease of sugar cane began in 1901 and lasted until 1914. The cultural materials received were from New South Wales.

(3) *Bacillus hyacinthi-septicus*, Heinz (1889); (4) *Bacillus tracheiphilus*, Smith; (5) *Bacillus solanacearum*, Smith; and (6) *Pseudomonas campestris*, Pammel. In Fischer's *Vorlesungen über Bakterien* (Jena, 1897) had appeared statements "so contrary to fact" that Smith sought, and obtained, space in the *Centralblatt* to answer him.²⁶ He first disposed of the German's totally erroneous declaration that infections by water pores are impossible and some equally false assumptions respecting wound infections. "Instead of there being no plant diseases attributable to bacteria," Smith contended,

if the whole truth were known, there are probably as many plant diseases due to bacteria as there are animal diseases caused by these organisms. I made this same statement more than two years ago and see no reason for retracting it. On the contrary, a wider outlook and a better grasp of the subject lead me to reaffirm it. . . . Six diseases of cultivated plants have already been referred to, viz. pear blight, cucurbit wilt, brown rot of potatoes, black rot of cabbages, the yellow disease of hyacinths, and the bacteriosis or water-spot disease of beans. I will add two other plant diseases, viz. the soft rot of hyacinths and the olive tuberculosis. It has been definitely settled that these eight diseases, not to mention several others, are due to bacteria, and the evidence on which this opinion rests is of the same sort and is just as conclusive as the evidence of the bacterial origin of anthrax, glanders, symptomatic anthrax, tuberculosis, diphtheria, hog cholera, typhoid fever, or asiatic cholera, eight well recognized animal diseases due to bacteria, or at least to well known organisms commonly classed as bacteria. . . . I select these eight plant diseases because I am familiar with each one, and know from my own experiments in six of the eight that they are due to as many different bacteria. Each of these six organisms I have studied in pure cultures and am familiar with their morphology and cultural characters. The specific bacterial growth is constantly present in each of these six diseases. . . .

Without known exception, Smith enjoyed the endorsement of American scientists, once the controversy had assumed proportions and it was realized that eminent German scientists remained adamant and unmoved from the traditional position taken years earlier by DeBary, Robert Hartig, and others. Dr. Farlow frowned on the argument as undignified. But no rift between Smith and Farlow followed. In fact, later Farlow helped to secure Smith's

²⁶ Erwin F. Smith, Are there bacterial diseases of plants? A consideration of some statements in Dr. Alfred Fischer's *Vorlesungen über Bakterien*, *Centralb. f. Bakt.*, etc. II, 5(8): 271-278, 1899.

invitation to membership in the National Academy of Sciences. Farlow's letter of recommendation read:

Being at the last moment prevented from attending the session [April 22-24, 1913] of the Academy, I should like to say a word in favor of Dr. Erwin F. Smith who is a candidate for election. Dr. Smith in my opinion is one whose knowledge of the diseases of plants due to fungi [bacteria?] is greater than that of any other investigator either here or abroad. His work has not been confined to the practical aspects of the subject alone but has embraced a study of plant diseases in their widest sense including the higher scientific questions which they naturally suggest. I feel that his election would add to our number a member whose reputation is recognized by pathologists the world over.

Fischer's answer to Smith, "Die Bakterienkrankheiten der Pflanzen,"²⁷ was immediate and published in the same number of the *Centralblatt*. In 1899 the *Centralblatt*²⁸ had published in German Smith's paper on potato as a culture medium, "Kartoffel als Kulturboden, mit einigen Bemerkungen über ein zusammengesetztes Ersatzmittel." He had begun his discussion, "Are there bacterial diseases of plants?" by quoting directly from Fischer's book, and the latter's "antwort" was wholly in German. Smith believed this answer, as far as the profession was concerned,

destroy[ed] itself and need[ed] no reply. Pathologists and bacteriologists²⁹ will accept it for what it is worth, and no more. The coming years will also stamp their judgment upon it, and I have absolutely no fear of what that judgement will be. The "Registrierung" of an error by some self-constituted authority has sometimes, in the history of science, prevented a knowledge of the truth for a few years, or even for a whole generation, but it will not answer in this case. . . .

His reasons, taken together with the balance of the subject matter of this reply and his earlier presentment of proof, supplied science with a scholarly condensation of the established learning on plant bacteriology at that time.

During the summer of 1899 Smith received an important communication from Professor Emerich Ráthay, director of the oenologische pomological school of Klosterneuburg near Vienna, Austria. Written June 18, this letter informed him that Ráthay,

²⁷ *Centr. f. Bakt.* II, 5(8): 279-287, 1899.

²⁸ 5(3): 102-104; also, *Proc. Am. Asso. Adv. Sci.* 47: 411, 1898.

²⁹ Dr. Alfred Fischer in the role of pathologist, *Centr. f. Bakt.* II, 5(23): 810-817, 1899.

now convalescing from an illness of several months, was again taking up his studies

regarding a new Bacteriosis and hope[d] to be able [to publish] a preliminary [account of this in the proceedings of] the imp[erial] roy[al] Academy of Science in Vienna.

I [wrote Ráthay] am quite convinced, that there are Phytobacterioses and that the objections of Prof. Dr. Alfred Fischer concerning the existence of them are not at all founded.

I have learned very much by studiing your treati[s]es and am very grateful, that you had the kindness of sending me them.

I will only mention, that the Bacteriosis, which I found to be the cause of the malady [disease] has the same colour of citron-yellow as your *Pseudomonas*.

You may be sure, that I shall full enter in my treat[ise] for the existence of Phytobacteriosis.

Smith received other letters from Europe during this year. But none referred to the polemic with Fischer. December 2, 1899, M. C. Potter of the Durham College of Science, Newcastle-upon-Tyne, England, sent "an abstract of a paper on a Bacterial Disease of the Turnip caused by a *Pseudomonas* but evidently not the same as *P. campestris*." November 22, 1899, Smith's bulletin 17, "Wilt Disease of Cotton, Watermelon, and Cowpea (*Neocosmospora* nov. gen.),"³⁰ was published by the Division of Vegetable Physiology and Pathology. This publication contained a bibliography of the previous writings on the subject by Atkinson and Smith, and distinguished it from the wilt disease described the cotton-root rot of Texas and another wilt of cotton, cowpeas, etc., first described by Rolfs of Florida. December 1899, Dr. Franz Lafar of Vienna, Austria, thanked Smith for his publication. June 19, 1900, Dr. George Delacroix of the vegetable pathological station of the Ministry of Agriculture of Paris, France, wrote concerning a plant disease and referred complimentarily in his letter to Smith's publications. The first preserved letter received by Smith from Dr. Otto Appel of Charlottenburg, Berlin, Germany, was written in 1901 and expressed great interest, without mentioning Fischer, in Smith's studies. Recognition from Appel must have pleased Smith, as also a letter of January 11, 1901, from F. Král

³⁰ 72 pp. 10 pl., Washington, Gov't Print. Office, 1899.

of Král's Bakteriologisches Laboratorium,³¹ Prague, which read as follows:

You had the kindness to forward me 6 phytopathogenic organisms, whose cultures arrived in the best condition. These six species are of the highest value for my collection and I beg you to accept my *best thanks* for them.

I shall be happy to receive still other species from your laboratory, as you kindly indicated in your letter of December 18th.

All species, microbes and fungi which I possess, are at your disposition, you would oblige me by giving me the opportunity to send you what interests you.

Smith received this letter on January 30. In the next two months was published in three numbers of the *Centralblatt*³² his final reply to Fischer, "Entgegnung auf Alfred Fischer's 'Antwort' in betreff der Existenz von durch Bakterien verursachten Pflanzenkrankheiten." In the spring of 1900 Smith had prepared a memorandum, "Reasons for desiring to publish final reply to Alfred Fischer," and with its use, "carried [his] point and published [his] paper in *Centralblatt für Bakt.*" It read:

Numerous specific charges have been made by Dr. Fischer against my papers. He says he found them so full of mistakes that he was unable to use them and cites numerous cases in proof. These charges I have not met, but I have stated that I intend to do so. (1) What will be the result if I do not reply? There is not a German from one end of the Empire to the other who, in my position, would not reply, *whether the facts supported him or not*. If I do not meet these charges the majority will say: "He evidently remained silent because he could not meet the criticism." I know very well the habit of the German mind. This is what they will think, and what they will say.

(2) When American workers have been attacked outrageously, myself most of all, and when I have a full and *unanswerable* reply ready, why should I pigeon-hole it, and leave one-half the world in doubt,—say rather nine-tenths? To do this would be simply playing into Fischer's hands. He could desire nothing better. In order to save himself, he has tried in every possible way to discredit my work, and by implication that of the Department. I have smarted under it and will not endure it. The stealing of all my books or all of my money would have caused me much

³¹ March 23, 1900, Docent Král had written to Smith, "In accordance with Prof. Wehmer of Hannover (last number of the *Centralbl. f. Bakt. II Abth.*) I think it would be of great interest for all European Phytopathologists to make Experiments with the phytopathogenic Bacteria, described by you, Dr. Arthur, Savastano, and others. Would you be so kind and send me cultures of such Bacteria you dispose of?"

³² 7 (3, 4, and 5/6): 88-100; 128-139; 190-199; 11 pl., 1901.

less pain. I am not working in this Department for money, but for other ends. If I allow my work to be discredited what is left to me? I do not care for any credit *after I am dead*. I want it now as I go along, *and abroad as well as at home*. We cannot afford to let European workers discredit our work when we have the full and satisfactory answer at hand.

(3) It should be remembered that very little good work has been done in Germany on bacterial diseases of plants. Sorauer, who has never done a stroke of good work on this group and who is a laughing-stock to bacteriologists, although a good pathologist as regards fungi, is almost the only prominent German writer who has maintained the existence of such diseases. On the other side was formerly DeBary, and until *very recently*, Frank, and is now *Wehmer, Fischer and Hartig*. In his last revised general text book published this year, Robert Hartig takes substantially the same ground as in his older work, *i. e.*, nothing established. Consequently the bulk of German sentiment is against the existence of such diseases. How should it be otherwise? When the blind lead the blind both fall into the ditch.

(4) The time is ripe for such a defense as I am making. *It is a defense, not an attack*. All the facts are in my hands ready for a knock-out blow. The attention of the whole world is drawn to this controversy, owing to the character of the disputants and the importance of the subject. There is a better opportunity to make my influence felt than I have ever had before, or probably shall ever have again. Every word I say now counts and will have a dozen readers where I had one before. It is too late to back out now. If I do, we shall have Wehmer and Frank, and Fischer, and various other Germans, claiming that the matter is not settled, and coming forward themselves within the next decade with "*new facts and exact experiments*" drawn from their own researches and then we shall hear it said: "Ah, well, Americans, and Frenchmen, and Italians blundered and bungled over this subject for a decade or two, but Fischer and others showed that there was nothing in their writings, and it has remained for *Germans* to settle this matter *by bringing forward exact and full proofs*." They are already talking about the necessity of setting some one at work to settle this thing experimentally, as if it were not already settled in that way.

(5) Alfred Fischer is no mean adversary. He is a well-educated, well trained man. He occupies the position of Assistant Professor in one of the great German Universities, a position much more dignified than similar positions in this country, and one which is the life-long aspiration of hundreds of University graduates, who are willing to starve along for years as *privat Docents* for the sake of attaining it. Fischer is in line of succession to the full Professorship of Botany in the University of Leipsic, which has the finest physiological laboratory in Europe, and the greatest reputation at home and abroad. Every year English and American students go to Leipsic. Who comes to Washington? He is the author of numerous books, and of many papers in the best journals, all of which are frequently cited. He is a good teacher and is personally popular in Germany. What chance

have I of making any headway against him unless I can show by an appeal to chapter and verse that he is wrong? This I can do, *most thoroughly!*

(6) My final answer is ready. It is written for Europeans, not for Americans, and that all his colleagues may read it I have put it into his own tongue. It is dignified in tone. It is entirely free from personalities, and best of all it is *unanswerable!* I wish to use it both for the sake of my own reputation in Europe and for the sake of the dignity and honor of the U[nited] S[tates] Department of Agriculture, and of American workers generally.

Alfred Fischer never forgave Smith, but Americans were happy to possess such courageous leadership. By letter, June 30, 1899. F. C. Stewart had written Smith: "Yes, I have read your criticism of Fischer and his reply. It is awfully discouraging to have a man of Fischer's standing cast aside so lightly what we Americans have thought to be heavy weight work on bacterial diseases of plants."

For several years, Augustine D. Selby, botanist and chemist of the Ohio Agricultural Experiment Station, had been exchanging letters with Smith on problems of plant pathology. Soon after getting Smith's first publication on *Bacillus tracheiphilus*, Selby, also studying wilt diseases, asked him whether he would "be averse to stating what culture medium or media are preferred from your work on this organism (*B. tracheiphilus*)? I am doing some work on a greenhouse micrococcus, and have on hands an investigation of a bacterial tomato disease near Cincinnati." A few days later, June 22, 1897, Selby thanked Smith

for the suggestions concerning culture media. These will be serviceable indeed. What you state concerning each one's working out his own salvation, has been found true. The methods of manipulation for bacteria pathogenic in animal organisms require a good deal of adaptation in the vegetable world. I have been using beef broth and prune infusions as nutrients for agar and gelatine—also potato and sweet potato slices.

Later that year Selby informed that he had "fair material" for working out that season "the *Calla* bacterium." Selby, a graduate in science at Ohio State University, later a student at Washington University, St. Louis, the Shaw School of Botany, and Columbia University, was to deliver on November 29, 1901, an address as president of the Ohio Academy of Science on "The Future of Vegetable Pathology."³³ This, together with his work at the Ohio station at Wooster, would place him in the front

³³ *Science* n. s. 15(384): 736-740, May 9, 1902.

ranks of scientists in plant pathology and eventually culminate in his being elected to the presidency of the American Phytopathological Society in 1911. Smith in his address, "Fifty Years of Pathology,"³⁴ referring to several literary works which became available in 1900 to American plant pathologists, included one prepared by Selby. Said Smith:

In 1900 Sturgis published his "Literature of Plant Diseases." In 1900 Selby published his condensed handbook of the diseases of plants in Ohio. In 1900 appeared Liberty Hyde Bailey's magnificent "Cyclopedia of American Horticulture" in 4 volumes, useful to everybody interested in plants. That year appeared also the third edition of Hartig's "Lehrbuch."

Selby took a strong interest in Smith's controversy with Fischer. His letters were proof of his scholarship and thorough acquaintance with the historical background. Early in 1899 Selby read Smith's article, "Are there bacterial diseases of plants?" and Fischer's reply, and he hastened "to give expression to [his] interest in this matter and further to congratulate" Smith. Selby reasoned that "DeBary's sarcasm concerning what Dr. Burrill holds for the cause of pear blight, has served as a text for nearly all subsequent German authors who have treated the subject of plant diseases." Much other criticism of Fischer's answers was set forth. May 22, 1899, Smith thanked Selby for his letter, and with characteristic resolution, added,

I am not yet through with the discussion and when I have finished he will certainly be very sorry for what he has written. As I have facts on my side and he has only assumptions on his, the matter will, I judge, make rather spicy reading. . . . His reply is that of a special pleader, who is interested in bolstering up his own case rather than in finding out what is the truth. . . . DeBary was certainly wrong, but at the time he wrote, things looked very different in respect to bacteriological diseases of plants than they do at the present time. On the whole I have the greatest respect for the works of Anton DeBary. He is certainly one of the greatest German botanical writers of this century, if not the greatest.

Smith said more in his letter, but substantially what has already been set forth in his memorandum, "Reasons for desiring to publish final reply to Alfred Fischer." Selby did not wish Smith to interpret falsely what he had said about DeBary. In a letter of May 25, Selby urged, "I wish to make myself perfectly straight

³⁴ *Op. cit.*, 27.

on the subject of DeBary. I would, in no wise, reflect upon the work of that great botanist, but I would call attention to this, that so many German botanists, since his time, feel impelled to repeat the particular mistake he made as to bacteriological diseases of plants."

Again, despite his strong feelings, Smith endeavored to remain aloof from personalities, and stress that the main motivation of the discussion was the search for truth. When the year 1901 arrived, he received many more letters on the subject from American scientists. Dr. W. C. Sturgis, botanist of the Connecticut Agricultural Experiment Station, by letter of March 4, 1901, summarized the situation rather fully:

In view of the ascertained facts regarding B[acillus] amylovorus, I have never considered it worth while to repeat cultural work with it or to attempt infections. My work with it has been entirely of a preventive character.

I have not seen Delacroix's work on carnation disease, to which you refer. I am never surprised by ignorance, on the part of European investigators, of work done in this country. The Centralblätter and other Reviews pay little attention to American publications in general, and, with some show of reason, practically none at all to Exp[eriment] Sta[tion] Bull[etin]s and Rep[ort]s. These are apparently not classed as scientific publications worthy of note.

However, there is a good deal of excuse for Delacroix this time, inasmuch as my own work on the carnation-Fusarium consisted merely of a few trivial observations published in a Station Report which he probably never saw, while Stewart's work on it was equally inaccessible.

I am about getting out another edition of my Bibliog[raphy] of Fungous Diseases and I purpose distributing it pretty freely among European botanists so that there may be less excuse for ignorance regarding work done over here.

I have been following with extreme gratification your controversy with Fischer. He knows perfectly well he is wrong and his cumbrous attempts to hedge are most amusing. He is so badly whipped that it seems almost cruel to continue the fight.

April 23, 1901, L. R. Jones, then at Ann Arbor, Michigan, congratulated Smith:

I was much pleased with the appearance of the Centralblatt article and am glad that I followed your advice in so publishing.

Permit me to congratulate you upon your bulletin 26. Also your recent reply to Fischer. I doubt if he ventures to say anything more and so

thoroughly convincing a presentation of the matter must close the mouths of other critics as well. I have been asked by Dr. Spalding to occupy an evening—probably May 12th—with reviews (before botanical seminar here) of recent literature of bacterial and enzymal diseases of plants. I shall give your two publications a prominent place of course. It has occurred to me that possibly you might be willing to aid so good a work by permitting copies to be made for me of some of your lantern slides—providing you have an assistant capable of doing the photographic work. I could then illustrate the review. . . .

I am getting along pretty satisfactorily with my work on the *B. carotovorus* enzym. Have succeeded in isolating the enzym by precipitation methods and also in sterilizing the broth cultures without destroying the enzym. Have done the latter by chemical agents—and *more satisfactorily by heat*. The latter method has I think never heretofore been employed and is a very neat one. I heat tube cultures at 55° C for 10 minutes. This temperature sterilizes them and leaves the enzym pretty active since that does not weaken much until 58° C. But "don't say anything about it yet"—I quote that as one of the things I learned at Washington.

I have enzym action that far exceeds that described by Potter in speed. For example—sections of carrot immersed in the enzym solution show distinct swelling of the walls of the cells within *two minutes* (instead of 12 hours as with his) complete swelling and solution of the middle lamella within 20 minutes so that in less than a half hour the cells lose their cohesiveness—i. e. the tissues have passed through all the characteristic stages of the soft rot.

Regarding oxalic acid, I have as yet failed to detect a trace of it in my cultures but have not completed the search. The above action is in absence of any free acids—neutral solution—hence I doubt if the presence of such acid plays so prominent a part as Potter concluded. But I shall know better about that before I am through. . . .

R. A. Harper, Professor of Botany at the University of Wisconsin and possessor of a doctorate of philosophy from the University of Bonn, wrote:

You do up Fischer in great shape. If he would do a little work of his own in one line or another his judgments would be safer and gain more attention. He has been trying to convince us for many years that we don't know how to study nuclei but until he contributes something himself on the subject he will continue to pass for a mere theorizer.

The Canadian botanist A. H. Reginald Buller's inaugural dissertation on "Die Wirkung von Bakterien auf tote Zellen" had been done at the University of Leipzig in 1899. From Munich he congratulated Smith "upon the manner in which you have

maintained your position. I do not think anyone will have the hardihood to repeat Fischer's Vorlesungen statement again."³⁵

The polemic with Fischer was subordinate in importance to the larger consideration of Smith's scientific proofs and completeness of presentation. On August 20, 1901, G. W. Fuller of the New York firm of hydraulic engineers and sanitary experts, Hering and Fuller, thanked him for "a copy of Bulletin No. 28, describing the cultural characters of four bacteria parasitic on plants. It is certainly a very comprehensive piece of work," commented Fuller, "and I desire to congratulate you upon this important addition to our literature on this branch of bacteriology."

The next day Dr. V. A. Moore sent congratulations, saying, "You certainly are getting much on your side by way of research work." This was written of Bulletin 28 and he liked also "the excellence of Bulletin No. 26" ["Wakker's Hyacinth Germ"], concerning which he had written on March 8: "You are doing good work and I guess it is well you did not leave to take a place elsewhere." Dr. Moore was then working on his book, planned to be "valuable for students of comparative pathology," on *The Pathology and Differential Diagnosis of Infectious Diseases of Animals* (1902). His *Laboratory Directions for Beginners in Bacteriology* had gone to a second edition and been marketed, he told Smith, "the last of October. . . . I am under many obligations to you for the success it is having."

Theobald Smith, from his laboratory of comparative pathology at Harvard, wrote on May 16, 1901: "I am glad you took A[lfred] Fischer by the ears. I have the reprints on my table to read as soon as my course at the Med[ical] School is over, so that I can read without interruption. I had a similar bout about ten years ago. I believe the time, on the whole, well spent, although very irksome to the writer." Practically a decade had passed since he and Kilborne had proved that Texas cattle fever is transmitted by a tick; and now the principle of their work, aside from the insect transmission of plant diseases, was being applied in questions of the spread of human diseases. In 1899 the *Johns Hopkins Hospital Report*³⁶ published G. H. F. Nuttall's voluminous study

³⁵ Letters dated July 8, 1901 (Harper), August 16, 1901 (Buller). E. M. Wilcox, G. J. Peirce and other American botanists wrote Smith during this year.

³⁶ 8(1-2): 1-154, with bibliography, 1899.

"On the Rôle of Insects, Arachnids and Myriapods in the Spread of Bacterial and Parasitic Diseases of Man and Animals."

Erwin Smith's vigorous publications on pathogenic bacteria in plant diseases were also being read by pathologists in medical schools. Philip Hanson Hiss of the department of pathology at the College of Physicians and Surgeons of Columbia thanked Smith on September 13, 1901, for his "valuable paper on 'The Cultural Characters of *Pseudomonas Hyacinthi*, *Ps. Campestris*, etc.' . . . Some of the sections," he said, "are of particular interest to me, especially those relating to fermentation tests." Dr. William George MacCallum was then an associate professor of pathology at Johns Hopkins. In 1909 he was to be honored by a professorship at the College of Physicians and Surgeons, and in 1917, after his text-book on pathology had been published, was to return to Johns Hopkins as professor of pathology and bacteriology. On July 6, 1901, he sent Smith his "best thanks for your most interesting papers on the bacterial diseases of plants" and added: "I was especially interested in what you say as to the sharp specificity of these bacteria. Have not any of these bacteria the least pathogenic activity for animals?"

Smith was known at Johns Hopkins not alone for his work on plant bacterial diseases. At least once he had helped to diagnose a disease of humans caused by a parasitic fungus. In 1898 the *Johns Hopkins Bulletin*⁸⁷ published Benjamin Robinson Schenck's study, "On Refractory Subcutaneous Abscesses Caused by a Fungus possibly related to the *Sporotricha*," and, in the paper, Smith's work on "many of the points relating to the morphology and development of the organism," especially "the more difficult and obscure points pertaining to its life history and classification," was acknowledged. Since the specimen submitted to Smith proved to be only a "conidial fructification" and not "the perfect spore form," he had been unable to determine the species and to which of three possible form genera the organism belonged. But he believed that the description fitted "best into *Sporotrichum*." On April 18, 1898, Dr. Flexner and Schenck thanked him for his "very full report" and Schenck planned to "work over some of the points which [Smith] suggest[ed]." Dr. Welch was "much interested" in Smith's letter on the subject. That year Schenck

⁸⁷ 9(93): 286-290, Dec. 1898.

received his degree of doctor of medicine and became resident gynecologist of the hospital, later instructor in gynecology in the university. He may have continued to study sporotrichosis, for, Smith, later speaking³⁸ of this work, said:

In 1900 a fungus, identified by the writer as *Sporotricum*, was found, by Schenck of Johns Hopkins, to be the cause of chronic human abscesses, and since that time several hundred cases have been observed and a considerable literature has developed, mostly American and French. The parasite enters through wounds, often very slight ones, and the remedy is heavy doses of potassium iodide. Many other fungous parasites occur on man and other vertebrates, and they are nowhere studied at present (1926) as they should be. Thaxter has monographed those occurring on insects and arachnids (the Laboulbeniales).

On April 20, 1901, Dr. H. M. Hurd, superintendent of Johns Hopkins Hospital, wrote Smith: "I think your researches in Plant Pathology throw a great deal of light upon pathogenic bacteria; at least they have illuminated the subject in my mind more than anything else which I have read, and I congratulate you upon your thorough and successful work."

Smith always had wanted to study bacteriology under Dr. Welch. In 1902 a special reason was indicated why such further special study was desirable and, writing the great teacher, he received the following reply:

You are welcome to anything we can offer you, and so far as my lectures are concerned you can attend them without charge. But I do not believe that you would find anything worth coming here for. I teach very little by lectures, almost entirely by laboratory work. The arrangement is this: From October to Christmas we give the laboratory course in bacteriology. I usually give one or two lectures a week in connection with this course on infection and immunity, taking up particularly topics not sufficiently dealt with in the students' text-books. From January first to June is the course in pathology, consisting of three half days' laboratory work a week. Before each exercise either one of the assistants or I give a brief talk—fifteen or twenty minutes usually—on the topics to be studied that day in the laboratory. . . . I give, therefore, no systematic course of lectures on pathology, and I do not think that you would find it to your profit to attend the scattered, irregular ones I give to supplement the laboratory work. If you cared to take the laboratory course, the one given by one

³⁸ Fifty years of pathology, *op. cit.*, 31. See also, *Science*, August 15, 1947, on, The differentiation of the pathogenic species of *Sporotrichum*, by H. I. Lurie, where it is said that the tendency now is to regard all such species pathogenic to man as one species.

of the assistants during May and June is the only one at present open, as every place in the undergraduate course is occupied by the regular students, and they are crowded as it is. It may not be so another year, as our present second year class is unusually large. But I hardly suppose you would have time for the laboratory course. Moreover, I expect you could be[nefit] us more than we could you.

Since new laboratories were to be established in the Department of Agriculture and Smith was to be the chief of one, he doubtless felt the need of the latest in learning on pathology and bacteriology. In 1901 Theobald Smith had written:

What you say of the reorganization of the work and the new laboratory sounds very promising and I have no doubt that the prospects in Government service are destined to grow steadily better and that the work will eventually outstrip the work of our Universities in scope and thoroughness. . . .

University laboratory botanical instruction for several years had been completing a period of first growth and then transition. December 14, 1898, Professor F. C. Newcombe of the University of Michigan, in a letter to Smith, searchingly presented a problem gaining in proportions with every year: what, after all, was to be the fundamental function of university botanical laboratories? To teach, yes, but that was not the full answer; to teach what—pure science or practical science, or both? Around these inquiries clustered discussion which probed the bottom of classroom and laboratory policy. With humor but also an undercurrent of seriousness, Newcombe wrote:

Your letter of invitation to attend the coming meeting of the *Soc[city] of Plant Morph[ology] and Physiol[ogy]* came when I was in the throes of helping prepare a program for the annual meeting of the State Horticultural Soc[iety]. What do you think of that? To such things have we come. On the one hand are the allurements of so-called pure science who glances casually and perhaps too often disdainfully toward the *practical* barbarians, while on the other hand stand the regents, urging us to talk, talk, talk. We say "What shall we talk?" and the regents answer not. We say, "If you want the University to be represented in all sorts of state organizations, would you not better employ a few men for that work?" . . . Are we an agricultural college, or shall we eschew agriculture, or shall we divide our energies between agriculture and pure science. . . . But really I believe in the organization most thoroughly. In a few years it ought to be *the* society. . . .

February 16, 1898, Pammel had informed Smith that he was

"drifting out of bacteriological work," although still teaching the subject. October and November 1899, Hermann von Schrenk at St. Louis requested of Smith that cultures of pathogenic bacteria and fungi be sent to him. At his Mississippi Valley laboratory of the Department he had "such a fine opportunity for making greenhouse experiments" that he wanted some of his men to "work out the history of some of the pathogenic bacteria" and study things at first hand wherever possible. He was hoping to "get into the apple orchards this week to work on the root fungus."

In May 1899, Lilian Wheeler, daughter of C. F. Wheeler, who now was assistant professor of botany and forestry at Michigan Agricultural College, applied, at Smith's suggestion, to Secretary of Agriculture James Wilson for a position in Smith's laboratory. Effie A. Southworth some years before had left the Department, for a while taught at Barnard College until becoming the second wife of the widower V. M. Spalding, and in November, 1898, Spalding exuberantly had written from San Bernardino where, after some time spent in New Mexico, they had gone to enjoy California's "paradise of palms and olives, oranges and fig trees . . . multitudes of roses in full bloom . . . long avenues of eucalyptus and pepper trees and yards full of green shrubs and trees most of which were new to" them. To have an assistant as well as many improved facilities in the laboratory heartened Smith.

In 1900 Spencer Ambrose Beach, acting for the American Association of Agricultural Colleges and Experiment Station, invited Smith to present "a paper on the progress and present status of our knowledge of bacterial plant diseases. At this, the last meeting of the Association in the nineteenth century," Beach said, "it seems fitting to set a stake showing the progress of investigations along certain lines during the closing part of the century. When this subject of bacterial plant diseases came up it was at once decided that Dr. Smith was the man to present the topic. No one can do it so well. . . ."

Smith already had proved his fitness to lecture on this subject. During the previous July, 1899, he had delivered at the Marine Biological Laboratory of Woods Hole the course of lectures arranged for the year before. July 12 he wrote to Galloway: "My lectures did not begin until Monday. Today I give the third.

It is *new* business for me and rather hard, the more so as I found no time to prepare them in advance."

The following year, in July, Smith was again at Woods Hole, but as to whether he lectured on either plant pathology or plant bacteriology or both is not known. On August, 1901, however, again there, he wrote to Woods:

We have now been here a week and are well into the spirit of the place and of the season.

The summer school closes Tuesday next, after which there will be an exodus and then the place will be quieter. The laboratory season has been prosperous. 285 people eat at the mess table. How many are actually registered and doing work in the various laboratories I have not enquired. Zoology get the major portion of the students, but in the Bot[anical] lab[oratory] I counted 23 at one of [Rodney H.] True's lectures. Next year [Bradley Moore] Davis wishes to have the Bot[anical] Lab[oratory] enlarged. This needed improvement will cost about \$1600. There have been four teachers this year—Davis, True, [George T.] Moore, and Shaw—the latter, one of MacFarlane's students, has conducted the class in ecology.

Lectures have been fewer than usual this year. Generally there is a superfluity. I have heard three—two by Dr. Parker of Harvard on the sense perceptions of Crustaceans—very interesting, and one by Dr. Reighard of University of Michigan—a lantern slide lecture on the breeding habits, nest building, care of young, etc. of certain fishes—viz. Lamper eel, sun fish, and dog fish—also interesting.

My own lecture (the lantern slide one on bacterial diseases of plants) closed the season. Had a good audience, and apparently an interested one.

I have been so busy getting started in my own work that I have not been around much and don't yet know all of the Zoologists, who are teaching or at work here. I have met Whitman, Loeb, [Caswell] Grave (of Johns Hopkins), Bigelow (of Boston), Lillie,³⁹ and two or three others. In the Fish Commission Laboratories there are fewer special workers than last year. Generally these laboratories have been overcrowded, partly, no doubt, owing to Dr. Bumpus' very attractive personality. I miss his genial presence about the Fish Com[mission] buildings very much. He is now director in chief (practically at least) of the Natural history museum in New York and is probably so tied by executive duties that he will never find time to come here any more. He will do great things for the Museum, however. Dr. Hugh Smith, of Washington, who has taken his place as Scientific Director of the Fish Com[mission] work here, is interested in the work and very nice, so far as I have observed, but he is very reserved and quiet, too much to be popular. By his invitation we have been out once on the

³⁹ Frank R. Lillie, *The Woods Hole Marine Biological Laboratory*, 35-52, Univ. of Chicago Press, 1944. Much of the material of the next paragraph has been based on this book, some chapters of which Edwin Grant Conklin, professor-emeritus of biology of Princeton University, wrote.

Fish Hawk dredging, and have been out once with Swingle and Moore on the Sagilta up the coast for lupins. The Fish Commission tanks here are full of interesting things and new strange creatures are added every few days. . . .

Woods Hole was to Smith more than a place where scientific research was pursued. He and Mrs. Smith enjoyed there some of their happiest days, weeks, and months. In this region he was inspired to write some of his best poetry and to investigate some most difficult problems in science. Much of his best scientific writing was done there. The Marine Biological Laboratory stood for the highest ideals of scientific scholarship. Being of a national character, its direction had sought to cooperate with all scientific organizations, universities and colleges, and avoided "any one-sided alliances that might alienate large numbers of scientific men." With all scientific agencies interested in research in biology, the laboratory was to work and with none was it to amalgamate.

Situated on Cape Cod where one of the nation's first large scale experiments in forest tree culture was pursued by J. S. Fay, Woods Hole, when Smith first visited it, presented a picture which contrasts interestingly with the scene of today—a region of fashionable summer resorts, a town of many beautiful homes built among hills, fronted by Buzzard's Bay, Vineyard Sound, and the Atlantic Ocean, in full view of several beautiful islands, and on the mainland scientific laboratories which resemble more the buildings of a great university or research center of modern proportions. On July 12, 1899, Smith described Woods Hole to Galloway:

The place is small and crowded (about 200 people here at the laboratories and for pleasure) and hardly even a room to be had now. The one I vacate is already engaged. Farther up the coast, I suppose there must be many [cottages], there are so many sea coast summer resorts—Falmouth, Vineyard Haven, Cottage City, Sconset etc.

Woods Hole is an old fishing village, inhabited by three classes: first, the natives, a curious lot; second, the rich summer residents who own extensive grounds and have built large fine houses facing the shores and go about in yachts and steam launches, and are said to regard the rest of us as intruders on their peace and quiet; and, third, the college and university people who come here for a brief period in the summer and who are known locally as the "bug hunters." Much of the land is owned by a few rich men, and although there are many good sites for inexpensive cottages the building of such is discouraged. There are . . . very few bathing houses

and none now to be rented—all taken. . . . Today it is warm in the wind-sheltered places, but cool near the water, which is very beautiful in clear weather and dotted with ships large and small.

The things dredged out of the sea are also very curious and interesting. Boats go out every day, and each brings back strange animal and plant forms.

The Marine Biological Laboratory had rescued and resurrected the region's great scientific inheritance bequeathed by Louis Agassiz from his historic Anderson's School of Natural History on nearby Penikese Island. While not a direct descendant of Agassiz's institution,⁴⁰ the Laboratory was, nevertheless, an heir to the great tradition of scientific instruction and investigation, and had added much in branches of study. Invertebrate zoology established in 1888, marine botany at least half of which consisted of algal studies and commenced about 1890, general physiology started about 1892, and embryology about 1893, had each been kept in the foreground of research and instruction. About 1892 studies in the medical as well as biological⁴¹ sciences became more prominent, although for a while these consisted mainly of physiology, biochemistry, and embryology. Whereas in 1892 but five investigators were registered, by 1902 the number aggregated fourteen and by 1906 twenty. Among them were Councilman (1893), Flexner (1894-1895), F. P. Mall, Jacques Loeb, J. P. McMurrich, Ludvig Hektoen (1900), A. P. Mathews, E. P. Lyon, W. E. Garrey, Leo Loeb, A. J. Carlson, Hideyo Noguchi (1902, 1907), and C. R. Stockard. Some of this work had a definite significance from the standpoint of comparative pathology. In the next decades medical research workers as well as preclinical and scientific students ambitious to study medicine increased numerically.

That Smith arrived in time at Woods Hole to help further botanical interests at the Marine Biological Laboratory is indicated by two letters of the years 1899-1900. December 23, 1899, Bradley Moore Davis of the Department of Botany of the Uni-

⁴⁰ Frank R. Lillie, *The Woods Hole Marine Biological Laboratory*, *op. cit.*, 15-23.

⁴¹ *Idem*, 87-88, where it is also said that the course in general physiology was the first of its kind, not only in America but in the world. It was a prime factor in the rapid development of the subject in the universities. Concerning the researches in botany, pp. 144-145. Concerning the researches and investigators in medicine, pp. 146-147.

versity of Chicago, later of the University of Pennsylvania, and still later of the University of Michigan, promised to send to Smith

in a day or two the program of Botany for next season at the Marine Biological Laboratory. You will see [wrote Davis] that we have made some advance but not as much as we had hoped. A course in plant physiology is added to our program but we have not been able to introduce ecology for lack of room.

We hope to have an addition to our building soon and then can add this subject which is very appropriate for our conditions at Woods Hole.

You remember your promise to bring up before the proper persons the proposition to have the Dept. of Agric. subscribe for one or two rooms to be reserved for members of the department and where they might work during the summer in a comfortable climate and pleasant surroundings. Have you taken this matter up yet and if so how far has it progressed?

July 30, 1900, Galloway addressed a letter to Smith at Woods Hole, part of which read: "All arrangements have been made for the laboratory facilities at Woods Hole, so that you can feel perfectly free in the matter now."

In 1902 Smith was to be honored by being elected a trustee of the Marine Biological Laboratory and was to retain this position for twenty years or until 1921.⁴²

The year 1901 was a triumphant one for him in many ways. July 17 of that year, H. C. Ernst, bacteriologist and editor of the *Journal of Medical Research*, writing from Manomet, Plymouth County, Massachusetts, invited him to submit to the *Journal* the results of his further investigations on diseases of plants. So pleased with his articles already published was Ernst that he was re-reading them carefully, and he assured Smith that the *Journal* which was just then being started had the support of "many of the best men in the country." Dr. George M. Sternberg, by letter of May 7, requested Smith to send him "the publications containing an account of the bacteria pathogenic for plants, which you have described. I wish," said the Surgeon-General, "to introduce a brief description of these bacteria in a revised edition of my Text-Book of Bacteriology, which I am now at work upon."

March 29, 1901, Professor Doctor Beyerinck of the Bacteriological Laboratory of the Polytechnic Institute of Delft, Holland, "respectfully and obediently" thanked Smith very much and with

⁴² Frank R. Lillie, *The Woods Hole Marine Biological Laboratory*, *op. cit.*, 255.

pleasure for his "interesting paper on Wakker's hyacinth disease, this day received." Further recognition came from Europe; Lafar of the bacteriological laboratory of Vienna, Alfred Möller of Eberswalde, Germany, Luigi Savastano of the Royal School of Agriculture of Portici near Naples, Italy, and by 1901 practically every world known botanist, bacteriologist, and many eminent medical authorities and hygienists were among his correspondents.

Among other letters from foreign scientists during this year, F. A. F. C. Went, another pathologist of the Netherlands, for a while located in Java and later of the University of Utrecht, informed by letter of July 21, 1901, that he was at Paramaribo, Dutch Guiana, South America. Under orders of the Netherlands government, he was inquiring into agricultural conditions in the Dutch West Indian Colonies and making "some researches about diseases of Theobroma Cacao and the Sugar-Cane. For this end," he wrote, "I will be staying here till the middle of October, then proceed to Georgetown (Demerara), Barbados and Trinidad, thence to Curaçao and the smaller Dutch Antilles." He wanted to learn about the cultivation of sisal hemp in Florida, and on November 22 announced that he would arrive in Washington by way of either Florida or New York from the Bahamas and several of the West Indies island possessions of the Netherlands.

Early that autumn Smith had received a letter from N. A. Cobb of the department of agriculture at Sydney, New South Wales, concerning the gumming of sugar cane disease. He had promised "at the proper season to secure for [him] some further specimens" of the malady, enclosed a copy of his article on the subject, and added the important information that his recommendations had "resulted in quelling the trouble so completely" that specimens were now not easy to secure. Dr. Went had written that the disease was not in Java and that, while in the West Indies, he had visited many sugar producing colonies and spoken with scientific men and not located it. Cobb wrote that he had been "busy on other lines since [his] return" from the United States where he had met Smith at Woods Hole. "The bacteriologist of the Linnean Society of N[ew] S[outh] W[ales]," he believed, was "doing some excellent work with pure cultures of the microbe, but [had] published nothing as yet." Other workers may have begun studies.

At the next meeting of the Society for Plant Morphology and Physiology, Smith reported that Cobb had "pointed out a way to avoid the gumming of sugar cane . . . viz., by the selection of healthy cuttings. This practice [had] greatly reduced the amount of gummed cane in New South Wales."⁴³ Smith's study, "Ursache der Cobb'schen Krankheit des Zuckerrohrs,"⁴⁴ not published until 1904, noticed the prior work of the Australian Greig Smith and Cobb, verified and extended Cobb's conclusions, and for years Went and he exchanged letters on the subject. His study was a reason why Went visited him at Washington, and, since later he wrote on the disease in his three volume monograph, it and other studies were probably among the reasons why Smith in 1906 arranged to visit Went in the Netherlands.

The establishment of research centers of plant pathology in tropical countries was an advance of first magnitude. On July 15, 1901, Paul C. Freer, of the United States Philippines Commission and recently appointed superintendent of laboratories, asked Smith to recommend some properly qualified, "bright and energetic young man," capable of performing research in plant pathology. Buildings were being planned, equipment provided, and a staff selected, for laboratories being created in the Islands to promote research and economic progress in public health, forestry, agriculture, mining, and customs. "I have read with interest your article in regard to bacterial diseases of plants," Freer wrote Smith, "and I am sure that you can help me better than any one else. . . . As certain plant diseases, especially one of the coffee plants, are prevalent in the Islands, one of the first things to undertake is to find some means, if possible, of combatting these conditions."

The year 1901 was significant in Smith's life for a still more important reason. On July 1, in accordance with the authority and directives of a Congressional enactment, the United States Bureau of Plant Industry, as one of four Bureaus of the Department of Agriculture, was organized to bring together under one unit the allied work of plant physiology and plant pathology, botany, pomology, grass and forage plants, and experimental gardens and grounds. By executive order and Congressional approval the Arlington experimental farm, investigations in the

⁴³ Plant pathology: a retrospect and prospect, *op. cit.*, 608-609.

⁴⁴ *Centralblatt f. Bakt. etc.*, II, 13(22/23): 729-736, 1904.

production of domestic tea, the office of foreign seed and plant introduction, and the Congressional seed distribution, were added. B. T. Galloway was appointed chief of the Bureau which now had more than two hundred efficient workers. A. F. Woods was in charge of the office of vegetable pathological and physiological investigations. F. V. Coville remained as botanist in charge of botanical investigations and experiments. F. Lamson-Scribner who for nearly eight years had been agrostologist of the Department was appointed chief of the insular bureau of agriculture of the Philippine Commission,⁴⁵ and William Jasper Spillman, since 1894 a scientific agriculturist with the Department, was made agrostologist in charge of grass and forage plant investigations. G. B. Brackett continued to serve as pomologist in charge of pomological investigations. L. C. Corbett was placed in the position of horticulturist of the Bureau.

Smith's work in plant pathology and bacteriology had merited recognition for several years. This now came, not simply as another increase in salary, this time from \$2,000 to \$2,500 a year. A Laboratory of Plant Pathology was created and placed in his immediate charge. Its importance as a scientific research unit within the Bureau was not to be exceeded by any other, although, of course, the equally important Laboratory of Plant Breeding placed in the immediate charge of H. J. Webber represented a similar recognition. Webber in 1899 had been the Department's official representative at the first International Conference on Hybridisation and Plant Breeding which was held at Chiswick (London), England.

In 1898, when the full amount of a requested appropriation for plant breeding experiments was not granted, Woods and Webber refused to be discouraged. Woods wrote Smith, "A good deal can be done with \$5000," and the work continuing, within three years such valuable cotton improvements had been secured by Webber and Orton that the former on September 4, 1901, urged Woods to seek a \$40,000 annual appropriation for plant breeding.

Smith took an active part in obtaining increased sums of money for "experiments in the breeding of orchard fruits, cereals, tobacco, and other plants to secure improved varieties and varie-

⁴⁵ *Yearbook of the U. S. Dep't of Agric. for 1901, op. cit.*, 525.

ties and stocks resistant to disease, and for the investigation of the physiological principles underlying plant breeding." ⁴⁶ In 1898, when the Secretary of Agriculture approved \$12,000 for the study of tobacco problems, Woods called on Smith, if he could find time, to prepare "a brief article of four or five hundred words stating what [was] known of the relation of bacteria to the fermentation and curing of the tobacco leaf" in compliance with a request from Orange Judd of the *American Agriculturist* and *New England Farmer*.

Smith's estimates of monetary losses suffered by different regions from crop diseases always proved helpful in convincing Congressmen and the public why appropriations for the Bureau of Plant Industry should be increased. The losses were enormous and the work's value obvious. Especially plain was the urgent need of more research. At one time he estimated that losses in the west during two seasons from sugar beet diseases had been more than a million dollars. He ardently worked for the passage of a bill in Congress which would increase appropriations sufficiently to permit further study of the Michigan little peach disease. Special investigations were still wanted for peach yellows, bitter rot of apple, and other diseases of orchard, field, and garden crops.

Since 1897-1898, appropriations for the Department of Agriculture had steadily been increased. That year the figure was \$3,182,902. In 1898-1899, the total sum was augmented by \$326,300; in 1899-1900, by \$216,820; in 1900-1901, by \$297,478; and by 1901-1902, by \$558,920, until the allowance was \$4,582,420.

September 12, 1901, Woods wrote Smith, then at Woods Hole:

The new laboratory building is very nearly completed and we expect to get into it at an early day. . . . The estimates are in and we have asked in them for about \$125,000. It may be we can get it up to \$143,000. This is what I want and what I think from developments last year, we will certainly get,—possibly an even \$150,000.

Webber is having great luck with his cotton,—has one of the finest long staple uplands in existence. Orton's resistant cotton is resistant again this year. He has a resistant cowpea and Webber has found that the cowpea resistant to wilt is also resistant to Nematode. This is fully as important, as the cowpea cannot be used in the peach orchards in the south, you know, on account of the danger from Nematode to which the cowpeas are usually

⁴⁶ Quotations and facts based on memoranda and letters found among Smith's papers.

so subject. Carleton's macaroni wheat is booming. I think we are going to make a big hit in this industry.

Smith regarded Carleton's work as of great value. Of his wheat introductions and breeding work which like Webber's and Orton's cottons added millions of dollars to the American economy, Smith said: ⁴⁷

In 1898 Carleton made his first trip to Russia, bringing back the Kubanka durum wheat, resistant to heat and cold and to rust. In 1900 he went to Russia again, bringing back the Karkoff red winter wheat and various other valuable cereals. In 1899 his bulletin "Cereal rust of the United States" was published.

The second International Conference on Plant Breeding and Hybridization, held September 30-October 2, 1902, in New York City, heard an address by Orton "On the Breeding of Disease-resistant Varieties." ⁴⁸ In this, he explained the experiments from which his Sea Island cotton, cowpea, and watermelon wilt resistant varieties resulted. Accomplishments of the Division of Vegetable Physiology and Pathology since 1898 ⁴⁹ had been notable.

For the southern states, the value of the new hybrid cold-resistant oranges, developed by Swingle and Webber at Eustis and Miami, was being demonstrated. European table grapes were now being grown for foreign markets. At Summerville, South Carolina, the Department maintained a factory and a one hundred acre tract for tea growing, and near Charleston one thousand acres of tea had been started on rice lands. In Texas and Louisiana the Department's work had augmented by many millions of dollars the invested capital and income from the rice industry. New cottons were being secured for distribution, and Egyptian cotton was being grown and proving valuable for crossing with other

⁴⁷ Fifty years of pathology, *op. cit.*, 27; M. A. Carleton, Macaroni Wheats, Bull. 3, Bur. of Pl. Ind., Dec. 23, 1901.

⁴⁸ *Experiment Station Record* 14(3): 212, Nov. 1902.

⁴⁹ A. F. Woods, Work in vegetable physiology and pathology, *Yearbook of U. S. D. A.* for 1898: 261-266; W. T. Swingle and H. J. Webber, Hybrids and their utilization in plant breeding, *Yearbook* for 1897: 383-420. Concerning plant breeding work, B. T. Galloway, Industrial progress in plant work, *Yearbook* for 1902: 219-239; Willet M. Hays, Progress in plant and animal breeding, *Yearbook* for 1901: 217-232; H. J. Webber and Ernst A. Bessey, Progress of plant breeding in the United States, *Yearbook* for 1899: 465-490; H. J. Webber, Improvement of plants by selection, *Yearbook* for 1898: 355-376; A. F. Woods, The relation of nutrition to the health of plants, *Yearbook* for 1901: 155-176, especially those portions dealing with diseases of plants.

varieties. A new method of securing pastures of Bermuda grass from seed had been shown to be of much value in the south. The possibilities of growing rubber, coffee, banana, and cacao were indicated, and methods for cultivating many other tropical products were being improved.⁵⁰ Pineapple culture was being studied. Hundreds of crosses for vigor, quality, largeness of fruit, and disease-resistance, had been made.⁵¹ These were a few from many other examples.

Among the western states, the value of Pierce's hybrid disease-resistant grapes had been demonstrated in California. New varieties of prunes had been introduced along the Pacific Coast. In the arid southwest dates from Egypt and Algeria were being planted and grown. The introduction of many new, or the amelioration of many old, horticultural products in states of the far and central west was transforming agriculture from a condition of bare subsistence in many places to one of abundance or at least a promise of plenty. The demonstration of the practicability of growing macaroni wheat, the extensive importation of seed, and the establishment of factories made possible competition with the foreign product and saved the nation millions annually.

Improved hops and barley were imported and disseminated. New forage crops were secured for the entire country. The discovery of new methods for growing nitrogen-gathering bacteria made practicable the cultivation of leguminous crops in many new regions. Western ranges were being improved. American-grown clover had been shown to be superior over seed from foreign sources. Blue grass seed was harvested and handled by new methods, and the crop's value enhanced from thirty to forty per cent. Better transportation facilities for fruit and perishable products were being enlarged and quickened, and the way opened to cross-continental and European markets. Each advancement meant money and better practice in the many various plant industries, but with each extension new problems beset the plant pathologist.

Smith's new laboratory of plant pathology was not only to share in saving crops from partial or total destruction but also to assist in the work of crop amelioration for quality and quantity yields.

⁵⁰ Scope of work, Bureau of Plant Industry, U. S. Dep't of Agric., 4 pp.

⁵¹ Work in vegetable physiology and pathology, *op. cit.*, 266.

The introduction and breeding of disease-resistant plants were always encouraged and aided by him. This branch of research, however, was never his specialty. His was the pioneer laboratory of the Department in economic plant pathology, and when it was created the imperative need was for standardized methods of investigation and thorough studies of all the bacterially caused diseases of plants. During his lifetime, approximately one-half of the known bacterial plant maladies would be discovered and worked out in his laboratory. Extensive additional investigations, furthermore, would be made of a large percentage of the other one-half of those diseases then recognized as serious. Some idea of how widely Smith covered the field of bacterial diseases among agricultural crops may be gathered from the following list, prepared in 1926,⁵² each of which was "studied in detail, the causal organism determined and in many cases means of prevention and control established:

1. *Truck crops*: tomato, potato, tobacco wilt diseases; lettuce leaf and stem rots and spots; bean leaf, stem, pod and seed diseases; cucumber, melon, pumpkin and squash leaf and wilt diseases; sugar beet leaf spot; cabbage, cauliflower, turnip, mustard, kohlrabi rots and leaf spots; cotton boll rot, black arm and leaf spot.

2. *Cereal and forage crops*: sugar-cane, sorghum and broom corn diseases; sweet corn wilt; millet leaf spot; wheat, oats, barley and western wheat grass diseases of leaf, stem and kernel; Kudzu, soy-bean leaf spots; alfalfa and clover leaf and stem spots and root rots.

3. *Fruit diseases*: crown gall infecting the majority of large and small fruits, vegetables and ornamentals; coconut bud rot; stone-fruit leaf and fruit spot; mulberry leaf spot.

4. *Miscellaneous*: florists' troubles, gladiolus bulb rots; nasturtium wilt; calla soft rot; iris soft rot; hyacinth and other lily rots; leaf spots of nasturtium, gladiolus, delphinium, canna, geranium, martynia; lilac blight; castor bean wilt.

5. *Plant tumors other than crown gall*: olive (bacterial), apple(?), ash (bacterial), sugar beet (bacterial), citrus (fungus), sapodillo (fungus).

6. *Non-bacterial plant diseases*: garlic rot, oat blast, Smyrna fig rot (in California), sweet pea fasciation, banana wilt, cotton, cowpea, watermelon and tomato wilts, geranium stem rot, snapdragon wilt, dry rot of potato.

From this laboratory, altogether about 180 publications would

⁵² Found among Dr. Smith's paper. Prepared by request of A. F. Woods as to Scientific work in the Department and addressed to Dr. W. A. Taylor, chief of the Bureau of Plant Industry. Transmitted to heads of offices, memorandum 249, October 1926.

be issued. Add to this the regular diagnostic and remedial services furnished plant growers from every state of the Union, and one comprehends in brief compass its contribution to plant pathology and bacteriology during Smith's tenure. During these years, crop losses from bacterial and fungous diseases ranged from two to ninety-five per cent annually. First, the etiology of these maladies had to be understood, and proof of how infections start and are transmitted were often necessary preliminaries to working out methods of control or prevention, and remedies. If infections were transmitted by insects or through soils or on the seed, it was often as valuable to know these facts as to know the pathogenic organism responsible for the malady. Fundamental, nevertheless, was the isolation by pure culture technique of the organism, and each disease was studied from every available aspect.

In 1902-1903⁵³ Smith found on Japanese plums in Michigan a disease which he proved to be caused by *Bacterium pruni* EFS. He worked on this disease for twenty years, yet never published a full account of the organism. His first paper, entitled the same as his preliminary note in *Science*, was presented before the sixth meeting of the Society for Plant Morphology and Physiology. Dr. Spalding was then its president. At this meeting Smith offered also his "Completed proof that *Ps. Stewarti* is the Cause of the Sweet Corn Disease of Long Island." At the December 1903 meeting of the Society he began to discuss the bacterial origin and the parasite of the tumor of "The olive tubercle,"⁵⁴ due to *Bacterium Savastanoi* EFS, the results of study begun that year with James Birch Rorer.

In 1903 Smith published his Bulletin 29, "The Effect of Black Rot on Turnips,"⁵⁵ a series of photomicrographs accompanied by an explanatory text.

For the next several years he studied mainly bacterial diseases of plants, work aimed at preparing to place in written form various portions of his three-volume monograph, *Bacteria in*

⁵³ E. F. Smith, Observations on a hitherto unreported bacterial disease, the cause of which enters the plant through ordinary stomata, *Science* n. s. 17(429): 456-457, March 20, 1903; Synopsis of researches, *op. cit.*, 31-32; Fifty years of pathology, *op. cit.*, 29; Beobachtungen ueber eine bis dahin unbekannte, durch Bakterien verursachte Krankheit, die durch die gewöhnlichen Stomata in die Pflanze eindringt, *Centralblatt f. Bakt. etc.*, II, 10(22/23): 744-745, 1903.

⁵⁴ *Science* n. s. 19(480): 416, March 11, 1904.

⁵⁵ Issued Jan. 17, U. S. Bur. of Pl. Ind., pp. 9-20.

Relation to Plant Diseases. Among the organisms studied were *Bacillus delphini*,⁵⁶ *Bacterium andropogoni* EFS or stripe disease of broom corn and sorghum (1904-1922),⁵⁷ *Bacterium mori* (Boyer and Lambert emend, EFS) or mulberry blight (1905-1921),⁵⁸ *Bacterium lachrymans* Smith and Bryan or angular leaf spot of cucumber (1906-1915),⁵⁹ and *Bacterium syringae* (Van Hall) EFS or lilac blight (1906-1907).⁶⁰ In 1903 Otto Appel in Germany described a destructive bacterial potato rot due to *Bacillus phytophthorus*. Smith regarded this "masterly paper" as letting "a flood of light into an obscure situation,"⁶¹ and soon Appel's and two or three similar organisms were found in the United States by Smith and others. To study this disease, black-leg of potato, and its distinction from *Bacillus solanacearum*, Smith in 1906 would make his first journey to Europe.

In 1902, before the fifty-first meeting of the American Association for the Advancement of Science, B. T. Galloway, vice-president and chairman of Section G for 1901, commented in an address, "Applied Botany, Retrospective and Prospective,"⁶² "Probably in no other field of botanical science has the applied work been of more value to mankind than in bacteriology, surgery, and sanitation. . . . Bacteriology, in its relation to surgery and sanitation, has passed out of the field of applied botany, but problems will still arise. . . ."

Galloway spoke truthfully, and perhaps more wisely than he knew. October 10, 1901, Dr. Harvey Russell Gaylord of the Cancer Laboratory of the New York State Department of Health had addressed a letter of uncommon interest to Smith. It read:

Prof[essor] Welch of Johns Hopkins suggested that you might be able to assist me in a matter relating to the research which we are carrying on in Buffalo on carcinoma. You may possibly have seen an article by v. Leyden in the *Zeitschrift für klin[ischer] Medizin*, vol. 43, page 5, in which he describes having found organisms in the peritoneal fluid of a case of carcinoma, the description of which is very much like our own experiments. He

⁵⁶ Bacterial leaf spot diseases, *Science* n. s. 19(480): 416-418, March 11, 1904.

⁵⁷ Synopsis of researches, *op. cit.*, 33.

⁵⁸ *Ibid.*, 30.

⁵⁹ *Ibid.*, 33.

⁶⁰ *Ibid.*, 34-35.

⁶¹ *Introduction to bacterial diseases of plants, op. cit.*, 264 ff.; Fifty years of pathology, *op. cit.*, 29.

⁶² *Proc. Am. Ass'n for Adv. of Sci.*, 463-480, at pp. 469-470, June-July 1902.

likewise describes the appearance of these organisms in the tissues, and we find that they are practically identical with the so-called "Plimmer's bodies," with which we have been engaged.

v. Leyden compares them to an organism, the *Plasmodiophora brassicae* Woronin, which is described by Navachine⁶³ in the Russian Archiv for Pathology, 1900, vol. 9. Dr. Welch was kind enough to send me Navachine's article, and a comparison of his illustrations with our own specimens leaves no doubt that the organism which he has studied possesses a morphological appearance scarcely distinguishable from the bodies which we have found in epithelium in carcinoma. There has likewise appeared, in volume 27 of the *Centralblatt für Bakteriologie*, by Podwyssotzky an article dealing with the same organism, which he has injected into animals and which there produces mesoblastic tumors. Dr. Welch suggested that you might possibly be able to put us in the way of getting some material from which we could work up this side of the question. The organism, as described by Navachine, produces tumor-like formations in cabbages. . . .

Dr. Gaylord had graduated in medicine in 1893 from the University of Pennsylvania. From 1895 to 1898 he had been an assistant in pathology at the University of Göttingen, and in 1899 become professor of surgical pathology at the University of Buffalo and director of the State Institute for Study of Malignant Disease.

On April 15, 1901, at a meeting of the Johns Hopkins Medical Society, Dr. Gaylord had spoken on "The Parasite of Cancer."⁶⁴ Dr. Welch, then president of the Society, introduced him, and, after Dr. Gaylord had presented his demonstration of materials, Welch complimented him for his work by saying:

Dr. Gaylord has brought before us something more than the mere description of the so-called cell-enclosures observed in hardened specimens of cancer. Of the enclosures hitherto described in preserved material the only ones which present anything like a definite organization and which, it seems to me, have not been altogether satisfactorily explained are the bodies first accurately described by Thoma and Sjöbring, and subsequently noted by most of those who have studied this subject. These bodies in English and American writings are often designated without much propriety as "Plimmer's bodies." No conclusive evidence that these bodies, still less that any other of the various enclosures, are parasites, has been furnished, and it now seems evident that no further progress in the search for parasites is likely to be made by the examination of hardened material with our present methods.

⁶³ See also, Dr. S. Nawaschin, Beobachtungen über den feineren Bau und Umwandlungen von *Plasmodiophora brassicae* Woron. im Laufe ihres intracellularen Lebens, *Flora*, 86: 404, 1899.

⁶⁴ *Bull. Johns Hopkins Hospital* 12(126): 295-296, Sept. 1901.

Under these circumstances it is important to turn to the examination of fresh material and to make attempts to cultivate parasitic organisms, provided such exist in cancer and other malignant tumors. This direction of study has therefore been followed in recent years by several investigators, and it is especially his results along these lines which Dr. Gaylord has reported to us this evening.

Dr. Welch welcomed studies which shed light on "cellular degenerations and metamorphoses" in cancer. Until, he said, "we are in possession of more evidence than has yet been furnished in favor of the parasitic hypothesis" of the origin of cancer, research workers would have to be prepared for much skepticism following the announcements of their results. He recognized that, as yet, there was

not much agreement among the different observers either in the description or the interpretation of the various bodies regarded by them as parasites to be seen in fresh cancerous material or fluids, or in such material kept free from bacterial contamination, whether mixed with some cultural fluid or not. Dr. Gaylord lays especial emphasis upon the presence in cancers and other conditions of homogeneous, yellowish, spherical bodies resembling droplets of fat but without the usual reactions for fat, and he considers that he finds evidences of multiplication of these bodies and of their passing through a definite cycle of development which he describes.

Of Dr. Gaylord's experimental results in reproducing malignant tumors. Dr. Welch concluded: "Dr. Gaylord has presented an instance of multiple nodules in the lungs of an adeno-carcinomatous nature following the intravenous injection of cancerous ascitic fluid. With this exception and one or two more doubtful cases, his experimental results . . . are, like those of other investigators in the same line, negative."

Nevertheless, when Welch read Nawaschin's article describing *Plasmodiophora brassicae*, he forwarded it to Gaylord and must have suggested that, in view of the similarity between the parasitic organism which "produces tumor-like formations in cabbages" and "the bodies which [Gaylord had] found in epithelium in carcinoma," vegetable material might be obtained from Smith to study the parasitic aspect of the question.

Welch urged thorough "microscopic examination of fresh, macerated, and preserved cancerous material. . . . As regards artificial cultures," he also said,

it is certain that no forms of bacteria demonstrable by existing methods are directly concerned in the causation of cancer, and, notwithstanding the stronger claims made in behalf of Blastomycetes, I am glad to learn that Dr. Gaylord rejects these claims and takes a position in this regard opposed to that of San Felice, Roncali, Plimmer, Leopold, and others. He interprets as Protozoa the bodies which he regards as parasites.

In 1901 Smith had not really begun to study the physiology and pathology of plant tumors, the mechanisms involved in the formation of pathogenic overgrowths in plants. He furnished specimens of *Plasmodiophora brassicae* to more than one specialist in human cancer research; and, years later, in his textbook *Introduction to Bacterial Diseases of Plants*⁶⁵ referred to a period when much study was "put upon the club-root of turnips and cabbages (due to *Plasmodiophora brassicae*) by various persons who thought they saw in it certain resemblances to animal cancer." Club-root of cruciferous plants, he pointed out, is attributable to a myxomycete and, while cell hypertrophy and more or less cell multiplication are characteristics, among other differences the overgrowth is not an active hyperplasia.

Nothing preserved among his laboratory memoranda shows that during the years 1901-1903 he was studying either this subject or crown gall in plants. If he did study either, the research was more incidental than primary, and what, if any, crown gall study was made was doubtless confined to investigating another plant disease of possible bacterial origin. This does not seem probable, however, since crown gall at that time was not thought to be caused by a bacterium, and Smith himself affirmed 1904 as the year when first he and his laboratory assistants began the real investigation which led to the discovery and isolation of *Bacterium tumefaciens* Smith and Townsend as its cause. He said:⁶⁶

My first acquaintance with this disease was on peach trees from California and the South in 1892-3, at which time an effort was made, chiefly by means of the microscope, to find in the galls a fungous or plasmodial parasite. As nothing constant of this nature was discovered and bacteria were not then in mind the subject was dropped. It was taken up again in 1904 on Paris daisies received from New Jersey with bacteria definitely in mind because the overgrowths superficially resembled olive tubercle⁶⁷ in

⁶⁵ *Op. cit.*, 551.

⁶⁶ Synopsis of researches, *op. cit.*, 39.

⁶⁷ See Smith's and Rorer's Bull. 131, iv, "Recent Studies of the Olive-Tubercle

which I was then interested and knew from pure culture inoculations to be due to bacteria. . . . Earlier than any positive work in the United States Department of Agriculture, Cavara in Italy studied the disease as it occurs on grapes, isolated a white organism and with it produced a few tumors, but of this I knew nothing until I began gathering together the literature references for

Bulletin 213 of the Bureau of Plant Industry which recorded the history of the early work on crown gall of plants.

Crown gall research seems to have been recognized by authorities of the Division of Vegetable Physiology and Pathology as principally the work of J. W. Toumey, biologist of the Arizona Agricultural Experiment Station. He, in 1900, had published his bulletin 33,⁶⁸ "An Inquiry into the Cause and Nature of Crown-Gall," which tabulated accounts of other studies made of the disease and presented the results of his own work. At the conclusion of his inquiry, he compared his results with those of some recent investigations made of *Plasmodiophora brassicae*. Among these were the recent studies by Dr. S. Nawaschin (1899).

During 1897-1898 Toumey had been director of the Arizona station and from 1898 to 1900 superintendent of tree planting in the Division of Forestry of the United States Department of Agriculture. By 1900, however, when the Yale Forest School was founded, he was chosen to be an assistant professor of forestry and for the remainder of his distinguished career as an American scientist he was at this school, becoming in 1909 professor of silviculture and in 1910 its dean and director.

Since 1892 he and Smith had had some correspondence on plant diseases; and in 1900 they evidently exchanged letters concerning a microscope to be used by Toumey presumably for some research on the physical properties of timber. On November 13, 1899, Toumey had written Woods:

You remember when I was in Washington I had one or two talks with you regarding the crown gall of the peach and allied trees. Since my return to Tucson I have been giving my entire time to the further investigation

Organism," May 13, 1908, Bur. Pl. Ind., U. S. D. A. This study was based on materials from California and Italy. *Bacillus oleae tuberculosis* was described and previous work in Europe and the U. S. considered. Also, E. F. Smith, Some observations on the biology of the olive tubercle organism, *Centralb. f. Bakt.*, II, 15: 198-200, 1905.

⁶⁸ Publ. Univ. Ariz. Agric. Sta., April 13, 1900, pp. 9, 39, 63.

of this disease. You might be interested in knowing that I have well developed specimens of the gall produced by inoculation since I returned to Tucson the 25th of last month. What is more, I have finally run down the organism that causes the disease. It is a myxomycete, something that we have all been looking for for several years. One of the most interesting things regarding it is that it evidently only infests the gall as a plasmodium. Under certain conditions the plasmodium creeps to the surface of the gall and forms amoeba-like bodies which escape into the soil. From my investigations so far, I find two kinds of these amoeboidal bodies, namely, a large one, and another that is very minute. I believe these two forms fuse as they escape from the gall and after a time the resulting amoeboidal mass comes to a resting condition. As yet I do not know the result of this resting condition, but I presume that after a time it breaks up into minute amoeboidal bodies, which, finally reaching other plants, enter them and producing plasmodia and the resulting abnormal tissue. I have traced out the life-history of the organism, with the exception of one or two breaks, which I hope to complete within the course of the next two or three weeks.

He sent for literature on the myxomycetes and other subject matter which might help him in this study. In his published bulletin,⁶⁹ he admitted:

I fully recognize the incompleteness of this investigation and the uncertainties regarding certain portions of the work, but, as other duties make it necessary for me to lay aside the work for an indefinite period, I believe it best, on account of the great economic importance of the disease, to publish the results that I have obtained without waiting to make additional spore inoculations or to complete certain cytological investigations which I have in view and hope to make as opportunity permits.

Crown gall was to be studied by Smith and his laboratory assistants for many years and in many genera and species of plants. He had never planned to devote his laboratory researches to purposes other than investigations in pathology directly or indirectly pertinent to agriculture and the study of plants. But, while studying crown gall, he discerned an analogy between plant teratoid tumors and tumors of animals and man, and, while no complete analogy was ever claimed to be established, he offered his authenticated proofs of resemblances to scientists in the hope of aiding medical research to solve the difficult problem of human cancer. He often consulted pathologists in medical science but at no time claimed to be a medical pathologist. Throughout his career he remained a plant scientist and, by his studies, established

⁶⁹ *Idem*, 63.

that cancer, or a disease in points quite similar to cancer, exists in plants. In so doing, he evolved a logical theory of cancer development.

In plant bacteriology Smith occupies "a position comparable with that of Koch in the field of medical bacteriology."⁷⁰ The foremost importance of his work on crown gall, furthermore, may be said to have been his study of this as a disease of plants caused by a bacterium. His background in pathology and bacteriology, however, had been closely linked to research in sanitation and hygiene, public health, and medicine, and medical research men gradually became interested in his crown-gall-animal cancer analogy. Dr. Gaylord was among the first to recognize its importance from the standpoint of fundamental biology, perhaps even from its indicated values to the knowledge of cellular physiology and pathology. He called on Secretary Wilson and persuaded him to allow Smith sufficient latitude in his study of plant overgrowths to include the animal cancer analogy. Great economic and scientific importance to agriculture attached in solving the problems of this and other similarly destructive diseases of plants; and during the century's first half-decade Smith was almost totally absorbed with studying diseases of plants as such. But after 1907, for reasons yet to be divulged, the work as to crown gall began "to offer a clue which might lead to the solution of the greater and very obscure problem of the origin of malignant human and animal tumors."⁷¹ Dr. Gaylord interested other medical scientists in Europe as well as in America in Smith's work. Other doctors, leaders in the field of cancer research and among them Dr. James Ewing,⁷² we shall see encouraged Smith to elaborate the analogy on the basis of a fundamental study of pathological growth.

In October 1901, Smith began to move his laboratory into the larger quarters in the Department's building situated in the 1300 block on B Street—two large rooms on the third floor which were far more spacious and adequate than the quarters of the dwelling on 13th Street Southwest. In these years, the Department of Agri-

⁷⁰ Arthur T. Henrici (revised by Erling J. Ordal), *The biology of bacteria*, 353, Boston, D. C. Heath and Co., 1948.

⁷¹ E. F. Smith, Mechanism of overgrowth in plants, *Proc. Amer. Philos. Soc.* 56: 437, 1917. Address read April 13, 1917.

⁷² A. J. Riker *et al*, Some comparisons of bacterial plant galls and of their causal agents, *Botanical Review* 12(2): 59 (as to Dr. Ewing), Feb. 1946.

culture was scattered over an area covering several blocks and was housed in several buildings, the most important of which was the Administration Building of red brick which faced northward and was surrounded by a beautiful horticultural park called the Agricultural Grounds. Some of the laboratories, including those of vegetable physiology and pathology, had been located in nearby dwellings.

During the years 1901-1903 several very able plant scientists came into the employment of the Bureau of Plant Industry, and with the selection of some of them Smith had considerable to do. For instance, Rodney Howard True,⁷³ after an exchange of letters with Smith, was appointed plant physiologist for, among other studies, drug plant, poisonous plant and fermentation investigations. For four years, 1895-1899, True had been a member of the faculty in pharmacognosy at the University of Wisconsin, and from 1899 to 1901 had lectured in Radcliffe College and Harvard University. He hesitated before accepting the position. Plant physiology was his preference, he told Smith in a letter of March 12, 1901, but he was not sure that his present interest in toxicological science would fit into the work as planned for him. The two men had been acquainted since the Madison meeting in 1893 of the American Association for the Advancement of Science and, as True has revealed in his biographical sketch of Smith many years later,⁷⁴ his interest in Smith's work had heightened when at the University of Leipzig he had attended Alfred Fischer's lectures in bacteriology and heard "him mention an American named Smith who claimed that bacteria could grow in plants, that they did so grow and produced diseases in them as in animals. The naturally rather bitter tongue of Fischer," True told, "denied this claim and laid Smith's 'blunder' to a dirty technique in terms that an American present felt were intended to reflect rather broadly on the state of science in Smith's country. This was an episode in that most significant polemic in which, like Pasteur in his day, Smith fought and fought strenuously for his glimpse of a very important truth."

⁷³ H. L. Shantz, Rodney Howard True, *Science* n. s. 92(2398): 546-547, Dec. 13, 1940.

⁷⁴ Rodney H. True, Erwin F. Smith, *Phytopathology* 17(10): 675-680, quotation at pp. 675-676, Oct. 1927.

Dr. Galloway, chief of the Bureau, and Dr. Woods, in charge of investigations in plant physiology and pathology, consulted Smith, Webber, and other officials when choosing new scientific talent. In 1901 George Thomas Moore, a graduate in science from Wabash College, possessor of three degrees from Harvard University, for a while assistant in cryptogamic botany at Harvard, who had taught botany both at Radcliffe College and Dartmouth College, accepted a position as physiologist and algologist of the Bureau and in 1903 was to be placed in charge of the Department's Laboratory of Plant Physiology.

Karl Frederic Kellerman was a son of Dr. W. A. Kellerman. He was a former student of the Ohio State University, a graduate in science at Cornell, and during 1900-1901 an assistant in botany there. In 1901 he was appointed an assistant physiologist in the Bureau. During 1905-1906 he was to have charge of the laboratory of plant physiology and then be physiologist in charge of investigations in soil bacteriology and water purification. In 1902 W. T. Swingle took charge of investigations in crop physiology and breeding. With such leaders in American botanical research, the Department's working force in plant physiology was composed of able men.

Smith interested himself in building up the research personnel in plant pathology. On March 19, 1901, he had addressed a letter to Dr. Thaxter of Harvard in which he asked the learned associate of Dr. Farlow to suggest students suitable for scientific aides who would accept positions with the Department "in the nature of fellowships." Thaxter replied that he could think of no one at that time who was "in a position to take such a place," but he promised to advise Smith should he learn of any. The new chief of the laboratory of plant pathology may have sent similar letters to other American botanists, and he corresponded with some botanists with a view to employing them for general or special reasons.

In 1898 he had helped Charles Orrin Townsend secure his position as professor of botany at Maryland Agricultural College and as state plant pathologist there. In 1884 Townsend had graduated from Michigan State Normal School and obtained in 1888 and 1891 his bachelor and master of science degrees from the University of Michigan. After periods of teaching at St. John's

College, Annapolis, and at Wesleyan College, Macon, Georgia, he had specialized in botany and plant physiology, studying in 1897 at the University of Leipzig. In April, 1898, while filling a temporary position as instructor of botany at Barnard College, Columbia University, he inquired of Smith whether he knew of a vacancy "in teaching, experiment station, or U. S. Dep't of Agriculture."

Townsend accepted the offer from Maryland Agricultural College. But he left there a few years later to take a position with the Department of Agriculture to investigate sugar beet diseases. J. B. S. Norton became professor of botany and vegetable pathology at College Park, and state pathologist of Maryland. "If I take up the study of sugar beet diseases," wrote Townsend to Smith on March 9, 1901, "it will probably be necessary to do considerable in the way of planning experiments before July 1st. My year here closes June 30th." That summer he began work in Smith's laboratory of plant pathology and studied, among other things, a blight in cherry, a disease of seed onions from Bermuda, and beet diseases. He canvassed beet sugar factories for information regarding beet diseases he was to study: damping off, curly top or blight, leaf spot, leaf scorch, beet scab, rhizoctonia rot, root gall, etc.⁷⁵ Soon he was to begin a journey to the west to investigate diseases in the field. The beet sugar industry had expanded more rapidly than perhaps any other in the United States, and was exacting much attention from agriculturists.⁷⁶

The demand for American cotton had been increasing of recent years, and the cotton-growing industries more and more were being extended to Texas and other southwestern states.⁷⁷ On April 24, 1901, Benjamin Minge Duggar announced that he had accepted a position with the Department, his work to be in part to investigate cotton diseases in Texas and the southwest, and by August he was planning a reconnaissance to his investigation-field. He had studied under Atkinson and Farlow, and at one time worked with Forbes's Illinois natural history survey. Before and after taking advanced work at the University of Leipzig, he had been a graduate instructor and assistant professor of plant physi-

⁷⁵ See, C. O. Townsend, Some diseases of the sugar beet, *U. S. Dep't of Agric. Report* 72: 90-101, Aug. 30, 1902.

⁷⁶ *Yearbook U. S. Dep't of Agric. for 1901*: 487.

⁷⁷ *Idem*, 196, 203, 205.

ology in the College of Arts and Sciences of Cornell University. "As you know," he wrote Smith, "I have always devoted a considerable part of my time to pathology; but during the past two years, particularly, I have given much attention to physiological matters, and certain problems are now under investigation which have a distinct pathological bearing." He believed that the cotton disease research would "undoubtedly well repay study," and on August 9 wrote Smith of his preparations: "I have been looking up the soils of Texas and the distribution of cotton areas with reference to the probability of disease." He promised to send any specimens of Rhizoctonia disease he might find in the southwest.

On July 8, 1901, Dr. R. A. Harper, Professor of Botany at the University of Wisconsin, expressed satisfaction that one of his students, Deane B. Swingle, had been appointed to a position. "He is a good worker,"⁷⁸ wrote Harper, "and will push things, I think." By August he was at work in the laboratory, and during the middle of the month Randolph E. B. McKenney arrived as another assistant. He was a former student of W. P. Wilson, had been an assistant in botany under Macfarlane at the University of Pennsylvania, and came from a position as a teacher of biology at the Santa Ana, California, high school.

Smith was always proud that he had been first to recommend a woman for scientific work in the Department. "The first research woman in the Department of Agriculture," he later said,⁷⁹ "was Effie A. Southworth [later Mrs. V. M. Spalding], appointed in 1887 and I am proud to say at my suggestion. Since then there have been many, especially in the Bureau of Plant Industry." They had "demonstrated marked ability in pathological research, not only in [his] laboratory and other laboratories in the Department of Agriculture, but elsewhere." In July 1901 Dr. Galloway employed Miss Agnes J. Quirk and she became an assistant in Smith's laboratory. In school she had not been specially prepared for the work, but he recognized her ability as a student, prescribed a

⁷⁸ Swingle by 1903 had prepared B. P. I. bulletin 37, Formation of the spores in the sporangia of *Rhizopus nigricans* and of *Phycomyces nitens*; by 1904, with Smith, bulletin 55 on Dry rot of potatoes due to *Fusarium oxysporum*; and in 1904 they made "more than 100 freezings of various bacteria in liquid air, and in salt and crushed ice, showing that the critical temperature is around zero Centigrade and that repeated short freezings and thawings are much more destructive than a single longer freezing." See, Synopsis of researches, *op. cit.*, 21.

⁷⁹ Fifty years of pathology, *op. cit.*, 45.

course of reading and study, found her proficient, and we shall see that eventually she capably assisted him in some of his most valuable work. Until her retirement recently, she has been in the employ of the Bureau, and for years was herself in charge of one of its main laboratories.

Dr. Smith had several women scientific assistants. On September 24, 1901, Dr. Spalding asked how he should "advise a graduate student who thinks of preparing for work in your department: Miss Florence Hedges, from Lansing, Mich[igan], graduated last June from the U[niversity] of M[ichigan]." He recommended her as "a really bright, strong woman" whose "work in botany, plant physiology, and ecology was a good deal above the average." If she were to teach he planned for her "certain research work in ecology." But if she were to prepare for work in the agricultural department, which she rather preferred, he thought it "best for her to study fungi with [him], bacteriology with Novy, enzymes etc. with Newcombe," and whatever other subjects Smith suggested.

In 1902 Miss Lilian Wheeler resigned from her position to marry. Spalding again recommended Miss Hedges and she was employed that year for work in the laboratory of plant pathology.

Smith also employed Lloyd Stanley Tenny, a University of Rochester graduate where C. W. Dodge was a member of the faculty. Tenny had been prepared in botany and biology, and was to achieve in several capacities an illustrious career in American agriculture.

Two other men of future prominence in agriculture of the Americas who soon became assistants in the laboratory were James Birch Rorer, a graduate at Harvard in 1899 and for two years while earning an advanced degree an assistant in botany there, and John R. Johnston, also from Harvard, who later did important work as pathologist for the United Fruit Company and in charge of their investigations of economic plant diseases.

Still another botanist of the laboratory who was employed during this period was Miss Alice Haskins.

The work of the three main laboratories—physiology, pathology, and breeding—was supplemented by nation-wide field research. In many states where the Department did not have branch facilities, cooperative investigations with local agricultural experiment stations were in effect. Recently, in 1901, under P. H.

Rolfs's direction, the new Sub-tropical Laboratory at Miami, Florida, had been established. True, in 1902, for drug and medicinal plant investigations, had located in Vermont and elsewhere experimental plantations which included the growing of plants in alpine regions. Fungicides and insecticides were still being discovered. During the decade the self-boiled, so-called, lime sulfur mixture came into usage as a fungicide to control without injuring foliage a number of diseases, and later, replacing the liquid, the use of dry sulphur in restricted application.

At this time, in the central west, the disease known as bitter rot of apples, occasioning enormous losses, was being investigated at the Mississippi Valley laboratory and several state experiment stations. Smith later said of this:⁸⁰

In 1905 Scott, of the Bureau of Plant Industry, had remarkable success in preventing bitter-rot of the apple by use of the Bordeaux mixture. This disease which caused losses estimated at \$10,000,000 in 1900, and very great losses in the years immediately following, was controlled in his experiments to the extent of 93 to 98 per cent. Expressed in bushels, the sprayed trees (those that received 5 or more treatments at the proper time) yielded 50 to 60 bushels of sound apples, while all the apples rotted on the control trees, that is, *A* bore 1 sound apple, *B*, 6 sound apples, and *D*, 2 sound apples. The fungus *Glomerella*, cause of this rot, was extensively studied by Hermann von Schrenk, later by many others.

Among others, Cornelius Lott Shear, a plant pathologist of the Department, contributed very valuable studies on *Glomerella*. L. L. Harter, another Department plant pathologist, also was to contribute very valuable work on sweet potato diseases.⁸¹ Smith continued to hold in high esteem the work done in breeding disease-resistant plants. Full discussion of this subject belongs to a later period. Yet, to illustrate, it may be pointed out that in 1896 asparagus rust swept over and alarmed large sections of the nation, and by 1913 and before then J. B. Norton of the Bureau of Plant Industry had bred plants⁸² resistant to the destructive fungus. Smith's work mainly was now in indoor laboratory

⁸⁰ Fifty years of pathology, *op. cit.*, 29. Concerning lime-sulfur and self-boiled lime-sulfur as a remedy against apple scab, peach leaf curl, brown rot of peach, etc., see H. H. Whetzel, *Outl. Hist. of Phytop.*, *op. cit.*, 112-113.

⁸¹ *Idem*, 33. Smith said of this: "In 1913 Leonard Lee Harter described foot-rot, a new disease of the sweet potato, and in 1913, 1914, and 1915, he published on stem rot of the sweet potato." See, T. S. Harding, *op. cit.*, 81 for an account.

⁸² *Idem*, 33.

research. He seldom did field research at a distance, although he continued to direct field investigations the nation over. His real speciality now was, as it had been for several years, plant bacteriology.

Perhaps this was the real reason why in 1904 he found it impossible to give the address on plant pathology before the Congress of Arts and Science held at St. Louis as a part of the Universal Exposition.⁸³ When Dr. Simon Newcomb, president of the Congress, received his refusal, he asked Smith to suggest "three or four men eminent in this line who might be asked to deliver the address in question." Waite presented an address on "Vegetable Pathology, an Economic Science," and Arthur gave an address on "The History and Scope of Plant Pathology." Several American scientists discoursed before this conference. Duggar, now Professor of Botany at the University of Missouri, spoke on "Plant Physiology—Present Problems"; Theobald Smith, on "Some Problems in the Life-History of Pathogenic Microorganisms"; and in E. O. Jordan's address on "Relations of Bacteriology to Other Sciences," special attention was drawn to the "new division of technologic science," including now bacteriology of the soil, dairy, barn-yard, tan-pit and canning factory. Jordan did not fail to mention other advances made possible by bacteriology including new knowledge of diseases of domestic plants and animals, and advances in public hygiene. Similar to the past warfare of man against malaria and typhoid fever, the recent movement to study and suppress tuberculosis had become "one of the first attempts to apply bacteriologic knowledge in a determined and radical way to a problem of public hygiene." Furthermore, and somewhat indicative of future research in animal and plant bacteriology, the "study of the ultramicroscopic, or perhaps more correctly, the filterable viruses, [was] being prosecuted with great energy and in a sanguine spirit. The extension of bacteriologic method into the field of protozoön pathology," constituted, Jordan said, "one of the latest and most helpful developments in the study of infectious diseases." Included in this last was "renewed study of the remarkable protozoa called trypanosomes." Indeed, so great had

⁸³ Volume 5, edited by Dr. Howard J. Rogers, *op. cit.*, in which each and all of the following referred to addresses and quotations may be found. The quotations from Dr. Jordan's address may be found pp. 208-214.

the advances in bacteriology been that the comparatively new science had virtually appropriated the field of microbiology.

A second reason why Smith may not have found it possible to deliver the address on plant pathology at the International Congress of Arts and Science was that he was completing the first volume of his treatise, *Bacteria in Relation to Plant Diseases*. January 25, 1904, Secretary Charles D. Walcott of the Carnegie Institution of Washington had informed that the executive committee had agreed to publish the monograph and requested "a statement of the extent of text and illustrations." June 6, the day Smith sent to Newcomb his refusal of the honor of the address, Galloway recommended to Secretary Wilson that Smith's publishing arrangement with the Carnegie Institution be approved since the work would "be a valuable addition to our knowledge of bacteriology, and [would] place before the world very fully the advances that have been made, especially by this Department, in the last fifteen years."

Dr. Ira Remsen "grilled" Smith on the subject matter and Smith believed that the outcome of this conference secured the executive committee's approval. His friend, Dr. Robert Simpson Woodward, was not yet president of the Institution. Before the interview with Dr. Remsen, the author had conferred with Dr. Osler, Dr. Welch, and others of the Johns Hopkins Medical Faculty, and, perhaps, with Dr. Weir Mitchell, Dr. Billings, and others influential in medical bacteriology. November 6, 1903, when Smith had appealed to Farlow to help him find a publisher, he had examined experimentally and photographed forty-eight of the one hundred and twenty-five bacterial diseases of plants to be dealt with in the work. Part of the material had then been in manuscript for four years. His desire then was to publish all in one, or several, volumes; at least, to publish parts and at regular intervals. For some years he had planned to include an outline of laboratory methods, and upon the history and growth of plant bacteriology he had written and spoken. By 1904 he possessed about five hundred illustrations, most of these original and unpublished, and an uncompleted text of some two thousand typewritten pages. So pleased had Dr. Welch been with his work he at first suggested that Johns Hopkins University might publish it. "I am contemplating a trip to Europe next year," Smith told Farlow in

1903, "in the hope by personal observation of clearing up much that the literature of particular diseases has left in doubt."

Farlow answered promptly:

I am very glad to hear that the work on bacterial diseases of plants on which you have been engaged so long has now reached the stage of publication. Such a work is very much needed and I am sure that there is no one so competent as yourself to treat the subject. . . . [T]he work is sure to be of such a character as to be not only a credit to the [Carnegie] Institution . . . but also a boon to those numerous botanists, vegetable pathologists and scientific agriculturists who, without such a compendium of the results of modern investigation, have at present to derive their information from an endless number of scattered papers in different journals and publications of different societies.

Farlow characterized the prepared material as a "very valuable work." During these years plant pathologists were adding several systematic productions to the accumulated literature of the science. Such a volume as H. W. Conn's *Agricultural bacteriology*, published in 1901 and reviewed in the *Experiment Station Record*⁸⁴ as "one of the first books in the English language that covers the whole range of the relation of bacteria to agriculture in its broadest sense," was a "good general treatise on the subject." Later, Smith, in his second volume (p. 18) of *Bacteria in Relation to Plant Diseases*, would notice that but "5 pages out of 419" in this work were devoted to the bacterial plant diseases. More technical and restricted in purpose were several other important literary works. In 1904 Klebahn published *Die Wirtswechselfen Rostpilze* which Smith regarded as an "important book on the rusts that have more than one host."⁸⁵ That year, also, appeared Clinton's⁸⁶ "North American Ustilagineae." Furthermore, several important papers on the parasitic *Gloeosporium*s were coming out of Europe from various authors. From the standpoint of North American mycological science, however, the most important literary appearance of these years was in 1905 when the first and only installment of Farlow's *Bibliographical Index of North American Fungi* was published. By 1919, the year of Dr. Farlow's death, approximately 300,000 references were indexed and available.

Not until 1909 would a comprehensive American textbook on

⁸⁴ 13(7): 623, 1902.

⁸⁵ Fifty years of pathology, *op. cit.*, 29.

⁸⁶ Charles Thom and E. M. East, George Perkins Clinton, *Nat'l Acad. of Sci. Biog. Mem.* 20: 183-196, 1939.

plant diseases be published—*Fungous Diseases of Plants*, by B. M. Duggar. This book would appear almost two years after the first departments of plant pathology in American universities had been created—at Cornell University under H. H. Whetzel, author in 1918 of *An Outline of the History of Phytopathology*, and at the University of Minnesota under E. M. Freeman, author in 1905 of the pioneering work, *Minnesota Plant Diseases*. On February 1, 1910, L. R. Jones would establish at the University of Wisconsin one of the leading and, in instruction and research, most influential departments of plant pathology in any American university. The creation of this department would be practically contemporaneous with the founding of the American Phytopathological Society and precede by less than a year issuance of the first number of the Society's journal, *Phytopathology*. Smith would find⁸⁷ Duggar's book compared "very well indeed" with the standard European literature and manuals of plant pathology by Tubeuf, Sorauer, Kirchner, and Frank in Germany; Prillieux and Delacroix in France; Comes in Italy; and Ward of England, whose new treatise, *Disease in Plants*, was published in 1901. Whetzel began his instruction in diseases of plants, both in the college of arts and sciences of Cornell and in the department of the New York State College of Agriculture, with the use of mimeographed sheets prepared by himself from Küster's *Pathologische Pflanzenanatomie* which in first edition was published in 1903. Sydow began this year his *Annales Mycologici*, and about this time Willam H. Park's *Bacteriology in Medicine and Surgery* and the great four-volume work on infectious diseases of man and animals by W. Kolle and A. Wassermann, *unter Mitwirkung von 54 Fachmänner. Handbuch der pathogenen Mikroorganismen* (1902-1904), were making their appearances.⁸⁸

At the Washington meeting in 1902 of the American Association for the Advancement of Science, the mycologists present gathered and discussed forming a permanent organization. The response being favorable, an arranged meeting to organize was held the following year on December 29 at St. Louis. Dr. Arthur was temporary chairman and Dr. F. E. Clements secretary. Arthur

⁸⁷ *Science* n. s. 32(810): 56-58, July 8, 1910.

⁸⁸ Fifty years of pathology, *op. cit.*, 30-31. See also *Bacteria in relation to plant diseases* 1: 206, bibliography.

recently had been president of the Botanical Society of America and the subject of his address as retiring president was on "Problems in the Study of Plant Rusts." Farlow, at the St. Louis meeting, was elected president of the American Association. He, therefore, could not accept the presidency of the newly organized American Mycological Society. Dr. Thaxter became president, Dr. Earle vice-president, and Dr. Clements secretary-treasurer.⁸⁹ This year Dr. Thaxter had resigned as president of the Society for Plant Morphology and Physiology, and his "presidential mantle," so he wrote Smith on July 22, 1903, had fallen to Conway MacMillan. Mycologists and plant pathologists were now to have separate organizations, although many were to be members of both the mycological society and the American Phytopathological Society when at the Baltimore meeting of the American Association (Dec., 1908-Jan., 1909) the Phytopathological Society was organized on a preliminary basis and held its first annual meeting at Boston the following December.⁹⁰

Until 1904 not many papers concerned with study of bacterial diseases of plants had been presented before the American Society of Bacteriologists. At its third annual meeting, held at the University of Chicago in 1901,⁹¹ and over which Dr. Welch presided, every paper had either a medical or milk or water bacteriological significance, or some other special phase of research in bacteria other than plant diseases.

Before the sixth annual meeting of the Society of American Bacteriologists, held at Philadelphia December 27-28, 1904, Smith presented papers: one, "The Effect of Freezing on Bacteria,"⁹² the results of more than one hundred experiments using about a dozen different bacteria—saprophytes and plant and animal pathogenic forms. These experiments tended to confirm and extend the researches of Prudden, Park, Sedgwick, and Winslow, and were performed with Deane B. Swingle. Six conclusions were set forth with the added deduction that "Probably an enormous number of

⁸⁹ The American Mycological Society, *Jour. Mycology* 10(69): 46-47, Jan. 1904.

⁹⁰ C. L. Shear, First decade of the American Phytopathological Society, *Phytopathology* 9(4): 165 ff., Apr. 1919. See, also, H. H. Whetzel, *Outl. Hist. of Phytop.*, *op. cit.*, 110-111.

⁹¹ H. W. Conn, American Society of Bacteriologists, abstracts of papers presented, *Science* n. s. 15(375): 361-379, March 7, 1902.

⁹² *Science*, n. s., 21(535): 481-483, March 31, 1905.

bacteria are destroyed by every winter, and those which survive come through in the form of endospores or some other resistant shape." Smith also submitted before the meeting an "Exhibition of Cultures on Starch Jelly and on Silicate Jelly."⁹³ Nutrient starch jelly had been described in 1898 at the Boston meeting of the American Association for the Advancement of Science: and the second, to appear more fully in the first volume of *Bacteria in Relation to Plant Diseases*,⁹⁴ represented an extension of his earlier paper, "Some little-used culture media which have proved valuable for differentiation of species." More than once he combined the subject matter of his papers with materials for the text of his monograph. His paper, "Growth of Bacteria in the presence of Chloroform and Thymol,"⁹⁵ first given at Baltimore before the Society of American Bacteriologists in 1900, was elaborated more fully several years later in the first volume of his text.

In 1904, at Philadelphia, Smith presented before the eighth meeting of the Society for Plant Morphology and Physiology three papers: one, results from experiments continued in 1904 on stomatal infections of black spot of plum; another, on "Burrill's Disease of Broom Corn,"⁹⁶ results of continued study that summer with Miss Florence Hedges on one of the Department's farms at Washington; and, third, the results of his studies at Baracoa, Mata, and Yumuri in eastern Cuba of "The Bud Rot of the Coconut Palm in the West Indies."⁹⁷ In the spring of that year he and Rorer had journeyed via Miami to the Bahamas and Cuba, and studied the disease in April.

April 26, 1902, W. A. Kellerman, announcing to Smith that he was "arranging to take up again the Journal of Mycology, and make it a Quarterly to represent all phases of the subject, except the purely economic side," asked Smith for a list of his recently published writings. This scholar was preparing "a Bibliography and Index (author and subject—also host index of new species)

⁹³ *Idem*, 482.

⁹⁴ *Op. cit.* (starch jelly), 50, 196; (silicate jelly) 36, 198. See also, *Proc. Amer. Assoc. Adv. Sci.* 47: 411-412, 412-413, 1898.

⁹⁵ *Science*, n. s., 13(322): 327, 1901; *Bacteria in Relation to Plant Disease*, *op. cit.*, 1: 74.

⁹⁶ *Science*, n. s., 21(535): 502-503, March 31, 1905.

⁹⁷ *Idem*, 501-502.

of North American Mycology for the year 1901." He asked Smith not only for the list but also for something written to be published in the second number of the *Journal*. "I wish to include the botanical side of Bacteriology," said Dr. Kellerman, "and will be pleased to have your assistance along this line from time to time." In January of the previous year, James McKeen Cattell of the editorial department of *The Popular Science Monthly* had asked Smith to supply "some notes on botany and especially on physiological botany." Smith already had more work than he could do with the thoroughness and exactitude which characterized his every effort. Each year his work had increased, not lessened. Because of many things to do in his laboratory, aside from completing the manuscript material for the first volume of *Bacteria in Relation to Plant Diseases*, he had to refuse some offers which a few years before might have been esteemed as opportunities. Bacteriologists encouraged Smith to complete his "very valuable work."

June 28, 1905, Smith's salary was increased to \$2,750 per annum. September 22, he addressed a letter to Farlow:

It is with mingled feelings that I am sending you the first volume of my new book. I hope that at least some things in it will meet your approval and seem to you to have been worth the doing. I have made quite a number of changes in generic nomenclature, but have at the same time tried to be conservative. If I were to do the book over again I think I should enlarge some portions and restrict others, but good or bad, it is too late for any regrets of that sort. The printers were unconscionably slow and the thing has been running through the press for 15 months, but I am in hopes we can make shorter work of the remaining volumes.

I did hope to get up into New England this summer, but the time has passed, and every minute of it was very fully occupied here in Washington. . . .

Dr. Farlow was pleased with the volume. Dr. Kellerman wrote that

It is a splendid work, gotten up in nice shape, and as useful as attractive. Such work as this is certainly most creditable to American mycology—I will not say bacteriology, because the botanists who work in that field will not object to being called mycologists. Your illustrations are as good as illustrations can be made to-day. I wonder if the next generation can possibly laugh at the work we are issuing to-day, that is to say, particularly the illustrations that we regard as almost perfect!

Your bibliography will be most useful to everyone—as I know the remainder will be to those who work with bacteria.

I have in mind to issue a number of the *Mycological Bulletin* devoting the entire space to bacteria. . . . I would like to have this number refer mainly to your splendid volume.

C. E. Bessey, "very much pleased with the new book," announced that he would have it reviewed in *Science* by himself or a friend in bacteriology. P. H. Rolfs, pathologist in charge of the Subtropical Laboratory at Miami, called the book "an epoch making one in plant bacteriology." He was sure "that the vegetable pathologists [would] feel very grateful to [Smith] for bringing together this immense amount of material." L. H. Pammel offered to review it in the *American Naturalist* or the *Centralblatt*. He congratulated the author "on this important monograph, certainly the most complete monograph on bacterial diseases of plants published anywhere and a work which will rank foremost in bacteriological lines of investigations. You have, indeed, prepared," he wrote, "an excellent manual." L. H. Bailey believed that the work would "place the subject of the bacterial diseases of plants on a safe and rational foundation" and showed good judgment in its presentation.

L. R. Jones described the volume as a "*magnificent* work." Recently he had published in the *Centralblatt*⁹⁸ his "Studien über die cytohydrolytischen Enzyme, die durch die Bakterien, welche weiche Fäulnis bewirken, erzeugt werden." At the University of Vermont, Warner Jackson Morse, assistant botanist of the experiment station and assistant professor of bacteriology, was "just starting a dozen enthusiastic bacteriologists off in lab[oratory] work," and they were monopolizing the laboratory equipped for the study of plant bacterial diseases.

In the first years of the twentieth century had appeared two epoch-making discoveries in science, both great biological classics of all time. Hugo de Vries, scientific genius of the botanic garden of the University of Amsterdam, Holland, led by a bibliographical reference appended to L. H. Bailey's published lecture, "Cross Breeding and Hybridizing" (1891-1892), had discovered a paper of tremendous potential value to evolutionary understanding. This

⁹⁸ *Centralb. f. Bakt.* II, 10: 746-747, 1903, Abstract, *Proc. Soc. Plant Morphology and Physiology*.

was the monk Gregor Mendel's celebrated study of "Plant hybrids," a paper submitted without ostentation and printed in 1865, without any recognition of its substantive worth, in the proceedings of the Society of Natural History of Brünn (Brno), Austria (now of Moravia, Czechoslovakia).

De Vries, himself at the time perfecting his celebrated mutation theory based on years of experimental study of *Oenothera lamarckiana*, the evening primrose, was enabled thus in 1900 to bring Mendel's famous paper into widespread scientific notice by announcing its discovery together with his first elaboration of his own similarly famous hypothesis. His mutation theory and the theory of genetic inheritance implicit in Mendel's hybridization results provided science with not merely an extension of Charles Darwin's theory of natural selection but really a new theory as to the origin of some species. Experimental research workers were, therefore, soon at work investigating the genetic truth of each theory. At about the same time that De Vries became aware of the extraordinary value of Mendel's paper, two other European botanists of world renown, C. Correns and E. von Tschermak, properly evaluated its significance. De Vries because of his mutation theory, however, became the dominant figure of the scientific world. His work had strengthened the Darwinian theory of evolution by adding a profound, new, and fundamental truth: at least, an hypothesis to be investigated by scientists for many years to come. In 1904, invited by the Carnegie Institution of Washington to give the dedicatory address of the Station for Experimental Evolution at Cold Spring Harbor, Long Island, he visited America, lectured at several universities and institutions of science, and was a principal speaker and attraction at the Congress of Arts and Science of the Universal Exposition held at St. Louis.

At the Missouri Botanical Garden then, Frank N. Meyer, once a gardener at the botanic garden of Amsterdam and a plant explorer for the Department of Agriculture, was working under a part-time employment secured perhaps through a recommendation from Smith to Trelease. He kept Smith advised as to De Vries's movements in this country, and it is probable that at St. Louis, Washington, or some other point, De Vries and Smith became acquainted. On November 20, 1905, De Vries sent "a photograph of Wakker in his earlier days" and added:

Some days ago I received your magnificent volume on the Bacteria in relation to Plant Diseases. I have studied it in a preliminary way and with much interest, and find that it contains many points which will be useful even to those who suffer from plant diseases, although they are no[t] bacteriologists—as is my case. I intend to give it to one of my students [] to thoroughly study it with him and have him work on some bacterial disease of our garden according to your methods. I think that by this means he will be able to bring quite a useful contribution to your Science. Kindly recommending myself for the second volume and wishing you all the success you so well deserve, Yours truly Hugo de Vries.

Savastano, Peglion, and other important European students of bacterial diseases of plants reviewed Smith's volume in scientific journals which accorded the work the highest recognition.

Henry Kraemer, editor of the *American Journal of Pharmacy*,⁹⁹ wrote: "I have not seen a book in some time that pleases me like your book. It is in my judgment a most needed work and if the other volumes are like this it will be the most important contribution yet made to the study of bacteriology."

A reviewer in the *Plant World*¹⁰⁰ spoke of Smith as "the foremost American student of phytopathological bacteriology," and in *The Nation*¹⁰¹ plant physiologist G. L. Goodale welcomed his volume as "a treatise on the right use of the necessary appliances" and methods of investigation for "professional workers" in the "comparatively new field" of "plant pathology and therapeutics." N. A. Cobb, director of the division of pathology and physiology of the Hawaiian Sugar Planters' Association's experiment station at Honolulu, wrote that he had grown mentally since perusing Smith's "beautiful work on Plant Bacteriology." Oscar Loew of the Imperial University of Japan thanked Smith for his "splendid and comprehensive work." They had been correspondents for many years, both before and after Loew had participated in this country in a tobacco improvement program sponsored by the Department of Agriculture. While his work had been some highly specialized investigations in plant physiology, his results had had important bearings on morphology and

⁹⁹ Published by authority of the Philadelphia College of Pharmacy. 78(2): 96-98, Feb. 1906, contained a review of Smith's volume.

¹⁰⁰ 8(12): 307, Dec. 1905. Francis E. Lloyd, editor, publisher, and presumably the reviewer.

¹⁰¹ 83(2162): 493, Dec. 6, 1906. Review was not signed by Dr. Goodale but Smith knew that he had prepared the review.

pathology. He wrote of the book: "It is a capital standard work, for which please accept my heartiest thanks!" Many letters of appreciation from American and European botanists attested their high estimate of the volume's value.

Since 1899 Dr. James Ewing had been professor of pathology at the Cornell University Medical College in New York City. His degree as a doctor of medicine had been obtained at the College of Physicians and Surgeons of Columbia University and at that institution he had been a tutor in histology and an instructor in clinical pathology. He was a holder of a bachelor and a master of arts degrees from Amherst College. Throughout his entire career he was one of America's leading medical pathologists and bacteriologists. He wrote Smith on September 26, 1905: "I am delighted to see your magnificent volume on the Relation of Bacteria to Plant Diseases. I have often wanted to know something about plant pathology and the chances of learning now seem much brighter." He and Smith had had at least one consultation, and Dr. Ewing promised that all of the publications of the medical college would be sent him.

Dr. Simon Flexner of the newly established Rockefeller Institute for Medical Research, located at 50th Street and Lexington Avenue, placed a copy of the volume in the Institute's library and wrote that he was "confident that it will be of great value to workers in bacteriology." He asked for a copy of the second volume when published and invited Smith to visit the Institute.

From Harvard Medical School, Dr. W. T. Councilman of the department of pathology thanked and congratulated Smith on his "admirable work" which, he believed, "will form one of the most useful books in our laboratory." To Dr. Theobald Smith the volume invited "thorough study and the illustrations promise[d] to be very helpful." He hoped to read it carefully since, he said, "I am very much interested in the phenomena of parasitism and intend to learn something of the work among plant parasites. It is a monumental piece of work. Your kind reference in the preface is scarcely deserved, but it is accepted with due appreciation of the good will which prompted it."

Dr. Sternberg found it a "valuable work . . . evidently very complete."

Dr. V. A. Moore wrote: "I am satisfied that you have done an

excellent piece of work and that it will result in much credit to you and the good of the cause."

Since 1904 Dr. William Osler had been Regius Professor of Medicine at Oxford University, England. He, who with Welch, Halsted, and Kelly had formed the great four of Johns Hopkins Medical School, sent a letter on November 21, 1905: "Dear Smith:—Congratulations on your superb volume which arrived last week. It will be an enduring monument to your fame. How splendidly, too, the Carnegie Trust has published it. I hope you are not working too hard."

That year on December 28 Dr. Smith presented before the ninth meeting of the Society for Plant Morphology and Physiology a paper on "Channels of entrance and Types of movement in Bacterial Diseases of Plants."¹⁰² Most of his papers on plant bacteriology before this society had dealt with specific diseases as, for example, his papers at the sixth meeting on *Bacterium pruni* and on *Ps. Stewarti*, at the seventh meeting on the olive tubercle and on bacterial leaf spots, and at the eighth meeting on Burrill's bacterial disease of broom corn. At the fifth meeting he was scheduled for a twenty-five minute paper on "The Destruction of Cell walls by Bacteria."¹⁰³

His paper, "Effect of Freezing on Bacteria," before the sixth annual meeting of the Society of American Bacteriologists, had considered a general subject, and before the seventh meeting of this society he offered no paper at all. This meeting took place at Ann Arbor on December 28 and 29, 1905, and coincided with the ninth meeting of the Society for Plant Morphology and Physiology. At this time these organizations were still affiliated with the American Society of Naturalists, and Smith, evidently because of his paper on "Channels of entrance and Types of movement in Bacterial Diseases of Plants," had to attend the meetings at the University of Michigan rather than go to New Orleans during convocation week to preside as chairman over Section G of the American Association for the Advancement of Science. In his stead, S. M. Tracy and B. L. Robinson served as chairmen of the meeting of the botanical section at New Orleans. Smith attended the meeting at Ann Arbor of the Society of American Bacteri-

¹⁰² *Science*, n. s., 23(585): 424-425, March 16, 1906.

¹⁰³ *Science*, n. s., 15: 405, 1902.

ologists and in the new university medical building was elected president of this society for the year 1906. He followed in that office Edwin O. Jordan who on October 4, as an editor of the *Journal of Infectious Diseases*, had thanked him for his "fine monograph on the Bacterial Diseases of Plants" and said: "It is this sort of work that gives dignity and standing to American scholarship." Other officers elected were: F. P. Gorham, vice president; S. C. Prescott, secretary-treasurer; W. T. Sedgwick, delegate to the American Association for the Advancement of Science; and members of the council, Jordan, Vaughan, Flexner, and Joseph McFarland.

CHAPTER IX

FIRST EUROPEAN JOURNEY. PLANT BREEDING AND THE THIRD INTERNATIONAL CONFERENCE ON GENETICS. STUDIES ON CROWN GALL OF PLANTS AND THE ETIOLOGY OF PLANT TUMORS. RECOGNITION OF ITS IMPORTANCE AS AN AID TO MEDICAL RESEARCH ON ANIMAL CANCER

ON MARCH 1, 1906, Smith closed up his laboratory work, bade farewell to his co-workers, and the next morning he and Mrs. Smith sailed from New York for Naples on the North German Lloyd steamship, *Princess Irene*. He took with them his manuscripts, photographs, and drawings for the unpublished volumes of *Bacteria in Relation to Plant Diseases*, his camera and microscope, many small sterile Petri-dishes, and a large sack of culture media—nutrient agar in small 6 c.c. thick test tubes.

On the journey across, he enjoyed discussions on questions of pathology with Dr. David I. Wolfstein of Cincinnati and Dr. Albert Vander Veer of Albany who was on his way to attend the International Medical Congress at Madrid. They landed at Naples and two days later Smith went to the University and spent an afternoon with Professor Fortunato Pasquale who immediately began to acquaint him with facts of Neapolitan agriculture.

He visited the Zoological Station in the morning of March 19, and that afternoon, invited by Pasquale, visited Portici. Pasquale taught systematic and pharmaceutical botany, however, and it was not until the next day that he began to become acquainted with European plant bacteriologists and study their work.

He visited the more than a century old Botanic Garden where he met Dr. Fridiano Cavara who had just been appointed director of the garden and professor of botany in the university. He showed Smith through the buildings and grounds, and it was arranged for him to return later.

The next day he again went to Portici and this time met Dr. O. Comes, director of the R. Scuola Superiore d'Agricoltura. A class of twenty-six students, introduced to Smith as a "very distinguished scientific man" from the United States, stood and

bowed. Comes showed him his botanical collections and presented him with almost enough of his papers to make a full set of his writings. He regarded himself as "the earliest man in the field" of plant bacteriology, or at least "as early as any, the contemporary of Burrill."¹ His assistant took Smith through their chemical laboratories where he met Dr. Giacomo Rossi, a bacteriologist, and his assistant—"younger men," Smith commented, "and better equipped intellectually than Dr. Comes for the solution of problems involving the culture of bacteria. They showed [him] their *Bacillus Comesi* which destroys the middle lamella of various plants, leaving only the framework of leaves, etc. It grows copiously on potato as a gray-white, pitted layer. The organism produces spores and some gas. At the end of two years' cultivation it loses its power to disintegrate parenchyma, or at least," he added, "this is what I understood." He then sketched the organism when stained gentian violet and as seen under an oil immersion objective.

Smith had a slide of *Bacterium pruni*, a stained section of a green plum. He regretted that he had not brought more slides. But he conducted his conference as best he could, and Rossi gave him reprints. His assistant told Smith that "much gummosis of the vine" could be found on the slopes of Mount Vesuvius. He hoped to get specimens of this disease for "sections and cultures."

On March 22, at the agricultural school at Portici, he became acquainted with Savastano who was preparing for the Milan exposition an exhibit of photographs on Italian arboriculture. He spent several days with this scientist who reminded him somewhat of Dr. G. M. Sternberg and became one of his most valued correspondents. A son of the noted authority on olive tubercle would study in Smith's laboratory at Washington before going to one of the California experiment stations to complete his research training in America.

At the Naples zoological station, Smith twice encountered some American students, one or two of whom were from Johns Hopkins. He enjoyed meeting members of the staff there, and might have stayed longer at Naples had not Vesuvius erupted and compelled them to move several miles distant to Sorrento.

¹ Facts and quotations taken from Smith's unpublished journal of this trip.

By mid-April they were in Rome where he spent some time with Giuseppe Cuboni, director of the R. Stazione di Patologia vegetale. He found his aide, "Dr. Lionello Petri, at the Museo Agrario was very agreeable." Peglion and Brizi were pupils of Cuboni. Smith learned of "a new disease of oleander, supposed by Peglion to be bacterial." He wrote,

He has published a short paper on it. Petri gave me cultures of it. Cuboni showed me rognas of the vine, the tubercles of which he says contain bacteria, although I could find none in the sections I cut and examined. The vine stem was enlarged to two or three times its natural diameter over a length of two feet and was very warty and much fissured. Cuboni is well informed as to literature; has a good card catalogue, takes and reads foreign publications and seems to be a very capable man. The laboratory is not well equipped but has all the absolutely necessary apparatus for ordinary pathological and bacteriological work. Petri showed me stained sections of *Dacus oleae*, a dipterous insect in which he has found enormous masses of bacteria in the head, thorax and abdomen. He thinks it may be a case of symbiosis as he says the flies are not injured. His sections were well made and the bacteria well stained. Cultures of the organism on agar looked liked a yellow *Pseudomonas* or *Bacterium*. He said, however, that the organism was peritrichiate.

While in Rome, Smith met the distinguished English chemist, Sir Henry E. Roscoe, "known the world over. He is a man of very simple ways and charming speech," he observed,

just as approachable as most truly great men are, and as all should be. I was delighted to meet him. He is here to attend an international conference of chemists. People with them told me that Lady Roscoe is very fond of flowers and is a diligent gardener. Sir Roscoe asked me to call on him in England and promised introductions to various people. He is much interested in the new Lister Institute of which he is one of the trustees. He spoke of [John Bretland] Farmer's² studies of cell development in Cancer, and of Dr. William Osler, whom they have recently put on their advisory board.

Farmer's statements and similar statements made by English students of malignant animal tumors later suggested to Smith certain studies in peach, onion, and Paris daisy to determine whether "chromosomes undergo any change in number or location in the rapidly dividing cells of crown-galls and similar plant tumors."³

² Professor of the Royal College of Science, London, an outstanding authority on botany and cytology.

³ *Bacteria in relation to plant diseases* 2: 93, 1911.

From Rome Dr. and Mrs. Smith went to Florence, and there he found in several centers "a good deal of scientific activity." On April 26 he began several conferences with Dr. Pasquale Baccarini at the R. Istituto di Studi Superiori, "the most suitable place for research work [he had] seen," the building devoted to botany being new and the equipment consisting of a very large herbarium, suitable rooms and good apparatus for laboratory work, extensive preserved materials and collections, and a botanic garden and hothouses showing capable gardening care. The director of botanical investigations and his assistants were always "very courteous and friendly," as was also Stephen Sommier, systematic botanist and author of many papers on the Italian flora. Dr. Baccarini gave Smith reprints of his papers on mal nero and other subjects, and offered his aid to secure material of "a lemon spot disease from Catania. This," Smith wrote, "he has described as due to bacteria. His description suggests the lemon-spot disease of California which has proved so serious in recent years and the cause of which is still somewhat uncertain, but with the preponderance of evidence at present in favor of a fungous origin.⁴ In California the lemons on the lower part of the tree are the ones principally attacked." At the natural history museum Dr. Antonio Berlese showed him "an elaborate work," being published in parts, on anatomy of insects. Dr. Giacomo del Guercio, another entomologist, showed him the museum's "library, collections, apparatus, etc and promised to send [him] material illustrating certain bacterial diseases, one of grapes, and one of olives. He desires," noted Smith, "Aphides, especially gall-forming species." The medical school laboratories and clinics there were also visited, and their treatments for lupus by Roentgen's rays and otherwise were among the many points of special interest to the American scientist.

At Pisa Smith met Dr. Giovanni Arcangeli, another able systematic botanist, who showed him about their herbarium which was "of considerable size and in good condition" and their botanic garden which, while less attractive than the garden at Naples, had

⁴ In 1926, in his address, Fifty years of pathology, *op. cit.*, 29, Smith said: "In 1906 Ralph E. Smith, in California, described a brown rot of lemons due to a peronosporaceous fungus, called by him *Pythiacystis* nov. gen. but said by Leonian to be only another *Phytophthora*." In his journal, however, E. F. Smith did not mention R. E. Smith's description of that year.

"some extremely interesting old trees" and a plantation of about 1,600 species arranged according to natural orders and in such a way as to remind him of the Harvard Botanical Garden. For Dr. Townsend's sugar beet investigation work, Dr. Arcangeli promised to send seeds of a wild beet.

On May 18 at Bologna, Smith found Dr. Victor Peglion at the Scuola a Gradia Superiore "just fitting up a bacteriological laboratory [with] some good new apparatus." Peglion's school work had just been completed and he was living at Ferrara as a travelling inspector of vineyards and other crops. European studies in plant pathology during the past decade had been "largely devoted to fungous diseases of plants,"⁵ But from him Smith began to learn anew concerning the research status in Italy of several crop maladies believed possibly due to bacteria. Peglion suggested that the tubercle disease of oleander "might perhaps be caused by the same organism as the olive knot," but this Smith thought "unlikely" however possible it was that the malady might prove bacterial. The youthful and able Italian scientist promised to send material of a bacterial disease of hemp in the region. Brusone on rice was believed caused by root-asphyxiation and not a bacterium. A leaf-spot of *Trifolium* was also believed by Peglion to be non-bacterial in origin. So was a disease of strawberry though this was believed by at least one authority to be bacterial. Smith learned that "everyone smiles a little when [Comes'] *Bact. gummis* is mentioned, Peglion included." They discussed olive knot and one or two other diseases. The plant pathologists of Italy were "good men," Smith was told, especially Savastano, "a very capable man." Funds, however, were lacking, and Peglion ended their conference by saying, "Your Bureau of Plant Industry is a model."

En route to Milan, Dr. and Mrs. Smith stopped at Modena where at a technical school he consulted Professor Luigi Macchiatti and went on an excursion with Professor C. A. Marozzi to examine rognà of the vine. This malady was believed by Peglion to be due to bacteria. But, since he had not studied it and Baccharini said it was distinct from mal nero, Smith interested himself in the subject. He found the disease unlike anything he had seen on vines in the United States, and he secured a promise that fresh

⁵ Fifty years of pathology, *op. cit.*, 29.

material be sent him in August. Marozzi described its symptoms, and Smith photographed one specimen "seriously diseased from the level of the earth to a height of about seven feet. . . . The signs," he wrote,

are great numbers of rough tubercular swellings accompanied by deep fissures. These are generally first on one side but the vines are finally girdled or otherwise exhausted and killed, death occurring in about four years. . . . I searched in vain on many vine stocks for living tubercular tissue. . . . The tubercles when fresh are said to be soft, pale reddish externally and white within.

Professor Macchiatti showed him slides of mal nero and a rot of grape fruits, and on close study under good light Smith concluded that neither malady was a specific disease due exclusively to bacteria, although in mal nero some evidence of "mixed infection" was present and in the grape fruit rot "little oval or elliptical red bodies" were "sparingly" seen in various cells. These, he thought, were "possibly proteid bodies . . . normal constituents of the tissue," and "almost certainly . . . not bacteria." Nothing was decided definitely as to any one of the diseases. At the Stazione Agraria, he examined Professor Gino Cugini's "bacterial rot of grape flowers" (1891) and "a bacterial soft rot of fennel" which resembled bacterial celery rot. But he saw no indications of cultures, or inoculations, and, more than discussing these and one or two diseases from materials available with Professor Cugini, no further study was made.

"The most intellectually active scientific man in Modena," Smith decided, "is the director of the Botanic Garden, the well-known algologist, Dr. G. B. de Toni." He was in Pavia, but a gardener showed Smith about, and late that day Dr. de Toni called at his hotel. He was reading proof on the seventh volume of his *Sylloge Algarum*, and next morning in his work shop he showed Smith one hundred bound volumes of separates relating to algae, "a rich collection and one any algologist would give much to possess. He has also," commented Smith,

many other rare and expensive systematic works, and a large herbarium of algae, together with another representing other groups of plants. He works entirely, he said, on "pure science." On leaving he gave me his inaugural discourse delivered at the Royal University of Modena, November 4, 1905, entitled "Di una interessante scoperta del Modenese Giambattista Amici

e de' suoi progressi," a paper describing modern discoveries concerning the part the nucleus plays in sexual reproduction in plants, foreign workers (Ikeno, Hirase, Webber, Davis, etc.) being cited freely. Amici discovered the pollen tube growing through the stigma and style into the micropyle.

De Toni's list of American correspondents began, Smith noticed, "with the good Dr. Asa Gray."

At Milan Smith went to the International Exposition grounds and tried unsuccessfully to locate Dr. Ugo Brizi. He would have gone again but Mrs. Smith, suffering from rheumatic fever, had to be taken to Varese near Lake Maggiore and placed under the care of a physician. Days passed into weeks and Mrs. Smith began to show enough improvement to permit Dr. Smith again to visit the International Exposition at Milan. He found the plant pathological exhibit prepared by Giovanni Briosi of Pavia "the only one of any importance. Much care," he wrote in his journal, "has been taken in its preparation and installation. It proved so instructive that a trip to Pavia was undertaken at once to talk over matters with him and his assistants," especially the illustrations of "the obscure and wide-spread Italian rice disease concerning the origin of which there [were] at least three distinct hypotheses: (1) Voglino's, ascribing it to a bacterial infection of the roots; (2) Brizi's, ascribing it to asphyxiation of the roots; and (3) Garovaglio's, ascribing it to a fungus, *Pleospora oryzae*." He saw also a jar labelled "Bacteriosis del Gelso" which showed mulberry stems bearing leaves. He wrote of it: "The stems are not attacked, neither are the petioles but only the blade of the leaf in numerous irregular small yellow or brown spots, discrete or fused. It looks unlike our bacterial disease of mulberry, which kills the stems after the manner of pear blight."

At the Orto Botanico of Pavia, "a place," Smith said, "where they get work done," he interviewed Briosi who told him that at the University of Pavia he has "sometimes as many as 200 students including medical and pharmaceutical students." He met also Rodolfo Farneti who had most of the work in pathology, privat docent Luigi Montemartini who edited *Revista di Patologia Vegetale*, Pollaci, and others. Briosi was still publishing with Cavara their *Fungi Exsiccati*, and Smith heard with interest how for ten years he had been trying to get an appropriation for a bacteriological laboratory.

Smith arranged that gifts of his first volume of *Bacteria in Relation to Plant Diseases* be sent them who had not received the work. Cavara had "no sufficient words" to thank him. He called it "the most accurate monography on the bacteria which interest the agriculture. . . . No one better than you could develop such an object of t[h]eorie and practical bacteriologie. . . . I hope," he added, "that you will continue the great works that you have undertaken so to give us also the special division with the description of all species till now described of bacteria injurious to our cultivated plants." He supplied Smith with a bibliography of his writings and invited him to visit them again at the R. Orto Botanico of the University of Naples.

Smith was cordially received everywhere. Not less his volume on bacterial diseases of plants than his eminence as a plant pathologist obviated the necessity of letters of introduction. Before he had left America, Savastano and Peglion had written letters of appreciation for his valuable work. Professor Dr. M. W. Beyerinck, whom Smith esteemed as "the most distinguished bacteriologist in Holland," had, as director of his laboratory at Delft, thanked him for his "most admirable book on bacterial Plant-Sickness" and sent a paper of his work on gummosis. Dr. Carl Wehmer of the Technische Hochschule of Hannover, Germany, had sent a card of acknowledgment; W. Busse of the Phytopathological Institute of Berlin, a letter; Dr. Alfred Möller of Eberswalde, a letter; also, Dr. Rudolph Aderhold, director of plant pathology in the Imperial German Board of Health, the highest position of its kind in Germany, so Smith believed. Furthermore, Smith exchanged letters with S. Winogradsky of St. Petersburg, Russia.

Dr. Galloway had directed Smith, while in Europe, to visit places in Italy, France, Belgium, Holland, Germany, Austria, and other parts of western Europe to make "further investigations of the diseases of bulbs, potatoes, vines, rice, olives, and other fruit and crop plants which are introduced into the United States," and also to inspect the equipment in the laboratories of the countries visited.

Duggar in 1901 had conveyed to Delacroix and Prillieux of France Smith's requests for materials of a bean and a grain disease. From Paris on December 1, Duggar had informed: "Dela-

croix has a bacterial disease of potato stems which he thinks may not be the same as your B[acillus] solanacearum(?), for he finds some important differences as to the form of the organism in the plant. He will write you concerning it, and if possible I will get some material before leaving Paris. Delacroix," he added, "was able to help me greatly with *Rhizoctonia*. In all I secured about ten specimens from him, representing two species and about six host plants."

Smith, therefore, had reasons of his own for visiting France and possibly also England. On July 12, 1902, M. C. Potter of the Durham College of Science, Newcastle-upon-Tyne, had thanked him for a copy of his "most interesting review" of "Plant Pathology: a Retrospect and Prospect." He had promised to send soon his "paper extending [his] work on the action of *Pseudomonas* on living cells," and at that time said that he regarded his study of bacteria as his more important work. Smith knew other scientists in England, too.

England was not included on Smith's proposed itinerary when on the morning of June 13 he left his "wife miserably ill with intercostal rheumatism in the Albergo del Europa at Varese and started for Berlin. "It was hard to tear myself away," he wrote in his journal, "and only necessity forced me to it. She has now been confined to the hotel for two and one-half weeks and the prospect of an early release is poor indeed, although I am hoping for the best."

What was to have been one of the high points of their journey—the trip through the Alps—turned out to be a lonely, heart-breaking experience. Delight was found in the landscapes of Switzerland, along the valley of the Rhine from Basel to Frankfurt-am-Main. Early in May, he himself had been ill for a week. His worry and anxiety, his sadness and, indeed, moments of frenzy, over his wife's condition had not helped to restore his usual vigor. Since 1904 her strength had been declining, from childhood having courageously fought the effects of an illness from arthritis. To look on her delicate beauty, one could scarcely realize the full extent of her valiant struggle against a weakened heart and stiffening joints. She was always gentle and kind, ever resolute and sensitive to goodness, never complaining, and never prone to affectations.

Smith had seen one advantage in making the journey alone to Northern Europe. While in Berlin, he might consult specialists for himself and at the same time determine whether his wife's ailment was being treated properly. By mid-June he was in Berlin, had secured lodgings, and located Dr. W. D. Miller, an American dentist who in 1882-1884 had "associated bacteria with dental caries and subsequently called repeated attention to the danger of systemic infection from neglected teeth."⁶ Dr. Miller introduced him to Dr. John H. Cleves Symmes, an American physician, who, after an hour's conference, pronounced Mrs. Smith's treatment in the main correct. The suggestion that a majority of practitioners were adhering to a belief that possibly rheumatism was due to an organism of some sort encouraged Smith to believe that a cure would be ultimately effected. But the next day he learned from a highly reputable German doctor that he himself might have to undergo surgery. This, however, never proved to be the case.

Smith wrote in his journal much about medical practice and diseases of current prevalence. For instance, in Italy, the situation as to malaria fever interested him and he gathered facts concerning it. On this journey, however, he was preeminently a plant bacteriologist and pathologist. Pages of his journal were devoted to describing the cities he visited, their art galleries, and beautiful public buildings. Pages were devoted to describing beautiful landscapes and the vegetations of the various regions seen. Often he listed the plants of unusual interest observed in gardens. Indeed, wholly separate volumes could be prepared from the pages of his journal which dealt with his artistic and literary appreciations. But art and literature remained avocations while science was his foremost business. He found beauty in the study of microorganisms and glory in the conquests of disease they cause. His highly specialized, intellectually rigid, technological work required some freedom of mind. His escapes from its exacting disciplines, when he needed any, were the enjoyments of poetry, music, literature, and art, and these served him well when later grief invaded his well ordered life.

⁶ Fifty years of pathology, *op. cit.*, 18. Smith had met Dr. Miller in Washington the year before and in his journal he characterized him as a "dentist and student of dental caries."

Some years before, he had lost his father. Only a year or so past, Louisa Frink Smith, his mother, had died in her son's home cared for by an affectionate daughter-in-law. Erwin Smith had centered all of his love in one "luminous and beautiful personality. . . . Of all the women [he had] known and revered, this woman best satisfied [his] ideals, and of all her traits," he said, "that which deepest impressed itself upon [him] was her divine simplicity, a characteristic of all really great souls." And now she lay ill a far distance away.

Smith continued with his work as he was sure she wished him to do. On July 12, 1906, a letter arrived from H. J. Webber of the Laboratory of Plant Breeding of the Department at Washington. He wrote that it would be impossible for him to attend the International Conference on Genetics to be held in London late that month. He asked Smith to attend and present a statement of the plant breeding work of the Department in his stead. He explained in a letter of July 28 that Mrs. Webber was ill.

Dr. Smith had planned to go to the Netherlands and so he accepted the honor of addressing the conference. He wrote Webber: "Next to my own work in the Depart[ment] I am most interested in yours, and I think I can interest the Conference for a half hour at least." Webber thought so, too. On July 28 he told Galloway: "Dr. Smith understands the breeding work of the Department sufficiently well so that I think he can represent us intelligently and aid the Department by an explanation of the knowledge of its work."

Scientists in Berlin had been cordial to Smith and he had visited many points of interest. Soon after his arrival, he had enjoyed "an hour or more talking plant pathology and kindred subjects" with Dr. Paul Sorauer. A few days later they strolled through the new botanic garden "inspecting first the plants arranged in families on the south side of the grounds and then those arranged geographically on the north side near the new buildings." Smith believed that this garden would "soon become the most famous in the world." He saw there "many interesting plants" and particularly commended Dr. Engler's "efforts to show the flora of a given region (alpine plants, etc.)." Sorauer and he spoke in German and, in contrast to his experience in Italy, he found no difficulty in understanding him. Sorauer told him that his "new

edition of his 'Pflanzenkrankheiten' was his 'testament.' His whole soul," Smith observed, "is wrapped up in getting this last work well done. He brought me a good review of the first volume of my book which he had just printed in his 'Zeitschrift.'"

Dr. O. Uhlworm was "equally friendly" and Smith's visit with him led to a dinner invitation, at which occasion the American met Dr. Friedrich Löffler. He was in Berlin to attend a meeting of the Imperial Board of Health, and Smith and he talked of Koch's Institut für Infektionskrankheiten. Löffler told stories of his student days in Berlin, invited Smith to call on him at Greifswald, inquired of his work, and accepted with pleasure the promise of a gift of *Bacteria in Relation to Plant Diseases*. Smith liked his familiarity with literature.

Soon thereafter, Dr. Rudolph Aderhold, director of the Biologische Anstalt of the Imperial Board of Health at Dahlem, entertained with a dinner in Smith's honor. Berlin had early appealed to Smith as "the most interesting city of [his] acquaintance." He enjoyed its cleanliness, its buildings, parks, gardens, and forests of pine, birch, and beech. On a walk through Grunewalde with Dr. Aderhold and his son Hans, Smith had been told to visit the much older forests at Eberswalde. This he had done and while there tried to call on Dr. Alfred Möller. Later a day had been spent at Lübbenau, a small village on the edge of the Ober Spree-wald and in the midst of a famous trucking region, to study cucumber diseases. But no bacterial diseases or any other maladies of consequence were found. So, after visits to several new points of interest around Berlin including forests of pine, oak, birch, and various other tree species, Smith centered his attention on the work of the Biologische Anstalt and Koch's Institut. Dr. Koch was away from Berlin in the heart of Africa on Victoria Nyanza studying with half a dozen assistants "the terrible sleeping sickness." But Dr. Aderhold and Smith became close friends, and later the American plant pathologist met and spent about half an hour with Dr. Gaffky, director of the Institut.

At the dinner in Smith's honor was Dr. Otto Appel who was studying among other subjects *Fusarium* in all of its forms and had about seventy sorts under cultivation in Erlenmeyer flasks on strips of filter-paper soaked in synthetic media. He promised Smith cultures of his *Bacillus phytophthorus* and photographs of the

bacterial potato disease, Schwarzbeinigkeit (black leg), due to this organism—a subject which was one of the main reasons why the journey to Berlin had been made. Also guests at the dinner were several of Aderhold's assistants and Dr. Busse whose work was then sugar beet diseases in Germany and tropical diseases in the colonies and Dr. Maassen who had worked with Dr. Petri, was now studying "diseases of bees and honeycomb, fowl brood, black brood, etc." and whose papers and acuteness of observation Smith highly respected. Smith consulted these men in their laboratories several times. He gave Busse a serial slide of "stained sections of a green-plum fruit attacked by *Bacterium pruni* . . . one fairly illustrative of an early stage of the disease." A few days later Smith secured four "usable negatives and prints" which admirably illustrated Dr. Appel's work on black leg of potato. He had sent Dr. Aderhold a culture of *Bacillus amylovorus* and was pleased to learn that not only was his culture in the Anstalt's pathological museum but also that Aderhold recently had secured with it "very interesting infections on green pear fruits."

Smith heard with interest their account of how much difficulty they had experienced in isolating their *Bacillus spongiosus* from diseased cherry stems. He was reminded of his years of trying to isolate in pure culture *Bacillus tracheiphilus*, cause of cucumber wilt. Furthermore, he was told that *Bacillus spongiosus*, isolated from gummy cherry trees, "had no effect on the green pears, neither would it blight green pear shoots in an earlier test. Dr. Aderhold," wrote Smith,

showed me a very instructive photograph of a young pear tree with two shoots, one inoculated with the *Bacillus amylovorus* of my sending and the other shoot with a young culture of his cherry organism; the former shoot was killed in the course of a few days, the latter was not in the least injured. There are some other interesting differences i. e., *B. spongiosus* forms in gelatin plates (agar do.?) very typical, and curious buried colonies, "spongelike," Dr. Aderhold called them. It is a white organism making a copious sirup on sugar agar. I saw plates under a low power of the microscope. The body of the colony produces curious involutions or twisted folds, so that the main lines of growth or laminae of the colony are turned in different directions as one lowers or raises the objective slightly. He showed me cherry trees inoculated for some weeks and with no very definite signs of disease beyond a little gum flow from the wound which is not specific, but symptomatic of many things. I saw in the Museum

other cherry trees inoculated for half a year or more, with large branches destroyed by the bacteria. It is a slow disease. I believe I have seen the same thing in the United States on cherry branches sent to me some years ago from the Northwest Coast (Oregon and Washington). Bacteria were present in the tissues in great numbers and were recognized as such, but I did not attempt to cultivate them, having at the time more than I could do with other things. Dr. Aderhold has promised to furnish me photographs and other material for illustrating this cherry disease.⁷

The building of the Biologische Anstalt was "new, large, and admirably adapted," Smith believed, "to the needs of the station." Its hothouses and experimental grounds were adjacent; the equipment "admirable," and its pathological museum was "extremely interesting," supplying him with ideas for the new Department of Agriculture building at Washington. He saw "a large copper apparatus in compartments with an ice tank above . . . designed for study of organisms at various low temperatures. The compartment temperatures ranged from 1.5°C at one end to 16°C at the other. Such an apparatus," he decided, "would be very useful in my laboratory." He called on shops and dealers in bacteriological apparatus and got new ideas concerning laboratory equipment from them as well as from other sources.

He visited the extensive laboratory of bacteriology and the animal houses maintained by the Imperial Board of Health, and at Koch's Institut Dr. Claus Schilling, in charge of the division of tropical diseases, showed him "very thoroughly" through their main building and animal houses. The very complete Koch collection in the museum and library was examined; and Dr. Schilling and he discussed for a while Fritz Schaudinn's (and Hoffman's) recent discovery of a flagellate protozoan, *Treponema pallidum*,⁸ "the real cause of syphilis."

At a botanical meeting of about twenty persons, presided over by Dr. Engler, Smith became acquainted with, among others, Dr. Hugo Fischer, a bacteriologist interested in soil organisms. Dr. Gustav Lindau, mycologist, systematist of higher plants, and a pupil of Schwendener and Brefeld, and Dr. Georg Volkens, who had charge of plant introduction and disease investigations for

⁷ See *Bacteria in relation to plant diseases* 2: 69, 1911. Also, *Introduction to bacterial diseases of plants*, *op. cit.*, 474.

⁸ See, *Fifty years of pathology*, *op. cit.*, 31. There Smith also mentioned Castellani's discovery (also 1905) of a flagellate protozoan, "the cause of that dreadful tropical disease 'Yaws'" and its distinction from syphilis.

the German colonies, were at this meeting, and Smith interviewed them both in their work rooms a few days later. He also chatted for a half hour or more at the Dahlem school for gardeners with Dr. Carl Miller who served as secretary of the meeting. He called on Dr. Oskar Brefeld but the aging and almost blind German scholar was not at home. During his homeward journey, he received a letter from Dr. Karl Goebel who wrote to thank him for a copy of his "magnificent book" on bacterial plant diseases. Berlin had been a veritable feast of science.

On the morning of July 16, Smith left Berlin to go by way of Hannover and Essen to Utrecht, the Netherlands, where he arrived that night. There he saw the botanic garden, and immediately went on to Amsterdam. Since 1902 when he had entertained in Washington Dr. F. A. F. C. Went, professor of botany in the University of Utrecht, he had been invited to visit there, and had written the professor of his plans to be in Europe this year. Dr. Went, however, was in the Hague for two weeks and had not been able to reach Smith by letter to arrange a later time for the visit. Smith spent many days at Amsterdam, Haarlem, Hillegom, Leiden, and other places of interest. At the University of Amsterdam he visited the laboratory of Dr. Hugo de Vries. At Haarlem and Hillegom he collected diseased bulbs of hyacinth yellows, dissected them, and confirmed his earlier work. Observing that in some growers' bulb catalogues varieties were designated as susceptible or resistant to the disease, he discussed with them the matter of varietal resistance and how, if at all, disease-resistant varieties could be obtained by hybridization.⁹ A "glorious display of hybrid Gloxinias" and a "good collection of single and double, extraordinarily large-flowered tuberous-rooted Begonias," obtained by crossing and selection, were among the many floral displays he examined. But his most important study began when he started bacterial cultures at the Phytopathologisch Laboratorium of the Willie Commelin Scholten of Amsterdam,

⁹ In his *Bacteria in relation to plant diseases* 2: 95-96, Smith recommended systematic cross-breeding experiments in many plants for resistance to bacterial disease: "These should be taken up as a part of the regular work of Departments of Agriculture and Experiment Stations and carried on without interruption for a long series of years—long enough to insure good hybrids of fixed quality. In some cases systematic selection within the limits of susceptible varieties might lead to useful results." Practically the same as this statement was written into his diary at this time.

of which Dr. Johanna Westerdijk, a former student of DeVries and Goebel, was director. With pure cultures, by which he was "the first to obtain typical infections,"¹⁰ he began to study the blight of lilac caused by *Bacterium syringae* (C. J. J. van Hall) EFS.

In 1905 Dr. Went, recently President of the Faculty and then Rector of the University of Utrecht, had told Smith of the Dutch government's plans to open at Wageningen the next year a laboratory of plant pathology, with Ritzema Bos as director. From Bos he had received another letter of thanks for his "magnificent book" on bacterial diseases of plants. Plant pathology, as a laboratory science, was gaining in importance. Within the past few years, Smith had been consulted with regard to laboratory equipment for work in Mysore, Bangalore, India, and, to supply a trained scientist for the position, he invited into his laboratory Leslie C. Coleman, honor graduate of the University of Toronto and later Director of Agriculture for the government of Mysore.

Smith probably visited the laboratory at Wageningen. But he did not go again to Utrecht, probably planning this for after his return from England. He may have wanted to study, from Went's materials, two or three sugar-cane diseases. In 1904, reading Smith's article "Ursache der Cobb'schen Krankheit des Zuckerrohrs" in the *Centralblatt*, Went had written him of this and other diseases of sugar-cane which he had studied in Java: Sereh disease, "top rot," and Cobb's bacterial malady.

Dr. Went did not return to Utrecht until the last of the month, and on July 30, 1906, Smith left via Rotterdam for London. In his journal he professed a strong interest in plant breeding, especially in "that phase of the subject which relates to the production of races of plants resistant to disease."

On July 30 the Third International Conference on Genetics began, and Smith's address, "Plant Breeding in the United States Department of Agriculture,"¹¹ was presented at the fourth session on the morning of August 2. Six days later he reported to Galloway:

¹⁰ *An introduction to bacterial diseases of plants, op. cit.*, 35.

¹¹ *Rep't Third Int. Conf. on Genetics, 1906, 301-309*, ed. by Rev. W. Wilks, M. A., Sec'y, London, Print. by the Royal Hort. Soc. Also reprint by Spottiswoode & Co. Ltd., London, 1907.

I attended the meetings of the Int[ernational] Conference of Plant Breeding held under the auspices of the Royal Horticultural Society and spoke for about twenty five minutes. . . . The speech was well received and very many nice things were said about the progressive spirit of our Department which I wish you and the Secretary could have heard. They seemed to be so genuine. They admitted freely that we are far ahead of the English in all sorts of experimental work for the benefit of agriculture, that we put more money and more men into it, all with astonishingly good results, which makes them ashamed of the little England is doing. "When will our own country wake up to the necessities of the case?" seemed to be their common cry. They also praised the generosity of the Department in furnishing information and publications for all who would be benefited, irrespective of nationality. Sir Daniel Morris¹² and Mr. Fawcett of the West Indies praised us particularly for the courtesies we have extended to them.

The Royal Society ended by drawing up a resolution which has been transmitted to our minister here to be forwarded through the Secretary of State, thanking the President of the United States and Secretary Wilson for sending me over! I had nothing to do with it, and don't know what the resolution is like but should like to see it if it comes into your hands. I was treated with great courtesy. . . .

Morris read an important paper on hybrid sugar canes,¹³ some of which might be useful to our Louisiana planters. He said their best seedling cane was No. 208. This is very resistant to disease. It is a large yellow cane and yields three tons of sugar cane per acre in the Barbados. He stated that the sugar industry in the Br[itish] West Indies would have gone to the wall during the last eight years from diseases, but for these new canes which they have been able to produce by hybridization and are now in general cultivation.

The most important paper¹⁴ of the Conference was by Mr. Biffen¹⁵ who has found a perfectly rust resistant wheat which he calls *x*, having been unable to determine its name. This when crossed with Michigan Bronze, a variety which rusts with him very badly every year, gave a certain portion of hybrids *entirely free* from rust. The patch as a whole appeared

¹² Imperial Commissioner of Agriculture for the West Indies.

¹³ The improvement of the sugar-cane by selection and hybridisation, *Rep't 3rd Conf. on Genetics, op. cit.*, 310 ff. Note appended that seedling canes mentioned had been grown successfully in Louisiana: announced since paper was written.

¹⁴ Rowland H. Biffen, Experiments on the breeding of wheats for English conditions, *Rep't 3rd Conf. on Genetics, op. cit.*, 373-377. See also, E. S. Salmon, On raising strains of plants resistant to fungus disease, *idem*, 378-382. At p. 382, susceptibility to, and immunity from, at least rust were said to have been shown by Biffen's work to be Mendelian characters transmitted as regards inheritance in accordance with the ratios enunciated by Mendel in his paper on "Plant Hybrids," *supra*.

¹⁵ University Department of Agriculture, Cambridge, England. See *Bacteria in relation to plant diseases, op. cit.*, 3: 154, where Smith told of a conversation in 1906 with Biffen concerning a disease of mangolds and sugar-beet on which the latter had published in 1901.

to be "swamped with rust" but when he came to examine it plant by plant *one plant in four* was entirely free from rust. There were several hundreds of these plants "without a pustule on them." He thinks that in a few years we shall be able to breed wheats with any quality of grain and productivity we desire and at the same time have them absolutely rust resistant. Please show this to Mr. Carleton. I shall try to get some of the x wheat for him to experiment with.

You will find a report of the Conference in *Gardeners' Chronicle* for August 4th and 11th. I go back to Amsterdam for some weeks tomorrow.

Smith, aware of the "three and one" ratio significance of Biffen's discovery in plant inheritance, attempted no evaluation or prognostication of its future value to plant breeding or plant pathology. This had been done by other speakers at the Conference, by Biffen himself and by Dr. E. S. Salmon of the South-Eastern Agricultural College at Wye, Kent, and the addresses were to be published.

The next year when Smith saw a note on a bacterial mangold disease published by Biffen, he sent for material, and again asked for "the wheat in question." The English scientist promised to send the material and, as to the wheat, said that it "is immune only to *P[uccinia] glumarum* as far as I know. *P. graminis*," he wrote, "is far from common here and rarely appears on my plots, but late in the season flecks of it appear on the wheat often mixed with *P. dispersa*."

Biffen's honors were many. He was knighted for the achievement, and in 1908 was appointed professor of agricultural botany at Cambridge University. He prophesied to the Conference that plant breeding thenceforth would be "conducted on the lines which Mendel once for all laid out," that it would become highly specialized, and be placed on "a fresh and a practical basis." He exhibited hybrid wheats and barley,¹⁶ and Salmon, believing it "impossible to overestimate the importance" of his discovery, thought that "in those cases, as, for example, in the mildews, the rusts, and probably many other diseases, where the place of the decisive conflict between parasite and host is intracellular," he had answered the important question whether the inheritance is "determined by the 'constitution' of the plant" and, if so, whether the constitutional characters, especially with reference to susceptibility and immunity, are "Mendelian" in the ratio of

¹⁶ *Rep't 3rd Int. Conf. on Genetics, op. cit.*, 33-37.

their transmission. In breeding for disease-resistance against rust, this appeared to be now established.¹⁷ That year Biffen received from the Royal Horticultural Society a gold medal for his wheat variety resistant to rust.

Several years before the Conference, the Englishman William Bateson, the Canadian William Saunders, the Americans L. H. Bailey, W. J. Spillman, H. J. Webber, and other authorities on hybridization and plant breeding, had begun to reinterpret the literature and experimental knowledge of the subject in the light of an improved science fraught with vast potentialities for agricultural advancement. In fact, Bateson, in his address¹⁸ as presiding officer of the Conference, introduced the word, "Genetics," to define the basis for a new science, that sphere of scholarly research "devoted to the elucidation of the phenomena of heredity and variation: in other words, to the physiology of Descent, with implied bearing on the theoretical problems of the evolutionist and the systematist, and application to the practical problems of breeders, whether of animals or plants."

Since their first conference in 1899, much of the "bewildering complexity," he said, which characterized their early deliberations had been dispelled, and the science now was destined "not merely to add new regions to man's knowledge and power, but also to absorb and modify profoundly large tracts of the older sciences." Hybridization and plant breeding were no more to be "a speculative pastime to be pursued without apparatus or technical equipment in the hope that something would turn up," but "a developed science,"¹⁹ based on order and system, a new craft with terminology, tools, and other accoutrements of an exact science. Geneticists would probe deeply into the fundamentals of plant and animal life, study the mechanisms involved in breeding, and improve and extend the practice. Results of the earlier and late research on the DeVriesian mutation hypothesis were before the conference. Its report²⁰ published a letter written by Gregor Mendel at Brünn May 4, 1868, to Carl Nägeli in which he told of his change from a "hitherto humble position as a teacher of experimental physics"

¹⁷ *Idem*, 382.

¹⁸ The progress of genetic research. An inaugural address to the Third Conference on Hybridization and Plant-Breeding, *Report etc.*, *op. cit.*, 91.

¹⁹ *Idem*, 91.

²⁰ *Idem*, 89.

to a sphere strange but dear in which he was continuing his "experiments in hybridisation." Perhaps Mendel, like others of the science who followed him, was content to explore only the specialized and outlying functions involved in plant hybridization. He may have realized, but not placed in written form, the theory of heredity deducible from the results obtained with his plant hybrids. The body of doctrine was there, however, and great minds of the twentieth century—preeminently, DeVries, Correns, and Tschermak, and also, Bateson and Saunders—gave form to the primary, central, and all-controlling function of heredity on which the specialized and outlying functions depend.²¹ Smith met many notable men of the Conference. Tschermak he found a "very energetic man," and his address on "The importance of Hybridisation in the Study of Descent" pleased him. W. Johannsen of Copenhagen was also there and read an address, "Does Hybridisation increase fluctuating Variability?" an extension of his famous theory, based on experiments with beans, of breeding pure line species. Smith knew Bateson. Once, when the great Englishman was visiting in America, he and Smith enjoyed a stroll together at Woods Hole.

Smith followed on the program Miss E. Saunders of Newnham College, Cambridge, England. That he was acquainted with recent progress in plant breeding in America, especially that of the Department of Agriculture, was demonstrated by his address, part or all of which was given from memory. He chose to start with the breeding objective with which he was most familiar and on which he had been a consultant. He illustrated breeding for disease resistance by narrating the story of Orton's (and Webber's) South Carolina work originating "from rare mutations,"²² seed selection, and/or hybridization, the now famous wilt-resistant Sea Island cotton, Upland cotton, cowpea, and watermelon. Nor did he forget Pierce's earlier California hybrid grape vines resistant to the Anaheim disease so-called, and his crosses of raisin grapes to resist *coulure*. Breeding for resistance to frost and cold was

²¹ W. Bateson, *op. cit.*, at p. 97.

²² W. A. Orton, pathologist in charge of cotton and truck diseases, Bureau of Plant Industry, The development of farm crops resistant to disease, *Yearbook of the U. S. Dep't of Agric. for 1908*; *op. cit.*, 453-464. A list of examples of disease resistance was given at pp. 463-464; also, a consideration of "Mendelism" and Biffens's discovery at p. 463.

illustrated by Walter T. Swingle's and Webber's crosses in citrus fruits, especially the orange. These had been reported by Webber at the first or 1899 conference on hybridization and cross breeding, so Smith focussed attention on the progress since that time. Under the sub-title, breeding for greater productivity and for quality in edible fruits, foliage, fibers, etc., Smith described Webber's and Swingle's cross-breedings to improve the pineapple not only for increased resistance to disease but also for increased vigor, absence of spininess in the foliage, medium size, shallow eyes, juiciness, better flavor, absence of hard core, attractive top, good shipping and keeping quality, and other desirable attributes which made for increased marketability. Webber's long staple cotton strains, their increased length of fibre one-third more than the ordinary and bearing two or three times the amount of fruit, provided an excellent example of breeding for greater productivity and improved quality. Likewise, A. D. Shamel's cigar-wrapper tobacco improvements by breeding and selection begun in the autumn of 1903 in the Connecticut River valley.²³ Or, Willet M. Hays's "blue stem" spring wheat in Minnesota, the yield of the best strains, Smith said, increased two to five bushels an acre by simple selection. A Departmental program of corn improvement had been in effect now several years and was adding some results to the state experiment station work in this regard. Breeding for increased vigor, better productivity, and improved quality were time-honored objectives industrially, and the utilization of plant introductions both foreign and domestic in aid of the work had been approved practice for many, many years, not only among nurserymen, orchardists, and horticulturists, but also within the Department itself since the 1860's when William Saunders had inaugurated the work.²⁴

Achievements in breeding for disease and cold resistance were to be extended gradually. Although Smith in his address did not mention this, Townsend in 1902 had begun selecting sugar beets for resistance to the curly top disease, and in 1907 would report

²³ A. D. Shamel, The improvement of tobacco by breeding and selection, *Yearbook of the U. S. Dep't of Agric.* for 1904; 435; New tobacco varieties, *idem*, for 1906; 387 ff.

²⁴ William Saunders, by the editor, *Yearbook of the U. S. Dep't of Agric.* for 1900: 625-630.

some evidence of progress.²⁵ The etiology of several diseases of plants, now known to be of virus origin, was being studied. But many of the problems were not understood. Curly top has been shown to be a disease caused by a virus, and an insect vector—the beet leaf hopper *Eutettix tenellus* (Baker)—has been demonstrated to be the agent which transmits the virus. The development of a resistant variety awaited, therefore, more complete understanding of the disease and further perfection of technique in selecting and breeding for disease resistance. In the next decade of the century, L. R. Jones and others of the faculty and graduate personnel of the University of Wisconsin would brilliantly control *Fusarium* disease of cabbage, cabbage yellows so-called, by breeding and selecting plants resistant to this “seriously prevalent” disease. These skillful men obtained by “further hybridization and selection excellent commercial varieties of cabbage perfectly developed in fields where all the common sorts died from the attacks of *Fusarium conglutinans*.” Smith would praise their “beautiful photographs, showing long rows of well-headed and healthy cabbage plants surrounded by thousands of dead and dying ones.”²⁶ For several years Jones’s and his co-workers’ reports of progress would be among the most important papers presented at meetings of the American Phytopathological Society and published in the society’s journal, *Phytopathology*. Eubanks Carsner, later a student of Jones and prominent as an originator of varieties of sugar beet resistant to curly top,²⁷ modeled his breeding for disease-resistance after the work of Jones. Carsner’s work will be mentioned further later.

At Cornell University, in coordinate work of the departments of plant pathology and plant breeding at the New York State College of Agriculture, Walter H. Burkholder would explore the incidence of the Mendelian “three and one” ratio in disease resistance and susceptibility, while seeking by selection and hybridization to obtain an anthracnose-resistant white marrow bean. Burkholder’s accomplishment was accounted by Smith an outstanding event of the half century of American pathology. By

²⁵ Technical Bulletin 360: 4, U. S. Dep’t of Agric., May, 1933; also U. S. D. A. Bur. Pl. Indus. Bull. 122, 1908, 37 p., illus.

²⁶ Fifty years of pathology, *op. cit.*, 33,37.

²⁷ Eubanks Carsner, Curly-top resistance in sugar beets and tests of the resistant variety U. S. No. 1, Tech. Bull. 360, U. S. D. A. May, 1933, 68 pp., illus.

1918, when Burkholder announced this in *Phytopathology*,²⁸ a multiple factor hypothesis would have found its way also into the literature, and more than the two factors of resistance and susceptibility would be necessary to explain all of the phenomena in breeding, at least to the minds of some authors. By 1918 *Phytopathology* and other journals would have published many articles, abstracts, and notes relative to breeding disease-resistant plants. Smith estimated as of especial importance J. A. McClintock's study of "Spinach blight."²⁹ He said of this in his address, "Fifty Years of Pathology:"³⁰ "In 1918 McClintock and L. B. Smith published on a destructive mosaic blight of spinach and showed that it is not only transmitted by aphids, but also from one generation of aphides to another. L. B. Smith (1920) bred spinach resistant to this disease."

Not less important than breeding for resistance to disease and cold and for increased productivity and improved quality was breeding for resistance to alkali and to drought. To illustrate the Department's work, Smith at the Genetics Conference of 1906 selected alfalfa and wheat as the crops and described first the program of bringing into cultivation western areas of alkaline lands by introducing "certain types of leguminous and other plants" more tolerant of alkali than ordinary plants. Part of this work was in charge of one of Dr. Webber's assistants, T. H. Kearney, known also as an agricultural explorer and for valuable cotton importations from Egypt. Next Smith told of the nation's westward extension of its wheat belt into areas formerly pasture or unoccupied lands, plains, or deserts by introducing durum wheats from Russia. This was the triumph of Mark Alfred Carleton, in charge of the Department's office of cereal investigations. In semi-arid states of the central west and west where, though soil was adapted to wheat-growing the rainfall had been insufficient to bring winter and spring crops to maturity, the drawback of climate had been overcome, and immensely valuable industries, including the famous macaroni wheats, been either established or extended. Again without sacrificing scientific content or permitting his audience to go unacquainted with the facts

²⁸ 8(7): 353, July 1918, and authorities cited; Fifty years of pathology, *op cit.*, 37.

²⁹ *Phytopathology* 8(2): 74, Feb. 1918.

³⁰ *Op. cit.*, 37.

of Carleton's foresight, explorations, workmanship, and struggles against strong opposition, Smith told almost in the form of a story how one man's vision became a reality and thus westwardly extended "our wheat belt several hundred miles over many degrees of latitude. . . . The end is nowhere in sight," he prophesied.

From small beginnings six or seven years ago the durum wheat crop of the United States has increased steadily until last year [1905] it amounted to twenty million bushels, and this year to fifty million bushels, largely grown on semi-arid land where ordinary wheats will not grow. . . . In passing it is interesting to note that some of these wheats are also very resistant to rust (*Puccinia graminis*, *Puccinia Rubigo-vera*); one variety is absolutely resistant.

Smith regarded this as "one of the most brilliant of [the Department's] economic achievements." This was saying much since some of its foreign plant introductions—grasses and forage plants, rice from Japan, cotton from Egypt, the Smyrna type fig, dates, vegetables, oats, rye, barley, buckwheat, and many other crops—had either established whole new industries or so revitalized and expanded existing industries as each of themselves to reimburse for their importation cost, some to pay for the cost of the entire work of the office of seed and plant introduction. Chief Jared Gage Smith of this office in 1900 volunteered the opinion that from Turkey wheat from Russia, the Washington navel orange from Brazil, or sorghum and Kafir corn from Africa and China, so many millions of dollars had been realized as to be sufficient to defray "the cost of the whole work of the Department of Agriculture since its inception."³¹ The Swedish Select oat proved to be of outstanding value at one time. Wheats were imported not alone from Russia, but from Hungary, southern Europe, Australia, and the Orient as well. Carleton's macaroni or hard wheats were, however, the most valuable. This work, and that of reclaiming large areas of otherwise unusable lands by crops resistant to alkali and drought, proved abundantly beneficial to the national economy. Another such crop which Smith mentioned was the date palm, imported for the Department by Walter T. Swingle and at that time

³¹ Commercial plant introduction, *Yearbook of the U. S. Dep't of Agric. for 1900*: 131-144, at p. 133.

"growing satisfactorily in several places in Arizona and California." Since 1902 Kearney, agrostologist, botanist, and physiologist, had been physiologist in charge of alkali and drought resistant plant investigations. Experiments with several crops including alfalfa were then under way. Smith eagerly awaited their outcome and, after describing them, urged his audience to watch for the results when announced. When Smith concluded his address, the president of the Conference thanked Smith and said: "I regret that Dr. Webber has not been able to attend this meeting, but I know that we will all agree that Dr. Smith has been an excellent substitute."

Neils E. Hansen, professor of horticulture at the state agricultural college and experiment station at Brookings, South Dakota, also attended the Conference from the United States. At the time in the employ of the Department of Agriculture as an agricultural explorer, he was making a six-months exploration tour around the world through Lapland, Finland, Russia, Siberia, Manchuria, and Japan.

August 17, from the Willie Commelin Scholten at Amsterdam, Smith addressed a letter to Woods concerning the Conference:

Yours of July 24 was awaiting me on my return from London. The plant breeding conference was one of the most delightful events I have ever been a part of. There were quite a good many foreign delegates and the English bestirred themselves to do everything possible for our pleasure. They entertained royally. Only this great Society with 10,000 members, a big building free of encumbrance, a great prestige, and a big bank account could do what they did. They gave two or three stand up luncheons in the building, a great banquet one evening in the Exhibition hall, to which about 200 persons sat down, a trip on omnibuses to Kew and South Kensington, and two elaborate luncheons (dinners really) at two great country estates. The first was at the home place of Sir Trevor Lawrence, president of the Royal Hort[icultural] Society. This is about 25 miles south of London in Surrey. For this trip they provided a special train of twelve railway cars to take and bring us back. Music for the occasion was furnished by the crack band of the Royal Something (Artillery I believe). The tables were spread under a big green and white tent on one side of the spacious lawn. After which we looked through the orchid houses and strolled on the lovely grounds. The rich English certainly love lawns and gardens and know how to make good ones. I considered myself very fortunate in having the opportunity to see such good examples of their horticulture and landscape work. The second luncheon was given by the millionaire banker and stock broker, Leopold de Rothschild, at one of

his country houses near London. Here we were treated in princely fashion. Wittmack of Berlin was called on to thank Sir Trevor Lawrence for his luncheon and I to thank Mr. de Rothschild on behalf of the foreign delegates. It was a tough thing in prospect, but I got through it all right. I knew it was coming for two days but like an idiot, I could not tell what I should say until just before I began to speak! Before and after the luncheon we saw his lawns, gardens, hothouses, lily ponds, fountains, etc. His lily pond is very extensive, perfectly free from weeds, has an excellent setting of shrubs and fine old trees and containing many choice hardy lilies (tender sorts hybridized on the English water lily). These were in blossom, in clumps with much clear water between. The effect was remarkably good. It is the first lily pond which has appealed to me from [a] landscape point of view. Most are too small, too dirty, and too crowded for good effects.

Some of the attendants were the Vilmorins (Maurice and Phillip) from Paris, Plate and Wittmack from Berlin. Tschermak from Vienna, Cuboni from Rome, Lotze and others from Holland, Morris and Fawcett from the West Indies, the Director of the Botanic Garden at Buenos Ayres, who knows Fairchild, Francis Darwin, Carruthers, Elwes, Masters, Rich, Salmon, and Bateson who presided etc. I was much interested in an exhibit of picotees and other sorts of carnations by one of the leading English hybridizers of carnations. He had many odd colors, but most of his flower stalks branched and have several flowers sprawling out very awkwardly. There was not a single variety which in my judgment an American grower would look at! Afterwards I talked with Mr. Scott of New York about it and he quite agreed with me. The English and American carnation growers must have quite different ideals, and ours is the better. With his permission I carried off as many of the sorts as I liked to the ladies at the boarding house (quite a handful), and I had to break off the side flowers to get any sort of decent looking bouquet.

I am planning to sail from Genoa, Italy, on September 20 by S. S. Princess Irene, North German Lloyd line, if I can get enough money raked together to buy tickets. My wife is still quite ill and is anxious to return and I shall have had enough of it by that time. Already I am beginning to want to see the Stars and Stripes, but not the customs house officials in New York. . . . I have bought altogether less than the permitted amount, but have many little things and all my apparatus (camera, microscope, etc.) clothing and other stuff . . . 3 trunks and 7 hand bags . . . I will get some resistant rice if any is to be had. I leave here for Italy about the time you will receive this letter.

Early the next week Smith began work at the Phytopathologisch Laboratorium. A memorandum indicates that he took notes on seven agar stab cultures of *Bacterium hyacinthi*. He wrote³² of his study of lilac blight:

³² Journal, August 20, 1906.

Today I made my first inoculations of the greenish fluorescent organism (which I isolated from the Roozeward lilacs) into leaves and shoots of lilacs in Mr. Scholten's garden using pure cultures grown 5 days in nitrate bouillon. The tubes were inoculated from agar stab cultures made from 9 poured plate colonies. Two varieties of lilac were tested but owing to the lateness of the season and to consequent inability to find much growing tissue, most of the pricks were made on one plant, Marie Legary. Nine colonies were tested on leaf blades, petioles and soft shoots by a few needle punctures into each. In all about 30 inoculations were made.

That evening a distressing telegram from Varese brought news that Mrs. Smith's condition was "much worse." Immediately he left Amsterdam for Italy, and Director Westerdijk reported by various letters on the lilac inoculations, each of which showed a good bacterial growth. Later this work was re-checked by Smith in the United States. Miss Westerdijk expressed her pleasure that he had worked at the laboratory since from him, she said, she had learned much. He found Mrs. Smith seriously ill but recovering from a partial paralysis induced by a small brain clot.

Weary and worried, Smith wrote to Haven Metcalf, now a pathologist with the United States Department of Agriculture, and he showed Smith's letter to Dr. Woods, who responded with instructions to "let Department matters rest for awhile. . . . Don't worry about things here," Woods wrote, "give your attention to your wife . . . cable us if there is anything we can do. All your friends here love you both dearly and we don't want anything to happen to you."

Metcalf in 1907 was to be placed in charge of forest pathology in the Department but at this time was working in Smith's laboratory. In 1901-1902 he, a Brown University graduate who had studied also at Harvard, had taught bacteriology at the University of Nebraska and while a fellow in botany there had secured his degree of doctor of philosophy. Of him Dr. Bessey had written to Dr. Woods in October, 1901, concerning his good training in animal bacteriology and his growing interest in the study of "bacteria causing pathological conditions in plants." Bessey had suggested cooperative bacteriological investigations in sugar beet diseases. The next year, however, Metcalf had become professor of botany at Clemson Agricultural College, South Carolina, and there, interesting himself in rice and other crops, had learned of several Italian varieties of rice resistant to the disease known as

brusone. He had written to David Fairchild of the office of seed and plant introduction and distribution who in turn had written to Smith to secure samples. Metcalf, by letter of August 17, 1906, replying evidently to a still earlier letter from Smith, described more fully the rice disease as he understood it, asked Smith to visit the Italian rice fields, and said that everything was "going smoothly in the laboratory." John R. Johnston, he informed, was "working on the potato material, and also on the cultures of Appel's organism, of which cultures were received apparently from Aderhold. We have as yet no results from the oleander inoculations. . . . I have examined the rice material from Farneti with a great deal of interest, and so far as I have examined it, it seems to be the same as what I have gotten in South Carolina." Hemp leaves also had been received from Peglion.

When Smith had hastily left Amsterdam, he went directly to Varese and omitted all of his planned conferences with scientists in Holland, France, and Belgium. It is possible that, after consulting with the doctor, Mrs. Smith insisted that he complete his work before their departure, and that then he went to Montpellier, Nancy, Paris, and other places in France, indeed even again into Holland to visit the station at Wageningen and other points which he had planned to visit. Most surely he had planned to confer with J. Ritzema Bos, H. M. Quanjer, Beyerinck, and others in the Netherlands, and in France, with Delacroix, Prillieux, and others at Paris, Boyer and Lambert at Montpellier, and many other pathologists, whether students of animal or plant diseases. Foremost a plant bacteriologist on this journey, he certainly must have already either met Wakker or satisfied himself as to his inquiries before he began his already mentioned culture studies of *Bacterium hyacinthi*. No later than 1896 Wakker, that year in Java, had written Smith about his papers on this subject.

He visited the Italian Lakes and Venice. But most of the remainder of his stay in Europe was spent with Mrs. Smith. What was to have been a happy ending was converging on tragedy, although she had not allowed him to believe that any real danger was imminent. Financially the trip had been exceedingly expensive, especially in view of the costs of Mrs. Smith's illness, for one of so modest a salary as Smith's. In a small note book he tabulated the name of almost one hundred plant pathologists of

France, Germany, Austria, Holland, England, Russia, and the Scandinavian countries. Beneath the names of each he listed the plant diseases on which he knew or was informed each had material, or had worked, and was an outstanding authority.

On the day before their scheduled sailing from Genoa, objections were raised to Mrs. Smith as a passenger because of her illness. Dr. Smith and her doctor telegraphed the American Ambassador at Rome who, in turn, communicated with the American Consul-General, and officials of the North German Lloyd Company were assured that she was suffering from a non-contagious ailment, chronic rheumatism. On September 20 they sailed on the steamship, *Princess Irene*, and voyaged by way of Naples and Gibraltar to New York.

That year Smith could not attend the meeting of the Society for Plant Morphology and Physiology. He had been present at every meeting of this society, served once as its president, and nearly if not always presented one or more papers.

Nor could he enjoy his second presidential distinction and preside at the meeting of the Society of American Bacteriologists. Its eighth annual meeting was held during convocation week at the College of Physicians and Surgeons and Rockefeller Institute for Medical Research. Formal ceremonies opening the laboratory of the institute³³ had been held on May 11, 1906, while he was in Europe, and Dr. Welch had been a principal speaker of the occasion.

The first session of the annual meeting of the Society of American Bacteriologists was held at the College of Physicians and Surgeons. There the papers more strictly treated topics of morphology, physiology, and other aspects of the science. But at the second session held with Section K of the American Association for the Advancement of Science the papers were of interest to bacteriologist, pathologist, and chemist, and were largely given over to researches in sera, toxins, and the biology of pathogenic bacteria and other organisms. This joint session met at the Rockefeller Institute, and on Saturday morning the Society again gathered at the College of Physicians and Surgeons to consider

³³ *Science*, n. s., 24(601): 1-18, July 6, 1906. See also, S. and J. T. Flexner, *William Henry Welch* etc., *op. cit.*, chap. XIII: 269-296, at 284-285 (account of the founding of this and other American medical research laboratories).

laboratory procedures, bacterial cultivation methods, milk bacteriology, and other branches. A brilliant Canadian bacteriologist, Dr. James Carroll, was elected to Smith's place as president of the society. Smith was chosen the society's delegate to the American Association for the Advancement of Sciences.³⁴

Carroll was the survivor of an heroic experience. He and Dr. Jesse W. Lazear, with Dr. Walter Reed and Dr. Aristides Agramonte, had been members of the United States commission or army board which had demonstrated that the mosquito, *Aedes aegypti*, transmits intermediately the organism which causes yellow fever. Voluntarily Carroll had permitted himself to be invalided by the disease. He recovered and returned to the United States. But Dr. Lazear, one of several truly great martyrs of American experimental science, lost his life in Cuba.³⁵

Samuel C. Prescott of Massachusetts Institute of Technology and secretary of the society reported to Smith, "We had a very good meeting, the only drawback being that you were not there to officiate." Smith had not prepared a presidential address or presided at the meeting because on December 28 Mrs. Smith had died. He had fought valiantly to save her life. He himself examined the cultures made from her blood. From a culture of the streptococcal infection, Dr. Rufus I. Cole, resident physician of Johns Hopkins Hospital and later director of the Hospital of the Rockefeller Institute, evidently prepared a vaccine. Dr. W. S. Thayer wrote Smith after the end: "You have been so brave throughout it all and it has been such a hard fight. But one thing I think I may say. It was I fear a losing fight from the beginning. The outlook in such cases is desperate and I fear that our hopes were too much raised by the first improvements."

In February he began to assuage his grief by writing his "Reflections and Memories." His haunting fear that death ends all cast heavy shadows on these pages but by April he was believing, "If any human love can survive death I believe hers will do so." By 1912 he concluded this "book" with a triumphant resolution: "I have begun a memorial of her. It is to be verse and prose. I have written 104 sonnets and various other poems."

³⁴ *Science*, n. s., 25(647): 805-806, May 24, 1907.

³⁵ See also Charles Morrow Wilson, *Reed of Virginia, Ambassadors in White, The story of American tropical medicine*, 98-124, N. Y., Henry Holt and Co., 1942; concerning Carroll and Lazear, pp. 102-120.

He found prose more difficult than verse to write, and he secured the well known medalist and sculptor, Victor David Brenner, to make a "beautiful low relief of her face" in bronze. He designed the reverse of the medal of her, and chose its inscription. He was confident

Her pure sweet deeds will ever wider flow, In other lives unceasing live and grow, As priceless part of earth's great heritage . . . Nature and Art, twin goddesses fair, Walked with her, my beloved, everywhere, Unfolding the beauty in common and lowly things, Till the varied earth, inwoven with mystic light, Darkened and gleamed, a haunting loveliness of form and tone, Proclaiming in rapturous hours the Master Will, The indwelling Soul, whose law unto love is wed. Oh would I could know if the heart's sweet music ends with the broken strings, Or sings to a lordlier harp beyond our mortal sight! With the clogs of the mortal body forever shed, Somewhere I trust, in the cosmic vastness, she liveth still, Wiser and statlier grown, more beautiful there, But finding still in the good of others her own.

For Her Friends and Mine, a volume of 380 pages of beautiful poetry, was privately printed and distributed in 1915. In English literature few more exquisite and appropriate poems have ever been written by a scientist, or by a husband, to the memory of his wife. These verses were proof of his genius, and of his ability to write poetry expressive of a scientist's view of life and nature. He also employed the prominent sculptor, U. S. J. Dunbar, to prepare a bronze bust of her, and concluded his description of her with the quotation, "Earth changes, but thy soul and God stand sure." Pursuant to her request, Mrs. Smith's body was cremated and her ashes scattered along the shore at Woods Hole.

Smith promised Farlow that "after a while" he would "get to work" again, since he knew that was "best" for him. One of his accomplishments in 1907 was the preparation of a chart or "society card," a standard form for describing and classifying species of bacteria, which was approved that year by the Society of American Bacteriologists.³⁶ Since 1903 he had been working with F. P. Gorham and F. D. Chester as members of a committee to study the problem of classification.

Frank R. Lillie, assistant director of the Marine Biological Laboratory and professor of zoology and embryology at the Uni-

³⁶ Agnes J. Quirk and Edna H. Fawcett, Hydrogen-ion concentration vs. titratable acidity in culture mediums, *Jour. Infectious Diseases* 33(1): 4, July 1923, and reprint. Introduction written and contents approved by Smith.

versity of Chicago, wrote Smith in February he was "grateful" that once again he was interesting himself in the Laboratory and that the Department of Agriculture would probably again take a room for research during the approaching summer. Soon after Smith had been elected a trustee of the Laboratory, the corporation had undergone a crisis in its affairs but had emerged stronger than ever with its traditional policies intact.³⁷ During 1903, 1904, and 1905, the corporation of the Marine Biological Laboratory had been enjoying an annual grant from the Carnegie Institution of \$10,000 for the privilege of using twenty tables at the Laboratory. In 1906, however, this annual grant had not been available by reason of the Carnegie Institution's plans to establish its own department of marine biology, which became located at Tortugas, Florida. Smith, therefore, in 1907 arranged, as he did at least once before, for the Department of Agriculture to have a room at the Laboratory and probably with a view to pursuing some of his own researches there that summer. For, on August 11, 1907, in one of a series of letters which told of his plans and activities and which were written at Woods Hole, he said: "I brought many notes and manuscripts relating to scientific things, for I hope to finish, or nearly finish, the second volume of my book while here this summer and autumn,"³⁸ *Bacteria in Relation to Plant Diseases*.

Other laboratory facilities away from Washington and devoted to plant science investigation were offered him that year. March 17, 1907, Hermann von Schrenk at St. Louis had invited Smith to be a visiting scientist at the laboratory there. Von Schrenk, in the sixteenth *Annual Report of the Missouri Botanical Garden*,³⁹ had published in 1905 on "Intumescences formed as a result of chemical stimulation," a research some of the experiments of which Smith later repeated to study the effect of ammonia, in addition to various copper fungicides, as causes of intumescences in some plants.⁴⁰

³⁷ F. R. Lillie (and E. G. Conklin), *The Woods Hole Marine Biological Laboratory*, *op. cit.*, 50-59, and Smith's collected materials.

³⁸ *For her friends and mine*, *op. cit.*, 43.

³⁹ St. Louis, Mo. (May 31, 1905), pp. 125-148. Also, *Science*, n. s., 17(424): 263, 1903.

⁴⁰ E. F. Smith, Mechanism of tumor growth in crown gall, *Jour. Agric. Res.* 8(5): 171-172, 181, Jan. 29, 1917; Mechanism of overgrowth in plants, *Proc. Amer. Philos. Soc.* 56: 443-444, 1917; *An intro. to bact. dis. of plants*, *op. cit.*, 483, 572.

During the summer or early autumn of 1907, Smith visited the new laboratory of the Rockefeller Institute for Medical Research located at 66th Street and Avenue A in New York City. From Washington, on November 20, 1907, he addressed a letter to the director, Dr. Simon Flexner:

I was much pleased at what I saw during my brief visit to the Rockefeller Institute. It seems to me quite as good a place to work as Koch's establishment in Berlin. I hope, if it would not inconvenience you in any way, that you will some time let me come and work in the institution for two or three months, so that I may get somewhat better knowledge of animal tumors for purposes of comparison with those on plants which I am now working with.

Flexner answered immediately: "I should regard it as a most flattering distinction to have you come to the Institute to work, for as long or as short a period as you choose, on the subject of tumors in animals, and I should put the facilities of the Institute wholly at your disposal." We do not know if or when Smith studied at the Institute; but that he often consulted research authorities there is sure, and it is quite likely that at some time he made use of the facilities to study animal tumors.

He was asked to recommend someone for an assistant professorship with "much time for research" which might soon become available at Harvard. He received a letter from Theobald Smith of the Laboratory of Comparative Pathology of the Medical School that, as a part of a reorganization of the work of the Bussey Institution, vegetable pathology and bacteriology, both in teaching and research, was planned to be included in the curriculum.

T. J. Burrill expressed to Smith his "admiration for your magnificent introductory volume on bacteria in relation to plant diseases. It is by far the best thing of the kind ever published and is bound to be a classic upon the subject. I congratulate you heartily in its production, and hope you may succeed as well with what is to follow." Within a year Howard Spurr Hammond of the department of botany at the University of Illinois was using the volume "extensively in some research work [he was] carrying on in connection with some of the graduate work." L. R. Jones wrote that he had been using the volume and that his "satisfaction in it increase[d] with time and usage. I hope." he said, "that you appreciate, in some measure at least, how

valuable this has proved to us in our Station work and in the class room, and also I hope that your courage is holding good for the pushing of the work upon the subsequent parts.”⁴¹

On April 26, 1907, Dr. Smith and Dr. C. O. Townsend of the office of sugar beet investigations published in *Science*⁴² their very important announcement of “A Plant-Tumor of Bacterial Origin.” They said:

The number of vegetable galls known positively, *i. e.*, by exact experiment, to be due to bacteria, is not very great. The discovery of a new one of undoubted bacterial origin is, therefore, of considerable interest to plant pathologists, and may be some interest to animal pathologists, especially to those interested in determining the origin of cancerous growth. For two years the writers have been studying a tumor or gall which occurs naturally on the cultivated marguerite, or Paris daisy. It has been difficult to isolate the organism and to demonstrate it unmistakably in stained sections. Recently the bacteria (seen in small numbers in the unstained tissues on the start) have been plated out successfully. With sub-cultures from poured plate colonies, thus obtained, the galls have been reproduced abundantly and repeatedly during the last few months, the inoculations having been made by needle-pricks. . . . The organism attacks both roots and shoots. It frequently induces abnormal growths on the wounded parts of young cuttings. Its power to produce hyperplasia is not confined to the marguerite. Well-developed small tumors have been produced in a few weeks on the stems of tobacco, tomato and potato plants and on the roots of sugar beets. More interesting economically is the fact that galls closely resembling the young stages of crown-gall have been produced on the roots of peach trees by needle-pricks, introducing this organism. In eighteen days these growths have reached the size of small peas, the checks remaining unaffected. It is too early, perhaps, to say positively that the cause of the wide-spread and destructive crown-gall of the peach has been determined by these inoculations, but it looks that way. Of course, the most that can be affirmed absolutely at this writing is that we have found an organism which when inoculated into the peach produces with great regularity galls which in early stages of their growth cannot be distinguished from the crown-gall. The matured daisy galls also look astonishingly like the peach gall. Numerous experiments which ought to settle the matter definitely in course of the next three months are now under way.

During 1892-1893, when Smith first studied this disease, he had not really begun his work in bacterial diseases of plants. Under the microscope and with the use of culture media, he had examined

⁴¹ Burrill's letter was written February 6, 1907; Jones's, December 19, 1907; and Hammond's, January 14, 1908.

⁴² N. s., 25(643): 671-673, Apr. 26, 1907.

specimens of crown gall of the peach, but only for plasmodia or fungi as parasitic causes of the malady. Since then, other able scientists, among them, Waite, von Schrenk, P. J. O'Gara, and George Grant Hedgcock, had made some study of the disease but never discovered its cause. Hedgcock was a graduate of the University of Nebraska and since 1901 had been with the Department in several capacities as a plant and forest pathologist.

Toumey's "Inquiry into the Cause and Nature of Crown-Gall"⁴³ had been principally concerned with the disease as found in almond trees, and its causal organism, he believed, was myxomycetous in nature. *Dendrophagus* had been chosen for the name of his proposed new genus "to suggest the cancerous character of the gall tissue," and *globosus* was suggested for the species because of "the form of the mature sporangium." He regarded it as "a specific organism, belonging to the slime-molds, or Myxomycetes. Altogether," he said,

about 200 species of slime-molds have been published as occurring in North America. Of this large number but a single species is known to be parasitic, and this is considered by some authors as a doubtful Myxomycete. The parasitic species heretofore described is the common *Plasmodiophora brassicae*, which causes the disease known as "club-root" in cabbage and allied plants.

Smith had "never seen anything on or in crown galls which ha[d] in any way led him to think that Myxomycetes have anything to do with their production. Phenomena," he wrote in 1911,⁴⁴ "similar to that described by Toumey were seen independently by the writer as long ago as 1893 in cells of the crown gall of the peach, and after six months' work upon the subject, the conclusion was reached that the appearances were artefacts and not living slime molds." He had seen and studied Toumey's slides, but had never been convinced that the organism described and figured by him "had any connection whatsoever with the cell-changes in the gall also described and figured by him. . . . But even granting Toumey's interpretation as correct," he wrote further,

⁴³ *Op. cit.*, Bulletin 33, April 13, 1900, publ. of Univ. of Arizona Agric. Exp't Sta., 64 pp. See pp. 14, 21, 25, 26, 39, 55.

⁴⁴ E. F. Smith, Crown gall of plants, *Phytopathology* 1(1): 7-11, Feb. 1911; quotation taken from concluding note, pp. 10-11, at p. 11.

the etiological significance of the plasmodium has not been made out, since all sorts of non-parasitic soil organisms are likely to occur in galls when they are of some size. Toumey inoculated only ten plants altogether and obtained successful infections on *three* only of the ten.⁴⁵ The inoculations were made in a country where the disease prevailed extensively and with spore material taken from the *cut surface of the gall*. The *Dendrophagus* spores, therefore, would be liable to contamination with anything occurring on the cut surface, especially the virulent schizomycete which can be demonstrated to be present in such galls and with which in pure sub-culture the disease can be reproduced, if the plants are in a young and actively growing condition, 100 times out of 100.

Drs. Smith and Townsend in their preliminary announcement of "A Plant-Tumor of Bacterial Origin" (1907) proposed for "the organism causing these tumors the name *Bacterium tumefaciens*," new species, "a schizomycete causing rapid multiplication of the young tissues of *Chrysanthemum frutescens*, *Prunus persica*, etc." At that time their experiments on crown gall of the peach were not completed; but they mentioned Toumey's work as not clearly establishing that it was due to a myxomycete. They admitted that "the cause, in spite of much study by many persons, [was] still in dispute," and acknowledged that the disease had been proved to be communicable by the experiments of Thaxter, Halsted, Selby, Toumey, W. E. Smith, von Schrenk, Hedgcock, and others. When minced galls were buried in the earth near the roots of sound trees, crown galls developed.

Sometime between February, 1904, and March, 1906, Smith had examined fresh, unstained, thin sections of undecayed galls, prepared by Townsend. He had detected bacteria in the interior of the gall material but was never thereafter prepared to say that these were actually the bacterium later isolated. Seeing bacteria in small clumps so positively, he (or Dr. Townsend) directed Miss Haskins "to make agar-poured plates from the interior of suitable galls; and with the bacteria so obtained, inoculations on healthy daisy plants."⁴⁶

The work proceeded for "many months without positive results."⁴⁷

⁴⁵ An inquiry into the cause and nature of crown-gall, *op. cit.*, 63.

⁴⁶ E. F. Smith, N. A. Brown, and C. O. Townsend, Crown-gall of plants: its cause and remedy, U. S. Dep't of Agric., Bur. of Pl. Ind. Bull. 213: 22 (issued Feb. 28, 1911). Miss Haskins's work summarized, pp. 20-25.

⁴⁷ *Idem*, 22.

Other workers contributed "here and there,—cultures set, sections cut, flagella stained, chemical analyses made."⁴⁸ But for two years Miss Haskins was the laboratory assistant, she preparing in all several hundred plate cultures. To Smith she had been recommended as a research worker in 1903 by her professor of botany, W. F. Ganong, of Smith College.

On February 21, 1904, the Department had received from one of the large commercial plant growers in New Jersey white and yellow marguerites or Paris daisies (*Chrysanthemum frutescens*) infected with gall-like growths on the stems and leaves, and it was stated that the galls appeared, without evident cause, on plants grown in the open in summer and under glass in winter.⁴⁹ Inability to secure the gall with regularity prompted Dr. Townsend to abandon for a while the theory that the cause was bacteria and to explore the effects of mechanical injuries on young and old plants. In May 1906, however, after Dr. and Mrs. Smith had sailed for Europe, one of the workers, presumably Dr. Townsend, observed, while studying microtome sections stained with anilin compounds having a strong affinity for bacteria, that although "no distinct bacteria could be made out, nevertheless, that part of the section lying deepest, i. e., bordering on the sound tissues, took the stain much heavier than the rest of the gall as though the living bacteria might be lodged most abundantly in this portion. It was suggested, therefore, that for the next series of plates deeper tissues should be used." New cultures were prepared and in four from six groups of plates a yellow organism developed; and in five from the six series "a few small, round, white colonies appeared in each plate. . . . Slant agar and potato cylinder cultures were made from both the yellow and white colonies, also cultures in litmus milk."⁵⁰ During June young healthy daisy plants growing in the greenhouse were inoculated with each of the four organisms at the top, middle, and base of their stems. At least two sets of inoculations were made. On June 18 knotty growths were visible and later developed into "considerable size" in some of the pricked spots.

Dr. Smith, before his departure, wrote in his journal: "Deane

⁴⁸ E. F. Smith, Crown gall in plants, *Phytopathology* 1(1): 7, Feb. 1911.

⁴⁹ E. F. Smith, The discovery of cancer in plants, *Nat'l Geographic Magazine* 24 (1): 57, Jan. 1913; Crown-gall of plants: its cause and remedy, *op. cit.*, 21.

⁵⁰ Crown-gall of plants: its cause and remedy, *op. cit.*, 23-25.

Swingle, my assistant, now in California assisting Merton B. Waite in his effort to exterminate pear-blight and save the orchards of a great state, is to be married in June to Miss Alice C. Haskins, who was formerly in my laboratory and who now works with Dr. C. O. Townsend. . . .”

Galloway wrote:

You probably know by this time that Deane Swingle has decided to leave the Department, having accepted a place in Montana [at the agricultural college at Bozeman]. . . . From the fact that at the last session of Congress a bill was passed giving each experiment station \$5,000 additional last year, \$10,000 the present year and \$15,000 next year, the stations will be in a position now to compete with us for men. We are already beginning to feel the effect of this; and several of our men have had offers. In three years, therefore, all the stations will be provided with \$30,000 annually for their work instead of \$15,000. The man who was responsible for this measure, Mr. H. C. Adams, Member from Wisconsin, has been a very good friend, not only of the stations, but of the Department as well.

The crown gall cultures and inoculations throughout most of that summer, as a consequence, were not cared for properly, and some of the labels on Miss Haskins's work were lost. In September Miss Nellie A. Brown, one of America's ablest women scientists and whose work in this connection deserves special recognition, became employed by Dr. Townsend's office of sugar beet investigations. She had been graduated at the University of Michigan under Spalding and Newcombe, and had studied at Dr. Rolfs's and the Department's sub-tropical laboratory at Miami. Dr. Townsend told Miss Brown to continue the work. She plated the bacterium with the use of suitable media, grew the germ and, with subcultures from poured plate colonies, reproduced galls on plants repeatedly. On November 27 she made

28 inoculations into marguerite daisies, using 4 different organisms plated from a daisy gall found in the greenhouse (probably produced by one of Miss Haskins's inoculations—labels lost off). Inoculated each organism into 7 different daisy plants at the tip. The cultures were 2 days old.

Result—December 12: All 7 plants inoculated with the white organism (designated B) had knobby outgrowths. No protuberances were visible on plants inoculated with the other organisms.

December 18: Galls formed on all those plants inoculated with B. The

same organism (B) was isolated by poured plates from one of the galls and its infectious nature proved by [subsequently described] inoculations.⁵¹

On February 18, 1907, Smith began experiments to verify and extend what had been done. His memoranda read:

Four vigorous plants of white flowered Paris daisy and 6 similar plants of the yellow-flowered Paris daisy were selected. Each plant of the white variety branched at the base into two equal shoots; 7 of these shoots were inoculated and the eighth was held as a check. On the inoculated shoots also check pricks were made an inch or two above the places where the infected needle entered. All of the inoculations were made by needle pricks, using a slant glycerine-agar culture, 7 days old, which had been streaked from another slant agar culture. The organism was derived from a strain which had been passed twice through the daisy by Miss Brown with the production of tumors (the organism designated B). It was probably a third or fourth subculture from the colony. Four of the inoculated white-daisy shoots received 3 needle pricks each; two received 1 prick each; one received 50 pricks. . . . Result—February 23, 1907: There was distinct evidence of infection on each of the 12 shoots at the end of 5 days, the protuberances on some being nearly a millimeter high.

During and from July, the "plants were removed at the end of 1 month, 2 months, and later, with well-developed tumors. Galls formed only where inoculated. The 122 sterile (check) punctures healed normally. Every infected prick resulted in a larger or smaller tumor." By November 25, he added, "During the summer some of the plants developed many secondary infections (metastases)." ⁵²

This appears to have been his first experiment using the daisy organism on daisy, and on February 18 he started other similar experiments using the daisy organism on tomato and tobacco. Experiments with the daisy organism, and with schizomycetes from galls on other plants, would be made on a wide variety of cultivated plants. Hard and soft galls would be studied according to type, distribution, cause, technique in study, cultural characters, histology, transmission, variability, etc. The work would bring

⁵¹ Crown-gall of plants: its cause and remedy, *op. cit.*, 25. Miss Brown's sets of inoculations here referred to were started on January 8 and 18 and February 6, 1907. Scientists interested in this subject should read the description of the technique applied by Miss Haskins (pp. 22-25) and compare this with pp. 25 et seq., "Further inoculations by [Smith, Brown, and Townsend] made with the daisy organism and with the same or similar bacteria plated from galls on other host plants."

⁵² Crown-gall of plants: its cause and remedy, *op. cit.*, 26-27.

about modifications in nursery and orchard practice, and lead to a rigid system of crop shipment inspection.⁵³ Later when Smith would prepare his text, *Introduction to Bacterial Diseases of Plants*, several of the photographs used to illustrate the chapter on "Crown Gall" would be taken from pure culture inoculations made during these first years, 1906-1907.⁵⁴

Smith did not present the discovery in 1907 before the Society of American Bacteriologists. At one of the sessions of the Society's meeting of December 29-31, 1908, however, he addressed the Society on "The Etiology of Plant Tumors," illustrating his lecture with a series of lantern slides, numerous stereopticon photographs of inoculated plants of various species and of widely different families. *Science's*⁵⁵ abstract of the address reported that

Hundreds of pure culture inoculations [had] been made. The organism cultivated from the Paris daisy has been inoculated many times successfully into the same and also into the peach, rose, hop, sugar-beet, white poplar and other susceptible plants. That from the crown gall of the peach has been many times successfully inoculated into the peach, and also into the Paris daisy, sugar-beet, hop and other plants. The schizomycete from the hop has been inoculated successfully into the hop and into the Paris daisy, sugar-beet and other plants. One of the astonishing things about this crown-gall organism is the number of families which are subject to infection; in other words, the very simple and generalized nutritional needs of the parasite. In some ways it resembles the root-tubercle organism of Leguminosae, but is not identical. It has been inoculated into clovers with the production of knots. Quite recently from the hard gall of the apple (selected by Dr. Hedgcock) we [Smith, Townsend, and Brown] have isolated an organism which appears to be like that occurring in other crown galls, and with this, hard galls have been produced upon the Paris daisy. A similar if not identical organism has also been isolated from the hairy root of the apple and successfully inoculated into the sugar-beet, i. e., with the production of similar root-tufts at the point of inoculation. There is now little doubt, therefore, that the hairy root of the apple is also of bacterial origin.

Metastatic growths occur on these plants, but up to this time we have not definitely determined the channels of infection from the primary tumor to the secondary ones. These are easily discovered in the case of the olive-

⁵³ Fifty years of pathology, *op. cit.*, 32.

⁵⁴ *Op. cit.*, 413-432.

⁵⁵ N. s. 30(763): 223-224, Aug. 13, 1909. Abstracts of three other papers presented to the Society of American Bacteriologists by Smith also published were: Seed corn as a means of disseminating *Bacterium stewartii*; The occurrence of *Bacterium pruni* in peach foliage; and Two sources of error in the determination of gas-production by microorganisms.

tubercle, but are not readily found in case of these crown galls. The same remark is true respecting the bacteria in the primary tumors. They are very abundant and easily discovered in the olive-tubercle, but not readily detected in the crown gall, although obtainable therefrom in Petri dish cultures on agar.

The abstract, probably prepared by Smith, suggested a possible, but unestablished, theory:

It is still too early in the course of our studies to make positive statements respecting the likeness or unlikeness of these growths to malignant animal tumors, but it is proposed to continue this phase of the inquiry. There is in these growths a very rapid multiplication of parenchymatic tissues with reduction and distortion of the firm conductive tissues of the plant and the final decay and sloughing off of the spongy tissues, leaving open wounds, on the margins of which fresh developments of the tumor may appear.

Smith's and Townsend's article, "A plant-tumor of bacterial origin," described the schizomycete *Bacterium tumefaciens* and its pathogenic properties; and was published in *Science*, and in the *Centralblatt für Bakteriologie II* ⁵⁶ with some additional data. The organism's description followed the provisions of the chart of the Society of American Bacteriologists, and its group number became 212.2322023. ⁵⁷ These were the first of many important statements on this subject addressed primarily to plant pathologists. In 1922 ⁵⁸ Dr. Smith presented a general summary, or outline, of the work. This read as follows:

The general outcome of these researches, continued for many years and still going on, has been an entire revision of views as to the nature of the disease and as to sanitary measures necessary for its restriction. We now know not only the morphology and biology of the organism causing the tumors but also that the type of the tumor varies with the part infected, that there are several strains of the organism and probably many, that isolations differ in virulence, that some colonies which look all right have no virulence whatever, that on culture media and probably in the plant some strains lose virulence much sooner than others, that isolations are cross-inoculable to a very surprising degree, i. e., to plants of many families, that some plants, immune or nearly immune to certain strains, respond vigorously to other strains, and that some species are resistant to all strains so far tested, e. g., olive, onion and garlic.

⁵⁶ 20(1/3): 89-91, 1907.

⁵⁷ Erwin F. Smith, Nellie A. Brown, and Lucia McCulloch, The structure and development of crown gall: a plant cancer, U. S. Dep't of Agric., Bur. of Plant Industry Bulletin No. 255: 16, issued June 29, 1912.

⁵⁸ Synopsis of researches, *op. cit.*, 39-40.

Things not yet determined are the number of strains, the extent of cross inoculability, the cause of resistance, the reason for loss of virulence on media, the nature of certain beet tumors and the question whether right looking but non-infectious colonies from such tumors and occasionally from other tumors are really the parasite deprived of infectious powers by sojourn in the plant under unfavorable conditions or are only deceiving saprophytes, extent of variability of the organism on culture media and in the tumor, production of metastasizing tumors in animals, etc.

Smith would spend many years trying "to think out the rationale of what goes on in the cell following the introduction of the crown gall organism. What we see is excessive and abnormal multiplication of the tissues resulting in a tumor, a hyperplasia," he would say⁵⁹ in 1917. "What we would wish to know is the mechanism of the growth—that is, the chemical or physical stimulus behind the observed phenomena—since, if we can comprehend it in the plant, we may be able to apply our knowledge to the understanding of similar phenomena in man and animals."

He admitted that at first he thought of "the crown gall phenomena as much more complex than it really is. . . . In the beginning I had," he wrote,

what I now believe to be a wrong conception of growth. I looked upon it as something that an outside substance could directly stimulate into development, but probably it is not that (Weigert, Ribbert, Loeb). Growth is the normal function of cells. They are always multiplying when they are not inhibited by one thing or another. Growth, then, if this view is correct, comes about not by the direct application of stimuli, but indirectly *by the removal of various inhibitions*. . . . In crown galls the removal of growth inhibitions is brought about, I think, by the physical action of substances liberated within the tumor cells as the result of the metabolism of the imprisoned bacteria.

From that time his use of the word "stimulus" was to be construed as "*the remover of an inhibition*."

He became a student of plant tumors and their etiology. He found indicated analogies in structure and function between the higher plants and animals in health and disease.⁶⁰ He approached his comparison between crown gall and sarcoma with "mental reservations." He was more confident of his analogy between crown gall and "a nontypical epithelioma or carcinoma."⁶¹ Crown

⁵⁹ Mechanism of tumor growth in crown gall, *Jour. Agric. Res.* 8: 165-167.

⁶⁰ The structure and development of crown gall, analogies, *op. cit.*, 53-57.

⁶¹ *Idem*, 54.

gall, where occurring, he said, is a plant cancer, structurally "unlike that of club-root of cabbage, which is a hypertrophy rather than a hyperplasia."⁶² The olive tubercle (*Bacterium savastanoi*) was "an excellent example of atrophy."⁶³ In suggesting resemblances in crown gall of plants to malignant animal tumors and certain malignant neoplasms in man he hoped to aid animal and medical pathologists in solving problems of the more obscure higher animal malignancies including human cancer. It is true that he consistently favored the view that in certain types of human cancer a parasite would be found involved, indeed the cause. But in his 1912 bulletin, "The Structure and Development of Crown Gall," he made it plain that nothing therein should be construed to indicate that the authors, Smith, Miss Brown, and Miss McCulloch, believed that "the organism causing crown galls is able also to cause human cancer, but only that [they believed] the latter due to a cell parasite of some sort . . .,"⁶⁴ and the possibility of a virus origin of human cancer was not excluded. If a tag must be affixed, Smith preeminently was a physiologist in plant pathology or a pathologist in plant physiology, interested in studying the life processes and structures, and the existent analogies, between created living forms, the intention being to help solve the problems of disease. He grew bolder as he grew more convinced. But in his first appearances before the American Association for Cancer Research, he maintained little more than his fundamental contention: that crown gall of wild and cultivated plants, while proved conclusively to be of bacterial origin, is a tumor growth, comparable to certain malignant animal tumors.

February 22, 1909, William Carpenter MacCarty, associate pathologist of the laboratory staff of the Doctors Mayo, Graham and Company—the renowned Mayo Hospital—of Rochester, Minnesota, addressed a letter to Smith:

Your letter of February 18th and reprints were received and thoroughly appreciated.

My scientific career was started as a botanist, and I therefore seek on every occasion to connect histological botany with histological pathology.

I have for some time wondered whether there could be any facts con-

⁶² *Idem*, 57, fact 5. See *Résumé*, pp. 57-60; 38 facts summarized.

⁶³ *Bact. in Rel. to Pl. Dis.*, *op. cit.*, 2: 92.

⁶⁴ *Op. cit.*, fact 38, p. 60.

nected with pathological growth in plant life which might throw some light on the many pathological conditions which we find in animal life. While with Dr. [Walter Bradford Cannon, professor of physiology of Harvard Medical School] I mentioned this subject to him and he immediately referred me to you. I shall read the reprints with great pleasure and hope that I may find something in them which I can apply to my work.

If at any time I can be of service to you in supplying you with pathological material, I shall be only too glad to send you what I can. Our material is very extensive and more than we can really handle, and we will be very glad to turn anything over to you which might be of service.

If I should find any connection between the new growths in plant life and animal life I shall immediately communicate with you.

On March 30, a little more than a month after Dr. MacCarty had written, Dr. Gaylord, now director of the Cancer Laboratory of the New York State Department of Health, sent Smith some references to literature on tumor transplantation, accelerated growth in mouse tumors, and other topics. A second paragraph of his letter extended a cordial and significant invitation:

I find that I shall be in Boston for the meeting. We will meet on April 8th and if you can find it possible to be present at the meeting and present your material it would give us the very greatest pleasure. As secretary of the society, I extend to you an invitation to present your material at that meeting. I expect to be in Washington on Wednesday or Thursday and will try to find time to discuss the matter further with you.

During 1909-1910 Dr. Gaylord served as president of the American Association for Cancer Research. Such interest was aroused at the Boston meeting by Smith's lantern-slide illustrated address that he was invited the next year to appear before the Association at its spring meeting in Washington. At this time Smith exhibited specimens of crown gall. Still again, and for the third time, he addressed the Association at its Buffalo meeting, April 13, 1911, on "Crown-Gall and Sarcoma,"⁶⁵ in which he stressed likenesses, *developmentally*, between crown gall, a plant cancer, and certain malignant neoplasms in man. He called attention to "the method of action of the parasite on the cells of the

⁶⁵ *Zeitschrift für Krebsforschung* 11, Bd. 1. Heft., an abstract of this address. See also Circular 85, Bur. Pl. Indus., U. S. Dep't of Agric., issued June 20, 1911, 4 pp. In his address, On some resemblances of crown-gall to human cancer, *Science*, n. s. 35(892): 161-162, Feb. 2, 1912, Smith spoke of this as his "third address before the American Association for Cancer Research, an abstract of which was published by the Department of Agriculture as Circular No. 85 . . . and in *Zeitschrift f. Krebsforschung*, 11 Bd., 1 Heft."

host." He realized that superficially crown gall "growths are like those due to various fungi, to gall insects, to *Plasmodiophora brassicae*, or to the olive-tubercle organism, but structurally," he argued, "they are different. Their manner of growth and their histology strongly suggest certain malignant animal tumors."⁶⁶ He was discussing only resemblances, facts of an analogy admittedly incomplete as yet, between crown gall of plants and malignant animal tumors, and his discovery in 1911 that a distinct strand of tumor tissue could be traced from primary to secondary tumors lent credence to his point of view. But he qualified the proposal:

There is perhaps no metastasis, in the strict sense of the term, but so far as regards the mechanism of tumor development it is a matter of minor importance whether the migrating tumor cells separate themselves entirely from the parent mass or remain connected with it by means of a tumor strand. The plant body favors the latter method of development, whereas the rapidly circulating blood stream in animals favors the former, i. e., the dislodgment and distribution of cells. The matter of chief importance is the method of action of the parasite on the cells of the host.⁶⁷

It was a worthy faith, but Smith advanced no more than a hope, that "some light" on the origin of malignant human tumors might be disclosed from his researches. That the light began to have faint glimmerings around the world was in part due to Dr. Gaylord. In October 1910 he attended the International Cancer Congress at Paris and heard Dr. C. O. Jensen of the Serum Laboratory of the Danish Veterinary and Agricultural School at Copenhagen address the conference. When Dr. Jensen expressed a belief that his experimental tumors on beets were probably non-parasitic in origin, Dr. Gaylord informed him of Smith's discovery and isolation of the parasite *Bacterium tumefaciens*. Jensen procured a copy of Smith, Brown, and Townsend's bulletin 213, "Crown-Gall of Plants: Its Cause and Remedy," and, after studying its contents and himself performing further experiments, began to share the view, quoting him, "that 'crown-gall' is a new formation which (irrespective of the cause of its formation) can be placed side by side with the real malignant tumor-formation in the higher animals. . . . Through all these properties this

⁶⁶ E. F. Smith, Crown gall of plants, *Phytopathology* 1(1): 10, Feb. 1911.

⁶⁷ Crown-gall and sarcoma, *Circular* 85, *op. cit.*, 1-4, at p. 1.

tumor shows very considerable points of similarity with the animal malignant tumors, especially with the carcinoma.”⁶⁸ And Dr. Jensen enjoyed a world wide prestige as a student of animal cancer!

Since 1906 Dr. MacCarty had been with the Mayo Clinic as a specialist in surgical pathology and biopathology. Graduating in science in 1900 from the University of Kentucky, he had obtained his degree of doctor of medicine at Johns Hopkins in 1904 and then gone to the Koenigin Hospital, Berlin, Germany, for further study specializing in surgical pathology. Returning to the United States, he had planned, together with teaching pathology at Washington University, to practice medicine in St. Louis. But Dr. Welch recommended him for appointment as a full time pathologist on Director Louis B. Wilson's laboratory staff, with facilities at St. Mary's Hospital, Rochester, Minnesota, and under the celebrated surgeons and brothers, Dr. William James Mayo and Dr. Charles Horace Mayo.⁶⁹ For a year while at the University of Kentucky, Dr. MacCarty had taught botany and was, therefore, admirably fitted to collaborate informally with Smith in the latter's new work. Dr. Gaylord for several years had made plain his certain interest in Smith's analogue of crown gall and human cancer. April 19, 1911, Dr. Roswell Park, reading Smith's monograph, "Crown-Gall of Plants: Its Cause and Remedy," sent him an immediate card of thanks and said, "As I have glanced over its pages it becomes more and more evident to me that the work on Comparative Pathology needs to be written and needs you for a collaborator."

At McGill University, Montreal, Canada, where Dr. William Osler had taught before going to the University of Pennsylvania and Johns Hopkins, the great English born pathologist, Dr. John George Adami, now professor of pathology of the Department of Pathology and Bacteriology of McGill, on March 8 of the same year had written to Smith:

I have read with intense interest your monograph on Crown-Gall of Plants, and congratulate you most cordially upon it, and upon the results

⁶⁸ See a translation of a paper by Jensen in Smith's address, Twentieth century advances in cancer research, *Jour. Radiology* 4(9): 299, Sept. 1923.

⁶⁹ An interesting book on these two great men has been written by Helen Clapesattle, *The Doctors Mayo*, Minneapolis, Univ. of Minn. Press, 1941. In this appears many references to Dr. MacCarty and his work at the Mayo Clinic.

you have achieved. It seems to me a most valuable contribution to the study of Neoplasms. If you know the chapters on Neoplasia in the first volume of my Principles of Pathology, you will understand that I am of an open mind on the subject of the relationship of microbes to the origin of tumours. I am wholly ready to accept what you have shown, namely, that there are bacteria of such a grade of virulence that they initiate cell proliferation rather than cell destruction. I rather incline to the belief, however, that those having initiated proliferation, the cells then acquire the "habit of growth" and may continue to proliferate in the absence of the primary stimulant. Thus I am not a little interested to see that in the older growths you note the difficulty in obtaining cultures. Is it your opinion that these growths continue developing actively for months after the germs have practically disappeared? There is a method, by the by, worked out by Duval, which is very-successful in staining refractory bacteria in animal tissue. I cannot at the moment put my hands upon the reference. It might be worth your while to write to him at Tulane University, New Orleans, for the technique.

Lastly, looking at your admirable and abundant photographs, I am impelled to ask you whether you have one or two examples of these galls that you could spare for our Museum.

June 20, 1911, the United States Bureau of Plant Industry published a four-paged circular, number 85, prepared by Smith, and entitled, "Crown-Gall and Sarcoma." Receiving a copy of this in England, Dr. William Osler, Regius Professor of Medicine at Oxford, quickly expressed to Smith his considerate encouragement: "What a suggestive paper on *Crown Gall and Sarcoma!*" he exclaimed. "Most interesting in every way!" Since this communication was written on July 4, 1911, Dr. Osler might have had before him a copy of Smith's address, "Crown Gall and Sarcoma," presented on the April 13 previous before the American Association for Cancer Research, meeting that year at Buffalo, New York. Proceedings of the Association that year were published in the *Zeitschrift für Krebsforschung* of Berlin, and reprints of the substance of Smith's main points and the discussion which followed were made available.

In this presentation, Smith called attention to the subject matter of his bulletin, "Crown-Gall of Plants: Its Cause and Remedy," and to three new and added facts:

1. When a stem-tumor produces secondary tumors in a leaf, the structure of the leaf-tumor is like that of the stem.
2. In a certain proportion of the cases (I do not know yet whether in

all) there is a deep-seated strand of tumor tissue extending all the way from the primary tumor to the secondary.

3. The bacteria accompany the secondary tumors the same as the primary tumors, and apparently in about the same abundance, that is to say, scarcity.⁷⁰

Five years of experiments were required before a method was believed to have been found to stain the organism within the cell structures, that is, with the exacting technique necessary to complete elucidation. Many stains had been used. But to differentiate lignified from nonlignified tissues, the method recommended by Charles Joseph Chamberlain in his *Methods in Plant Histology* (pp. 49, 68), "a prolonged stain in methyl green followed by a short exposure to acid fuchsin,"⁷¹ had proved best. Further, by impregnating with gold chlorid, Smith, Brown, and McCulloch had been able to demonstrate "that the bacteria are not abundant in the tissues, that they occur *inside the cells but outside the nucleus*, and that Y-bodies and variously branched forms are common."

At the American Association for Cancer Research meeting, Smith exhibited both photographs and specimens, and announced that another bulletin setting forth the additional facts was being prepared. In answer to a question from Dr. Frank Burr Mallory, associate professor of pathology at Harvard University, as to whether or not he could stain the organism, Smith answered, "Perfectly well. The organism is rare however. These organisms secrete acetic acid and this causes them to quickly go into involution forms." And in answer to a question from Dr. Beebe, "Is the actual division of the cell due to stimulation by these organisms?" Smith replied, "The cells divide by mitosis and the organism is in the tissues."

If the discussion was completely abstracted, little or no definite notice was taken of Smith's announced discovery of a parenchymatous tumor strand found in inoculated Paris daisy plants connecting the primary stem tumors with deep-seated secondary

⁷⁰ *Zeitschrift für Krebsforschung* 11(1), 1911, Berlin, and reprint.

⁷¹ The structure and development of crown gall: a plant cancer, *op. cit.*, 23-24; 22. In 1914 Smith made a "Statement" before the Committee on Agriculture of the House of Representatives, which said on this point: "it required five years of experiments before we were able to find a method of staining [the organism] inside of the cells." Presumably what was seen was mitochondria. *Intro. to bact. dis. of plants, op. cit.*, 419.

tumors which had developed in the leaves some weeks after the stem inoculations. These secondary leaf-tumors were shown to be possessed of stem structure, that is, to have the structure of the primary tumor, and thus to suggest a similarity following "the law of cancer in man and animals."⁷² Dr. William Bradley Coley, famous New York physician and surgeon, told the meeting that he believed Smith's work "extremely valuable." That he adhered to this opinion was borne out by letters written years later to Smith. Dr. Coley was not only professor of clinical cancer research in Cornell University Medical School but also surgeon-in chief, consulting or attending surgeon at several hospitals, including the Memorial Hospital of New York where some of the earliest and most valuable cancer research in connection with a hospital was done in this country. For a number of years Dr. James Ewing, also a participant in the discussion and whom Smith always held in high esteem because of his sincerity of beliefs and devotion to truth, directed this cancer research. At the Buffalo meeting he stated his view that, while he found "certain aspects extremely interesting," he believed, "We are making a great mistake in calling carcinoma one and the same disease. Cancer in man is different from cancer in animals." In a letter of May 31, 1916, Dr. Coley encouraged Smith by telling him that he had sent extra copies of Smith's papers to men who he was sure were "especially interested in the subject. Most of the pathologists that I have talked with, are still very skeptical about the possibility of being able to draw an analogy between plant tumors and human tumors. Needless to say I am not in accord with such views. I am a firm believer in applying the rules of logic in the field of Medicine as well as in other fields of science." June 25, 1916, Dr. Coley again congratulated Smith "upon the convincing and lucid arguments you so ably presented [in another article by Smith entitled "Further Evidence that Crown Gall of Plants is Cancer."⁷³] I have, for 25 years, in season and out of season," he continued,

held the same views, and in spite of the advice of my friends, the pathologists, that I would lose "caste" if I published them, I have kept on expressing my opinions. Each year has brought firmer conviction of the

⁷² Erwin F. Smith, Further evidence as to the relation between crown gall and cancer, *Proc. Natl. Acad. Sciences* 2: 446, Aug. 1916.

⁷³ *Science*, n. s., 43(1121): 871-889, June 23, 1916.

truth of my opinions, with larger clinical experience. I am surprised that men who profess to monopolize the "scientific" shrine in Modern Medicine should be so unscientific as to "stand pat" upon Cohnheim's theory of the origin of cancer, a theory without any basis in clinical facts, and frown upon any attempt to learn more about the possibilities of a parasitic origin. The lack of agreement among pathologists as to the classification of tumors and the hopeless confusion that now exists in classification has already forced the clinician to lay less and less stress upon the pathological diagnosis and more and more upon the clinical. I believe you have inaugurated a new era in Cancer Research. . . . I shall do all in my power to help along researches in the lines you have so splendidly started.

Not only were the older great physicians, surgeons, and teachers of Baltimore medical circles following Smith's work with interest, but younger doctors were also being attracted to his new offerings of scientific knowledge. Dr. Thomas Stephen Cullen, Baltimore surgeon and professor of clinical gynecology at Johns Hopkins, had attended the Buffalo meeting of the American Association for Cancer Research and been so enthused by the disclosures made by Smith in his address that immediately following the meeting he had telegraphed Secretary of Agriculture Wilson and congratulated the Department of Agriculture on having so able a man. Receiving a copy of bulletin 213, "Crown-Gall of Plants: Its Cause and Remedy," he again wrote to the Secretary on October 29, 1911,

I have been greatly interested in a recent monograph appearing from your Department, namely, "Crown Gall" by Dr. Erwin F. Smith. The results in this work are truly remarkable. Recently I have had the opportunity of seeing many of the photomicrographs showing the finer structures of these tumors. This work is of such a high scientific value and the photographs he has made so clear and convincing that I take the liberty of asking you to urge him strongly to publish his results most fully and with plenty of his photographs reproduced in the best possible manner. It will not only reflect great credit on the department of agriculture but add materially to the prestige of American Scientific work. I have been particularly interested in the subject of cancer since taking charge of Gynecological Pathology at the Johns Hopkins Hospital eighteen years ago and consequently feel sure you will make no mistake in urging him in doing this. It will redound greatly to the credit of your department.

Dr. Cullen had received part of his education at the Collegiate Institute, Toronto, Canada, and in 1890 his M. B. degree from the University there. First he had specialized in abdominal surgery

and then accepted the chair of clinical gynecology at Johns Hopkins University in the capacity of visiting gynecologist of the Hospital. Smith cordially welcomed so eminent and able a conferee near him. June 20, 1911, he invited Dr. Cullen to examine some of his materials.

In working over sections of some fish, which I inoculated three years ago with *Bacterium tumefaciens* from daisy, I have come across some phenomena which I cannot interpret very well. I should like very much to have you see some of the slides. If you are over here some afternoon, can you not run in for an hour or two and look at them? My room is 301 West Wing, Department of Agriculture. . . . Some of the things I have may be simple inflammation, but other phenomena seems to me a good deal like a sarcoma. Of course, I am not saying on the strength of two series of experiments that I produced what I have got, and when I first concluded the experiments I was so much discouraged that I did not cut any sections of them for a good while. If you come over here, I could show you some very pretty slides of plant diseases.

Again by a letter of October 17 Smith invited Cullen to examine some slides and photomicrographs which were nearly finished. The result was Dr. Cullen's letter to Secretary of Agriculture Wilson.

Secretary of Agriculture Wilson had become much interested in Smith's researches. Honored and respected by all of his co-workers, Dr. Albert F. Woods had left the Department the year before to accept an appointment as Dean of the College of Agriculture of the University of Minnesota and director of the state's agricultural experiment station. For some time Dr. Galloway's health had not been strong. During the first half of the year 1911 he and Mrs. Galloway had taken a trip to Europe. Smith wrote him of his visit to Minneapolis to attend the annual meeting of the American Association for the Advancement of Science, of what progress Dean and Director Woods had already made, of what great and promising plans he had for the future, and that the next meeting of the Association was to be held in Washington. "About a month ago," he said,

I went up to Buffalo at the request of the Academy of Medicine to give a talk on crown-gall, showing some lantern slides. They paid my expenses. The crown-gall bulletin ["Crown-Gall of Plants: Its Cause and Remedy"] which has been under way so long was published at about that time. The Secretary got interested in the matter and rushed it through. I have sent a

copy of it to you at Marseilles. We are still working on the subject, because there are a number of very interesting points in connection with it which are not yet settled. I have been crazy over the subject for a half year or more, because I think it is related to malignant animal tumors in its method of growth and development. . . . There is a very interesting tumor on pine trees in south France, not very far from Marseilles. It has been reported from Coarze near Hyères, but I fancy it occurs in quite a good many localities in the Departments of the Maritime Alps and Var. It looks somewhat like Peridermium galls. There has never been any good work done on the disease. I have been anxious for years to get hold of it. I got it once from Paris, but the thing was old and dead and I could not do anything with it in the way of cultures. If you come across any tumors on pines in southern France of such description I wish you would gather a lot of them for me, especially the younger living ones. Tubeuf in Munich has recently published a paper on a bacterial gall of other species of pine which he found somewhere in Germany, and I suspect they are identical, but no one can say.

Smith, therefore, must have delivered an illustrated lecture on crown gall of plants before the Buffalo Academy of Medicine prior to addressing the American Association for Cancer Research. But at each event he had been preeminently a plant pathologist and plant bacteriologist, and for good reasons. The work in his Laboratory covered a range of investigational materials—forest and cultivated tree species, garden and farm field crops, orchard and greenhouse materials, wild plant species—in such numbers and from all parts of the world that probably no similar laboratory of the North American continent, perhaps of the world, approximated it in scope and influence.

At the Boston meeting of the Botanical Society of America in 1909 Smith had been elected president of the society for the ensuing year of 1910. This was the year when as another affiliate of the American Association for the Advancement of Science the American Phytopathological Society⁷⁴ held its first annual meeting in cooperation with the Botanical Society and Section G of the Association. Dr. F. C. Newcombe congratulated Smith. "If you live a little longer," he said, "we shall have to form a new society; otherwise the field of presidencies will be exhausted by you. But do not let this remark bother you. I know you can be just as

⁷⁴ C. L. Shear, First decade of the American Phytopathological Society, *Phytopathology* 9(4): 165-170, Apr. 1919.

good private as general, and you are all the same to us whether you are the one or the other."

Newcombe had collected funds from former pupils of Dr. Spalding to erect the memorial tablet which in the Natural Sciences building of the University of Michigan honors the work of this great teacher. Spalding, interested in applying modern investigation techniques of plant physiology to ecology, had resigned from his university professorship and become a resident investigator at the Carnegie Institution's Desert Botanical Laboratory at Tucson. Smith's continued interest in ecology was shown in 1906 when in Berlin he sought out Georg Volkens, author of a "beautiful ecological paper on the Egyptian and Arabian desert," a study which had excited his admiration and "greatest interest" and which he thought "a model in its way." In 1906 Smith had something to do with honoring an American botanist who had helped to coordinate research in plant physiology and ecology—W. F. Ganong. That year the Society for Plant Morphology and Physiology was being merged with the Botanical Society of America, and E. C. Jeffrey, retiring president of the former organization, asked Smith, on behalf of its members, to present a silver cup to Ganong in appreciation for his many years of service as their secretary-treasurer. Farlow was first asked, but neither Smith nor the first president of the organization could attend the meeting at which the presentation was made. Dr. Ganong subsequently was elected president of the Botanical Society of America, and Smith followed him in office.

In 1910 Smith, as president of the Society, arranged a symposium on plant pathology for the Minneapolis meeting of that year. As an ex-officio member of the sectional committee, he was requested by Dr. H. C. Cowles of the department of botany of the University of Chicago to suggest "topics and participants" for the program of Section G.

L. R. Jones agreed to open the discussions at the symposium on plant pathology. He had served as the first president of the American Phytopathological Society and, following him, Frank Lincoln Stevens, professor of botany and vegetable pathology at the North Carolina College of Agriculture and Mechanic Arts, was to preside at the Society's second annual meeting, also to be at Minneapolis. In 1912 Stevens was to go to the University of

Puerto Rico as dean of the college of agriculture and mechanic arts there, and in February 1914 he would become professor of plant pathology at the University of Illinois. By this time, T. J. Burrill had retired from active duties with highest honors from his university and the scientific world generally.

Jones's letter to Smith was written as professor and head of his new department of plant pathology at the University of Wisconsin. He was equipping and moving into a new laboratory, and another "small laboratory [was] in process of construction, opening into [his] new greenhouse quarters." This department was a part of the college of agriculture, and C. R. Orton and F. J. Pritchard were his assistants.

At the Minneapolis meeting the American Phytopathological Society voted to establish its official journal, *Phytopathology*, and Jones, Shear, and Whetzel became its first editors. Associate editors were G. P. Clinton, E. M. Freeman, H. T. Güssow of the Dominion Experimental Farms, Ottawa, Canada, F. D. Heald, H. Metcalf, W. A. Orton, W. M. Scott, A. D. Selby, E. F. Smith, Ralph E. Smith, and Roland Thaxter. Donald Reddick was business manager.

Plant pathologists had been appointed at more state agricultural experiment stations. Their science was a part of the course of instruction and research in botany at several universities. For years their science had been acquiring independent status and now, with their own society and journal, an established profession was more than ever evident. In America, during Erwin Smith's lifetime, workers in the science had increased from a very few to numbers in the hundreds. Already mention has been made of three university departments which were among, if not, the first established exclusively (or primarily) for instruction in plant pathology: at Cornell under Whetzel, at the University of Wisconsin under Jones, and at the University of Minnesota under Freeman. The title of the Minnesota division of plant pathology included also botany but the division was created on August 1, 1907, with work in plant pathology as the object in view and, being in the agricultural college, was distinct from the botanical department of the college of science, literature, and the arts. Freeman, a former pathologist in the office of cereals investigation at the United States Department of Agriculture, a University of

Minnesota alumnus who had taken a year's graduate work with Ward at the University of Cambridge, in addition to securing a doctorate of philosophy from his alma mater, specialized in plant pathology. In his *Outline of the History of Phytopathology*,⁷⁵ Dr. Whetzel has said, "The first distinct department of plant pathology to be established, so far as I know, was the one at Cornell University in the autumn of 1907. Shortly thereafter (1909) the one at the University of Wisconsin, with Professor Jones at its head, was announced." Whetzel, as also Jones, had taught plant pathology before their respective departments were established, Jones ever since he had gone to the University of Vermont and even more after his half year of study with Smith in 1899. When the department of plant pathology was created at Cornell University, Bailey was dean and director of the New York State College of Agriculture of which the new unit became a part. Whetzel as assistant professor, Donald Reddick as instructor, and Mortier F. Barrus as an assistant comprised the department. Whetzel immediately began consulting Smith. His first letter thanked Smith for some cultures of *B. amylovorus* and suggestions as to culture media:

I am inclined to think that so far as we are concerned we shall follow your methods of making culture media since much of the work on the cultural characters of bacteria causing plant diseases has been done in your laboratory. Moreover, I find in attempting to follow the recommendations of the American Public Health Association that they are more or less cumbersome and unsatisfactory as compared with the methods which you recommend.

I have a letter from my man Mr. Reddick saying that he has had the pleasure of meeting you, and he has no doubt told you that I expect to be in Washington next Saturday and Sunday and part of Monday. It is not just the best time to be in Washington but seems to be the only time when I can get away. I hope that I may have the pleasure of meeting you and seeing your laboratories.

Before going to Cornell University in 1902 to be an assistant in George F. Atkinson's department of botany in the college of arts and science, Whetzel had earned two degrees at Wabash College, Crawfordsville, Indiana. His professor in botany had been Mason B. Thomas who was also dean of the faculty there

⁷⁵ *Op. cit.*, 109.

and who on March 6, 1911, also wrote to Smith congratulating him

on the great work on Crown Gall which ha[d] just come to [him] from [Smith's] laboratory. I hope you will know how much botanists in general value your convincing solution of this problem but better than this I want you to know how valuable it will be to teachers, as an example to their students who are preparing for the profession of botany. The influence of this is more than you can imagine. I hope to see you at Woods Hole next summer.

October 18, 1910, Carlton C. Curtis of the department of botany of Columbia University informed of his "desire to establish [there] within the next year a course on straight plant pathology" and asked Smith to send plant disease materials, particularly of "many of the common bacterial diseases." His second paragraph was of interest:

You must feel very much encouraged that so great an interest is being developed in your line of work. I have not had an opportunity to carefully examine Stevens' and [J. G.] Hall's [*Diseases of economic plants*⁷⁶], but I feel that this work and Duggar's work [*Fungous Diseases of Plants*] are bound to give great interest to the work in this country.

He concluded by telling how much they used his book, *Bacteria in Relation to Plant Diseases*—"you must take great satisfaction in contemplating this splendid piece of work," he added. Within another year and a half, in May, 1912, there came to Smith a more positive suggestion from Bessey of the University of Nebraska.

I have had it in mind a number of times, and yesterday I was urged by one of my assistants—therefore I will do it—that is, I will tell you that it has seemed to me for a good while that *you should prepare a textbook on Plant Pathology*. I think I told you this once before, but I wish to say it again still more emphatically. No man in the country I think is as able to prepare such a textbook as you are. I mean the *right kind* of a textbook. Almost anybody can write a book in which he enumerates the fungi or other things that bother plants, and in this way he can bring together a great deal of material and issue it as a book on Plant Pathology, but you have taught us in the two books on bacteria in relation to plant diseases that there is a better and higher ideal to be attained in Plant Pathology.

⁷⁶ Published by the Macmillan Company, 1910. In the *Botanical Gazette* 52: 155, July-Dec., 1911, this was reviewed together with George Masee's *Diseases of cultivated plants and trees*, also published in 1910 by the Macmillan Company.

Why can't you give us a textbook? Perhaps the best thing would be to give us an elementary work at first, so as to teach us all what Plant Pathology actually is or should be, and then give us finally another one which goes to the very bottom of things as you are able to see them.

An instructor of plant pathology at the university had told Bessey that Smith's two volumes were "the best ever published on plant diseases of any kind."

In December 1911, in the first volume of *Phytopathology*,⁷⁷ L. R. Jones had reviewed Smith's second volume on *Bacteria in Relation to Plant Diseases* and concluded that "Certainly nothing comparable to this—as fundamental work in plant pathology has appeared in this generation." The following year, also in *Phytopathology*,⁷⁸ appeared an article by H. A. Harding of the New York Agricultural Experiment Station at Geneva, "The Trend of Investigation in Plant Pathology," in which was stated:

If there is a doubt in the mind of anyone as to the objective point toward which the development of plant pathology is tending let him read the second volume of Erwin F. Smith's "Bacteria in Relation to Plant Diseases." There he will find two hundred pages devoted to the presentation of the science of plant pathology and its form of presentation is almost identical with that current in animal pathology. Whenever this is possible the terminology of animal pathology is applied to the corresponding tissue changes in plants. It is entirely natural that this growing correlation between animal and plant pathology should be most evident in the treatment of bacterial diseases since many of the bacteriologists, in the nature of the case, have come into closer touch with animal pathology than have the students of fungi. . . .

The present trend of investigation in plant pathology may be characterized as an effort to follow animal pathology in recognizing the host as the focal point of study and to consider the relation of the host to its environment in a comprehensive way.

On December 28, 1911, when Smith gave his address as retiring president of the Botanical Society of America, "Some Resemblances of Crown-Gall to Human Cancer,"⁷⁹ members of other affiliate organizations of the American Association for the Advancement of Science—Section G, the Society of American Bacteriologists, and the American Phytopathological Society—were present by invitation. He had been pleased with the accomplishments of

⁷⁷ 1(6): 204, Dec. 1911.

⁷⁸ 2(4): 161-163, Aug. 1912.

⁷⁹ *Science*, n. s., 35(892): 161-172, Feb. 2, 1912.

the Minneapolis meetings the year before, especially that between fifty and seventy botanists were in attendance. The charter membership of the American Phytopathological Society numbering about one hundred and thirty plant pathologists now exceeded two hundred. That Dr. Farlow "received twice as many votes for president (of the Botanical Society of America) as anyone else" also pleased Smith. Programs of three societies of botanists—Section G, the Botanical Society of America, and the American Phytopathological Society—was "*an embarrass de richesses*," he jubilated to Thaxter. Smith had two leading articles in the first issue of *Phytopathology*, one on Anton de Bary and the other on "Crown Gall of Plants," and the next year he would contribute several more to the journal, one on "Woronin," another leading article.

In 1911 Peyton Rous published in the *Journal of the American Medical Association*⁸⁰ upon the "Transmission of a malignant new growth by means of a cell-free filtrate." Up to this time, pathologists had been agreed

that only by means of living cells can neoplasms be transferred from one animal to another. This observation discouraged completely all idea of a virus, for if one did exist it should be separable from the cells under suitable conditions and tumors should therefore be transmissible with filtrates. But all such attempts failed until 1911, when Rous transferred in this way a sarcoma of the breast muscles in a Plymouth Rock hen . . . Rous's first papers aroused little interest, and as the infection hypothesis had always been vigorously opposed the general attitude was one of skepticism.⁸¹

At first, Smith's "idea that crown gall of plants resembles malignant human tumors and [could] be made to throw a flood of light on the origin of the latter [had] received only a cool welcome."⁸²

But by 1911, when Smith, Brown, and Townsend's bulletin 213, "Crown-gall of plants: its cause and remedy," was published, the theory began to receive "respectful attention." One reason appears to have been a revival of interest in the parasitic theory and virus hypothesis of the origin of cancer. Smith recognized

⁸⁰ 56: 198.

⁸¹ Charles Oberling, M.D. (translated by William H. Woglom, M.D.), *The riddle of cancer*, 130, New Haven, Yale Univ. Press, 1944.

⁸² E. F. Smith, N. A. Brown, L. McCulloch, The structure and development of crown gall: a plant cancer, Bull. 255, U. S. D. A. Bur. of Pl. Ind., 11-12.

at once the significance of Rous's research achievement, especially after he read in the *Journal of Experimental Medicine* what seemed "ample proofs of this contention" and still more so after April 4, 1912, when he heard Rous address the American Association for Cancer Research.⁸³ Smith, in his address "On some resemblances of Crown-gall to Human Cancer," gave his estimate of the value of Rous's discovery. Further, in his introduction to his, Brown, and McCulloch's bulletin 255 on "The structure and development of crown gall: a plant cancer," he listed this as first of three points which "tended strongly to unsettle the crystallizing belief in the nonparasitic origin of cancer."

In his address, he said:

Having found no parasite in the cancer cells, a majority of the animal pathologists have given up the idea that cancer can be of parasitic origin. For a generation the research workers fell back upon Cohnheim's hypothesis that cancers were due to the development of small fragments of tissue cut off from the parent layer during embryonal growth, to be enclosed in other tissue and lie dormant until acted on abnormally later in life by some unknown stimulus. But while studies of the animal body show that such separation of small portions of tissue from the germinal layer is not uncommon, research workers on cancer are now generally agreed, I believe, that there are many phenomena connected with the development of cancer for which this hypothesis of Cohnheim offers a wholly inadequate explanation. Moreover, what induces these dormant cells to develop was never determined. A very favorite theory with cancer specialists has been that the cancer cell is the only parasite, and that no infections could be obtained on animals unless the living cancer cell were present. This hypothesis must now be abandoned owing to the discovery by Peyton Rous (1911) that sarcoma of chickens may be produced in the absence of cancer cells, *i. e.*, by cancerous fluid filtered free from all traces of living cancer cells. So far as I know he has not expressed any opinion as to the nature of the infection which has been separated from his ground chicken sarcomata by centrifuging and also by filtration through Berkefeld bougies, but in the light of the evidence we have secured from plants I believe you will agree with me that it can be nothing else than a living microorganism, minute enough to pass through the walls of the rather coarse filter.⁸⁴

Francis Peyton Rous, Baltimore-born and educated at Johns Hopkins University and Medical School, during 1905-1906 had been resident house officer of Johns Hopkins Hospital and then

⁸³ *Idem*, 11.

⁸⁴ On some resemblances of crown-gall to human cancer, *op. cit.*, reprint, 4.

become an instructor in pathology at the University of Michigan. By 1912 he was an associate member of the research personnel of the Rockefeller Institute for Medical Research. After Smith had heard his address at the American Association for Cancer Research, he sent Rous a letter of congratulations:

I found your paper of tremendous interest and wish to congratulate you on the beautiful results you are obtaining. Whatever people may have said in the past about the possibility of some living cells of the chicken sarcoma passing through the filter or escaping into the fluid which has been centrifuged, there can be no dodging the fact that the tumor tissue dried for six months is as dead as a door nail.⁸⁵

To Smith, Rous had not only shown that "a chicken sarcoma is inoculable in the absence of living chicken cells, i. e., with fluid freed from the ground sarcoma by centrifuging, and also by filtration through moderately coarse Berkefeld bougies," but further that "tumor material dried for six months is still infectious."⁸⁶

Smith recapitulated in his address of 1912 the evidence bearing out his resemblance theory of crown galls and malignant animal tumors. In crown gall were to be found:

the cell itself a disturbing force, i. e., an enormous multiplication of certain cells of the body without reference to physiological needs and in opposition to the best interests of the organism; a non-capsulate tumor, with absence of the absciss cavities and of plainly visible parasites; peripheral growth and a well-developed stroma consisting of vessels and fibers; from this primary tumor the development of strands of tumor tissue upon which secondary tumors develop; in the secondary tumors a strong tendency to take on the structure of the organ in which the primary tumor has developed; frequent if not necessary origin of the primary tumor in bruises, wounds, or irritated places; complete recovery if all the tumor tissue is extirpated, failure if it is not; in some cases spontaneous recovery. The chief difference so far made out is that in case of cancer cells we know nothing whatever as to the cause of the abnormal growth, whereas in case of these overgrowths on plants we have definitely proved them to be due to the presence of an intracellular schizomycete which we have many times

⁸⁵ Written May 16, 1912, after the cancer research association's Philadelphia meeting. University of Pennsylvania Medical School. *Zeitschr. für Krebsforschung* 12: 430 ff., 1912. Rous, *et al's* paper, 430-431; Smith's paper, 435-436.

⁸⁶ The structure and development of crown gall, *op. cit.*, 11, introduction. A decade later, Smith again reviewed the work of Rous and his co-workers on Filterable virus chicken sarcomas. For this purpose, he read twenty-five of his papers. See, Twentieth century advances in cancer research, by Smith, *Jour. Radiology* 4(9), pt. III: 301-303, Sept. 1923.

isolated and reisolated in pure culture and by means of which we can produce the disease at will.

Again he answered the reply of certain cancer specialists that crown galls are evidently a granuloma and not a true tumor, citing in his discussion tuberculosis and syphilitic gummata as instances of granulomata and differentiating malignant animal and human tumors. In part, the difference was, as he viewed the problem, that in granulomata the parasite migrates whereas in cancers the cancer cell itself migrates, that is, "some of the body cells which under some unknown stimulation have been taken out of the physiological control of the body and have become thus, as it were, parasites on their fellow cells." In plants there is no rapid blood stream, such as animals have. How, then, was the tumor strand and its propagation to be explained? The strand had been discovered in the inner wood next to the pith between a secondary and a primary tumor and near the primary tumor, and, further, it was found to be not merely local but traceable for some distance and occurring with regularity in the normal tissue between the primary and secondary tumors. Smith exhibited lantern slides showing cross and longitudinal sections of such strands from inoculated plants. On this strand had been found developed secondary tumors. "In the Paris daisy, when the primary tumor is on the stem," he explained,

secondary tumors often develop on the leaves, and strands of tumor tissue have been traced in numerous instances all the way from the primary tumors through the stem into the leaf, and all stages of the development of the secondary tumors observed on many plants. This tumor strand boring its way through stems and leaves appears to be as much a foreign body as the roots of a mistletoe or the mycelium of a fungus. From these strands and from these secondary tumors we have isolated the same micro-organism that occurs in the primary tumors and with subcultures from such bacterial colonies have reproduced the disease. The discovery of this strand affords a satisfactory explanation for the fact that the morbid growth usually returns after excision.

The second striking fact to which I wish to call your attention is that when the primary tumor occurs in the stem and the secondary tumor in the leaf the structure of the secondary tumor is not that of the leaf in which it is growing, but of the stem from which the strand was derived.

The *Bulletin de l'Institut Pasteur* reviewed this address, and on June 22, 1912, Smith thanked Dr. Weinberg. He promised to send

a copy of his bulletin 255 which contained photomicrographs illustrating and confirming his statements. Since the address he, Brown, and McCulloch had stained,⁸⁷ they believed, the organism within the cells. Of the "intracellular bacteria," he added,

They are not very numerous, and have the same forms as in our flask cultures, growing under unfavorable conditions, i. e., they are club-shaped, y-shaped, and variously branched, just as they are in the old flasks, and in young test-tube cultures when exposed to dilute acetic acid, where also they are either dead or develop slowly on agar plates just as they do in a large proportion of the cases when plated out of the tumors.

In his address he had pointed out that,

Various researchers on cancer have mentioned finding rod-shaped and Y-shaped bodies in cancer cells. For example, Dr. Borrell, of the Pasteur Institute in Paris, and Dr. Reese, working in the cancer laboratory at Buffalo.

In 1912 the Ambassador of the government of France invited the United States Department of Agriculture to send Smith to Paris to address the first International Congress of Comparative Pathology. He did not attend the congress, which convened October 17-23, but sent an address which was presented, "Le Cancer est-il Une Maladie du Règne Végétal?"⁸⁸ Dr. Borrel addressed the meeting on "Le Cancer" and recognized Smith's crown gall research as etiologically significant in the problem of cancer origin. Borrel, the proponent of the ingenious hypothesis of the carcinogenic virus as the cause of cancer in animals and perhaps man, said of Smith's work on "cancer" of plants: "Voilà le premier cas d'un cancer véritable répondant à la définition du cancer et causé par l'inoculation directe d'un microbe; une tumeur de la betterave inoculable à la betterave par greffe et inoculable à la betterave et à beaucoup d'autres végétaux par des cultures."

Dr. Charles Oberling, chief of the Experimental Department of the Cancer Institute in Paris, in his recent book, *The Riddle of Cancer*,⁸⁹ describes the "classic infection hypothesis" as "imagi-

⁸⁷ Erwin F. Smith, The staining of *Bacterium tumefaciens* in tissue, *Phytopathology* 2(3): 127, June 1912.

⁸⁸ *Compt. Rend. 1er Congr. Internat. Patb. Comparée* (17-23 Oct., 1912) 2: 984-1002, 1914. Also, reprint, Paris, 1912, 19 pp. Borrel's address, Smith's quotation therefrom: *1er Cong. Int. Patb. comparée*, 1(2): 640-641.

⁸⁹ Lectures, Cancer Institute of the Fac. of Med. of Paris and Fac. of Med. of Teheran, transl. by Dr. W. H. Woglom, 67, New Haven, Yale Univ. Press, 1944.

ning" the cause of cancer to be "a microbe or protozoon or, in other words, an organism of very small size in comparison with the cell that it was supposed to infect." He says,

The possibility of a larger parasite, belonging to a higher species, was never seriously taken into consideration and it is not surprising, therefore, that Borrel's first communication on a sarcoma in the wall of a parasitic cyst of the rat's liver should have fallen flat. But no neglect could dampen his ardor, and with all his characteristic enthusiasm he threw himself into a search for parasites in tumors. Other rat sarcomas were discovered, identical with the first and all in the walls of cysts enclosing a *Cysticercus fasciolaris*, the larval form of a cat tapeworm, *Taenia crassicolis*. Acarids, or mites, were found in the milk ducts about mammary cancers in mice and another, *Demodex folliculorum*, a common inhabitant of the hair follicles and sebaceous glands, around, or even within, cancers of the skin in man.

For Borrel there was now no shadow of doubt whatever; to him the intervention of a parasite was obvious, though he did not assign it a direct role in causation. Cancer being due to a virus, an opinion that Borrel never relinquished, the parasite must act merely as its inoculator. Chance bearer of a carcinogenic virus, threading its way through the tissues, it infects in its progress whatever cells happen to lie in its path.

This brilliant hypothesis had the misfortune to be announced at a time when it still lacked any scientific foundation and the idea of a carcinogenic virus seemed the purest fantasy, for although it was supported by an observation or two here and there these could easily be explained away as coincidences; no experimental demonstration had yet been provided. Then, too, the role of *Demodex* in cancer of the skin seemed more than problematical, for the parasite occurred with equal frequency in cancers and in normal skin.

Discredited from the first by this medley of findings, true enough, to be sure, but unverified, and by more than questionable inferences, the parasitic hypothesis had little to commend it. It fell into complete oblivion, therefore, from which it could be rescued only by such dramatic testimony as that of Fibiger.

In 1913 Professor Johannes Fibiger, pathological anatomist and director of the Universitetets patologisk-anatomiske Institut of Copenhagen, after years of unwearied research, was to announce that by feeding nematode-infested cock-roaches to rats he had secured several cases of cancer,⁹⁰ at least changes in the stomachs of rats which appeared to be "true cancerous growths." The nematodes had been taken from muscles of a cock-roach: and the

⁹⁰ See a statement in English by Fibiger concerning his early work in Smith's address, Twentieth century advances in cancer research, *op. cit.*, 303-305. Quotations of this paragraph from this.

studies continuing and more cancers resulting, Smith read his "lucid papers . . . as fascinating as a romance," "By these investigations," Fibiger claimed in 1913, "carcinomatous tumours giving rise to metastases were produced experimentally for the first time, and the hypothesis put forward by Borrel and Haaland was thus verified, as it has now been proved that nematodes play a causal part in the development of cancer in rats."

In 1912 in his address, "Le Cancer,"⁹¹ before the International Congress of Comparative Pathology, Borrell considered in support of the parasitic hypothesis of cancer his own researches and conclusions, Rous's transmission of sarcoma in chickens by direct inoculation with a cell-free or virus filtrate, and Jensen's confirmatory work of Smith's main conclusions of the comparison of crown gall and malignant tumor formation in animals. The next year Smith, in an address⁹² before the seventeenth International Congress of Medicine, would quote from Borrel's estimate of his work and the intimation that he, Smith, had been "the first one to produce a cancer by inoculation with pure cultures of a micro-organism, and the first one to furnish proof that it is actually a symbiosis."

On November 23, 1912, Dr. Louis B. Wilson, director of the Laboratories of the Mayo Clinic, invited Smith, on the behalf of a committee, "to have at the Scientific Exhibit of the American Medical Association in Minneapolis next June as large a number as possible of pieces of research which shall be demonstrated by the men who have done the work. We will have," he explained,

the exclusive use of the new building of the Department of Anatomy, which contains an amphitheatre and two large lecture rooms besides numerous small rooms. We shall place the exhibits around in the smaller rooms (all of which are connected) and provide for a regular program of lantern slide demonstrations in the lecture rooms. Last year at Atlantic City we did something of the sort and the men who were on the program were much pleased with it. Several of them, in fact, said it was the best opportunity they had had to present their material to the sort of an audience that they most desired to reach. Besides general research, I am particularly interested in that on cancer and should like if possible to have an "exhibit

⁹¹ See an excerpt of three paragraphs from Borrel's paper in Smith's address, *Twentieth century advances in cancer research*, *op. cit.*, 299.

⁹² Cancer in plants, *Proc. 17th Internat. Cong. of Med.*, Section III, General Pathology and Pathological Anatomy, reprint, 18. Citing Borrel's paper, *Le cancer, 1er Cong. Int. Path. comparée* 1(2): 640-641.

symposium" on the most recent work. Can you not bring up enough material to illustrate thoroughly your studies on tumors of plants and, perhaps with an assistant from your laboratory, be on hand to demonstrate them? We will provide space and light, plenty of microscopes, a projection lantern and give you as much time as may be necessary for a lantern slide demonstration in an audience room that will seat one hundred people.

At the Mayo Clinic, Dr. MacCarty had reported at a hospital staff meeting on Smith's recent discoveries. So interested was Dr. William J. Mayo that he wrote both Smith and Dr. Cullen. He told the latter: "I will be glad to help Dr. Smith in any way that I can and would like to see him have an opportunity to develop this wonderful work he is doing." He read some of Smith's papers and wrote him: "I will be very glad indeed to do anything I can to help out with this splendid work you are doing."

Smith attended the Minneapolis meeting of the American Medical Association, evidently as planned presented his lecture and demonstration, and was given a Certificate of Merit in recognition of the value of his work on crown gall in relation to human cancer.⁹³

Just before this meeting, he had been advised of another extraordinary honor. On April 24, 1913, Arnold Hague, home secretary of the National Academy of Sciences, notified him of his election to membership in the Academy. His election had taken place on the occasion of the Academy's semi-centennial anniversary which was held in Washington from April 22 to 24 of that year. One hundred and twenty-five scientists, ten or twelve of them botanists, were now members, recipients of this highly coveted distinction. The botanists were D. H. Campbell, J. M. Coulter, Farlow, Goodale, Harper, E. W. Hilgard, Sargent, Thaxter, Trelease, David White, and Smith.⁹⁴ DeVries and Pfeffer were among the foreign associates. Many of Smith's friends from the medical profession were members: Billings, Councilman, Flexner, Prudden, Mall, Ira Remsen, Theobald Smith, and Welch among them, and such eminent scholars in biology as Conklin and Jacques Loeb.

In the year 1913 the Academy's semi-centennial celebration took place and Frederick W. True's *History of the First Half-Century of the National Academy* was published. Erwin Smith was elected

⁹³ *Phytopathology* 3(3): 253.

⁹⁴ *Idem*, 194. White was known as a geologist but he was also a paleobotanist. Hilgard was an agriculturist but he was also a botanist.

as a plant pathologist and his candidacy was sponsored by Farlow who addressed to the members his already quoted letter. Home Secretary Hague announced to Smith his election to membership as an expression of the Academy's "high appreciation of [his] services to science." For three years the honored plant pathologist served as chairman of its botanical section.

Another honor may have been Smith's this year. Neither he nor any official of the Department of Agriculture announced this. But in May, 1913, newspaper accounts of the country disclosed that at a salary of \$10,000 a year he was offered a position with the Rockefeller Institute for Medical Research.

In April 1913 Dr. Simon Flexner of the Rockefeller Institute contemplated appointing a plant pathologist in his department of the laboratories for the year 1913-1914. Minutes of the Board of Trustees of the Institute confirm this, and also that in 1920 the matter of forming a department of plant pathology was considered, each time Dr. Flexner consulting Dr. Smith. The latter's diaries of later years reveal that at some time an offer was made to him and that for the same reason he had refused other offers to remain with his Laboratory of Plant Pathology he declined this. This offer could have been either in 1913 or 1920, perhaps both, but as to the year 1913, the matter was settled by an exchange of letters in June. On June 30, Dr. Flexner advised Smith that the staff of the Institute was made up for the year 1913-1914 but that he would "probably have to call upon [him] again." None of the correspondence confirms the \$10,000 figure as to salary mentioned in the newspaper article published over the country.

Smith may have also been considered for a professorship at Harvard University. In 1909 he appears to have had a conference with Dean Wallace C. Sabine of the Graduate School of Applied Science concerning a teaching appointment and perhaps to have recommended Dr. L. R. Jones for the position. Smith wrote Jones who answered on April 13 "that if their school is to accomplish the things that they wish in the large way you are the one man essential to them. If then, they can afford two men I believe I could help in certain ways."

Harvard's courses and laboratory research in all branches of cryptogamic botany had maintained under Thaxter and various assistants the standards of excellence set by Farlow during his

years of active teaching from 1874 until about 1896. The valuable *Contributions from the Cryptogamic Laboratory of Harvard University* had reached by 1896 thirty-seven, with many unnumbered,⁹⁵ papers and many of them were of fundamental importance to plant pathology and its allied branches of science. By 1909 the number of *Contributions* had been increased to sixty-five papers and the list of graduates from the university and laboratory, or who had done special work there, included such men as G. F. Atkinson, C. E. Bessey, E. A. Burt, G. P. Clinton, B. M. Duggar, J. H. Faull, J. E. Humphrey, J. R. Johnston, G. R. Lyman, L. H. Pammel, L. W. Riddle, J. B. Rorer, F. C. Stewart, W. C. Sturgis, R. Thaxter, W. Trelease, L. M. Underwood, and H. von Schrenk. About this time H. P. Barss and H. S. Jackson did work there. Most of these men became important figures in plant pathology in America, and from this laboratory had graduated such very important algologists as W. A. Setchell who graduated also from Yale University under Daniel Cady Eaton, and Kingo Miyabe of Japan. Furthermore, such noted American botanists, leaders in the fields of physiology, genetics, cytology, taxonomy, or other branches, A. F. Blakeslee, B. M. Davis, G. T. Moore, G. J. Peirce, B. L. Robinson, J. J. Wolfe, and others, had taken their undergraduate and/or graduate work there, at least in part. Bruce Fink and others had studied lichenology with Farlow, and among Farlow's and Thaxter's many correspondents from all parts of the world were many leading students of plant pathology, physiology, cytology, and genetics, as well as the outstanding mycologists, algologists, bryologists, and students of other phases of research among the cryptogamia. Pertinent also is the fact that among their many other students were men who went into zoology, biology, and medicine.

At Harvard in 1909 W. J. V. Osterhout was appointed an assistant professor of botany and assumed mainly the work in plant physiology formerly taught by Dr. Goodale. Evidently an expanded program of teaching and research at the Bussey Institution, one to include experimental investigations in plant pathology, was contemplated but did not materialize perhaps because

⁹⁵ W. G. Farlow, *A sketch of cryptogamic botany in Harvard University, 1874-1896*, printed pamphlet, 16 pages; also, a valuable article on the work of the department of cryptogamic botany under Farlow and Thaxter, W. H. Weston, Jr., *Dr. Farlow's influence on mycology*, *Farlowia* 2(1): 85-95.

Jones went to the University of Wisconsin and Smith's ambition was not to teach but remain with the Department of Agriculture. E. M. East was appointed assistant professor of experimental plant morphology and began his noted work there in genetics. But that Dean Sabine was interested in Smith's work seems undoubted. On July 11, 1912, after he had read the latter's bulletin 255 on "The structure and development of crown gall," he wrote: "Your comparison of the Crown Gall to Sarcoma and animal tumors has interested me for several years past. I am very glad indeed to have this exact statement of the parallelism." Furthermore, Dr. Milton Joseph Rosenau of the department of preventive medicine and hygiene of the medical school congratulated Smith and his co-workers on July 31 on "this splendid piece of work."

By 1912 Jones's department at the University of Wisconsin consisted of five persons: himself, I. E. Melhus, Freda M. Bachman, A. G. Johnson, and R. E. Vaughan. On July 11 he wrote Smith:

Again I must congratulate you upon your Bulletin 255. I need add nothing to what I have said before in order to assure you that I am convinced that you are, in these contributions on crown gall, not only giving a stimulus which must be powerful in its influence upon human cancer research, but pointing to the highest plane yet attained in Plant Pathology. I wish you continued success along these lines for the sake of our science as well as for your own satisfaction.

American botanists were uniform in their high praise of Smith's work. Professor Whetzel—his department at the New York State College of Agriculture now composed of two professors, one assistant professor, four instructors, one investigator, five assistants, and seven fellows—called bulletin 255 "certainly a crowning contribution to [Smith's] many wonderful studies of this disease. While I fear," he wrote to Smith on July 15, 1912,

that the edition is so limited that I can hardly hope that you can help me out in my request, still I shall appreciate it very much if in any way you can make it possible for me to get ten or twelve copies of this for our classes in Plant Pathology. I am planning to give a course in Pathological Histology next winter and this would be one of the best things I could put in the hands of my students as a model of accurate work along this line.

Walter G. Sackett, bacteriologist of the Laboratory of Bacteri-

ology of the Colorado Agriculture Experiment Station, on the very next day styled the bulletin "certainly a most inspiring treatise, and I," he confided to Smith, "am sure that all plant pathologists envy you the success which you have achieved along this particular line. You have opened up a most suggestive field along the line of cancers and tumors, and I sincerely hope that you will be able to connect up the relation between plant and animal Carcinomas in the very near future." Professor T. D. Beckwith of the department of bacteriology of Oregon Agricultural College and Experiment Station characterized the publication as "magnificent"; Hermann von Schrenk, "a very extraordinary contribution" and he sent his "very best felicitations"; and P. J. O'Gara, now pathologist and entomologist for Rogue River Valley, Medford, Oregon, another scientist who when with the Department of Agriculture had done some work on crown gall, confessed in 1911 that if he ever returned to the Department Smith's Laboratory would "probably hold out the greatest inducement." When B. D. Halsted had read Smith's bulletin 213 and his article on crown gall in *Phytopathology*, by a letter of March 6, 1911, he heartily congratulated Smith "upon reaching, through many years of splendid research work, the clear-cut conclusions set forth in the bulletin. It must be a great satisfaction to be able to so completely settle many of the points of greatest importance concerning the Crown-Galls. If there is more to be done with them," he said complimentarily, "and doubtless you see more unsolved problems than any one else,—the foundations of fact have been so securely laid and the methods so fully worked out, that others can attack them in a rational manner." Halsted, too, had at one time written on crown gall.

Many other botanists and bacteriologists congratulated Smith. David Fairchild arranged for Smith to prepare for the *National Geographic Magazine*⁹⁶ an article on "The discovery of cancer in plants. An account of some remarkable experiments by the U. S. Department of Agriculture." Fairchild wrote to Smith, "I want to congratulate you on the production of such a remarkable piece of work. It has taken a great deal of your life but I am sure the world will be glad to honor you for what you have done."

⁹⁶ 24(1): 53-70, 12 pls., Jan. 1913.

The world's senior plant bacteriologist, Thomas Jonathan Burrill, reading the second volume of Smith's *Bacteria in Relation to Plant Disease*, addressed two letters to Smith within the month of February 1912. "I cannot express in words my appreciation of the way you have done this work—let me say the admiration I have for the notable accomplishment," he said on February 17. "I know nothing anywhere like it in botanical literature that can be called its equal. We will wait now with great expectations for what is further to appear." And on February 9 he had written, "Permit me to say that I am very greatly interested in your revelations concerning the similarity of crown gall and cancer in the animal body."

Dr. William Osler found the second volume of *Bacteria in Relation to Plant Diseases* of "the greatest interest," so much so that he passed it on to a very close and "great friend" in the Agricultural Department of Oxford. "What an extraordinary amount of work it represents," he uttered with characteristic spontaneity, "and in such a new and encouraging field. If you ever come over here, let us see you."

Dr. Adami of McGill University on May 22, 1912, sent Smith, "Heartiest congratulations over your continued good work with the Crown-gall organism." Reading Smith's *The Structure and Development of Crown Gall*, he rejoined, "It is a triumph to be able to demonstrate it within the cells."

Dr. Edwin O. Jordan, professor of bacteriology at the University of Chicago, in 1912 rated the whole work in crown gall as "one of the most important and fundamental contributions of recent years, and," he told Smith, "I heartily congratulate you on your really splendid success and on the far-reaching results that I think will flow from these discoveries."

"Please accept my most sincere thanks for this valuable contribution to the etiology of malignant tumors—Bulletin No. 255—which I have just received," wrote A. F. Coca of the department of experimental pathology, Loomis Laboratory, of Cornell University Medical College on July 8, 1912. "The plates are wonderful. It is a handsome piece of work. I heartily congratulate you." Coca had obtained his degree of doctor of medicine from the University of Pennsylvania, had studied at the University of Heidelberg, Germany, spending two of the years 1905-1909 at the Cancer

Institute there, and in 1910 become an instructor in pathology and bacteriology at Cornell. June 30, 1911, Dr. Coca had congratulated Smith on three other publications and exclaimed, "Who would have looked for studies on Sarcoma and Bacillus Coli from the Department of Agriculture. Please accept my very warm thanks for these interesting and important works." Moreover, R. Weil of the department of experimental medicine and of the same laboratory and medical college planned, he wrote Smith, to "undertake the inoculation of your plant tumor cultures into various species of animals," and asked Smith for culture material if Smith wished this done. Other medical laboratories similarly sent for culture materials. Also a number of prominent medical doctors.

Not only medical colleges and doctors of eminence sent interesting letters, but also public health laboratories, biological laboratories of private companies, and scientific research institutes public and private the world over from Norway and Canada on the north, Brazil and Argentina on the south in the western hemisphere and the Union of South Africa and Mysore, India, in the eastern hemisphere. M. A. Barber of the Bureau of Science of the Philippine Islands reviewed the work before its Science Club, and the university medical school. On September 28, 1912, the *British Medical Journal*⁹⁷ recognized the work, not in complete agreement with Smith's claims, but with full acknowledgment of its value as "thoroughly good" research stimulating to other workers to "enter upon this field of investigation." During 1912 William B. Brierley, lecturer on economic botany at the University of Manchester, whose principal work and interest was the study of plant diseases, wrote he considered it a privilege and pleasure to come in contact, "however remote, with one to whom the Science of Plant Pathology is so deeply indebted and for whose Scientific work I and all botanists have the greatest respect and admiration." "Those [papers and bulletins] dealing with Crown Gall and Human Cancer," Brierley wrote in his second letter of 1912,

I found of such extreme interest, that I presented them before a meeting of Botanical Teaching Staff and Researchers today. Dr. C. Powell White

⁹⁷ P. 811.

who at present is working in the Cancer Research Lab[orator]y in the University was also present. Text and the beautiful series of plates were discussed in some detail, our seminar lasting well over three hours.

Many research workers from various countries sent for experimental materials. One was M. Haaland, who for some years had worked at the Imperial Cancer Research Fund of England and was now with the pathological institute of Bergen, Norway. He had been shown by Jensen beet-root tumors produced by a daisy strain of *Bacterium tumefaciens* from Smith's laboratory. He wished to make a study of the subject, sent for cultures, and promised to review Smith's Bulletin 255, "The Structure and Development of Crown Gall: a Plant Cancer," in one of the medical journals of Norway.

CHAPTER X

SECOND EUROPEAN JOURNEY. INTERNATIONAL CONGRESS OF MEDICINE. FURTHER RESEARCH ON PLANT DISEASES, ESPECIALLY THE PLANT TUMOR AND VARIOUS COORDINATE PROBLEMS.

WHEREAS Smith's first journey to Europe in 1906 had been to study and gather materials to complete his second volume of *Bacteria in Relation to Plant Diseases*, his second journey in 1913 was planned with the same purposes in mind for the third volume of this epoch making work. Another reason was to deliver at London his address, "Cancer in Plants," before the seventeenth International Congress of Medicine to be held in August 1913. This was regarded by him as an "English equivalent" of his address of the year previous at the first International Congress of Comparative Pathology held at Paris. He had not been present at the Paris Congress, and so far as is known no exhibits were sent over with his paper. Since that occasion, however, he had prepared an elaborate exhibit of his crown gall materials for the Minneapolis meeting of the American Medical Association and substantially the same together with some new demonstration exhibits were taken with him to London. Sunday, August 17, from the Charing Cross Hotel, he wrote to his Laboratory co-workers:

My dear friends. It is eight days and sixteen hours from New York to London via Dover on S. S. "Lapland." . . . The sea was calm as an old pond all the way. There was singing and dancing and card playing and the usual ship amusements and toward the end of the trip I became acquainted with many of the passengers. This had its inconveniences for I then had to hide away to get any reading or writing done. Some of the persons I met proved very pleasant, e. g. Dr. and Mrs. [Lewellys Franklin] Barker, Dr. [Harvey] Cushing, Dr. and Mrs. Adolph Meyer, Dr. and Mrs. [John Alden] Lichty, and the charming young girl Dorothy, their daughter, Dr. Dana, Dr. Bosworth, Dr. Cunningham, Dr. [George Linius] Streeter, Dr. and Mrs. Foster. Later I met Dr. and Mrs. Murphy. All from Baltimore, New York, or Boston except the Lichtys who are from Pittsburg and the Murphys from Chicago(?) Dr. M[urphy] is the big surgeon who said, "We are all proud of you." There were twentyone doctors on the ship, most of whom I met and all of whom asked to see my exhibit.

That Smith made favorable impressions on these leaders in medical science even before they heard his address was further shown by letters later received. For instance, on May 25, 1916, Dr. Harvey Cushing, surgeon-in-chief of the Peter Bent Brigham Hospital, Boston, and famed as one of the great surgeons of all time on the brain, congratulated Smith, not as a friend of Dr. William Osler, of whom Dr. Cushing was later his authorized biographer, but because of his own estimate of the value of Smith's contribution to pathology. "I cannot tell you how pleased I am," he wrote, "to have received your papers on your most admirable studies. I remember with the greatest pleasure crossing with you at the time of the Congress in 1913, and have always hoped that I might have an opportunity of calling on you in Washington." Dr. Charles Loomis Dana was professor of nervous diseases at Cornell Medical College, and author of a textbook on nervous diseases and psychiatry. Dr. Adolf Meyer was psychiatrist-in-chief of the Johns Hopkins Hospital and perhaps the greatest American authority on combined neurology, pathology, and psychiatry. On March 5, 1917, he thanked Smith for his "very interesting and splendidly illustrated study of the crown gall" and remembered "with much pleasure" their voyage across the Atlantic.

That Smith adhered rigidly to his role as a plant pathologist was indicated by his description of his activities at the Congress. "The great XVIIth International Medical Congress has come and gone," he wrote on August 17. The Congress was

voted by everybody a great success. There were general sessions, twenty two sections for the reading of papers, a scientific exhibit, dinners, teas, excursions, and all sorts of functions, all of which I cut out. I attended none of the general sessions, and only one section meeting, that of General Pathology, one p. m., to read my paper—the English equivalent of the French paper—and did not go on any of the excursions.

My exhibit was accorded ample and very satisfactory space in the middle of one of the great exhibition rooms, close to Dr. Bashford's¹ than which it attracted much more attention. Indeed, many distinguished surgeons and physicians visited it and said it was "*the most* noteworthy and interesting of all the exhibits." I demonstrated it about 7 hours every day to whomsoever would listen. Some nights I was talked out. Last of all

¹ Of the Imperial Cancer Research Fund of England.

came Mr. Makins,² the great surgeon and Dr. [Sir Thomas] Barlow, president of the Congress. Some men I hoped might see it did not come, e. g. Dr. Osler and Dr. W. J. Mayo, but there were too many things to see and do and the time too short. The scientific exhibits, as a whole, were exceedingly interesting but failed largely of their purpose owing to the multiplicity of other things going on at the same time. One day in the middle of the Congress should have been set aside for this and called "Exhibit Day." . . . At least one third of my visitors were Frenchmen and Germans and Russians, there were many of the latter, also some Japanese, who mostly spoke German. The Russians spoke German and French indifferently. I got along very well with these people, but could have done nothing in English. I put up also an Italian sign over my show, but Italians were scarce. I did not meet any, but some Spaniards. The newspapers gave short notices. . . .

Tuesday evening, August 13, Dr. Osler sent Smith a note saying he was "desolated not to have seen you but I," he explained, "have been tied to my section every day. I hear your demonstration was wonderful." Dr. W. J. Mayo also sent a letter of explanation, saying:

Our stay abroad was of necessity very brief as we arrived in London on Monday evening and sailed from Liverpool the following Saturday, August 9th. I am sorry I did not have opportunity of again seeing your excellent exhibit, as would liked to have brought in a number of English friends who also are very interested in work of this character.

I am still calling attention of the profession at various times to the work in connection with the cancer question.

Among the many, many honors heaped upon this eminent graduate of the University of Michigan, his election to the presidency of the American Surgical Association was his latest. Already he had held the high honor of president of the American Medical Association in 1905-1906, and besides having served in 1895 as president of the Minnesota Medical Society he had occupied the same office of distinction during 1911-1912 in the Society of Clinical Surgery.

The plea of Smith's address, "Cancer in Plants," to medical scientists was presented near its conclusion. "I am persuaded," he affirmed,

that malignant human and animal tumours must be due to some kind of micro-organism occurring within the cells, and driving them into division through its action on the nucleus, in other words, that cancer is not a *cell anarchy*, but a *cell symbiosis*, "the incestuous product of the parasite and

² George Henry Makins of England.

its host," as Borrel paraphrases my thought. And this is what I would ask all cancer workers to reconsider, namely, whether in malignant tumours of man and animals it may not be possible that there occurs a concealed parasite, which, from its vantage ground within the cell, stimulates the nucleus into abnormal division, and passes over into the daughter-cells during this division, and so on, *ad infinitum*, as is the case in crown-gall.

No harm will be done if my hypothesis is found not to have any foundation in fact, but if I should happen to be right, then great results must follow. For, once the cause of cancer is ascertained, better methods of prophylaxis will immediately suggest themselves, and the remedy may eventually follow.³

Crown gall was believed to be a cancer in plants and *Bacterium tumefaciens* its parasitic cause. He had supplied cultures of this organism to European and American scientists, some of whom had reproduced the disease on sugar beets and other plants: Jensen of Copenhagen, Peklov of Prague, and von Dungern of Heidelberg. Dr. Peklov had sent him "a photograph of an inoculated sugar-beet, on which [was] shown as typical a tumour as any we have produced in the United States," Smith said. "Others, following my directions, have isolated the parasite for themselves, and with it have reproduced the disease, e. g. Clayton Smith in California, J. Bolle in Austria, and Pole Evans in South Africa."

He realized that the history of cancer research was strewn "with wrecks of parasitic theories." But the difficulty, he believed, had been that "substantial proof" upon an experimental basis under an adequate hypothesis of cancer development had been lacking. A fundamental study of pathological growth, made in plants of tumors in plants, was offered to medical science in the belief that experimental research in animal cancer might benefit. Plants were more abundantly available and easier of manipulation than the higher animals. "Cancer," he said, "has stood for many years a great sphinx-like problem of pathology, defying solution. Indeed, now that the aetiology of syphilis is solved, we may say it is the last great problem of human pathology to remain in a cloud of uncertainty."

In 1911 Hideyo Noguchi, youthful and distinguished Japanese pathologist working at the Rockefeller Institute for Medical Research, had isolated in pure culture from the brain and cord *Treponema pallidum*, cause of syphilis. He made possible

³ *Proc. 17th Inter. Cong. of Med.*, London, Aug. 1913, and reprint, p. 17

improvements in diagnosis and demonstrated the organism's connection with general paresis and locomotor ataxia.⁴ In 1909-1910 Paul Ehrlich's discovery of the organic arsenic compound, salvarsan or "606," had proved remedial, at least in part.⁵ Smith knew of other recent medical research triumphs. Drs. Simon Flexner and Lewis, at the Rockefeller Institute, in 1909, "by intracranial inoculation of tissue from diseased spinal cords [had] produced infantile paralysis in monkeys and transmitted it from one monkey to another through a long series. Eighty-one monkeys were infected with the virus and the average incubation period was 9.82 days." New discoveries have been made since this list was prepared in 1926 by Smith. By 1913 Flexner and Noguchi had "cultivated from the brains of children dead of infantile paralysis a very minute, filter-passing, coccus-like organism, about 0.2μ in diameter, with which they reproduced the disease in monkeys from the brains of which they reisolated the organism." Moreover, in

1910, in the Philippines, by inoculating diseased material containing the *Treponema pertenuis*, Henry J. Nichols [had] produced Yaws in a monkey, and from this monkey transmitted the disease to three generations of rabbits.

In 1909 Harold Taylor Ricketts [had] discovered the cause of the Rocky Mountain spotted fever, isolating the organism since known as *Rickettsia* from man and from the ticks which he had previously proved to be carriers of the disease, also from the eggs of the ticks.

In 1911 McCoy, in California, [later] Director of the Hygienic Laboratory in Washington, discovered *Bacterium tularensis* in ground squirrels dead of an epidemic disease resembling plague, which he was then studying.

[And in] 1913 Yamagiwa and Itchikawa, in Japan, produced cancer in the ears of rabbits by repeated tar-paintings.

Smith, in his address, "Cancer in Plants," offered the following in support of his analogy between tumor strands in crown gall which connect primary and secondary tumors and those malignant animal and human tumors which "show a marked tendency to reproduce themselves at a distance from the primary tumour by means of migrating portions of their own tissue," either independently or by a chain of tumor cells: "MacCallum, from the

⁴ Fifty years of pathology, *op. cit.*, 34; see also Smith's translation of *Pasteur, the history of a mind, op. cit.*, 341-342, Annotated list etc.

⁵ Fifty years of pathology, *op. cit.*, 34; also, John Drury Ratcliff, *Yellow magic, The story of penicillin*, 26, 27, N. Y., Random House.

⁶ Fifty years of pathology, *op. cit.*, 34-35.

Johns Hopkins Hospital, has reported a malignant growth of the testicle which he traced out of the scrotum and through the vena cava, in a continuous chain of tumour cells, as far as the diaphragm. The animal pathologists have drawn, I believe," he suggested,

too sharp a line of demarcation between malignant tumours on the one hand, where the cells of the host-animal, acting under some unknown stimulus are responsible for the tumorous growth, and granulomata on the other hand, such as tuberculosis or actinomycosis, where a visible parasite is responsible for the presence of the primary tumour, and the direct migrations of this parasite for any secondary tumours that may appear. At bottom, I think the distinction between such a disease, for example, as tuberculosis or leprosy and the malignant animal tumours is not as sharp as some of the histologists have been inclined to believe, but this is aside from the question at issue, and only a note by the way. The host-reaction is prominent in both.

In this, Smith came close to venturing a purely medical opinion but he remained a pathologist in the study of plants, interested in galls and overgrowths of plants, a subject, he believed, "well worthy the profound attention of animal pathologists, because plants, after all, are not fundamentally different from animals, and the study of their tumours may well prove just what is necessary to throw light on the aetiology of cancers in man and the lower animals." To this end, he discussed various kinds of bacterial plant diseases and infection, galls and overgrowths due to insects, fungi, and other causes; and among the fungous galls considered was "the cause of the West Indian fungous knot of citrous fruits, recently worked out in [his] laboratory." After he had returned to America, Dr. Peyton Rous, exchanging reprints, wrote: "I see by the medical journals that your work has made a deep impression on European observers and wish that I might have been in London last summer to hear your paper."

Paris and Vienna were Smith's two principal destinations for scientific research. At Paris he visited the Pasteur Institute and became acquainted with Dr. A. Besredka and Élie Metchnikoff, the latter the celebrated discoverer of phagocytosis and vice-director of the Institute. Besredka, himself an immunologist and pathologist of illustrious calibre, was to become one of Smith's closest friends and admirers among his foreign correspondents. The great Dr. Pierre Paul Émile Roux, director of the Institute,

was in southern France or northern Africa. Regrettably we do not know more of what took place on this first of Dr. Smith's visits to this world famous institution.⁷

About 1908,⁸ in a German catalogue of second-hand books, he had seen the title of a book, *Pasteur, histoire d'un esprit*, by Émile Duclaux. Straightway he had ordered it, because of its subject and its author, and was puzzled, since the book was published in France in 1896, why he had never come across it in any library or literary review or known any fellow scientist who had read it. A book on Pasteur! That was of utmost importance. And by Duclaux, director of the Pasteur Institute from 1895 until his death in 1904 and closely associated with the institution from its beginnings in 1888! His name was enough. Smith "devoured page after page, marveling more and more at the wonderful breadth and perspicacity of the presentation. Pasteur seemed alive in its pages, and Duclaux not less alive. No book about a scientific man ever interested him more, or could be written, it seemed, with a more appreciative and discriminating touch." When finished with his reading, he wrote on the margin of its last page, "The most useful book I have read in a long time."

While at the Institut Pasteur, Smith obtained permission from the Institut and Madame Émile Duclaux to translate into English *Pasteur, histoire d'un esprit*. Madame Duclaux, herself a figure in English and French literature as a poetess and writer of prose, had been Mrs. James Darmesteter before her marriage to Duclaux, and her maiden name had been Agnes Mary Robinson. An author of many books, she had written in 1906 a book on her husband's life, *La vie de Émile Duclaux*. This book, out of print by the year 1920 when Smith's and Miss Hedges' translation, *Pasteur, The History of a Mind*, was published, was made the basis of Dr. Smith's study of the life and work of Duclaux, which study comprised the "Introduction" to the translated volume on Pasteur. Madame Duclaux in 1913 presented Smith with a copy of her book.

The other scientific institution which Smith visited while in Paris in 1913 was the French government's Laboratory of Plant

⁷ These facts are taken from Dr. Smith's Journal of a third European trip 1924.

⁸ Introduction, *Pasteur, The history of a mind, op. cit.*, v-vi.

Pathology, 11 bis rue d'Alesia. There Smith studied the identity of the American and French mulberry blight.⁹ There also he examined, though hastily, materials of Delacroix's so-called bacterial wilt-diseases of tobacco,¹⁰ for purposes of his third volume of *Bacteria in Relation to Plant Diseases*. There also, through the courtesy of Director Étienne Föex, he carefully examined the matter he had wished to study at Paris in 1906—whether Delacroix's *B. solanincola* was a third organism different from *Bacillus phytophthorus* and *Bacterium solanacearum* and the cause of the French disease, brown rot of solanaceae. Delacroix had said in 1901, "This bacterium seems to me not different from the *Bacillus solanacearum* of Erwin F. Smith. Its cultural characters are the same; the signs of the disease observed in the United States on the potato, tomato, and egg-plant are exactly those I have myself seen."

Smith collected data and, after returning to the United States, revised his manuscript to include a footnote descriptive of his studies at the Station of Vegetable Pathology in Paris.¹¹

From Paris, Dr. Smith went to the Flemish cities of Antwerp and Ghent. He attended the International Exposition of that year where he found the English exhibit on tropical diseases, prepared by the Liverpool School of Tropical Medicine "very full and creditable—on cholera, typhoid, plague, beri beri, sleeping sickness and half a dozen other diseases." Literature in all branches of pathology had been increasing enormously during recent years. Delacroix and Maublanc had published not only their *Maladies parasitaires des Plantes cultivées* (1908-1909) but also their *Maladies des Plantes cultivées dans les Pays chauds* which Smith had accepted as the first handbook of tropical plant diseases.¹² This appeared in 1911 and two years later Melville T. Cook's *The Diseases of Tropical Plants*. New editions of older works and some first editions of important manuals and textbooks in bacteriology had been published, though Smith's three-volume monograph was being received as the standard work on plant

⁹ *Phytopathology* 4(1): 34, Feb. 1914. See also Smith's article, Bacterial mulberry blight, *idem* 2(4): 175, Aug. 1912; also, Bacterial blight of mulberry, *Science* 31 (803): 792-794, May 20, 1910.

¹⁰ *Bacteria in relation to plant diseases* 3: 266-267, 263, 1914.

¹¹ *Idem*, 214-215.

¹² Fifty years of pathology, *op. cit.*, 32-33.

bacteriology. Contrasting Smith's two published, and the promulgated third, volumes of *Bacteria in Relation to Plant Diseases* with the total knowledge of micro-organic pathology as shown by W. Kolle's and A. Wassermann's *Handbuch der pathogenen Mikroorganismen*, plant bacteriology as a growing science was developing commensurately. In 1907-1909 two supplementary volumes to the original four of the great *Handbuch* had been published, and in 1912-1913 a new edition by Kolle and Abel—eight volumes in all—had made its appearance.¹³ Plant pathology and bacteriology as experimental sciences were scarcely a half-century old; some might say, scarcely little more than a quarter of a century old.

At the French exhibit of the International Exposition, Smith saw some "cultures of cheese and butter organisms from the Pasteur Inst[itute] on which [he] made some notes for Thom." Charles Thom was at that time a mycologist in cheese investigations of the dairy division of the Bureau of Animal Industry of the United States Department of Agriculture and in charge of cooperative work in soft and fancy cheesemaking with the Storrs Connecticut Agricultural Experiment Station.

Investigation in microbiology for many years, of course, had figured prominently in the work, and the *Bulletin*, of the Pasteur Institute. In America, many microbiological investigations had taken place but had been known under the broader titles of dairy bacteriology, vegetable pathology, mycology, or various branches of medical science. Gradually, however, microbiology, as an organized segregate of experimental inquiry, was being recognized. In 1914 Dr. Thom, authority on the physiology and classification of the economic molds of the genera *Penicillium* and *Aspergillus* and at one time a student in Smith's laboratory, was to be placed in charge of a Microbiological Laboratory in the Division of Chemistry of the Department of Agriculture. In 1911 Charles Edward Marshall, professor of bacteriology and hygiene at Michigan Agricultural College, had edited, with many scientists collaborating, an important publication, *Microbiology*, and the next year he had gone to Massachusetts Agricultural College as director of the graduate school and professor of microbiology. In 1914 he became president of the Society of American Bac-

¹³ *Idem*, 34-35.

teriologists; and his continued interest in Smith's work was shown by a letter of 1912 praising one of his papers on crown gall, "On some resemblances of Crown-gall to Human Cancer."

During recent decades, microbiology and its branches of science have been greatly expanded and become far more specialized. In 1947, at Copenhagen, a fourth International Congress for Microbiology was held, attended by more than eleven hundred persons, eighty-seven of whom were Americans representing various segments of study. Recently at Rutgers University was established an institute,¹⁴ an outgrowth of a department, of microbiology under the brilliant leadership of Dr. Selman A. Waksman, a former student of Dr. Halsted. At the Copenhagen congress Waksman was the first American scientist to receive the Emil Christian Hansen prize for microbiology.¹⁵ His most famous discovery has been the antibiotic, streptomycin. Since then, among other discoveries of importance, there have been two which approximate in value and, as remedies against several serious diseases, may prove of comparable worth with the first great antibiotic, penicillin, and with streptomycin. Aureomycin,¹⁶ now introduced into medical practice, was discovered by B. M. Duggar, emeritus member of the faculty of the University of Wisconsin. Chloromycetin, the utilization of which is now being investigated, was the original discovery of Paul R. Burkholder of the Osborn Botanical Laboratory, Yale University. He isolated the *Streptomyces* species from a soil sample collected in Venezuela.¹⁷

Dr. Smith, while on his European journey of 1913, was interested in microbiology, but mainly those aspects which pertained to the study of plant diseases. Not until 1915 would the English bacteriologist, Frederick William Twort, announce in the *Lancet* his discovery of the bacteriophage. This, as a product of the bacterial cell, was a decade and more later to figure in studies of cancer viruses,¹⁸ and assistants in Smith's laboratory would publish on the influence of bacteriophage on *Bacterium tumefaciens*.¹⁹ In

¹⁴ S. A. Waksman, An institute of microbiology—its aims and purposes, *Science* 110(2845): 27-30, July 8, 1949. Six main research fields described.

¹⁵ *Science* 106(2754): 339, Oct. 10, 1947.

¹⁶ *Science* 110: 305, Sept. 23, 1949.

¹⁷ *Science* 106(2757): 417, 418, Oct. 31, 1947.

¹⁸ *British Medical Journal*, London, 515, March 26, 1949.

¹⁹ N. A. Brown and A. J. Quirk, Influence of bacteriophage on *Bacterium tumefaciens*.

Vienna he bought "one new book" and "a very important one for our work," he wrote to his laboratory co-workers.²⁰ This was *Microchemie der Pflanzen*, by Dr. Hans Molisch, "the leading botanist here in Vienna. . . . The book," he explained, "has something in it on the alkalinity of protoplasm and especially of that of the nucleus, and gives some tests which we may be able to apply to the tissues of crown gall i. e., to determine whether my theory that the nuclear contents of the daisy is alkaline, as I suppose." His plan was that, if a chemist could be associated with them, they might translate the book into English. "It would prove very useful not only for us in the doing," he said, "but for many English reading students," and he was very much pleased when in July, 1921, he received from Molisch a second edition of it.

Dr. Molisch himself had shown Smith a copy, and they had "a delightful visit of a couple of hours." Then most of his time that week was "spent at the K. K. Oenologisches und Pomologisches Institut at Klosterneuberg about twelve miles north of Vienna near the Danube," where Felix von Thüman, the mycologist, had been, and where Dr. Linsbauer was now director of the Lehr-Anstalt. There he examined the materials of Emerich Ráthay on a bacterial disease of orchard grass.²¹ Before leaving for Europe, Smith had received from Denmark specimens of a disease of orchard grass which resembled a description by Ráthay in 1899 of "a bacteriosis of *Dactylis glomerata*." In 1911, on Maryland carnations, he also had found what seemed to be this same disease. He, therefore, had prepared for *Science*²² an article entitled, "A New Type of Bacterial Disease." In this malady the chief growth of the parasite appears on the surface between closely appressed organs. The question yet remained, however, whether the Danish and Austrian diseases were one and the same. In oak woods on the Kahlenberg where Ráthay had collected his material, Smith found fragmentary specimens, and brought them back to America. In a hot house in Washington a diseased plant

faciens, and some potential studies of filtrates, *Jour. Agric. Res.* 39(7): 503-530, 1929 (considers work 1921-1922 etc. of F. D'Hérelle on the bacteriophage).

²⁰ Letter written at Vienna November 1, 1913.

²¹ *Bacteria in relation to plant diseases* 3: 160.

²² N. s. 38(991): 926, Dec. 26, 1913. Also, *Introduction to bacterial diseases of plants, op. cit.*, 48, and authorities cited.

produced fifteen flowering healthy shoots the next year.²³ Subsequently, O'Gara discovered a similar disease on *Agropyron* in Utah and Hutchinson on wheat heads in Punjab in India.

The Laboratory's scientific study of many of the plant diseases dealt with in the third volume of *Bacteria in Relation to Plant Diseases* had been performed in Washington, and little or no work on these by Smith was required in Europe. A few examples were the corn studies including further work on Stewart's disease of sweet corn, and the Grand Rapids tomato disease, wilt diseases of tobacco, vascular diseases of banana, etc.²⁴ After returning to America, he exchanged letters with A. Spieckermann of Munich who, with Kotthoff, described (1914) their bacterial ring disease of potato. But he did not go to Munich. In early November he was in Vienna and by the middle of the month in Bologna and Milan, preparing for visits to Genoa and San Remo and to sail for the United States in the not distant future. He wanted specimens or information about the oleander gall, the pine tumor of southern France, and other things of value or interest in his work. Although he regretted that he had not put over his sailing to a later time, he made his scheduled reservation and returned home on the same ship on which he had crossed, the *Lapland*. One day out of New York he was asked to present a lecture, and he sketched the material progress made by research in pathology since the early work of Pasteur and Koch and the major technical advancements in bacteriology since then.

Dr. Smith, soon after his arrival from his second European journey, married on February 21, 1914, Miss Ruth Warren at the home of her uncle and aunt in Springfield, Massachusetts. She was the daughter of Josephine Hopkins and Wilmot L. Warren, and her father and her uncle, Edward F. Hayes, for many years had been leading editorial writers for the *Springfield Republican*.

²³ *Bacteria in relation to plant diseases, op. cit.*, 3: 160 n.

²⁴ Corn: Erwin F. Smith and Florence Hedges, Burrill's bacterial disease of broom corn, *Science*, n. s., 21(535): 502-503, March 31, 1905. See also, Erwin F. Smith and Charlotte Elliott, A bacterial disease of broomcorn and sorghum, *Phytopathology* 14(1): 48, Jan. 1924.

Erwin F. Smith, Seed corn as a means of disseminating *Bacterium stewarti*, *Science*, n. s., 30(763): 223-224, Aug. 13, 1909.

Tomatoes: Erwin F. Smith, A new tomato disease of economic importance, *Science*, n. s., 31(803): 794-796, May 20, 1910.

Potatoes: Erwin F. Smith, *Bacillus phytophthorus* Appel, *Science* 31(802): 748-749, May 13, 1910. Also, *Phytopath.* 2(5): 213-214, Oct. 1912.

Left an orphan at the age of fourteen years, she had lived with members of her family and with her brother. Her education had been received in the Springfield schools and at Smith College, and her degree of master of arts had been obtained in Latin and Greek at Columbia University. She was a linguist of unusual ability, conversant with several modern as well as the classical languages, a student of literature and art, and had taught these subjects in several of the best private schools of the east. She and Dr. Smith read together of evenings the best books, and over more than a dozen years were very happy, their mutual interests in languages, literature, and art providing them with an abundant basis for calm, quiet satisfaction and joy. His diaries again and again reveal this.

The year 1914 brought new honors to Dr. Smith. On May 13 he was elected to membership in the American Academy of Arts and Sciences. On April 24 he was invited by Director F. R. Lillie of the Marine Biological Laboratory to participate the following July 7 in a series of evening lectures on general biological science. On April 17 President Charles R. Van Hise of the University of Wisconsin wrote him that, on recommendation of their faculty, the regents would confer on him at their next Commencement the honorary degree of Doctor of Science. Because of a necessary journey to the Pacific Coast, Dr. L. R. Jones could not be present at this ceremony but he wrote: "It is not necessary to add the very great pleasure it gives me that the University of Wisconsin could thus honor itself while honoring you by awarding you this degree."

On August 4 Smith announced to Thaxter: "The third volume of *Bacteria in Relation to Plant Diseases*, which has been grinding through the press for 18 months, will be published this week." V. H. Blackman, professor of physiology and pathology of the Imperial College of Science and Technology at South Kensington, England, was among the many who sent letters of appreciation. "May I congratulate you," he wrote, "on the publication of the third volume of your great work on bacteriology. The appearance of these fine volumes is eagerly welcomed by many botanists in this country." Jacob Eriksson of Stockholm ordered a copy of the "imposing work." Savastano of Italy sent for this and any other volume of the set which he did not possess. Ernst A. Bessey of

the department of botany of Michigan Agricultural College found the work "excellent, thorough and basic." Among the other Americans who wrote Smith, V. M. Spalding, now retired, living at Loma Linde, California, and proud of his student, said: "What a satisfaction it must be to you to bring this great work to a conclusion and still have years—I trust—of fruitful investigation in which at least some of the problems that you have met may be settled. Your studies of cancer in plants for years past seem to me of extreme interest and value, to mention one out of much more." Smith planned to visit California the next summer. But his hopes to renew acquaintance with his venerable professor were not to be realized. The next year his researches in crown gall required his presence in his laboratory, and on November 12, 1918, Dr. Spalding died.

In 1913 David Franklin Houston had been appointed United States Secretary of Agriculture. B. T. Galloway had been promoted to Assistant Secretary, and W. A. Taylor had been made chief of the Bureau of Plant Industry. Secretary Wilson had retired and, to honor him, a farewell reception at the United States National Museum had been held on March 7, 1913. Dr. Smith had delivered the farewell address, and presented duplicate larger than life portrait bronze busts of the Secretary, one to him and one to be placed in the library of the Department "to show the coming generations what manner of man he was who wrought" the great work of advancing agriculture on a nation-wide basis during his administration. Smith had originated the idea of the bust, persuaded the Secretary to sit for the sculptor whom Smith picked, collected the funds from employees of the Department, and in the course of his presentation, said:

You have been with us as Secretary of Agriculture for sixteen years, a longer period than any other man has ever served in a Cabinet position in this country. Rarely in any country has a man been so honored. In the sixteen years we have grown from less than twenty five hundred persons occupied with problems for the betterment of agriculture to nearly fourteen thousand workers. We then expended less than three million dollars per annum on our work. The bill that has recently passed Congress directs us to expend nearly eighteen millions. Sixteen years ago we had very little influence at home and none abroad. To-day there is not a civilized country in the world that does not speak with respect and envy of our Department

of Agriculture, and as for our standing at home one has only to ask any well educated farmer anywhere in this broad country,—in Maine or Missouri; New York or Minnesota; the Gulf States; the Carolinas or California.

You have been a part of all this vast growth. To you, more than to any other one man, all this is due. Your large foresight and wise administration have made it possible. . . .²⁵

Before his retirement, Secretary Wilson had recommended to the committee on appropriations of the House of Representatives certain additional monies which, had these been approved by the committee on agriculture, would very considerably have enabled Dr. Smith to extend his laboratories and field investigations in plant pathology. The recommended appropriation had also been approved by Secretary Houston and to secure favorable action thereon Smith in 1914 appeared before the house committee on agriculture.²⁶

Introducing himself to the committee members unacquainted with his work, he pointed out that he had been with the Department for twenty-seven years and during this time

revolutionized the study of bacterial diseases of plants . . . discovered a new type of fungous diseases of plants (the Fusariums) of wide distribution and great economic importance . . . demonstrated that a wide-spread bacterial tumor on plants (grape vines, roses, peaches, pears and many other cultivated plants) is a genuine cancer in its method of growth and [had] rendered it extremely probable that cancer in man and animals is due to a similar organism.

For the work on cankers and tumors of plants, he suggested that the Department "might well put an additional \$10,000 into this one subject." In the autumn of 1914 the Department had launched a large scale campaign, cooperating with states around the Gulf of Mexico, to eradicate the serious and widely disseminated canker disease of citrus fruit and trees. A scientific paper, prepared on this subject by Miss Clara Hasse of the Department, announcing discovery of the cause to be a bacterium, *Bacterium citri*, was more proof the next year of the tremendous economic good realized from investigations in plant pathology and bac-

²⁵ Typewritten copy of his address found among collected documents and papers; also, his diary, 1920, p. 239.

²⁶ Statement of Dr. Erwin F. Smith before the House Committee of Agriculture 1914, *op. cit.*

teriology. Miss Hasse's treatise, Smith believed, would remain a classic upon the subject. The fact that within less than a decade at a cost of about half a million dollars this disease was brought "very well under control" whereas when the campaign was started the disease already had seriously reduced the industry in some of its best and most productive regions exemplified another among several instances where plant industries had been saved from serious curtailment or total extinction as later did happen to industries connected with the chestnut due to its bark disease.²⁷

Smith wanted more funds to study also bacterial diseases among vine stocks, bacterial oat diseases and various other leaf-spot bacterial diseases of plants. His major emphasis, however, was directed to the need of field and laboratory investigations in tobacco, potato and tomato, and corn diseases. In recent years these situations had become so threatening that Smith pleaded, "If we could cut these diseases in two or even reduce them one-tenth part, we could save the country every year more than the cost of the entire Bureau of Plant Industry."

Several bacterial diseases of the tomato and potato were causing "immense losses, north and south, east and west, some part of which," he urged, "we ought to be able to cut out by further researches." The bacterial canker of tomato, first called "The Grand Rapids disease" (*Science* 31: 794) after the locality from which it had been first sent to him, had been described by him in 1909 as a phloem wilt disease caused by a yellow schizomycete, *Aplanobacter michiganense*. This was one. In 1910 Smith had published (*Science* 31: 748) his verification of Appel's work on black-leg of the potato, due to *Bacillus phytophthorus*. This was another widespread and destructive disease. To the "several bacterial diseases" of tomato and potato Smith had "personally given much attention." But, he argued, "I cannot cope adequately with them without further assistance." At least four more laboratory assistants were needed.

In 1908 he had determined that the "Granville tobacco wilt," (Bulletin 141, part II, Bur. Pl. Industry) was due to *Bacterium solanacearum*. In the southern states this wilt disease in tobacco

²⁷ Report of the Secretary, *Yearbook of the U. S. Dep't of Agric. for 1918*: 43. See also, *Fifty years of pathology, op. cit.*, 33, 38. Also, W. A. Taylor, *Campaigns against plant diseases, in A Quarter Century of Service*, found in report of the Chief of the Bureau of Plant Industry for 1926, and reprint, pp. 2-3.

and the tomato had now made it impossible to grow these crops on many fields. Smith explained to the House of Representatives' agricultural committee:

There is a bacterial tobacco disease in North Carolina and Florida of such magnitude and destructive power that if it continues to spread unchecked for the next 25 years we may confidently expect the tobacco industry of the United States to be practically wiped out. I know the cause of this disease. I wish to study further the habits of the parasite, hoping to find some way of attacking it. I think we ought to be able to find a way to cultivate tobacco again on the badly infested lands, something which is not now possible, as the organism persists in the soil.

As to diseases of corn, he stated his

wish to continue my studies of bacterial diseases of corn, broom corn and sorghum in the field, and to study critically the causes of the molding of corn in freight cars and elevators. The losses are now very great and if they could be eliminated, which seems to me quite possible, the price paid to the producers in our central and western states could be increased several cents a bushel, the prices now paid being made low enough to recoup all losses in transit.

In the West Indies, also, serious curtailment or perhaps eventual destruction of profitable plant industries was threatened by diseases. Two such which Smith had studied, determined the cause, and for which he was still seeking a remedy or control method were the bacterial bud-rot of the coconut palm (1905) (*Science* 21: 500) and the banana disease, due to *Fusarium cubense* (*Science* 31: 754) which by 1926 "belted the globe."

October 16, 1914, at the twenty-fifth anniversary celebration of the Missouri Botanical Garden, Smith by invitation read a paper, "A Conspectus of Bacterial Diseases of Plants,"²⁸ a scholarly condensation of principles and knowledge, and the substance of which formed the basis of the first chapter of his textbook, *An Introduction to Bacterial Diseases of Plants*, published some six years later. Paragraphs and some revisions in the light of new knowledge were added when the textbook was prepared. One paragraph, in connection with methods of control, should be regarded prophetically relevant to modern science today: "One of my fancies," Smith wrote in 1920,²⁹ "is that plant pathologists will eventually

²⁸ *Annals of the Mo. Bot. Garden* 2: 377-401, Feb.-April 1915.

²⁹ *Intro. to bact. dis. of plants, op. cit.*, 74.

discover competing saprophytes which when sown on infected soils will overcome and render harmless certain of the bacterial parasites present in them. We have some evidence that nature does this, and man working toward a definite end should be able to improve on nature."

Smith in 1914 placed confidence in the "rapidly increasing knowledge of the biological peculiarities of the parasites causing these diseases, and of the ways in which they are disseminated. . . ." His hope for the future was in wider application of control-methods already found, and in the discovery of new rules experimentally worked out by plant pathologists and based on "individual peculiarities of the parasites." That this hope was not yielded during his lifetime is shown by an utterance formally made in 1926 near the close of his life: "We know as yet," he said,³⁰ "very little about bacterial symbiosis and antagonism in relation to disease, but beyond doubt there is a great deal to learn."

In the second volume, indeed in all three volumes, of *Bacteria in Relation to Plant Diseases*, he suggested to plant scientists some "wholly unworked" fields for future investigation. This he did in many of his most important scientific writings. If the field was not wholly unworked, he suggested subjects which he confidently believed would yield more valuable results and subjects which he was sure should be studied and investigated further. Among wholly unworked fields, he suggested plant studies in acquired resistance to, and immunity from, disease.³¹

Originating valuable disease-resistant strains of plants by selection or hybridization, and this included the newer resources being discovered by research in genetics, represented a type of future investigation which he was sure would be still more useful in solving problems. Much of his writing had important bearings on animal and medical pathology, yet were based on results exclusively from the study of plants. It took scholarship of high order to answer such a question as, "Are any bacteria known to cause disease in both plants and animals?" and adduce his answer from available experimental data the world over resulting from inoculations of plant parasites into animals and animal parasites

³⁰ Fifty years of pathology, *op. cit.*, 40.

³¹ *Bacteria in relation to plant diseases*, *op. cit.*, 2: 93-96.

into plants. Likewise, he had to know his subject thoroughly to elaborate summary research results from the comparatively unexplored field of plants as carriers of disease.

It was as late as 1912 before the existence of the first vitamin was definitely proved—"as a *something*, a quality, a property, or perhaps a substance, which some foods had and others did not. Other vitamins," Leonard A. Maynard³² has pointed out, "became thus recognized in succeeding years, but it was not until the organic chemist commenced to give these "somethings" attention that the field really moved. It was over 15 years after the first vitamin was discovered that the chemical nature of any of them was definitely established. In the succeeding years, isolation, determination of structure, and, frequently, synthesis followed increasingly closely on the biologists' discoveries." In 1912, radiology, emerging from the discovery of "X-rays" so-called in 1895 and the discovery of radium in 1898, was comparatively still an infant science.³³

Smith admitted³⁴ that a more thorough knowledge of biochemistry and biophysics greatly would have helped him: "Handicaps have been," he confessed, "first, insufficient mathematical physics; second, insufficient biochemistry. But perhaps if I had specialized on these subjects, I should in the end have been like one of my teachers who decided he would be a naturalist but must first get a good ground-work in Latin and Greek. The result was that he never passed on into a study of nature but became a teacher of the classics. Moral: There is not room in one short life for everything." Dr. Smith considered that, after all, his "most important work [had] been the stimulating of other persons to undertake researches first in [his] own laboratory and then through [his] writings and teachings in many other laboratories."

This was written in 1922 and he lived only five more years. The search for chemo-therapeutical remedies against disease had been started many years before, but was chiefly a work of medical research. Smith's interest in the causes of diseases, human, animal,

³² Director of the School of Nutrition, Cornell University, A chemist's view of nutrition, *Science* 105(27): 399-403, Jan.-June 1947. Address given before the New York Section of the American Chemical Society, December 6, 1946. The discovery of vitamins by Sir Frederick Gowland Hopkins of England traces to the year 1906 (*Time*, May 26, 1947 issue).

³³ Douglas James Guthrie, *A history of medicine, op. cit.*, 384-385.

³⁴ Synopsis of researches, *op. cit.*, 45-46.

or plant, was always accompanied by an equally strong interest in remedies or control methods. In 1919 Noguchi had isolated from yellow fever cases a flagellate protozoan, *Leptospira icteroides*. Smith believed this to be "the probable cause of the disease." In the early 1920's, Drs. G. F. Dick and G. H. Dick, American medical scientists of Chicago, found a streptococcus in scarlet fever. Smith believed this "probably" the disease's cause, since the streptococcus was shown to be unvaryingly present and its behavior indicated it as the cause.³⁵ This discovery led to the elaboration of a susceptibility test for scarlet fever comparable to the Bela Schick susceptibility test for diphtheria.³⁶ E. C. Rosenow's studies of facial infection, endocarditis and arthritis during this decade 1917-1926 also interested Smith.³⁷

Experimental research in antibiotics emerged into definite notice after Sir Alexander Fleming observed in 1928 that when a culture plate fluid containing staphylococcal colonies was contaminated by spores of a species of *Penicillium* (*Penicillium notatum*), the staphylococci near the mold began to show signs of dissolving. For years botanists had noticed that when a bread mold culture was introduced on a seeded plate of bacteria a clear area around the mold colony often resulted. In 1877 Pasteur and Joubert had suggested a possible therapeutical importance from their observation that the growth of certain air-borne organisms inhibited the growth of the anthrax bacillus. John Tyndall had mentioned an observed "struggle for existence between the Bacteria and the *Penicillium*" in his 1881 *Essays on the Floating Matter of the Air in Relation to Putrefaction and Infection*.³⁸ Yet the production of the therapeutical medicine or drug, penicillin, eluded mastery until after Fleming had investigated this "new bacterial inhibitor" further. Fleming's initial report was not published until 1929, two years after Dr. Smith died. After the report a decade of more studies by other workers, especially research in the clinical uses of the isolated product penicillin, followed before the medical

³⁵ Fifty years of pathology, *op. cit.*, 41.

³⁶ Justina Hamilton Hill, *Germ and the man*, 34 ff., 96. G. P. Putnam's Sons, 1940.

³⁷ Fifty years of pathology, *op. cit.*, 41.

³⁸ H. Landsberg, Prelude to the discovery of penicillin, *Isis* 40, pt. 3(121): 225-227, Aug. 1949.

drug was manufactured and marketed.³⁹ During his lifetime other outstanding achievements in medicine were announced and made available. "One of the great advances of human medicine in this decade," he said in 1926,⁴⁰ "has been the preparation of insulin by Banting and Best, of Toronto, from animal pancreas (1922) for the treatment of diabetes."

Smith seldom studied experimentally questions of antagonism among the bacteria, although while studying soil parasites he became aware of great possibilities from such study. Often symbiotic diseases, Ardisias, Pavettas, etc., interested him. The era of sulfanilamide and sulfa drugs post-dated his life. Nevertheless, in direct and incidental ways, he contributed knowledge which has proved valuable in modern antibiotal research. We shall consider some instances soon, and much of the rest of this book will show some of the groundwork that was prepared.

While discussing acquired resistance to, and immunity from, diseases in plants, he had written on "antibodies," and called the subject of extreme interest theoretically and an almost wholly unworked field. Whether an attack by a disease confers immunity, in plants as in animals and man, was negatived by him, although he illustrated from his studies of crown gall and olive tubercle some conclusions which pointed to a "supposed" increased resistance after repeated inoculations. He admitted that another explanation was plausible. The observed phenomenon might also have been due to a loss of virulence by the parasites.⁴¹

In 1909 he and Miss Hedges had published on a "Diplodia Disease of Maize."⁴² caused by a soil organism like the fusarium. The fungus and its manner of infection were studied, and they suggested that it might be the

cause of the so-called "cornstalk" disease prevalent among cattle in the west. It is also possible that to *Diplodia* should be referred the great numbers of deaths of negroes in the south during the past three years from the so-called pellagra, following the consumption of moldy corn-meal and moldy hominy. This fungus (*Diplodia*) is also a cause of moldy corn in Italy. The only other fungi we have reason for suspecting in this connec-

³⁹ S. Epstein and B. Williams, *Miracles from microbes, The road to streptomycin*, 99 f., New Brunswick, Rutgers Univ. Press, 1946; J. D. Ratcliff, *op. cit.*

⁴⁰ Fifty years of pathology, *op. cit.*, 41.

⁴¹ *Bact. in rel. to pl. dis.*, *op. cit.*, 2: 93-94.

⁴² *Science*, n. s., 30(758): 60-61, July 9, 1909.

tion are species of *Aspergillus*. The writers would be very glad to receive for study samples of hominy or corn meal suspected of being the cause of pellagra.

The pathology of pellagra as a disease of humans was not studied by the Department, but the agricultural aspects of the problem were. From 1909 until 1912 the department's annual reports⁴³ referred to the work. Secretary Wilson's comment in 1911 was significant: "The question of spoiled corn and its relation to pellagara has been under investigation, the agricultural side of the problem only being considered. Toxic substances have been isolated from cultures of organisms occurring on spoiled corn and some new constituents have been isolated."

December 16, 1910, Carl Lucas Alsberg and Otis F. Black, chemical biologists of drug-plant, poisonous-plant, physiological and fermentation investigations, published their Bureau of Plant Industry bulletin 199, "The Determination of the Deterioration of Maize, with incidental reference to Pellagra." In this appeared a reference to a determination by Dr. Smith of a mold growth on corn, found by him to be "spores of *Aspergillus fumigatus*." Other molds observed to discolor corn kernels were believed to include "members of the genus of mold known as *Penicillium*." In this bulletin were described procedures to determine toxicity and to test for micro-organisms and for a tendency to become moldy. It was said:⁴⁴

The test for micro-organisms and the tendency to become moldy involves the quantitative determination of the number of organisms in the suspicious sample compared with a sound sample. The methods hitherto proposed for this purpose do not seem to be adequate. To devise improved ones and to determine the nature of the organisms present is beyond the limits of the present problem. This has been undertaken by Dr. Erwin F. Smith . . . and he will no doubt report in due time.

Dr. Alsberg was to become Chief of the Department's Bureau of Chemistry in 1912. Since 1908 he had been with the Bureau of Plant Industry as a chemical biologist, for two years before then serving part time as an investigator with the United States

⁴³ *Yearbook of the Department of Agriculture* for 1909: 75; for 1910: 60; for 1911: 60; for 1912: 78; and also as to investigations of transmission of pellagra, for 1913: 38.

⁴⁴ Examination of whole corn by inspection, 14 *et seq.*; Determination of toxicity, and Test for micro-organism and for a tendency to become moldy, 30-31.

Bureau of Fisheries. In 1896 he had graduated from Columbia University and obtained in 1900 his master's degree from the university and his degree of doctor of medicine from the College of Physicians and Surgeons there, also during the next three years taking post-graduate study at the German universities of Strassburg and Berlin. From 1902 until accepting a position with the United States Department of Agriculture, he had been an assistant in physiological chemistry and an instructor in biological chemistry at Harvard University. Smith thought so well of him that he helped to secure his appointment as Chief of the Bureau of Chemistry succeeding to the place left vacant by the resignation of Dr. Harvey W. Wiley. In an entry of his diary of January 10, 1920, Smith reveals: "I first called the attention of Sec[retar]y Ja[me]s Wilson to Alsberg as a suitable man to fill the place made vacant by Dr. Wiley's resignation and my letter to Pres[ident] Taft by advice of the Sec[retar]y was what set the ball rolling and led to his appointment. He has made good here."

In 1913, Bureau of Plant Industry bulletin 270, "Contributions to the Study of Maize Deterioration. Biochemical and toxicological investigations of *Penicillium puberulum* and *Penicillium stoloniferum*," also by Alsberg and Black, was issued. This was submitted first to Dr. R. H. True, physiologist in charge. Dr. Galloway, when he transmitted it to Secretary Wilson, called attention to the discovery that one of the molds, *Penicillium puberulum*, had been shown "to develop toxic substances in maize." The research was believed to have demonstrated primarily that the two organisms "have specific physiological properties," and that Smith performed some fundamental work in the study is undoubted. Alsberg and Black at page 11 of their bulletin said: "For this investigation the genus *Penicillium* was chosen, because it is well systematized in the recent monograph of Thom.⁴⁵ Five species inhabiting spoiled maize in the United States were obtained from Dr. Thom and cultivated separately by Dr. Erwin F. Smith. Without such help this investigation could hardly have been undertaken." Further it was said that from samples of deteriorated maize, Dr. Smith "isolated two species of *Penicillium*. One of these species was

⁴⁵ Charles Thom, Cultural studies of species of *Penicillium*, Bureau of Animal Industry bulletin 118, U. S. Dep't of Agriculture, 1910.

identified by Dr. Charles Thom, of the Storrs Agricultural Experiment Station as *P. stoloniferum* Thom."

The antibiotic, penicillic acid, was derived from *Penicillium puberulum* Bainier. This was proved to be capable of inhibiting to some extent the growth of yeast and quite effectively to inhibit the growth of *Bacillus coli*.⁴⁶ It seems a fair inference to say that Dr. Smith participated in this research which brought forth the announcement of what has been described as "the world's second recognized antibiotic."⁴⁷ He himself claimed no credit for the discovery, at least if he did this author has never found that he placed such a claim in writing. Alsberg and Black in their introduction stated the purpose of the research: "Whether molds or the products of their growth have an injurious effect on animals is a question which has not been conclusively settled," and in their summary their results were announced: "Of six species of *Penicillium* from maize examined, only two elaborated substances toxic to mice. Two of these species, one toxic, the other non-toxic, were studied in detail." *Penicillium stoloniferum* Thom was the organism found to be nontoxic.

The following was said under "General Considerations":

Since it has been definitely shown in the present paper that a distinct species of *Penicillium* produces a substance of moderate toxicity, the question very naturally arises, has it any pathological significance? At present it can only be said that it is too early to answer this question. . . . [R]esults of this investigation of *Penicillium puberulum* indicate the possibility of acute intoxication by moldy food. As already stated, the different species of *Penicillium* differ radically in their biochemical behavior. If there is so much difference in the ordinary products of metabolism, it is altogether likely that a series of toxic substances may be produced by different species. Some of these substances might very well be far more toxic than penicillic acid.⁴⁸

Penicillic acid, albeit, was found "not sufficiently toxic" to strengthen materially the "maize theory of the etiology of pellagra." Investigations were planned to extend to other species of *Penicillium*; and the "finding of penicillic acid indicate[d] that

⁴⁶ Samuel Epstein and Beryl Williams, *Miracles from microbes*, *op. cit.*, 69. Also, Bull. 270, Bur. Pl. Indus., U. S. Dep't of Agric., 26-27, 1913.

⁴⁷ S. Epstein and B. Williams, *op. cit.*, 69. See also, A. L. Baron, *Handbook of antibiotics*, 182-183, N. Y., Reinhold Publ. Corp., 1950.

⁴⁸ Bull. 270, *op. cit.*, 7, 40, 47. *Penicillium puberulum* Bainier, it was said, was originally isolated by F. D. Heald.

the relation of moldy corn to pellagra deserve[d] renewed attention." The Bureau's microbiological laboratory under Thom was established the next year. Special inquiries and requests for cultures and other materials were referred by the Laboratory of Plant Pathology to this and other laboratories. For instance, Thaxter in December, 1914, requested cultures of *Citromyces citricus*. Smith suggested that he apply to Dr. C-E. A. Winslow, curator of public health of the American Museum of Natural History of New York or Miss Westerdyk of the Willie Commelin Scholten of Amsterdam. Since, however, word recently had come that, owing probably to World War I, the latter's cultures had been lost, he doubted whether such could be obtained from there or from Wehmer. "The bother of keeping organisms going," Smith explained,

is so great that in recent years I have limited my efforts principally to those I am specially interested in and even these disappoint me frequently. . . . Before sealing my letter I thought best to make some inquiries of other laboratories in the Dept[artmen]t. No one has *C. citricus*, or any other *Citromyces* unless some citric acid producing sp[ecie]s of *Penicillium* are that.

Thom who has studied in Wehmer's laboratory recently says W[ehmer] told him that he had found other fungi producing citric acid and that his generic distinction based on this character "would not hold." . . . I understood Thom that W[ehmer] now thinks there are many species of *C[itromyces]* and that he [Thom] on the contrary doubts if there are any, i. e., any distinct from *Penicillium*. The Dep[artmen]t has species of the latter that produce citric acid and these I can get for you, I think, if you wish. They were isolated from Am[erican] soil by Thom and seem to come near to W[ehmer]'s *citromyces*. They have been sent to W[ehmer] and received by him, but he has not yet worked them over.

Should anyone doubt that Smith was familiar with technical principles involved in the study of bacterial antagonism, that person should examine his treatment of "Mixed Cultures and Mixed Infections" in the first volume of *Bacteria in Relation to Plant Diseases*.⁴⁹ There he cited in the text a reference to the "interesting paper on Antagonism" by William Dodge Frost of the faculty in agricultural bacteriology of the University of Wisconsin. Dr. Frost's article had been published in 1904 in the first volume⁵⁰ of the *Journal of Infectious Diseases*, and Smith described his "two new methods of studying this subject, viz., the

⁴⁹ Pp. 72-74.

⁵⁰ Pp. 599-640.

divided-plate method and the agar-block method." Still another method by Frankland and Ward was described. This author, however, doubts whether it may be said that Smith made any direct contribution to modern research in antibiotics. A recent *Handbook of Antibiotics*, already cited, lists nearly two hundred "antibiotic-producing organisms" and compiles and describes far more than half that number of antibiotics. Among the earlier substances announced, each discovery, like that of penicillin, either post-dated his years of research or became known after his work was mainly on crown gall.

In December 1915, at the seventh annual meeting of the American Phytopathological Society,⁵¹ Dr. Smith was elected president of the organization for the coming year. In 1916 he was made a member of the American Philosophical Society. Plant pathologists, and American scientists generally, recognized the importance of his work on crown gall.

In 1917, *The Plant World* a journal of general botany asked "some fifty American botanists what they consider[ed] to be the one or two most important botanical contributions of the last three years." Within a month and a half, Smith's "work on crown gall [had] received at least twice as many votes as any other important work."⁵²

Dr. Thomas S. Cullen requested Smith to send copies of his crown gall papers to President Nicholas Murray Butler of Columbia University. This was done, and Dr. Butler thanked Smith by a letter of May 17, 1915:

My own interest in this particular subject is very great, not only because we administer here a large fund for cancer research [George Crocker Special Research Fund], but because, purely from a layman's point of view, I suggested several years ago that comparative studies be made in animal and plant pathology with a view to ascertaining whether some of the parasitic growths, commonly observed in connection with plants, might not throw light on cancer research.

During the winter of 1915-1916, Dr. Smith discovered some "further evidence"⁵³ which he believed pointed to the cancerous

⁵¹ 7th ann. meeting, held in the Botany and Zoology building of the Ohio State University, Columbus.

⁵² Letters written by Forrest Shreve, managing editor, Feb. 23, Ap. 7; see also, *Plant World* 20(8): 264-265, 1917.

⁵³ Further evidence that crown gall of plants is cancer, *Science*, n. s., 43(1121): 871-889, June 23, 1916, and reprint.

nature of crown gall. He experimentally secured in plants a series of results of "embryonal teratomata" which he believed tended to correlate "the most diverse forms of neoplasm" and removed one of the strongest objections to regarding crown gall as a cancer in plants.⁵⁴

The facts of his discovery were described⁵⁵ in part as follows:

[U]p to the winter of 1915-1916 I do not recall that I ever saw a bud or shoot on a gall known definitely to be a crown gall. Early in our study of this disease, however, we demonstrated by cultures and by inoculations on apple, quince, and sugar-beet that a root-disease of the apple characterized by small flat tumors out of which grow tufts of fleshy roots and which was supposed to be a disease of non-parasitic origin, was actually due to bacteria scarcely distinguishable morphologically and culturally from those causing the ordinary crown gall. I was then inclined to assume complete identity of the organisms and to ascribe the difference in the phenomena, i. e., plain galls, or root-bearing galls, to fundamental differences in the tissues which happened to become infected, since the parasitic bacteria were not found in the new roots but only in the flat and often scanty tumor from which they originated. . . . Clusters of roots were also obtained on the stem of a collard plant which was inoculated with the crown-gall organism plated from a tumor on poplar, which tumor did not bear roots. To this extent only had I seen formed organs develop out of experimentally produced tumor tissue. Now I have seen much more, to wit, imperfect and fugitive leaf-buds and flower buds also developing from crown galls produced by pure-culture inoculations, using the hop strain of *Bacterium tumefaciens*. These were first observed January 8, 1916, in shoots of red-flowered Pelargonium inoculated in the growing point, five at least out of six plants showing the phenomenon. . . . More recently by bacterial inoculation restricted to the region of dormant buds in leaf axils, I have obtained the same phenomena on tomato and tobacco plants, on orange trees, on castor oil plants, and again on Pelargonium. Still later, on a tobacco plant by direct inoculation, i. e., without the intervention of a tumor-strand I caused a shoot to develop out of place, i. e., below a leaf rather than from the axil above it—an unheard of phenomenon!

The discoveries of January 8 had been made soon after Dr. Smith had addressed,⁵⁶ at the invitation of the Surgeon General, a meeting of the second Pan American Scientific Congress. On January 6 he had presented a lantern-slide demonstration of "Plant Tumors" before a joint session of subsection E of the

⁵⁴ E. F. Smith, Studies on the crown gall of plants, its relation to human cancer, *Jour. Cancer Research* 1(2): 231-309, at 243, and n. 3, Apr. 1916.

⁵⁵ *Idem*, 241-242, and n. 2.

⁵⁶ *Second Pan Amer. Sci. Cong.* The Final Act, 279, Washington, Gov't Print. Office, 1916. Surgeon General Wm. C. Gorgas was chairman.

Section on Public Health and Medical Science and the American Association for Cancer Research. Immediately he sent brief notices of the discoveries to *Science*⁵⁷ and the *Journal of the American Medical Association*.⁵⁸ He included the above-quoted materials in an article⁵⁹ sent to the *Journal of Cancer Research*, new, official, publishing organ of the cancer research association. On April 18, 1916, he read before the National Academy of Sciences a paper entitled "Further evidence as to the relation between crown gall and cancer."⁶⁰ After this occasion, "many individual members [visited the] hot houses to look at [his] inoculated plants."⁶¹

April 29 Smith reported to Dr. MacCarty of the Mayo Laboratories:

You will be interested to know that I had a string of distinguished zoologists and physicians to look at my inoculated plants during the meetings of the National Academy of Sciences in the week following your visit here, and all of them expressed themselves as greatly interested, and some of them went much further than that. . . .

The inoculated plants bearing teratoids have developed considerably further since you saw them, and a number of them are now dying with the disease. I had half a dozen photographs of these plants made today and will have about as many more made on Monday, to use as slides before the Ninth Annual Meeting of the American Association for Cancer Research, which begins here in Washington at the Willard Hotel on Monday, May 8, 1916. . . .

Something quite interesting is now visible on the inoculated plants of *Ricinus* which was not visible when you saw them. The tumors in the leaf axils have in a number of instances stimulated the neighboring glands on the leaf stalks so that they have become cancerous and have enlarged 100 times or more the normal size of the glands. This is extremely suggestive of what I saw in your laboratory in cancer of the rectum, namely, the marked enlargement of the intestinal glands 3 or 4 inches away from the tumor. This phenomenon is visible on as many as six of my plants. I have had sections made of one of these enlarged glands and they are now in the process of staining, but not yet ready for study.

I feel confident that I shall be able before very long to show on plants by the use of this one microorganism (*Bacterium tumefaciens*) all the leading types of cancer as it occurs in man, exclusive, of course, of Gliomata and the like.

Please show this letter to the good Doctors Mayo, and give them my kindest regards.

⁵⁷ 43(1106): 348, March 10, 1916.

⁵⁸ March 11, 1916, p. 833.

⁵⁹ Studies on the crown gall of plants etc., *op. cit.* (March 5, rec. for pub.).

⁶⁰ *Proc. Nat'l Acad. Sci.* 2: 444-448, Aug. 1916.

⁶¹ Letter, E. F. Smith to Thaxter, April 20, 1916.

That botanists as well as zoologists and physicians saw Smith's experimental materials, and were impressed, seems evident from a letter written by John Merle Coulter to Smith after the meetings of the National Academy:

It was a great pleasure to meet you again in Washington, and particularly to see your material. I did not get in to hear you present it at the meeting, chiefly because I had had a more satisfactory demonstration in connection with the plants themselves. I heard, however, that the fifteen minutes left at your disposal were about the best received fifteen minutes of the program.

The elderly Dr. William Williams Keen, president of the American Philosophical Society, author of the six—later eight—volume *Keen's System of Surgery*, and one of the truly great figures of medical history, wrote on May 29, 1916:

Thank you for supplementing my very interesting though too brief visit with you recently in your greenhouses. The work that you have done on the pathology of cancer in plants is very extraordinary.

I shall hope very sincerely that our Committee may be able to arrange with you for a paper at the General Meeting of the American Philosophical Society next April when you would have an audience of about one-hundred scientists from all over the country. A paper at that time I should hope would contain the *new* matter that you have accumulated of late as we always desire, of course, for the these General Meetings in April papers of original research as far as possible.

Smith replied on August 10: "I am finding my experiments so interesting this summer that I doubt if I shall be able to get in a vacation until some time in the fall. I am obtaining very wonderful results with the teratoids and also by direct chemical stimulation. I now think I shall be able to demonstrate chemical stimulus."

He enclosed a photograph of a tumor "produced in the top of a tobacco plant by a pure-culture inoculation (needle pricks only), the top being cut off in the middle of an internode and the pricks made into the cut surface. An irregular tumor covering the whole top [had] developed and out of this dozens of young leafy shoots [were] growing." Except when inoculated, such cut stems never developed growths. "If the inoculations are made toward the base of the plant," he said further, "I get only leafy shoots; if they are made well toward the top of the plant just before blossoms develop I get tumors containing both leafy shoots

and fugitive blossom buds." It was possible, furthermore, "to inoculate tobacco plants so as to obtain tumors with no shoots."

On August 9, Smith answered a letter from Dr. Charles H. Mayo:

Permit me to congratulate you on your election to the presidency of the American Medical Association. It is a high honor and one well deserved. I am getting some wonderful results this summer with my teratoid crown-galls, photograph of one of which I enclose. The needle pricks were made in the cut end of the stem (main axis of the plant) and you will see that a good sized tumor has developed, covering the whole top of the plant, and sprouting from various parts of it are dozens of embryonic fragments (diminutive tobacco plants). . . . I have a dozen other plants in the houses at the present time with tumors as strange as the one shown. . . . I am working mainly at present with tobacco, castor oil bean, and the common red house geranium. The same sort of tumors can be produced, as I now know, on many other plants. I am slowly gathering material in the shape of notes and good photographs for a special paper on atypical teratomata in plants due to bacterial inoculation. I am working also on the chemical end of the subject this summer and have obtained some quite interesting and very striking results, which will I hope form a basis of another paper. . . .

In June 1917 Dr. Mayo, in his presidential address,⁶² gave Smith the following splendid recognition: "We are proud of our agricultural department and its investigations as to the causes, control and cure by serums of the diseases of animals and the destruction of parasitic hosts. The work of Erwin F. Smith on plant diseases is monumental, especially his discoveries as to the causes of certain plant tumors. . . ."

One month before, April 1917, in *Proceedings of the National Academy of Sciences*,⁶³ had been published Smith's communication of the previous January 26 to the Academy, entitled, "Chemically Induced Crown-galls." In the January 29 issue of the *Journal of Agricultural Research*⁶⁴ had appeared Smith's even more important study, "Mechanism of Tumor Growth in Crown Gall." Previous to these, moreover, in the October 27, 1916, number of *Science*⁶⁵ Smith's report on plant tumors formed by products of bacterial growth, "Tumors in plants," had excited wide attention. Reprints

⁶² *Jour. Amer. Med. Ass.* 68(23): 1675, June 9, 1917.

⁶³ 3: 312-314, Apr. 1917.

⁶⁴ 8(5): 165-184, Jan. 29, 1917.

⁶⁵ 44(1139): 611-612, Oct. 27, 1916.

of another paper published in *Science*,⁶⁶ originally an address delivered before the Washington Academy of Sciences on May 11, 1916, had been circulated. Smith had said that the full title of the last paper should be "Further Evidence that Crown Gall is Cancer, and that Cancer in Plants because of its Variable Form and its Bacterial Origin offers Strong Presumptive Evidence Both of the Parasitic Origin and of the Essential Unity of the Various Forms of Cancer Occurring in Man and Animals." Any one of these studies may have been read by Dr. Mayo and evoked his tribute to Smith's work. But, since on February 15 Dean and Director A. F. Woods—soon to be President Woods of the Maryland State College of Agriculture—informed Smith that he planned to be in Rochester, Minnesota, the following Saturday and would take with him a copy of a recent paper by Smith and present it to Dr. Will Mayo, probably the study which Dr. Charles Mayo had read was "Mechanism of Tumor Growth in Crown Gall."

In this study, Smith had concluded that

it would seem . . . that in local osmotic action (possibly in some stages chemical action also) of various substances (aldehyde, acetone, alcohol, acids, alkalies) thrown into cells and diffusing from them in various directions, as the result of the metabolism of a feeble intracellular parasite or symbiont together with the resultant counter movements of water and food supply we have, in crown gall at least and presumptively also in animal neoplasms, the explanation of tumor growth—that is, of that extensive multiplication of cells in opposition to physiological control which has so long puzzled pathologists and all students of overgrowths.

In crown galls, [he began by saying (pp. 167-168),] the removal of growth inhibitions is brought about, I think, by the physical action of substances liberated within the tumor cells as the result of the metabolism of the imprisoned bacteria. . . . If the cell proliferation in crown gall is due to substances liberated within the cell by the parasite, as it seems reasonable to suppose, they must be substances either identical with or at least not differing greatly in their physical or physiological action from those acting on the non-parasitized cell during normal growth and division. . . . In our search for the direct exciting cause of tumors we need consider only the excretions of known tumor-producing organisms; but since little or nothing is known concerning the growth-exciting substance, or substances, produced in animal neoplasms, or liberated in plants by the various gall-forming fungi, or by gall flies and gall nematodes, we may for the present confine our attention exclusively to the bacterial tumors of plants. This narrows down the problem to a few species of bacteria and even some of these—

⁶⁶ 43(1121): 871-889, June 23, 1916.

that is, *Bacterium savastanoi* (the cause of olive tubercle) and *Bact. beticola* (the cause of beet tubercle)—are negligible so far as the purposes of this paper are concerned, because I regard the tumors they produce as granulomas, not neoplasms. In fact, thus far in seeking for the physical-chemical origin of neoplasms I have sought only for answers to the following questions:

(1) What are the products of the bacterial metabolism of *Bact. tumefaciens* . . . ?

(2) Are any of these products capable of inducing cell proliferation when injected into the growing plant?

(3) If so, is this substance a sufficiently common product of other bacterial and nonbacterial tumor-producing parasites so that it may be regarded hypothetically as produced by all of them and consequently as the exciting cause of all bacterial, fungus, nematode, cynipid, and other vegetable galls, or must we suppose that various chemical growth excitants exist?

(4) And finally to what extent can these facts be supposed to apply to human and animal neoplasms?

His answers or hypotheses were drawn from a wide range of experiments and included comments on plant diseases "of undetermined origin but suspected of being due to parasites" (p. 183) such as curly top of sugar beet, mosaic of tobacco, and other diseases today attributed to viruses. At one point in his paper he said that his recent experiments with crown gall teratoids had led him to "believe that not only the origin of fasciations but also of many other duplications is to be sought in local and feeble infections by a variety of deep-seated microorganisms" (p. 181).

While the origin and growth of tumors, benign or malignant, was regarded by Smith as more than a chemical phenomenon, he detailed many experiments making use of chemicals to trace the sources of tumor inducement and growth. Especially was this so when describing his experiments made to ascertain the nature of the action and changes wrought in plants by chemical by-products of the bacterial metabolism of *Bacterium tumefaciens*. Experiments were made with and without the use of the crown gall organism, sometimes only with the use of chemicals to study the phenomena. But the mechanism of tumor formation and growth was found, amid the complex of causes and conditions, to be fundamentally osmotic rather than chemical. Local osmotic pressure was believed to be the beginning of all tumor growth (p. 175). Cell proliferation is caused by something more than

bacterial infection. The plant tumor of *Bacterium tumefaciens* was more than the wilt of cucurbits (*Bacillus tracheiphilus*) and the softrot of potato (*Bacillus phytophthorus*). A physical-chemical action was necessary to explain tumors, and Smith believed

that any soluble substance whatsoever, except a killing, a plasmolyzing, or an oxygen-absorbing substance, if continually liberated in excess locally in tissues would be competent to induce tumor formation. . . . [D]ilute ammonia causes intumescences and [it also seemed] probable that ammonia liberated within the cell in small quantities by the imprisoned bacteria must be one of the causes of the excessive and abnormal cell proliferation in crown gall. Probably amin compounds also help to determine it. Since an acid and an alcohol are likewise produced by the crown gall bacteria and this alcohol and this acid (as well as many other acids) in pure dilution and also in combination with ammonia caused galls or intumescences in my experiments; the acid (or acids), the alkalis, and the alcohol must, I believe, act together in producing the tumors, and *osmotically rather than chemically*. . . . [C]hanges in stimulus can produce changes in structure. . . . (pp. 177, 183)

Smith's study of "Mechanism of Tumor Growth in Crown Gall" must have been one of his papers of this period which prompted Dr. Mayo to say to the American Medical Association:

The work of Erwin F. Smith on plant diseases is monumental, especially his discoveries as to the cause of certain plant tumors which show again that our bacteria and insect chemi[cals] are the prime offenders through the development of their varying acids, which may be stimulating or destructive to other cell life, causing tumors or decay. We should appreciate and aid the work of these departments.

Dr. MacCarty had kept Smith's work before the laboratory staff members of the Mayo Clinic. In a letter of July 4, 1916, he told Smith he had had lantern slides made of Smith's photographic illustrations. The impression made had been "very good indeed. What you are doing with plants, I have been doing with animal tissues," MacCarty wrote, "and I feel that we are presenting a correct, modern and scientific conception of neoplasms and their biological position. . . . I feel that we are on the right track so far as the histogenesis of neoplasms is concerned. I also feel that you have made great strides in the field of etiology."

The year 1916 had been a banner year for Smith in his research work on crown gall. He apprised Thaxter of this in a letter of October 21:

This year's work on crown gall has been more interesting to me than work of any previous year for two reasons. First, I have found a new type of the tumor full of abortive sprouts, and second, I have been able to produce small tumors with by-products of the bacterial growth i. e. by chemicals, the bacteria being absent.

This summer I have obtained many fine teratoid tumors on tobacco and on *Pelargonium* by inoculating the crown gall bacteria into the cut end of stems (middle of internodes) and some of these internodal tumors have given 500 separate buds. The thing is so astonishing that I would not have believed it possible.

Plant anatomist Irving W. Bailey of the Bussey Institution for Research in Applied Biology had begun to exchange scientific materials and papers with Smith. Physiologist W. B. Cannon of Harvard Medical School continued to express his appreciation. Dr. Adami, now with the medical service of the Canadian armed forces overseas, sent an "Hurrah! I am sure that you are on the right track and to me it is peculiarly delightful that this experimental confirmation is obtained in a sister science." June 22 he had written again that Smith's papers were

extraordinarily interesting and [he had] read them with intense appreciation, more especially as these teratoid studies seem to support my view that it is not so much the presence of the actual irritant or stimulant within the cells that is to be regarded as the essential factor as that, to employ your words, the changed stimulus (which may, as in your cases, be of microbic origin, or in other cases may be of other nature) induces the growth habit, which once properly started, continues with the development of a tumour mass. I am glad to see in your *Journal of Cancer Research* paper, page 253, that your observations are leading in this direction. I do most heartily congratulate you. These teratoid Crown Galls are a splendid advance forwards.

Chemist Jerome Alexander of New York City, pioneer worker with the ultramicroscope in America, in June 1916, believing that Smith was opening up a new way toward solving the cancer problem, offered to cooperate with him in examining specimens with the use of the ultramicroscope. M. C. Marsh, biologist of the New York State Institute for the Study of Malignant Disease, located at the Gratwick Laboratory at Springville, where Dr. Gaylord was still director, read Smith's article in *Science*, "Further evidence that crown gall of plants is cancer," and was moved to write on July 16, "That paper I hope and think is a beacon light in circumambient darkness. My congratulations. I have been

trying for 1½ years to start epithelial (mammary) growths in mice with something besides tumor cells. Results not yet much, but not completely negative."

Leo Loeb, of the department of comparative pathology of Washington University Medical School, St. Louis, seeing Smith's "interesting papers" in *Science* and the *Journal of Cancer Research*, sent a collection of their more recent publications. As has been pointed out, Loeb's transplantation to laboratory animals of a rat thyroid sarcoma had preceded the experimental work of even Jensen in Denmark.⁶⁷ In his address, "Further Evidence that Crown Gall is Cancer,"⁶⁸ Smith had ventured the opinion that heredity is not a sufficient cancer explanation. "The same thing was said repeatedly," Smith argued,

of tuberculosis prior to 1884. Now we see that heredity furnished the canvas but could not paint the picture. Miss Maude Slye's work on heredity of cancer in mice is astonishing and praiseworthy, but I do not feel sure that a similar picture could not be obtained by breeding together tuberculous animals, indeed I am quite certain that the results of such experiments would be a vastly increased number of tubercular animals, and if we knew no more about the cause of tuberculosis than we do about the cause of cancer, the interpretation of the results would be entirely wrong, *i. e.*, they would be ascribed wholly to heredity, whereas we know that two factors are involved: (1) heredity; (2) infection. I do not think Miss Slye has established the fact that cancer follows Mendel's law.

Loeb had not only taught pathology and experimental pathology at the University of Illinois and the University of Pennsylvania but during 1902 he had been an experimental pathologist with the New York State Pathological Laboratory at Buffalo and until 1915 had been for several years director of the department of pathology at the Barnard Skin and Cancer Hospital of St. Louis. Accompanying his collection of publications, he sent a letter to Smith, written on July 3, 1916:

I was not sure that you had seen our communications dealing with the heredity of cancer, inasmuch as you mention in your resumé in *Science* only Miss Slye's papers. I believe that I was the first one to undertake and to publish extensive studies on the heredity of cancer in isolated strains in animals of the same species, following them through a series of generations, establishing the tumor incidence and tumor age on a large scale.

⁶⁷ Erwin F. Smith, Twentieth century advances in cancer research, *op. cit.*, 297.

⁶⁸ *Op. cit.*, reprint, 4.

After I had begun my work in Granby, Mass[achusetts], in conjunction with Miss Lathrop, I went to see Miss Slye who I knew was at that time carrying on investigations on heredity of characters other than tumor incidence, in the Department of Zoology under Doctor Tower. I suggested that her's might be suitable material for studies of heredity in cancer, and proposed joint work.

I believe that our methods are not the same. While Miss Slye publishes isolated family trees, Miss Lathrop and myself give the history of succeeding generations of whole strains. Our conclusions are not entirely identical, although her results confirm the great importance of hereditary factors.

I think it very probable however, as I expressed in my paper in Science, that given a large quantity of external stimulation—microorganisms may supply under certain conditions this stimulation—hereditary factors may be dispensed with.

I am looking forward with much interest to your further publications.

Smith's position on heredity in cancer would be more fully elaborated later. He was not presuming to be an authority on medicine or animal pathology but a plant pathologist. His interest in this question was evident. He believed there were hereditary aspects. But his papers argued, from the standpoint of general or comparative pathology, the infectious nature of malignant tumors and that crown gall should be recognized as a neoplasm in plants. From its study light could be derived on the infectious nature and action of animal and human neoplastic growths.

Many surgeons, physicians, and teachers encouraged Smith to continue his studies. Dr. Robert Holmes Greene, surgeon of New York City and author in 1917 of a text book on surgery and in 1918 to publish a volume on *Cancer*, its nature, causes, diagnosis and treatment, wrote:

You have presented a strong case in favor of the infectious nature of cancer. On the other hand, Miss Maude Slye and Dr. Leo Loeb in their work strongly favor the theory of inheritance. It is indeed very interesting to see how the different experiments work out in their results. My own experience would seem to indicate that cancer is both hereditary and infectious.

Dr. Willy Meyer, another New York surgeon, widely known for his method of radical operation for carcinoma of the breast and other important contributions to surgical practice, wrote Smith on May 13, 1916,

Permit me to thank you once more . . . for the great courtesy shown me in conjunction with the colleagues who visited you on Tuesday afternoon. I was more than pleased to meet you again and see your wonderful specimens and convincing results in producing tumors in plants. I have no doubt that the future will prove the microbial origin of malignant tumors also in the human being. I have been looking for such proof for many years, and am delighted that it has now been definitely found as far as the vegetable kingdom is concerned. One's gratitude goes out toward the man who, by persistent and painstaking work, has succeeded in proving this contention.

Smith's papers had strengthened more the beliefs of Dr. W. B. Coley who, through his official capacity as chairman of the Archer M. Huntington Cancer Research Fund, was still rendering aid and encouragement. He wrote on May 24: "I agree with you that we are on the eve of important discoveries in the field of etiology of malignant tumors in man. For many years I have been absolutely convinced that such tumors are of extrinsic origin due either to the same, or slightly different varieties of microorganisms. Your wonderful work has brought the origin of these tumors nearer solution. I cannot tell you how much I appreciated your kindness in letting me see so much of your work while in Washington." On November 21 he sent for cultures of *Bacterium tumefaciens* "with the idea of taking up a series of experiments with them on animals. . . . Our Huntington Fund," he advised, "is now in a position to take up this work and we should very greatly appreciate any suggestions from you with regard to how to go about it."

Founded in 1899 by Caroline Brewer Croft, the Cancer Commission of Harvard University was a part of the medical school. At least, the commission, directed by Dr. E. E. Tyzzer, was made up of representatives from the fund, the college, and the medical school. Drs. Theobald Smith and W. T. Councilman represented the medical school. On January 24, 1918, Dr. Councilman sent Erwin Smith some constructive criticism, having to do evidently with the latter's monograph, "Embryomas in Plants. (Produced by Bacterial Inoculations),"⁶⁹ an expansion of an address given on December 18, 1916, before the Johns Hopkins Medical Society and entitled. "Is there any real relation between crown gall of plants and cancer?" Said Dr. Councilman:

⁶⁹ *Johns Hopkins Hospital Bull.* 28(319): 277-294, Sept. 1917.

I have read your monograph with very great interest and think it a valuable contribution to the subject of growth, but you will forgive me if I fail to see the exact relation to tumors. The fact that growth can be stimulated in various ways, both mechanical and chemical, or possibly the mechanical would resolve itself ultimately into the chemical, is a very important fact. There is such an enormous difference between the multipotent tissues of the plant and that of the animal. The explanation of tumors, when it comes, must take in all cases, the congenital and the senile tumors, as well as those in between. I feel that the senile tumors, which are more prominent in animals than in man, play a very great part, as for instance in dogs, where we get a general tumor formation which affects the tissues generally it is true, but certain tissues very much more, and where the tumors are never unicentric but multicentric. I think, however, that you should follow out your work continually, and that it does throw light on tumors in enabling us better to comprehend the essential factor which is growth.

In 1924 Smith and Councilman would meet in Rome, Italy, and, with crown gall photographs and actual tumor specimens from live oak trees before them, plant pathologist would explain more fully to medical pathologist the points of analogy between plant and animal tumors.⁷⁰

From Philadelphia, Chicago, St. Louis, and many other cities over the country highly reputable surgeons sent Smith letters expressive of interest in, and appreciation for, the value of his research disclosures. A few may be listed: Dr. Herman Morris Adler, Dr. Henry W. Abelmann, Dr. M. G. Seelig, Dr. Solomon Solis Cohen, Dr. Henry W. Stelwagon, Dr. Arthur A. Eisenberg, Dr. Bradley M. Patten, Dr. André Crotti, Dr. Rollin Howard Stevens, and many others whose letters or work will be considered elsewhere in this book. Dr. Adler wrote: "Together with all the pathologists of the world, I have been extremely interested in your work." Some of these doctors, interested in research, began experiments of one sort or another making use of the crown gall organism. For instance, Dr. Stevens obtained cultures of *Bacterium tumefaciens*, transplanted them several times on plain agar, and inoculated several red geraniums, all with a view to studying the effect of X-ray on the viability of the cultures, the tumor produced, and on immunity of the plant. He called to Smith's attention a recent editorial in the *Journal of the American Medical*

⁷⁰ See p. 610 of this book.

*Association*⁷¹ which elaborately described and characterized his work as bringing forward "important evidence in favor of the parasitic theory" of cancer etiology.

Once before, the *Journal*,⁷² in an editorial, "Is Cancer of Infectious Nature?" had mentioned Smith's work. One of the conclusions was that he was making out "a very strong case in favor of his view, and . . . that the view of the infectious cause of cancer in general [was] strengthened by his work." The "absence of complete analogy between the structure of plants and that of animals (and the fundamental difference in metabolism)" still held in abeyance the question whether crown gall was cancer in plants. Nevertheless, his studies of plant tumors were being considered, among other points, together with the experimentally induced tumors in animals (chicken sarcoma) announced by Peyton Rous. Rous during 1917 sent Smith two letters, one of which said: "You seem to go on gaining new and important facts all the time. Best wishes for those yet to come!"

During this period, Dr. W. J. Mayo sent Smith several complimentary letters. So did Dr. E. O. Jordan of Chicago University's department of hygiene and bacteriology. He believed Smith's work "most suggestive for its bearings on human pathology." Several workers at the New York State Institute for the Study of Malignant Disease—Dr. Marsh, biologist, Dr. John A. P. Millet, internist, and Dr. G. H. A. Clowes, biological chemist, aside from Dr. Gaylord—consulted Smith, especially in those instances where plant tumors were being studied comparably with animal tumors in matters such as conductivity and permeability determinations of normal and diseased tissues. Copies of Smith's crown gall papers were distributed through the Institute by Dr. Marsh, and a set sent to Dr. Gaylord who for a while was with the United States armed forces. Dr. Millet, planning experiments with animals similar to Smith's experiments chemically inducing crown galls on *Ricinus*, cauliflower, and other plants, wrote on March 29, 1917: "I have felt more and more since being here that we must look to some physical, or possibly physico-chemical explanation for tumor growth, and I therefore found much encouragement in reading your own beliefs." In 1918 Smith and his laboratory

⁷¹ 67(2): 123-124. Dr. George H. Simmons, editor of the *Journal*.

⁷² 59: 448, Aug. 10, 1912.

supplied Dr. Clowes with tumor and gall materials on sugar beet, *Pelargonium*, tobacco, and hop. An exchange of letters indicates that these plant materials were used in experimental work aimed at "determining the relative permeability of the tumor and normal tissues under normal conditions and under the influence of the various agents brought to bear on them." Dr. Clowes explained on April 2 that he was "making conductivity and permeability determinations on the materials . . . in comparison with cancerous and normal tissues derived from mice and human beings. Also the influence of electrolytes on the equilibrium of the tissues in question." Many doctors, students of the cancer problem in one capacity or another, sent Smith letters assuring him of their interest in his studies and the high value they placed on his research results. Students of such calibre as Dr. J. Collins Warren, professor emeritus of surgery at Harvard Medical School, Dr. Douglas Symmers, pathologist of Bellevue Medical College, Dr. Stanhope Bayne-Jones, pathologist and bacteriologist, Dr. Theodore C. Janeway, physician-in-chief of Johns Hopkins Hospital, Dr. Walter C. Burket of Johns Hopkins Medical School, and Dr. John J. Abel of Johns Hopkins's department of pharmacology were among many others and reveal the range of various scholarly work to which the studies had some pertinence. Such leaders as Dr. Flexner, Dr. Hurd, Dr. Keen, Dr. Cushing, Dr. Prudden, and Dr. Cullen continued to express their appreciation and belief in the value of Smith's studies. Indeed, Dr. Warren wrote that the crown gall studies were "of the greatest interest to those engaged in the cancer problem," and Dr. Burket told Smith, "as Pasteur approached the question of infectious diseases thru fermentation so you may approach the great problem of new growth and cancer thru your studies in plant tumors."

Biologists also were interested. In 1916 Director C. B. Davenport of the department of genetics of the Carnegie Institution and the Station for Experimental Evolution at Cold Spring Harbor read Smith's "Studies on the Crown Gall of Plants its relation to human cancer" and began an exchange of letters. In February 1917 he answered a letter from Smith and expressed his delight that he had "succeeded in producing the small plant tumors in the absence of many of the bacteria. This brings," he said, "the phenomenon in plants in closer relation to that in animals." The

next day he read "Mechanism of tumor growth in crown gall," and wrote again: "It seems to me tremendously important that you can by opposing the action of the growth inhibitor thru ammonia and various diverse organic acids encourage cell proliferation and produce curious races." Geneticist E. M. East of the Bussey Institution for Research in Applied Biology wrote Smith the following year after reading his paper, "Embryomas in plants," and congratulated him on "getting results of the greatest interest and importance."

The month April 1917 was distinguished by two addresses given by Smith before important gatherings of scientists, and at each he spoke on the subject on which he was now an authority. On April 13, reading a paper before the American Philosophical Society on "Mechanism of Overgrowth in Plants,"⁷³ he reiterated his view that

the growth in crown gall [is] due primarily to a physical cause, viz., to an increase in the osmotic pressure due to the heaping up locally of various soluble substances excreted by the bacteria as a result of their metabolism. . . . The reason I have for thinking the phenomenon of plant overgrowth is primarily physical is the fact that it can be obtained by a great variety of substances not the products of parasites, anything in fact, which disturbs tissue equilibriums without destroying cells, seems to be capable of causing overgrowths, which cease, of course, as soon as the stimulus is exhausted. . . . The first crown galls I studied seemed to me to be overgrowths of the conjunctive tissues and most of our many inoculations up to the end of 1915 produced that type of tumor which corresponds, I believe, to overgrowths of the connective tissue of animals and which I have called plant sarcomas. We had found indeed, as early as 1908-9, and had produced by bacterial inoculation, plant tumors bearing roots, but the full meaning of this discovery, as related to cancer, did not occur to me until early in 1916, when I found crown-gall tumors bearing leafy shoots on some of our inoculated hothouse geraniums. Beginning with this discovery I made numerous inoculations in the leaf axils of various plants which resulted in the production of leafy tumors, and subsequently I produced them freely on leaves and on cut internodes where no buds occur normally. Tumors bearing roots have also been produced by us on the top of plants, and in one cut internode of tobacco I succeeded in producing a tumor which bore flower buds. These perishable root-bearing and shoot-bearing tumors I regard as plant embryomas and have so described them.⁷⁴

⁷³ *Proc. Amer. Philos. Soc.* 56: 437-444, 1917.

⁷⁴ *Idem*, quotations at pp. 441-442, 442-443.

When Director V. A. Moore of the New York State Veterinary College had read in 1916 Smith's paper, "Further Evidence that Crown Gall of Plants is Cancer,"⁷⁵ he was "much interested in the statement relative to the variety of lesions due to the same organism. There is," Moore wrote, "some analogy between this statement and the one made by Vaughan in regard to the poison group of the protein molecule." Smith's concluding plea in his paper then had been: "Cancer, according to my notion, is a problem for the experimental biologist and the bacteriologist. The morphologist has gone as far as he can go and the energy of cancer research from now on must, I believe, be turned into new channels, if we are to expect results commensurate with the needs of humanity. . . . Whatever else may be denied," he had said in his discussion, "*the bold fact now stands out demonstrably that all the leading types of cancerous proliferation can be produced in plants with one microorganism*" and the "four principal groups" were, as Smith viewed the problem, the sarcomas, the carcinomas, the so-called mixed tumors, and the embryonal teratomas. In his paper, "Embryomas in Plants,"⁷⁶ he said: "Whether epitheliomas and carcinomas can also be produced in plants by bacterial inoculation remains to be determined, but I believe they can be," and, while admitting that this subject was for "further experiment," he seemed to have no doubt of the validity of what he called "plant sarcomas" and "plant embryomas" and of his view that "the kind of tumor depends on the type of cells stimulated."⁷⁷ This subject will be discussed further in Chapters XI and XII.

What was more important than Smith's classifying of various types of tumors in plants, during this period, was his splendid work on the mechanism of tumor growth in crown gall. Dr. Moore believed his paper on this subject "most excellent work. . . . It seems to me," Moore wrote in February 1917, "that you have clinched your points so thoroughly that future investigations cannot overthrow your deductions." Professor of Physiology Jacques Loeb⁷⁸ told Smith, "You have unquestionably opened

⁷⁵ *Science, n. s.*, 43(1121): 871-889, June 23, 1916.

⁷⁶ *Op. cit.*, recapitulation, point 14.

⁷⁷ Mechanism of overgrowth in plants, *op. cit.*, 442.

⁷⁸ Of the University of California but letter written on stationery of the Rockefeller Institute for Medical Research.

up a most interesting field for further research." In his paper Smith acknowledged that had he read, when it appeared in 1913, Loeb's "latest remarkable book," *Artificial Parthenogenesis and Fertilization*, his work would have been advanced by "at least two years." Loeb's "many positive and splendid results with acids on animal eggs" were correlated with his work more explicitly in his paper, "Mechanism of overgrowth in plants."⁷⁹ His first illustrative slide showed "chemical findings" and on this slide, he pointed out,

I have starred the substances with which I have now produced overgrowths in plants and have italicized those which Dr. Jacques Loeb had previously found in his experiments on animal eggs to be most effective in causing unfertilized eggs to begin to grow. That there should be so many of these egg-starting substances excreted by a tumor-producing parasite is not only astonishing but extremely suggestive. All of them are substances which pass readily through protoplasmic membranes.

TABLE I. Showing Products of *Bacterium tumefaciens*.

*Ammonia	Alcohol	*Formic Acid
*Amines	Acetone	Carbonic Acid(?) [supplied by Smith for reasons given]
*Aldehyd	*Acetic Acid	

Smith directed attention "especially to the substances the names of which [he had] starred as compounds with which to experiment singly and combined, and in a great variety of dilutions. With each one of these substances, in the absence of bacteria," he said, "I have obtained on suitable plants decided overgrowths, growths which I think I am warranted in designating as incipient crown galls." He showed slides presenting results obtained with ammonia, dimethylamine, formaldehyde, acetic acid, and formic acid. He reminded his audience of Von Schrenk's intumescences on cauliflower from copper salts, and he explained in some detail Dr. Bernhard Fischer's overgrowths of epithelium in rabbit's ears (1906) caused by injections of scarlet red and indophenol. Other substances than these which can cause overgrowths were now known, substances some of which were known to be products of tumor-producing parasites. The new investigations were a challenge.

On April 19 Smith participated in ceremonies which dedicated

⁷⁹ *Op. cit.*, 439-440.

the laboratory building and plant houses of the Brooklyn Botanic Garden. The topic of his address was "The Relations of Crown-Gall to Other Overgrowths in Plants."⁸⁰ "My own belief," he said, "is that all overgrowths are correlated phenomena, are the response of the organism to essentially similar (but not necessarily identical) stimuli, the visible difference in response when brought about by parasites being due to number and location of the parasites, age and kind of tissues invaded, and volume, direction, and velocity of the stimulus exerted." He admitted that "our work on crown gall has opened up more problems than it has settled," but out of it all would come "an eventual better understanding of the whole mechanism of overgrowth." He told of recently producing "galls with diluted crown-gall products," a procedure which suggested "a new method of attacking gall problems in general, especially those in which the gall parasites can be cultivated pure in sufficient quantity for chemical analysis, e. g., various fungi. . . ."

This technique, he believed, might be extended to the study of plant disorders other than galls. He suggested that

the application of chemical substances in various dilutions to growing plants or plant organs, such substances in particular as are known or suspected to be produced by living organisms, or are present in soils as a result of decomposition, may prove to be a hopeful way of attacking certain unsolved and difficult problems in plant pathology, e. g., the aetiology of the mosaic diseases, the cause of various growth limitations, etc.

In his address before the American Philosophical Society, he urged research "on the origin, through absorbed poisons, of certain plant diseases whose etiology [was] very obscure, such as peach yellows, peach rosette, and the various mosaic diseases." His discoveries in crown gall, he believed, might have "interesting bearings" on still other points of research: on the origin of insect, nematode and fungous galls; on the formation of thyloses in vessels; on the origin, through absorbed poisons, of various plant and animal monstrosities; on various problems of modification by slight changes in environment; on the possibility of normal wide distribution of dormant germ-cells among somatic cells; and on the etiology of various human and animal tumors.⁸¹

⁸⁰ *Brooklyn Botanic Garden Memoirs* 1: 448-453, 1918.

⁸¹ Mechanism of overgrowth in plants, *op. cit.*, 443.

By this time it had been shown that the causal agents of virus plant diseases "pass filters having pores so minute that they held back the smallest bacteria,"⁸² and virus diseases of plants were being differentiated from those believed due to bacteria. By 1936 more than three thousand papers, exclusive of short reports and casual references, would be published on these subjects, and about two-thirds of them would "deal with the virus diseases of the following six plants: potato, sugar cane, tobacco, tomato, sugar beet and cucumber."⁸³ By 1926 Smith would say⁸⁴ of the work on these maladies:

Known first from tobacco, they have been discovered in the potato and in a great variety of cultivated plants where they had hitherto been overlooked or had attracted but little attention, probably because they were not then so prevalent. They are now recognized as among our worst plant diseases and the obscurity of their origin renders them doubly interesting. . . . Up to this time the cause or causes remain in doubt, but much has been learned respecting signs of these diseases, host plants, and methods of transmission.

In Connecticut the further work on tobacco mosaic by G. P. Clinton⁸⁵ and at the Bureau of Plant Industry by H. A. Allard of the office of tobacco and plant nutrition investigations had added much to the knowledge of this disorder,⁸⁶ the virus of which in more recent years has been isolated at least in the form of crystal protein by W. M. Stanley and his co-workers at the Rockefeller Institute.⁸⁷ More recently, too, the very important work of Allard (and W. W. Garner) on the phenomena of "photoperiodism" has tended to overshadow the former's definitely valuable work on mosaic disease of tobacco. Smith, however, believed that the work of Allard, Schultz, and others on virus plant diseases had made the Bureau of Plant Industry one of the five principal research centers for their study in America. The other four were the work of Doolittle, Johnson, and others at the University of Wisconsin, the work of Eubanks Carsner in the far

⁸² L. O. Kunkel, Virus diseases of plants: twenty-five years of progress 1910-1935, *Brooklyn Botanic Garden Memoirs* 4: 51-55, May 7, 1936.

⁸³ *Idem*, 51.

⁸⁴ Fifty years of pathology, *op. cit.*, 35-36.

⁸⁵ *Conn. Agric. Exp't Sta. Rept.* 6: 416 ff., 1914; Charles Thom and E. M. East, George Perkins Clinton, *Natl. Acad. of Sci. Biog. Memo.* 20: 183-196, 1939.

⁸⁶ See the *Journal of Agricultural Research and Phytopathology* (1915-1917).

⁸⁷ G. Seiffert, *Virus diseases in man, animal and plant, op. cit.*, 34 ff.

west, the work of B. M. Duggar at the Missouri Botanical Garden, and that of L. O. Kunkel at the Boyce Thompson Institute for Plant Research at Yonkers, New York.⁸⁸

Kunkel, a graduate of the University of Missouri and the Shaw School of Botany, had secured in 1914 his doctorate of philosophy degree under Dr. R. A. Harper at Columbia University. Granted a scholarship, he had spent the next years in graduate study at the University of Freiburg, Germany. The comparatively new subject of virus plant disease research had attracted him, and, when an opportunity with the Bureau of Plant Industry was offered, he accepted. While at Columbia he had been a research assistant, and at the University of Missouri he had been an assistant in botany.

His first work as a pathologist with the Department of Agriculture was on potato diseases. In 1913 W. A. Orton had published⁸⁹ on "Leaf roll, curly leaf, and other new potato diseases" among which a mosaic disease similar to the mosaic of tobacco, tomato, and other crops was mentioned. Kunkel, beginning his work under Dr. I. E. Melhus and Orton, spent his summers in Maine and his winters at Washington. In one of his first interviews with officials of the Department, he had told Dr. Charles Brooks that he wanted to study peach yellows, and in the autumn of 1914 he became acquainted with Dr. Smith. So commendable was his work regarded by Smith that in 1916-1917 he recommended Kunkel to Dr. Van Hall of the Instituut voor Planten-Ziekten en Cultures for an appointment at Buitenzorg, Java. World War I made it impossible for Kunkel to accept this appointment. But early in 1919 Smith again recommended him to Dr. H. A. Lyon of the Hawaiian Sugar Planters' Experiment Station and persuaded him to accept the offer to go to Hawaii and study mosaic disease of sugar cane. Kunkel left the Bureau and went to Hawaii in 1920.

Mosaic disease of sugar cane recently had been found in Louisiana and other southern states, and since the middle of the decade had caused severe lossess in parts of Puerto Rico. E. W. Brandes, formerly pathologist of the Puerto Rico Agricultural Station at Mayaguez and now in sugar-plant investigations with

⁸⁸ Fifty years of pathology, *op. cit.*, 35-36.

⁸⁹ *Phytopathology* 3(1): 69, Feb. 1913.

the Department at Washington, had published⁹⁰ in 1919 on "The Mosaic Disease of Sugar Cane and Other Grasses." In 1917 he, while of the department of plant pathology of Cornell University, had submitted to Smith his description of *Fusarium cubense* Smith, cause of the banana blight in Puerto Rico. He gave a short account of his "experiments proving [its] pathogenicity" and its seeming identity with "the 'Panama disease' of other tropical American countries." Smith had studied it from Cuban material.⁹¹ At the Washington greenhouses, Brandes also proved that mosaic disease of sugar cane is transmitted by the corn aphid, *Aphis maidis*, and his work was followed by field studies in Puerto Rico.⁹²

On October 9, 1918, J. B. Rorer, mycologist and secretary of the Board of Agriculture, Port-of-Spain, Trinidad, wrote Smith that he was returning to Ecuador "perhaps for good. At any rate," he said, "I am going for a year and if things work out all right I will probably stay on. The cacao association wants me to start a station for the study of cacao diseases following the recommendations I made to them in my report last year." In November 1918⁹³ J. R. Johnston, also formerly of Smith's laboratory and now with the United Fruit Company in charge of their work in the study of tropical plant diseases, reviewed Rorer's report to commend its subject matter and "emphasize the necessity for more pioneer work in plant pathology and economic entomology in tropical American countries." Sometime, a month or so later, was made known Rorer's acceptance of a position as mycologist of the Association de Agricultores del Ecuador at Guayaquil.⁹⁴ Johnston in 1916 had published an article in *Phytopathology*⁹⁵ on "Phytopathological Work in the Tropics," in which was outlined the efforts being made by several inadequately equipped but functioning laboratories: two in Puerto Rico and one each in Surinam, Demerara, Trinidad, Barbados, and Jamaica. A station in Mexico was also in existence but practically inactive. A privately maintained station had been located in Costa Rica. Johnston's article was sent from the Estación Experimental Agronomica Santiago de las Vegas, Cuba. Despite the difficulties of pioneering work,

⁹⁰ Bulletin 829, U. S. Bur. of Plant Industry, professional paper, Oct. 29, 1919.

⁹¹ Letter, Brandes to Smith, Feb. 6; see also, *Phytopathology* 9(9): 339-389, Sept. 1919.

⁹² *Phytopathology* 13(1): 24 ff., Jan. 1923.

⁹³ *Idem*, 8(11): 550.

⁹⁴ *Idem*, 9(1): 48, Jan. 1919.

⁹⁵ 6(5): 381 ff., Oct. 1916.

some of the accomplishments of scientists in these laboratories had been of value. But the field was large, and workers few in comparison with the magnitude of the task.

Rorer and Smith in 1904, while in the West Indies—Cuba and the Bahamas—to study the coconut bud rot, had visited the Harvard University Station for Tropical Research and sugar cane investigation on the Atkins estate at Soledad, Cienfuegos, Cuba. There Smith studied for the first time specimens of sugar cane imported from Java and infected with the Sereh disease. About this time American botanical research, including the study of plant diseases, had been extended to several points in the tropics. The New York Botanical Garden had negotiated an agreement with the colonial government of Jamaica to maintain as a botanical laboratory and in conjunction with the government's department of public gardens and plantations the famous Cinchona Botanical Garden there. Nor were the extensions confined to experimental research laboratories and agricultural stations. After serving until 1906 as plant pathologist in charge of the Department's subtropical laboratory at Miami, Florida, and until 1921 as director of the Florida Agricultural Experiment Station and dean of the college of agriculture of the University of Florida, P. H. Rolfs, was selected to locate, organize, and conduct an agricultural college for the State of Minas Geraes, Brazil. Other South American universities were founded which attached especial importance to investigations in plant pathology. More and further each year, plant pathology, as a working science and reality, was being extended internationally.⁹⁶ Research in disease of plants had made great strides in Japan,⁹⁷ in the Philippines,⁹⁸ and some very important work was being done by Smith's former laboratory and scientific assistant, Leslie C. Coleman, in Mysore, India.

In Canada a phytopathological society would be organized. Sectional societies in the United States would be established. In England, the laboratory of plant pathology, formerly located at

⁹⁶ January 2, 1913, at the Cleveland, Ohio, meeting of the American Phytopathological Society, a symposium on International Aspects of Phytopathological Problems had been presented by C. L. Shear, L. R. Jones, and W. A. Orton. See *Phytopathology* 3(2, 3): 77, 143 ff., 1913.

⁹⁷ H. Atherton Lee, Plant pathology in Japan, *Phytopathology* 9(4): 178-179, Apr. 1919.

⁹⁸ Otto Reinking, college of agriculture, University of the Philippines, Philippine plant diseases, *Phytopathology* 9(3): 114-140, Mar. 1919.

the Royal Botanical Garden, Kew, would be transferred in 1919 to Rothamsted Experimental Station, Harpenden, and an Institute of Plant Pathology, with W. B. Brierley in charge of a mycological department, be created. July 15, 1919, Brierley would write to Smith,

Crown Gall as described in your investigations has had practically no attention paid to it in this country and in consequence I have asked Dr. Henderson Smith to carry out some experimental work upon it. Dr. Smith is a medical pathologist of some eminence and was until lately in charge of the bacteriological department of the Institute of Preventive Medicine. He is now devoting himself to the study of bacteria in relation to plant disease and in future will be working here with me.

Brierley sent for both cultures and literature.

In 1915⁹⁹ Ralph Eliot Smith, professor of plant pathology at the University of California, had written: "It is scarcely twenty years since Erwin Smith challenged the skepticism of the European world in regard to the existence of bacterial plant diseases. Still more recent has been most of the development of knowledge of protozoa, spirochetes, sporozoa, and filterable viruses in animal pathology. With these examples before us," he asked, "who shall say that organized living parasites of wholly unknown types do not exist?"

Insisting that our "knowledge of parasitism, either in plants or animals, is by no means exhausted," he maintained that "In the present status of plant pathology the following broad principle cannot be denied: '*There is no single instance of a positively demonstrated inciting cause of any specific plant disease except a parasite.*' We have theories, indications, appearances, and effects which point otherwise, but of proof we have none in any case."

His real plea was for renewed investigation of many of the obscure "physiological" plant diseases on the basis of their cytology and histology, all changes of structure as far as possible detected from the disease's inception and followed through its effects. "Animal pathologists long ago," he urged,

arrived at *cellular pathology* as the last resort in diagnosis, and the plant pathologist can do no less if he would solve his most obscure problems. . . . Let one read the recent edition of Mallory's *Pathological Histology*, written

⁹⁹ The investigation of "physiological" plant diseases, *Phytopathology* 5(2): 83, 87, 90, Apr. 1915.

from the standpoint of human pathology, and he will realize the value of such knowledge and its deficiency in plant pathology. Let him study Erwin Smith's (1912) photomicrographs of crown gall. . . .

Erwin Smith's work in crown gall was already being accepted as a model for research in plant pathology. But no parasite in peach yellows had ever been cultivated on artificial media and the disease reproduced by reinoculation in accordance with the "standards" set by his work in crown gall. Nor has this been done even today.

L. O. Kunkel in 1933 established that peach yellows is transmitted by an insect vector, *Macropsis trimaculata*.¹⁰⁰ Heat treatments for the cure of peach yellows and other virus diseases of peach have been found effective,¹⁰¹ although as yet no way of practically applying the knowledge in large or even small orchards has been worked out. Research has been in keeping with the most exact standards. But any "organized living parasite" remains unproven.

Smith attached special value to his laboratory's work in "determining methods of transmission of various bacterial diseases. I have myself," he wrote Paul Moore in 1923,¹⁰²

worked out the dissemination of two bacterial diseases by insects and Dr. [Frederick V.] Rand of this laboratory has confirmed and extended my work on one of the diseases (cucurbit wilt)¹⁰³ and now recently has shown conclusively that a third disease (Stewart's disease of maize)¹⁰⁴ is transmitted by a beetle. This is an entirely new and extremely interesting discovery of which no one had the least suspicion when he began work. This makes two bacterial diseases of plants that we now know to be carried in the bodies of Diabroticas. Most of the beetles of these two species, *Diabrotica vittata* and *Diabrotica duodecempunctata*, after eating the diseased material, evidently soon throw off the organism but in a certain small proportion it persists in their bodies for a long time and they serve as the spring starters of new infections.

¹⁰⁰ Insect transmission of peach yellows, *Contr. Boyce Thompson Inst.* 5: 19-28, 1933.

¹⁰¹ L. O. Kunkel, Heat treatments for the cure of yellows and other virus diseases of peach, *Phytopathology* 26(9): 809-830, Sept. 1936; also, abstract, *Phytopath.* 25: 24, 1935.

¹⁰² Letter November 7, 1923, to Moore of the National Research Council.

¹⁰³ F. V. Rand, Dissemination of bacterial wilt of cucurbits, *Jour. Agric. Res.* 5(6): 257-260, Nov. 8, 1915; *idem*, 6: 417-434, June 12, 1916; also, with Ella M. A. Enlows, Bull. 828, U. S. Dep't Agric., 1920.

¹⁰⁴ F. V. Rand *et al.*, Insects as disseminators of plant diseases, *Phytopathology* 12(5): 225-240, May 1922.

Modes of overwintering by disease-producing organisms and their transmission by living carriers had for many years been investigated in animal and plant pathology. These studies almost invariably had practical bearings, since by eradicating the overwintering agency and by exterminating the insect carrier effective weapons against the malady's spread were provided. For example, S. P. Doolittle's discovery that wild cucurbits harbor the virus of cucumber mosaic and that the striped beetle transmits the mosaic virus enabled workers to attack this virus disease in part by destroying wild cucurbit vines and by exterminating as far as possible the beetle transmitter.¹⁰⁵

Smith, in his address "Fifty Years of Pathology,"¹⁰⁶ after devoting considerable attention to carriers of human and animal diseases (to be quoted in Chapter XI), expressed his belief that there

are also many carriers of plant parasites, as many as of animal diseases, and as in animals there are some very striking adaptations, for example, the relation of the striped beetle, *Diabrotica vittata*, to the bacterial wilt of cucurbits, worked out in detail by F. V. Rand in my laboratory. Here all the beetles, apparently, transmit the disease in the summer, but only a small proportion of those that have wintered over are able to do so, that is, those only whose intestinal juices have not been able to destroy the ingested microbes. From the feces of these individuals come the first spring infections. In other words, these particular beetles behave exactly like human typhoid carriers. Rand lists nearly 50 plant diseases known or suspected to be transmitted by insects.

In 1920 Rand and W. Dwight Pierce published in *Phytopathology*¹⁰⁷ a study entitled "A Coördination of Our Knowledge of Insect Transmission in Plant and Animal Diseases."

In his letter to Moore, Smith told also that "recently Dr. Harry Braun of this laboratory has devised a new seed treatment which bids fair to be of great importance in controlling various seed-borne diseases." In his *Introduction to Bacterial Diseases of Plants*,¹⁰⁸ he had said that by Braun's discovery "(1919) . . . injury to seed-wheat, which is considerable when formalin solutions or copper sulphate solutions are used, is eliminated by

¹⁰⁵ Fifty years of pathology, *op. cit.*, 37. At p. 36 Smith also mentioned I. C. Jagger's transmission of cucumber mosaic diseases by rubbing crushed leaves of diseased plants on healthy plants (1917).

¹⁰⁶ *Op. cit.*, 42.

¹⁰⁷ 10(4): 189-231, Apr. 1920.

¹⁰⁸ Pp. 71-73.

soaking the seed in water for 10 minutes and then keeping it moist for 6 hours before treating it. This," he explained,

allows it to absorb sufficient water to become resistant, since most of the injury is caused by the formaldehyd that is carried into the grain along with the imbibition water. It is then soaked in one part of formalin to 400 parts of water for 10 minutes, drained, and covered for six hours to get effective action of the remaining aldehyd vapor on all the parasites, whereupon it is dried and planted. His work, repeated many times on different varieties of wheat and to some extent also on other grains, shows nearly as full germination and quite as good growth in the treated plots as in the control plots. In fact, growth from the treated seed is stimulated a little.

Seed disinfection, as a practical investigation technique to combat diseases of plants, had been studied by Smith for many years. In 1914, for instance, one of his reasons why he requested an appropriation to study further a corn bacterial disease was because he had learned that the malady "is distributed on the kernels and [could] be overcome largely by proper disinfection of the seed-corn, at slight expense."¹⁰⁹

In 1917 Smith published preliminary announcements¹¹⁰ on "A New Disease of Wheat," black chaff. A careful study of it was being made in his laboratory "to determine the biology of the parasite and whether it is actually transmitted from the seed to the young plant and so again to the seed," as he suspected. During several years this seriously prevalent trouble was studied on this continent and in Europe. Smith negotiated a cooperative arrangement with workers at the Kansas and Wisconsin experiment stations. Drs. Jones, A. G. Johnson, and C. S. Reddy in Wisconsin were studying a bacterial disease of barley, and evidently one of the points to be settled was whether the barley was the same as the wheat disease. The organism finally described was *Bacterium translucens* var. *undulosum* n. var.,¹¹¹ and of this Smith said in 1926:¹¹² "The parasite is carried over on the seed from one crop to the next and seed grain should not be obtained from diseased fields."

¹⁰⁹ Memorandum presented before an agricultural committee, House of Representatives.

¹¹⁰ *Jour. Agric. Res.* 10(1): 51-53, July 2, 1917; *Plant Disease Bulletin* 2, Sept. 1, 1917. Samples of the disease from various localities requested.

¹¹¹ Smith, Jones, and Reddy, The black chaff of wheat, *Science* 50(1280): 48, and literature cited, July 11, 1919.

¹¹² Fifty years of pathology, *op. cit.*, 36-37.

"How much my work has directly profited agriculture and horticulture," wrote Smith to Moore, "I have no means of knowing. It has been largely a breaking of new ground for the benefit of other workers. Some practical procedures have been modified as a result of it, e. g., inspections for certain diseases," and this held true from the start of his work on peach yellows through his study of crown gall, especially as to rules of orchard and nursery practice and of inspection of shipments. In 1926,¹¹³ for a descriptive study of various types of work done in the Department of Agriculture, Smith placed at a half a million dollars the estimated annual saving made possible to the nation by the work of his laboratory. This, he believed, was probably a conservative estimate, and, compared with the annual sum of (until then) less than one-tenth that amount received for his laboratory from the total appropriation for federal agricultural work, its value was easily demonstrated. "About half of the bacterial diseases of cultivated plants now recognized as serious," he said in this report, "have been discovered and worked out in his laboratory and extensive investigations have been made of the greater number of the remainder." There was, furthermore, the work in fungus, virus, and other types of plant disease research.

On March 9, 1920, Robert S. Woodward, president of the Carnegie Institution of Washington, wrote Smith that "the two works published by the Institution which [had] been most in demand during the world war [had been] two works which [had] emanated from the United States Department of Agriculture"—*Mosquitoes of North and Central America and the West Indies*, by Dr. L. O. Howard and others, and Smith's *Bacteria in Relation to Plant Diseases*. Dr. Woodward quoted from his recent report to the Institution's trustees: "Few of our contemporaries two decades ago, say, would have thought it more than a trivial matter whether a mosquito is designated as culex, anopheles or stegomyia. Similarly few of us would have thought ten years ago that a work on bacteriology could have become at any time a 'better seller' than the Institution's works on archaeology, history or literature." Smith was pleased, and especially by Woodward's added comment: "These instances serve to illustrate the truth

¹¹³ Memorandum 249, October, 1926, addressed to the chief of the Bureau of Plant Industry, *op. cit.*, chapter VIII.

well known to men of science that what is commonly condemned as lacking in 'practical' value often turns out to be of prime utility."

Smith's and Miss Hedges' translation of Émile Duclaux's *Pasteur, The History of a Mind* brought many more congratulatory letters than may be quoted here. Dr. M. P. Ravenel, then president of the American Public Health Association and a former student at the Pasteur Institute, was among those who commended and thanked Smith for the volume. Dr. Flexner thanked him for his "most valuable service in giving Duclaux's work to the English-speaking public. . . . I shall look forward also," he wrote, "to your forthcoming book [*Introduction to Bacterial Diseases of Plants*]. It is splendid to know that an authoritative book on that subject is about to appear. Some day before long I may wish to speak with you again about plant pathology for the future." Dr. Welch found "the greatest pleasure" in *Pasteur*. "I knew that this book existed," he wrote,

but I had never seen a copy. I agree with your estimate of the work, which is in many ways and especially for a scientific man more interesting and informing than the familiar life of Pasteur by his son-in-law. I enjoyed your sketch of Duclaux, of whose life I was previously but little informed. Delighted as I am to have this book with the dedication in your handwriting, I am even more glad to have your remembrance and expressions of good will. I reciprocate most heartily with all my best wishes for your own health, happiness and continued capacity to advance scientific knowledge.

During the year 1920 Smith learned that at the last meeting of the American Association for Cancer Research crown gall as a plant cancer was discussed. He immediately wrote in his diary: "They will be doing this for the next ten years."

CHAPTER XI

FURTHER DISCUSSION OF THE CROWN GALL—ANIMAL CANCER ANALOGY—VARIOUS ASPECTS OF FACT AND THEORY. A SUGGESTED MULTIPLE FACTOR HYPOTHESIS OF PATHOLOGICAL GROWTH.

FURTHER RESEARCHES IN BACTERIAL, FUNGOUS, AND VIRUS DISEASES OF PLANTS

BY 1920 Smith had published more than twenty-five papers, aside from his books, on crown gall. His laboratory was becoming recognized as doing probably more research on bacterial diseases of plants than any other laboratory in the world. He realized that he was breaking "new ground for the benefit of other workers," and in his text, *Introduction to Bacterial Diseases of Plants*,¹ more than writing on methods of research, fourteen selected diseases including crown gall, and a general conspectus of the subject, he suggested fifty-one subjects for special study and supplied notes on thirty-four additional maladies. Mention of a few of his topics for future research gives some idea of their scope: natural and acquired immunity and resistance to disease in plants and the effect of hybridization; symbiosis and antagonism among the lower plant organisms and the possibility of solving diseases by discovery of new methods; causes of mosaic and virus diseases, tobacco mosaic, peach yellows and rosette, pecan rosette, etc.; new adaptative and differential culture media; critical study of the chemistry of tumor-producing species from variously aged flask-cultures in various media; the determination of all acids produced by plant pathogens, and the hydrogen-ion content of media as related to growth of plant pathogenic species; the continued study of plant nutritive components, soil and climate relations, in plant diseases; insect and other animal distributors of diseases and their control; and many other problems.

Since 1917, especially since his treatise, "Mechanism of Tumor Growth in Crown Gall," scientific interest in the crown gall-animal

¹ W. B. Saunders Co., Phila. and London, 1920. At pp. 474-477 appeared his "suggestion of subjects for special study"; at p. 473, "notes on some additional diseases." At this time, furthermore, Smith offered to research pathologists study materials.

cancer analogy had been steadily accumulating. Among botanists the response was reassuringly vigorous. On the Pacific coast, Director H. J. Webber of the Riverside Citrus Experiment Station of the college of agriculture of the University of California found Smith's monograph "characteristic of the thorough work" he had always done. Venus Pool McKay of the Oregon agriculture college at Corvallis wrote that she had "practically 'worded' through" his article. "'Now,'" she said,

I am wondering if any one would dare go so far to say that in peculiar cases of malnutrition, the resistance of the animal cell becomes lowered and proliferation results. This process then is kept stimulated by the NH_3 radical which is constantly being cut off from some complex ammonium compound formed from the malnutrition condition. If so, then a changed nutrition should give a restored resistance and an inhibition of cell proliferation. A great field for thought and serious endeavor, that you have opened up.

P. J. O'Gara, chief pathologist in charge of the agricultural investigations of the American Smelting and Refining Company, with laboratories and experiment station at Salt Lake City, addressed a six-paged single-spaced typewritten letter to Smith, and described the results of his researches, made over three years, "determining the effects of dilute concentrations of sulphur dioxide gas on the growing organism"—wheat, oats, barley, corn, red clover, alfalfa, sugar beets, tomatoes, potatoes, beans, and other plants. On January 7, 1921, O'Gara visited Smith in his laboratory, gave him a specimen of an Arizona disease of *Pinus ponderosa* "supposed to be bacterial," and further described his experiments on crop injuries due to " SO_2 diffusion in air." Smith thought it "a remarkable piece of research." No bacteria in the pine disease, however, were found.

B. W. Wells, of the department of botany of the University of Chicago, at the time completing a thesis on galls, believed that Smith's paper constituted "a real and highly significant advance toward the solution of what Küster calls the 'kataplasma' type of gall or those forms in which the tissue suffers an interference with the normal inhibitors present and proceeds to develop a structure approximating the normal."

At a faculty seminar at the University of Pennsylvania, Smith's papers on plant overgrowths were discussed, especially the reasons

why as yet he had not "secured an epithelioma in plants." Drs. J. W. Harshberger and Macfarlane were most interested, in fact in 1917 Harshberger had written Smith of his interest in his "very important hypothesis as to the inactivation of the inhibitors of growth by the chemic products of *Bacterium tumefaciens*. I believe," he said, "that you have simplified the problem very much. Your experiments with ammonia and other substances point the way to a probable solution of some of the insurmountable obstacles in the way of the study of cancer in plants and animals." In two letters he suggested various plant materials with which to continue experiments. Dr. B. M. Davis of the department of botany found his paper "mighty interesting." "I wish," wrote Dr. Joseph McFarland of the McManes Laboratory of Pathology of the medical school, "that I had a modicum of your energy, industry and originality." The reprints were "most interesting."

Dr. A. F. Woods, now president of Maryland State College of Agriculture, praised his former associate, saying: "The work you have done in this field is most important and I am sure will be greatly appreciated by everyone interested in the development of Pathology."

Smith, pleased with Dr. E. C. Jeffrey's new book on *The Anatomy of Woody Plants*, promised in November 1917 to do with it what now he seldom did with books—"read the whole of it." Jeffrey replied,

It is a satisfaction to find expert opinion so favorable to my book. . . . I trust that you may feel encouraged to carry out your intention of reading the book through, particularly as there seem to be very close connections between our two fields of work. I have just been looking over your last interesting contribution on the subject of plant tumors. I am pleased to note that the "new growth hypothesis of the origin of tumors" is passing into the back ground because it has always seemed to be particularly absurd and impossible. Your article is all the more appreciated at the present time because one of my students is carrying on investigations as to the effect of purely physical stimuli on hypertrophy and similar phenomena.

On January 4, 1919, Smith reported to Dr. Cullen:

This summer some of my experiments on begonia phyllomaniaca turned out well, and I am now getting a paper² ready for publication. The plant

²The cause of proliferation in *Begonia phyllomaniaca*, *Proc. Nat'l Acad. Sci.* 5: 36-37, Feb. 1919. Read Nov. 18, 1918.

has been known for a long time to throw adventive shoots from leaves and stems (a very queer phenomenon) but nothing was known as to the cause of this peculiar behavior. I began to experiment with it thinking it might throw some light on non-cancerous tumor-cysts and the like. I find the shoots arise from the skin of the plant and that by certain forms of stimulus I can get them in great numbers at will. They are not vascularized into the mother at least not on the start, and most of them never were, and consequently they die in course of a few weeks. I have sometimes had as high as several thousand of these baby plants develop from a single leaf while still attached to the mother plant.

Jensen of Copenhagen (the mouse cancer man) has published this year (in Danish) a long paper on Crown Gall of plants summing up his observations and experiments of the last ten years, but also discussing critically the work done on it here in the Department of Agriculture especially as it bears on the question of animal cancer. I have had the whole paper translated, and find it so very stimulating that I wish it could be published somewhere in English. The enclosed excerpts³ will show what he thinks of some of my notions. I have always believed that the pull of heredity is so strong that as the disturbing factor is removed (bacteria in case of crown gall) the tumor cells will cease to behave abnormally whereas Jensen develops the very interesting idea that once the bacteria have converted normal cells into tumor cells the microorganisms may then die out, while the cells still retain and transmit their acquired characters. In other words, he believes in the transmission of acquired characters while I do not, or perhaps I should say, don't think there is as yet any good experimental evidence of it, for I would keep an open mind. You may have the whole paper to read if you wish.

Dr. Cullen answered, expressing much interest "in the memorandum . . . of Professor C. O. Jensen's work on Animal Cancer." He and Smith many times discussed various aspects of problems of cancer research.

Smith was now studying the beginning and middle phases in the production and development of tumors in plants. Some of his experiments were made with the aid of bacterial infection: and some by simple woundings and various experimental interruptions of normal physiological functioning and healing. In each experiment, he was interested in determining the minimum stimulus necessary for tumor production and tumor growth. He wanted, for purposes of his crown gall-animal cancer analogy, to arrive

³ These, or similar, excerpts were included in Smith's address, Twentieth century advances in cancer research, *Jour. Radiology* 4(9): 299, Sept. 1923. Jensen's paper was described in a letter to Dr. Gaylord as "Investigations concerning some tumor resembling growths in plants," *Serum Laboratory Rep't* 54 or 59, Copenhagen, 1918.

at some fundamental knowledge of what was then described as the "pre-cancerous stage"⁴ in plant and animal malignancies. The "chemical and physical stimuli underlying tumor-formation" was his basic theme and the subject was extended to include "the production of teratosis in the absence of tumors and of parasites."⁵

Proliferation experiments with the cultivated plant, *Begonia phyllomaniaca*, supplied material for many of his conclusions. It, because of its watery nature and especially sensitive and very permeable cell-membranes, was suited to studies of temporary loss of function by chilling, heat, anesthetics, acids, or excessive loss of water supply due to woundings or other causes. His paper on "The Cause of Proliferation in *Begonia Phyllomaniaca*"⁶ had been read on November 18, 1918, before the National Academy of Sciences, and, in it, among other important points, he attached special significance to his discovery that proliferation may occur in young tissues at a long distance from the point of wounding. In fact, the wounding might be in the root, and astonishing proliferation take place in the young tissue at the top of the plant. He accounted for this on the basis of shock induced by the "sudden interruption of the water current," causing "cell-precipitates or plasmolysis of the young totipotent cells which begin to grow when they have recovered from the shock." These adventive shoots are not branches, he said, "since they have no initial connection with the ordinary cambium, or xylem-phoem of the mother plant. They are rather to be classed with filial teratomas. Later, a small proportion of them establish connections with the conductive tissue of the mother and persist, i. e., become abnormally situated branches."

His paper, "Embryomas in Plants,"⁷ the original basis of which was his address in 1916 before the Johns Hopkins Medical Society, had had to do mainly with tumors in plants, produced by bacterial inoculations. When writing of growths from cut surfaces in tobacco, however, he had pointed to a *tendency* "always there" toward adventive buds or shoots "if the stimulus is strong enough," and this was so "not only in tobacco but

⁴ *Intro. to bact. dis. of plants, op. cit.*, 571. Also, pp. 483, 511.

⁵ *Idem*, part. IV, 3rd, 4th, and 5th subsections, pp. 477-632.

⁶ *Proc. Nat'l Acad. Sci.* 5: 36-37, Feb. 1919.

⁷ *Johns Hopkins Hosp. Bull.* 28(319): 277-294, Sept. 1917.

probably in all other plants, and presumably also in all animals, since a great many plants and animals are known to give rise readily to adventive buds even under a slight stimulus, *e. g.*, willows and begonias; hydras and tubularias. Just what the stimulus is in many cases," he had said,

remains to be worked out. The shoots are especially apt to appear when the plant or animal has been wounded or otherwise thrown out of balance. Such shoots are normal, or pathological only to the extent of being more or less fused, crowded, distorted and starved. For these reasons some of them should undoubtedly be classed as typical (non-cancerous) teratoids. Several interesting cases in plants have come to the writer's attention lately. . . . These are on tomato, cauliflower, cabbage, pond lily, etc. In one very curious variety of begonia (*Begonia phyllomaniaca*) buds in great numbers grow out of many parts of the leaf and stem on the slightest provocation.⁸

He wrote the preface of his textbook, *Introduction to Bacterial Diseases of Plants*, in August of 1920. He had completed the manuscript in 1915 but revised it twenty times, re-read galley-proof five or six times, and page-proof three or four times; and he said that his manuscript was to be construed as "revised down to the end" of the year 1919.

His address, "Production of Tumors in the Absence of Parasites,"⁹ was read on May 1, 1920, before the Section on Dermatology and Syphilology at the seventy-first annual session of the American Medical Association, held at New Orleans, Louisiana. The subtitle of his paper was "Varieties of Tumors in Plants," and, when outlining his conclusions from fifteen years of study of crown gall, he included on the authority of Jensen the point that this "tumor possesses the power of continuous growth through several generations."¹⁰ In his text book of the same year, moreover, he said: "Whether the cells thus originated may then continue to grow in the absence of the parasite, as Jensen believes, is a subject for further consideration."¹¹ Plant tumors, he said, were attributable to a variety of living causes: gall flies, plant lice, nematodes, fungi, myxomycetes, and bacteria. Frost (Harvey) and mechanical irritation (Wolf) could also cause tumors in

⁸ *Idem*, reprint, 5-6; or p. 279 of *Bulletin*, fn. 4.

⁹ *Archives of Dermatology and Syphilology* 2: 176-180, Aug. 1920.

¹⁰ *Idem*, reprint, 4.

¹¹ *Intro. to bact. dis. of plants, op. cit.*, 569.



Photograph taken in his laboratory in the early twenties.



Making free-hand sections of inoculated wilted watermelon. 1895.



In the library of his home, Washington, D. C.

plants, and he offered experimental data tending to establish that oxygen hunger followed by increased acidity in the tissues of plants could, in the absence of parasites, result in tumors. "I hope to convince you," he urged, that tumors may be also due "to a lack of aëration and the resulting increase in acidity of the imperfectly aërated tissues."¹² He described four series of experiments "following out [his] theory that disturbed cell respiration is at the bottom of tumor formation . . . results [obtained] in the absence of parasites by limiting the intake of air in various ways, and in each experiment [he reached] the same result—increased acidity of tissues and the production of small hyperplasias." A point of distinction followed.¹³

In crown gall and various other tumors due to parasites the parasite furnishes the acids and other stimuli as a direct result of its metabolism, but the small benign tumors I am describing are not the result of parasitism. The increased acidity probably results from the incomplete combustion of carbohydrates, i. e., from anaërobic cell-respiration due to poor circulation and imperfect aëration, which we know to be a peculiarity of tumors in general, since most of them are poorly vascularized and out of the general circulation.

If, for any reason, conditions such as I have outlined should occur in parts of the animal body, I should expect to see similar phenomena arise in such parts and, the nutrition being more abundant, the tumors might become large.

This paper, though resulting from his studies of crown gall and the general phenomena of tumor formation, treated "the experimental production of small benign tumors," their developmental physiology and morphology. Crown gall tumors and animal malignancies were considered, but his references to the problem of human cancer were few. At one point, concluding a paragraph about the plant cancer, crown gall, he said: "Like cancer, if taken early, the tumor may be removed completely; but in later stages of growth it often returns after removal." He summarized his address thus:

I have produced hyperplasia (1) by the direct application of dilute acids and alkalies to susceptible plant tissues; (2) by the introduction into the tissues of certain foreign organisms (especially the crown gall bacterium

¹² Production of tumors in the absence of parasites, *Arch. of Derm. and Syph.*, *op. cit.*, reprint, 1.

¹³ *Idem*, 5, 6.

which produces acids and alkali in flask cultures and probably also inside the cells as a result of its metabolism), and (3) I can now bring about the same results in the absence of parasites and their products by limiting the intake of oxygen, thus compelling the cells themselves to manufacture the stimulus which leads to the development of hyperplasias. I can conceive of this taking place only as the result of an anaërobic cell respiration acting on the youngest, most active cells of a tissue; but there must be always some oxygen of the air present, otherwise there will be complete asphyxiation of the tissues, as happened in my first experiments. Probably overgrowths due to freezing and to mechanical injuries will also be found to depend on local interference with cell respiration.

The most striking tumors are obtained by inoculating the crown gall organism, *Bacterium tumefaciens*. Some of these are simple tumors and others are tumors containing roots and shoots. Some also show the beginning of secondary tumors, and in the embryomas the invasion of tumor tissue into the embryomatous parts (young roots and shoots).

His diary of May 1, 1920, revealed that at New Orleans he exhibited photographs of tumors produced in the absence of parasites and live specimens of inoculated crown gall on sugar beet, cassava, castor oil plant, tomato, geranium, and Bryophyllum. Embryomas of the last three plants were demonstrated. The tomatoes illustrated root-bearing tumors, and the Pelargonium and Bryophyllum tumors bore leafy shoots. There was praise of his work in the discussion. The next problem had been indicated in this address and in preceding papers: how and by what method did tumor cells invade normal tissue, in crown gall and in animal cancer? In crown gall research, thus far at least, *Bacterium tumefaciens* had not been cultured out of the gall tissue, and it was believed almost beyond doubt that the action of the parasite and its by-products was confined to the cells; in other words, the disease began in the cells intracellularly and, spreading by contact of one cell with another, overcame in its invasion one cell after another until at last tissue and all organic substances fell subject to the disease so far as the disease spread within the plant body. This was the scientific view adhered to by Dr. Smith at this time.

In July 1920 he purchased and began to read several important texts on human pathology: "the part on tumors," he noted in his diary. These texts were Aldred Scott Warthin's edited and translated version of Ziegler's *General Pathology* and Francis Carter Woods' revision of Delafield and Prudden's work on the

subject. The third volume was F. B. Mallory's *Principles of Pathologic Histology*,¹⁴ and by July 20 he entered in his diary an important observation:

Finished reading the middle 96 pages of [G]. Hauser's *Das Cylinder-epithel-Carcinom des Magens und des Dickdarms*, the most important part for my purposes, the first part being chiefly history and classification and the last part a history of cases. It impresses me as a thoroughly good piece of work based on original research with a full knowledge of the literature. He is opposed to the doctrine of parasites as a cause but I can answer all of his objections. Striking is the resemblance of crown gall to cancer in its conversion of normal cells into tumor cells by apposition. He says in tumors big and little, ulcerated or not, there is a periferal conversion of normal gland cells into cancer cells all stages of which can be seen under the microscope. I must copy out some of his remarks. It would be interesting to write a commentary on his discussion of cause of cancer, entitled *30 years after*.

Three days later his diary read: "Two hot sticky days. 96° F. in the hothouse where I made inoculations of crown gall on tobacco (Res. Daisy and Hop.) I wish to get tumors duplicating one of several years ago showing conversion of normal cells into tumor cells by apposition." In a pencil notation, at the foot of the page, obviously written some while later, but undated, appears: "I got many such which have since been cut in serial sections stained, studied and photomicrographed."

On September 10 he examined under the microscope "a box of stained crown gall sections from tumor (3 weeks old) on young tobacco stem cortex, the inoculation being from Resistant Daisy Strain. They show," he wrote in his diary, "invasion of the phloem and wood and distinct conversion of cortex cells by apposition. I have marked certain ones to photomicrograph." Late in October he noted also:

In some of my Ricinus inoculations into vicinity of leaf glands I have found superb invasion of the cortex by crown gall cells and will pick out some for photomicrographs. . . . Working over the Ricinus slides. Some show very beautiful clumps of crown gall cells between much larger and

¹⁴ In Dr. Smith's scientific library at the time of his death in 1927 were: E. Ziegler's *General Pathology*, N. Y., Wm. Wood, 1918 (translated from 11th revised German edition, Jena, Fischer, 1905), and, F. Delafield and T. M. Prudden, *A text book of pathology*, Ed. 13, N. Y., Wood, 1925. His diary, however, reads: "Ziegler by Warthin, Delafield & Prudden by F. C. Wood, and Mallory (Pathologic Histology)." A pencil-written notation says of the Delafield & Prudden text, "revised by" F. C. Wood.

differently stained cortex cells, and their study in series shows that the surface of the tumor resembles a branching short root invading the cortex.

November 3 was spent partly in "making photomicrographs of No. 1188-62 crown gall on *Ricinus* petiole, showing invasion of tumor cells in cortical parenchyma." By early December he was concluding "There can be no question as to the growth of these cortex tumors *by apposition*. The stimulating influence extends out from the tumor on all sides about 1 mm."

On January 13, 1921, he began to plan a paper on this subject. His diary for the day read: "Continued making crown gall photomicrographs (tobacco cortex) to illustrate a projected paper on manner of growth of the tumor. It is peripheral by apposition in these particular tumors." His intention was, as he later¹⁵ explained to Professor A. Borrel of the Pasteur Institute, to add "one more piece of evidence pointing to the cancerous nature of the plant tumor, and I shall have," he hoped, "several additional ones to add in the near future." By June 11 he had "read all the more important literature pro and con of growth of tumors by apposition and made copious abstracts especially from Virchow, von Hansemann, and Borst." Three days passed and he was still studying the "origin of small tumors in the pith of Tobacco cortex tumor 1548D. They are close," he wrote in his diary, "to outer tumor but not clearly connected to it; but they are in tissue which I should call precancerous. There is a whole nest of little tumors in this region, some very small and in some instances they have caused the formation of a *small* island of wood on the inner face of strands of the inner phloem i. e., in the pith." For months he had been examining various transition stages in the contact of diseased with normal cells. A few quotations from his diary indicate his precision:

May 24, 1921. In all of the thousands of dividing cells I have never seen a karyokinetic figure. I have thought that it was because I collected them in p. m. and not at midnight or after, but I begin to think that the cells may divide amitotically. At the border of the tumors examined today under high powers i. e., in the transition many of the nuclei are notched or cleft deeply and a few wholly divided. I did not find the same in nuclei of an embryo root or in the very fine-cells [of the] tumor tissue. I will study further.

¹⁵ Letter, November 20, 1922.

June 6: Working at the microscope on nuclei of c[rown] g[all] cells in tobacco cortex (all the material was fixed in p. m.). I found very few mitoses, but some in each tissue i. e., fine celled tumor, and two grades of coarse-celled on the periphery. Notched nuclei occur also in all cell grades, but they are most abundant in the transition tissue. Many tumor cells contain 2 nuclei, and in one cell on the margin I *saw four*, with no trace of any cross wall.

June 7: Read Ziegler's Pathology. Worked at the microscope. Saw mitosis (one case) in stretched cell on border of tobacco cortex tumor. Searched especially for dividing cells which show clearly their origin from stretched cells and found 2 or 3 that are good enough to photograph.

The beginnings-of his study of appositional growth in crown gall tumors were traceable to the year 1907 when he had first observed the phenomenon, and definitely to 1916 when he had tried to reproduce experimentally what he had seen in 1907. He had followed the same procedure, "using the hop-strain of the crown gall organism and single needle-pricks as before, but the gall was growing very slowly when collected and [he] got nothing comparable to the earlier results which were produced"¹⁶ with the Paris daisy strain of *Bacterium tumefaciens*. In 1920 his repeated experiments were with this strain of the organism.

In the spring of 1908, he had "made fourteen sets of inoculation experiments on fish and frogs with pure cultures of *Bact. tumefaciens* derived from tumors on the hothouse daisy (*Chrysanthemum*) to determine whether this organism would induce similar abnormal growths in cold-blooded animals, experiments on warm-blooded animals being considered unnecessary because of the low maximum temperature of the organism (about 36.5° C.)."¹⁷ These experiments were continued at intervals of various years throughout the remainder of his research career, and were made on gold fish, trout, and salamander. While inoculations yielded some results,¹⁸ nothing conclusive was ever established and as late as the year 1926 he would say:¹⁹

I will never believe that cancers in animals can be produced by means of [the crown gall organism] until it can be done so many times and in such

¹⁶ E. F. Smith, Appositional growth in crown-gall tumors and in cancers, *Jour. Cancer Research* 7(1): 1-3, 1922.

¹⁷ *Bacteria in relation to plant diseases, op. cit.*, 2: 182-184.

¹⁸ For example, on February 19, 1916, Smith thought that perhaps some inoculations on gold fish had resulted in tumors which he described in a note-book.

¹⁹ Letter to Dr. James Young, Edinburgh, Scotland, February 26, 1926.

a clear-cut way as to leave no doubt whatever. Many years ago I got what I considered to be small sarcomatous tumors in fish, using the crown gall organism, but some of these receded and I never knew for certain that all would not have receded if I had left them longer, that is, it is so difficult to be sure that a proliferation confined to the injected part is not merely a response to the irritation which later on will all be absorbed. If I had got definite secondary tumors in my trout, then I would have thought I had something, but these I never got. In the same way (1914) I started a series of inoculations on salamanders and I got one definite adeno-carcinoma of the intestine, but that did not convince me, since "one swallow does not make a summer," to quote the proverb. It may have arisen naturally and not been due to anything I did.

Dr. Smith remained preeminently throughout his life a student of tumors in plants. His paper on appositional growth in crown gall tumors and in cancers was addressed to cancer research men; in fact, as we shall see, its first real presentation was before the American Association for Cancer Research. He, however, carefully qualified his purpose:

I am the more inclined to publish my observations on appositional growth in these plant-tumors,²⁰ because of very positive statements by many cancer specialists, from Waldeyer, Cohnheim and Virchow down to Hauser, Krompecher, v. Hansemann, Petersen, Cornil, Fabre-Domergue, Menetrier, and others, as to the occurrence of appositional growth in carcinoma, and because in many respects crown galls are better adapted to the study of this phenomenon of growth by apposition than animal tumors, not only because we know them to be due to an intracellular schizomycete so that there is a definite reason for such growth, but because they can be reproduced at will and collected for examination at any period of growth, and finally, because there are no migratory cells to confuse the picture.

As early as 1908 Dr. James Ewing, professor of pathology of Cornell Medical School and director of cancer research of Memorial Hospital, New York City, had been among those medical scientists who encouraged Smith to make, with crown gall, a fundamental study of pathological growth.²¹ Ewing later became a sincere, honest, and forthright critic of Smith's theories. Among other points, he thought that Smith, as a plant scientist, went too far in his crown gall-cancer analogies.²² On November 9, 1921,

²⁰ Appositional growth in crown-gall tumors and in cancers, *op. cit.*, 4-5.

²¹ See, A. J. Riker, E. Spoerl and Alice E. Gutsche, Some comparisons of bacterial plant galls and of their causal agents, *Botanical Review* 12(2): 59-60, Feb. 1946.

²² Told author by Dr. Riker who was told so by Dr. Ewing.

he wrote Smith that, while not denying the importance of his work, he believed that Smith still had "to point out how this process differs from chronic inflammation in plants." As late as November, 1926, these two scientists enjoyed discussions. On the occasion of their last conference, Dr. Smith wrote in his diary of Dr. Ewing: "About cancer etiology we do not agree, but I am very fond of him. He is an honest man and a brilliant one." That year he had written Dr. James Young,²³ gynecologist of Scotland, and said of Dr. Ewing:

He is a good friend of mine and I am never hurt by any of his criticisms because he is just as much in earnest to find the truth as I am, a very honest man and an excellent diagnostician and histologist. We have been scrapping back and forth for years over the question of etiology of cancer. The trouble with Ewing is that he is not a biologist and as I look at it, the cancer problem is peculiarly a biological problem.

To his last year, Smith's main work was his study of crown gall, and primarily he was studying this as one of many bacterial diseases of plants. In 1920 he prepared with G. H. Godfrey an article on the "Bacterial Wilt of Castor Bean."²⁴ With Godfrey in 1918 he had published on the "Brown Rot of Solanaceae on Ricinus";²⁵ with Miss McCulloch in 1919 on "Bacterium solanacearum in Beans";²⁶ with Miss Mary Katherine Bryan in 1915 on "Angular Leaf-spot of Cucumbers";²⁷ with L. R. Jones and C. S. Reddy in 1919 on "The Black Chaff of Wheat";²⁸ and with R. E. B. McKenney in 1921 on the tobacco blue-mold (*Peronospora*) disease of Georgia and Florida.²⁹ In 1921 he published on the "Effect of Crown-gall Inoculations on Bryophyllum,"³⁰ and in this considered his own work and that of Michael Levine on *Bryophyllum calycinum*, a West Indian plant which Jacques Loeb had also used in "some of his physiological experiments."³¹

²³ Letter, February 26, 1926.

²⁴ *Jour. Agric. Res.* 21(4): 255-261, May 16, 1921.

²⁵ *Science* 48(1228): 42-43, July 12, 1918.

²⁶ *Science* 50(1288): 238, Sept. 5, 1919.

²⁷ *Jour. Agric. Res.* 5(11): 465-476, Dec. 13, 1915. See also Miss Bryan's paper on, A nasturtium wilt caused by *Bacterium solanacearum*, *idem* 4(5): 451-458, Aug. 16, 1915.

²⁸ *Science* 50(1280): 48, July 11, 1919; E. F. Smith, A new disease of wheat, *Jour. Agric. Res.* 10(1): 51, July 2, 1917; and other authorities cited in *Science* 50: 48.

²⁹ U. S. Dep't Agric. Dept. Circulars 174, 176, 181, Apr.-June 7, 1921.

³⁰ *Jour. Agric. Res.* 21(8): 593-597, July 15, 1921.

³¹ Letter, Smith to Dr. Cullen, Oct. 6, 1920.

Dr. Gaylord and Smith exchanged some letters in September, 1920, concerning a similar study published recently in the *Journal of Cancer Research* by Levine and Dr. Isaac Levin. Dr. Smith's laboratory was still, first and foremost, one of plant pathology. He tried in every way practical to perfect his technique in studying crown gall. He had been "trying for two or three years to get hold of Dr. Bovie's apparatus for registering, electrically, the exact acidity of culture media"³² to improve his study of the hydrogen-ion concentration in tumors of plants and thus his study of the crown gall-cancer analogies.

By a letter of January 8, 1924, Smith explained to Dr. John M. T. Finney of the Johns Hopkins Hospital his hopes and intention: "I do not consider it at all likely that I shall ever be able to settle the cancer question. I only may have thrown some side lights on it which might help somebody working directly in that field to solve it. That is the most I have ever hoped to do, and sometimes I feel as if I had not done even that."

Dr. Charles Oberling, in his recent book, *The Riddle of Cancer*,³³ has said:

Nearly every organism advanced as the cause of cancer has turned out to be a familiar and harmless inhabitant of the skin or mucous membranes, and not a single one, whether isolated from man or from the lower animals, has ever elicited a genuine neoplasm. The only exceptions are certain plant tumors, caused by a microorganism (*Bacillus tumefaciens*) isolated and described by Erwin F. Smith.

Discussing the three more important hypotheses of the origin of cancer—the irritation, the embryonal, and the parasitic, or microbic—Dr. Oberling drew from numerous instances of results of modern research in virus diseases of plants and animals.

Smith, like many other students, found truth in all three hypotheses. While to the parasitic theory he subscribed more zealously, he did so largely on the basis of his learning in bacteriology and to some extent fungology. A student of parasitic diseases might be expected to indulge some bias, especially where the behavior of a pathogenic organism indicated analogous action. But prejudice was an unwritten word in his scientific language. He sincerely believed that the behavior of plant cancer was similar to animal

³² Letter, Smith to L. R. Jones, Mar. 11, 1920.

³³ New Haven, Yale Univ. Press, 1944, p. 35.

malignancies, and if so, the rationale indicated a parasite in cancer, an organism with capacities for adaptation and reproduction, and possibly other describable characters. Observe, however, his suggestions as to the nature of this possible, discoverable organism or carcinogenic agent invariably were drawn from his knowledge of plant diseases or from those human and animal diseases in which a known parasitic microorganism was the cause.

Unyielding search in tuberculosis, leprosy, syphilis, malaria, yellow fever, and other diseases had ended each time in finally isolating the real causal parasite or had brought the final isolation closer. This argued that experimental scientists dared not yield in their relentless search for a parasite in cancer. Especially was this so, since it had been shown that cancer in animals can be caused by parasites, *Gongylonema neoplasticum* (Fibiger), *Cysticercus fasciolaris* (Borrel) and by viruses—Rous's (and James Murphy *et al.*) chicken sarcoma and Akira Fujinami's independently discovered fowl myxosarcoma, which was tantamount to a verification of Rous's work. To these have been since added, among others, the Shope rabbit papilloma, Lucké frog kidney carcinoma, Bittner mouse breast cancer, and Yoshida rat ascites sarcoma.³⁴

Smith never maintained at all points completeness of analogy between crown gall of plants and human and animal cancers. Yet, he insisted, studies of the genetic origin and environmental development of tumors were made possible by comparatively studying malignant and non-parasitic tumors in plants. Since the causal organism of malignant plant tumors had been isolated, it followed, however slight the presumption, that a parasitic organism causing malignancies in animals and man might also be found—not the same organism but one with similar capabilities. At least, clues or leads helpful to ultimate discoveries or solution were available through the study of plant tumors.

Under controlled conditions, he examined the processes of tumor formation and growth, the chemico-physical stimuli which induce cell division and multiplication, and the nature of malignant in contrast to benign growths. To determine the nature of chemical substances liberated in tissues by parasites to cause plant

³⁴ *Science* 110(2844): 20, July 1, 1949 (review by Kanematsu Sugiura of *Cancer: radiations, virus, environment*, vol. II, by Dr. J. Maisin).

tumors, Smith used a flask culture technique. Various gall-forming fungi and bacteria could be cultivated in flasks "on simple culture media in any desired quantity and their products determined with a minimum of labor. This, rather than the analysis of tumors," was the proper procedure. He gave as his reasons³⁵ that

the cells of a tumor are only the cells of a plant or animal grown under an abnormal stimulus, which stimulus, it is very likely, is not only very minute in quantity but also used up during the growth of the tumor cells, that is, converted into something quite different and entirely inoffensive. For this reason analyses of tumor tissue should give only about the same kind and quantity of products as normal tissues in which there is an equally rapid movement of food-stuffs, and in which there is an equally rapid growth, and this is about what tumor analyses thus far have shown. In flask cultures, on the contrary, the products of parasitic growth accumulate and can be locked up for future study.

From 1911 to 1917 he prepared flask cultures of various substances produced by various strains of *Bacterium tumefaciens* out of river water, peptone and grape sugar, "substances corresponding to or approximating those which occur naturally in the cells of the plant."³⁶ These were sent to expert organic chemists of the Department of Agriculture. But, before submitting them, he tested each culture as to purity by pouring Petri-dish agar plates and as to pathogenicity by inoculating susceptible plants. From substances found present in the culture flasks and not in the controls he, with the aid of the chemists, determined "the most interesting . . . products of a cancer parasite, or, if one prefers so to express it, of a schizomycete which is the cause of a plant tumor possessing many features in common with animal cancers." These substances were "for the most part" ammonia, amines and fatty acids.³⁷

Proceeding on the hypothesis that "the parasite acts through its excretions" similarly as "most communicable diseases are due not to the parasites but to their toxins,"³⁸ he continued his study

³⁵ Mechanism of overgrowth in plants, *op. cit.*, 437-438.

³⁶ *Idem*, 438 (flask culture technique explained); Chemically induced crown galls, *op. cit.*, 312 (Smith described the "very simple culture media [as] in flasks of distilled water containing 1% dextrose and 1% peptone with a little calcium carbonate added to neutralize any acids formed and thus to favor long continued growth since the crown-gall organism is very sensitive to its own acid products.")

³⁷ Chemically induced crown galls, *op. cit.*, 312-313; Mechanism of overgrowth in plants, *op. cit.*, 439.

³⁸ Production of tumors in the absence of parasites (New Orleans address), *op. cit.*, 4.

of the crown gall bacterium and its by-products to establish within a range of probability what takes place within a cell, as a result of a changed metabolism, when a hyperplasia develops. Since these years, Smith has been accredited³⁹ with suggesting, if not establishing, that "the living cells of the host react to some substance generated by the bacteria, and that the bacteria need not be in direct contact with the cells which have been stimulated to divide."

Cell respiration, disturbed by want or imperfect functioning of the aerating organs of the plant, was believed to be one of the causes "at the bottom of tumor formation"—an oxygen hunger within the cell set up by "limited intake of air," reduced aerating water, etc.⁴⁰ Professor Albert Joyce Riker of the University of Wisconsin, and co-workers, more recently have correlated several individual factors suggested yet today to account, by their lack of balance for changes in plants from normal into pathological growth. These include "'oxygen hunger,' changes in osmotic pressure, rearranged amounts of growth substances and vitamins, 'irritating' substances, and altered amounts of food materials."⁴¹ In another study, "Inhibition of Respiration in Plant Tissues by Callus Stimulating Substances and Related Chemicals,"⁴² Dr. Riker *et al.* credit Smith in 1917 with opening "the field of chemical induction of gall formation by using materials found in cultures of crown gall bacteria. . . . These bacteria were grown in beef-extract peptone broth, and various compounds were recovered and tested for their ability to induce galls. Many other investigators continued this work and reported numerous compounds of various kinds which were active in gall production."⁴³ Mitchell *et al.* found that the best chemicals for inducing chemical galls actively induce "oxygen hunger." Several amino acids which inhibit growth are now known.⁴⁴ Further evidence has been

³⁹ E. J. Kraus, Nellie A. Brown, and K. C. Hamner, Histological reactions of bean plants to indoleacetic acid, *Botanical Gazette* 98(2): 414, Dec. 1936.

⁴⁰ Production of tumors in the absence of parasites, *op. cit.*, 5 f.

⁴¹ A. J. Riker, E. Spoerl, and Alice E. Gutsche, Some comparisons of bacterial plant galls and of their causal agents, *Botanical Review* 12(2): 57-82 at p. 66, Feb. 1946.

⁴² J. E. Mitchell, R. H. Burris, and Riker, *Amer. Jour. Botany* 36(4): 368-378 at p. 368, Apr. 1949.

⁴³ Riker and T. O. Berge in 1935 reviewed this subject, Atypical and pathological multiplication of cells approached through studies on crown gall, *Amer. Jour. Cancer* 25: 310-357.

⁴⁴ Riker and A. E. Gutsche, The growth of sunflower tissue *in vitro* on synthetic

adduced to show that a considerable variety of organic acids interfere with growth in plants.⁴⁵ In both cases concentration was critically important.

Another laboratory technique which Smith used effectively to advance his work was tissue cultures. He was among the first American scientists to recognize the need and "the potential value of a tissue culture technique in the study of plant neoplasia."⁴⁶ Dr. Philip R. White has said that he "did all in his power to encourage the development of such a technique." The origins of "the culturing of isolated fragments of tissues, especially of the vascular plants, under controlled conditions" to enable workers with more accuracy and completeness to observe plant life processes, has been traced to Haberlandt and the last years of the last century.⁴⁷ Many biological students of plants and animals have had a share in perfecting the method, including Dr. W. J. Robbins of the New York Botanical Gardens. Smith learned how to prepare tissue cultures for his work from an animal embryologist, Mrs. Margaret Reed Lewis, who was then with the department of embryology of the Carnegie Institution of Washington. Her laboratory was then in Baltimore where her husband, Dr. Warren Harmon Lewis, was professor of physiological anatomy at Johns Hopkins University. On April 28, 1921, Dr. and Mrs. Lewis had called on Dr. Smith in his laboratory at Washington. After examining slides of crown gall, they were given by him cultures of *Bacterium tumefaciens*. Smith had been acquainted with Dr. Lewis for some years, having met him at the Marine Biological Laboratory of Woods Hole while the latter was studying under Dr. Franklin P. Mall. Their plan in 1921 was to secure, if possible, tumorous overgrowths in chickens by cultural inoculation of *Bacterium tumefaciens*, and, by means of tumor transplantations to other chicks, to prepare tissue cultures and study the possibilities of viruses and toxins in the culture media.

In March 1922 it was thought that an animal tumor had

media with various organic and inorganic sources of nitrogen, *Amer. Jour. Bot.* 35: 227-238, at 230.

⁴⁵ A. C. Hildebrandt and Riker, The influence of various carbon compounds on the growth of marigold, paris-daisy, periwinkle, sunflower and tobacco tissue in vitro, *idem* 36(1): 74-85, at 78, Jan. 1949.

⁴⁶ P. R. White, Plant tissue cultures, *Ann. Rev. of Biochemistry* 11: 624, 1942.

⁴⁷ P. R. White, Plant tissue cultures. The history and present status of the problem, *Archiv f. exper. Zell. bes. Gewebz.* 10: 501-518 at pp. 502, 511, 1931.

resulted from inoculations with the crown gall organism, but, the tumor receding, Mrs. Lewis on March 30 reported to Dr. Smith:

My plans have failed owing to the fact that all of the overgrowths obtained with the new cultures of *Bacillus tumefaciens* from the peach and from euonymus have been absorbed again. There was quite a marked enlargement in some cases but it lasted only a few days. The cells which contain the organism after it has become changed resemble the human cancer cells. I inoculated several dozen developing eggs also and in every case where the embryo survived, I was able to recover the organism from the liver, after three or four days. A number of the embryos had malformations of the head. . . . The important point is to get the organism inside of the cells without any accompanying reaction of the tissue against them.

An enclosed memorandum ended: "Very difficult to stain organism without destroying all cytological structure of the cell." Again in 1925 these experiments were to be tried on rats and frogs, but result in only one "small tumor in the lymph sack" of a frog. Again in 1926 Smith was to repeat some experiments on salamanders and reexamine, in the light of new experimental disclosures from Germany, whether several years earlier he, when he tried similar experiments, had erred in any research particular. From a series of inoculations with *Bacterium tumefaciens*, he had obtained but one seemingly definite salamander carcinoma.

By 1921, and even before then, *Bacterium tumefaciens* was being recognized as a carcinogenic agent of more than ordinary significance. In April 1922, when he decided to consult cancer research specialists before completing his paper (and address) on oppositional growth in crown gall tumors and in cancers, he went first to Baltimore where Mrs. Lewis showed him tissue cultures from their first set of inoculations with the crown gall organism. His diary of April 17 reveals that he found them "indistinguishable from reactions and similar cultures (ectoderm) inoculated with two or three cancer cells. She is to give me cultures of an organism (rod) she obtained from a Sarcoma and I will try it on plants." April 18, he wrote: "Mrs. Lewis showed me additional cancer preparations. Whether (1) *Bacterium tumefaciens* is used (2) an organism from cancer, or (3) a few cancer cells or (4) Rous's chicken sarcoma ground, in her tissue cultures, there is the same type of reaction viz. small and deeper staining cells (3 or 4 times closer together) with notched nuclei. Material stained at end of 48 hours or sooner." April 27 he learned "how

she makes tissue culture"; and, on May 13, after several times working together, he placed the following comment in his diary: "By injecting the heart of a chick embryo with *Bacterium tumefaciens* (peach strain) and then at once starting tissue cultures from a bit of the peritoneum she has a tissue culture with giant cells and also with abnormal masses that look like a roundcell sarcoma. She is a genius!"

After he had adapted the tissue culture technique to the study of plants, he brought his first plant tissue cultures to her.⁴⁸ Together the Lewises and he interested Dr. D. S. Johnson, professor of botany at Johns Hopkins and a friend of Dr. Smith, in perfecting the procedure. Later Miss Nellie A. Brown of Dr. Smith's laboratory learned from Mrs. Lewis how she prepared tissue cultures.⁴⁹ About 1924 Dr. Johnson suggested that his brilliant student, Philip R. White, be placed at work on the subject as a specialty. He spent the summer of 1929 at the Mt. Desert Island Biological Laboratory at Salisbury Cove, Maine, studying the methods of Dr. and Mrs. Lewis in their laboratory quarters.⁵⁰

Since Smith's years, tissue culture methods have greatly aided scientists in studying problems of organic metabolism. By this procedure, not only interesting confirmation of his earlier beliefs about growth stimulations and inhibitions by acids has been provided, but also the study of acids and their influence has been directed toward determining their effects on host plants and the bacterial organism.⁵¹ Smith helped to advance the work, especially by his efforts to determine with exactness the factor of pH or hydrogen-ion concentration in plant tumor growth.⁵² Years were spent by him in investigating growth-promoting, growth-regulating, and growth-retarding substances. So far as this author is aware, the word "phytohormone" was never used once by him in his writings. The vast, organized study of plant hormones, as such, postdated his years of research. Yet the subject matter now embraced by hormones and their effects is an integral part of

⁴⁸ Told the author by Mrs. Lewis. The Lewises were then still living in Baltimore.

⁴⁹ Told the author by Miss Brown.

⁵⁰ White, Plant tissue cultures. The history, etc., *op. cit.*, 511.

⁵¹ A. J. Riker *et al.*, works cited; also, memorandum by Dr. Riker to the author.

⁵² A. J. Riker, Studies on the influence of some environmental factors on the development of crown gall, *Jour. Agric. Res.* 32: 83-96, 1926; also, *Amer. Jour. Botany* 32: 357-361, 1945.

the science of plant pathology, and much of the fundamental study which has made possible its evolution was the very work which he performed, though under the broad titles of plant physiology and plant pathology. Since 1893 the theory based on observation and experiment that plant organs, leaf, root, stem, flower, etc., are started by some chemical phenomena produced by the plant's metabolism had been announced by Sachs. Sachs' theory was announced for plants practically a decade before the discovery of the first animal hormone, secretin, by Bayliss and Starling (1902), and many years before definite experimental knowledge of plant hormones was made available.⁵³ Smith interested himself in the study of the fundamentals of plant life, regardless of definition or name. The study of crown gall infection now includes the investigation of hormone effects.⁵⁴ But, for our purposes, we shall employ the descriptive phraseology and terminology used by Smith.

While at Baltimore in April, 1922, Smith worked in the laboratory of Dr. Joseph Colt Bloodgood,⁵⁵ associate professor of clinical surgery and associate surgeon of John Hopkins Medical School and Hospital, an excellent, able man. Smith studied stained slides, microscopic sections, and other detail, of cancers of male and female organs, among these being the stomach, liver, and intestines, and the uterus and other abdominal parts of the female. One afternoon he watched Dr. Bloodgood, whom he regarded as a "great teacher," demonstrate cancers to a group of students. Several times he enjoyed discussions of pathology with Dr. Welch, Dr. Cullen, Dr. MacCallum, and others of his faculty friends there. Dr. MacCallum told Smith "he would be glad to have [him] as a plant pathologist in the new pathological building. He," Smith wrote in his diary, "would let me have several rooms."

April 24 Smith found, while reading past numbers of the German cancer journal, *Zeitschrift für Krebsforschung*,⁵⁶ an article

⁵³ William Crocker, Botany of the future, *Science* 88(2287): 388-389, Oct. 28, 1938 (discusses also Boysen-Jensen's work on the significance of chemical substances in the oat coleoptile); *Growth of plants, Twenty years' research at Boyce Thompson Institute*, chap. VI, Plant hormones, pp. 204 f., by P. W. Zimmerman, N. Y., Reinhold Pub. Corp., 1948.

⁵⁴ *Jour. Agric. Res.* 57: 21-39, 1938.

⁵⁵ William Carpenter MacCarty, Joseph C. Bloodgood, *Surgery, gynecology and obstetrics* 68: 965-966, May 1939.

⁵⁶ 18($\frac{1}{2}$): 110-125 (with Dr. Hans Hirschfeld), 11 figs., Berlin, 1925.

by Ferdinand Blumenthal, its editor, on crown gall. Dr. Blumenthal was a reputable medical authority on kidney secretion changes in various human diseases and director of the cancer laboratory of the important Berlin Charity Hospital of Universitäts Institut für Krebsforschung. *Zeitschrift für Krebsforschung* was "the oldest and most influential cancer journal in the world."⁵⁷ Smith remembered having read another article⁵⁸ on the same subject by the same authors in the same journal. Some years before, he had challenged German scientists who were skeptical of his claim of resemblances between crown gall and cancer. When one of his manuscripts was rejected by a German journal, he sent cultures to some of them, and later revealed:⁵⁹ "One of my manuscripts was rejected by a German journal as 'zu Botanisch'; a second German critic said, 'The disease has nothing in common with cancer but its name,' while a third likened the tumor to smut balls in maize, which it does not in the least resemble, and said all this had been known in Germany for a long time." Now Smith was reading papers on crown gall, prepared by German scientists. A special section for the study of plant tumors was to be a part of the Imperial Institute. He commented in his diary that night, "The Germans are waking up a little!"

April 29 Smith interrupted his studies at Johns Hopkins for a few days to return to Washington and arrange his exhibits for an address before the American Association for Cancer Research. The meeting took place in the Army Medical Museum on May 1, and of his address he said: "My paper came about ten thirty. Room fairly well filled. I showed about 20 lantern slides and spoke on Appositional Growth in Crown Gall and in Cancer. No one contradicted me and Levin of New York spoke most flatteringly of my work." At the meeting he found Dr. Lazarus-Barlow, pathologist of the Middlesex Hospital, London, with whom he had become acquainted at the International Medical Congress in 1913. He took this British scientist and Dr. Howard T. Karsner of Cleveland to lunch and later "spent two hours showing Dr.

⁵⁷ E. F. Smith, Recent cancer research, *Amer. Naturalist* 60: 247-249, May-June, 1926; also, Some newer aspects of cancer research, *Science* 61(1589): 600, June 12, 1925.

⁵⁸ *Zeit. f. Krebsf.* 16: 51-58 with 2 pls.

⁵⁹ E. F. Smith, Twentieth century advances in cancer research, *Jour. Radiology* 4(9): 299, Sept. 1923.

Arrhenius, cancer research man of the Christiania, Norway, Serum Laboratory stained sections of crown gall." Smith saw he was "much interested" and added to his diary account: "He is one of the few Europeans who believe cancer due to a parasite. I gave him slides and literature."

On May 3 Dr. Smith returned to Johns Hopkins for the second of his lectures to Dr. Bloodgood's students. Two or three times in 1922, and once in 1923, he lectured before the third year medical students. They were "a very interesting lot" and on April 19 his audience had consisted of ninety men and women, some of them members of the staff of the laboratory of surgical pathology. His second lecture was followed by microscopic demonstrations and for this purpose he had brought suitable materials from his own laboratory. He spoke for an hour and showed lantern slides of

crown gall as follows: 1. Primary tumor. 2. Secondary tumors. 3. Tumor strands. 4. Embryomas. 5. Sections showing structure. 6. Granulomas—*a.* nematode galls. *b.* tumors on orange due to fungus. *c.* olive tubercle. 7. Tumors due to chemicals produced by *Bacterium tumefaciens*—*a.* ammonia. *b.* acetic acid. *c.* formic acid. 8. Structure of acetic acid tumors—*a.* early hypertrophied cells. *b.* later hyperplasia. 9. Hyperplasia growths in fish due to crown gall bacteria.

At the conclusion, he "received vociferous and prolonged applause."

The following year, on March 26, his subject was: "Some resemblances between plant and animal tumors." Dr. Finney and Dr. Robert Miller were among the eighty persons present, and, at the end of this occasion, he received "a kind of ovation."

Smith completed on May 17 his research study of 1922 at the three Baltimore laboratories: the Carnegie Institution's laboratory of embryology, Dr. MacCallum's laboratory of pathology, and Dr. Bloodgood's laboratory of surgical pathology. He returned to Washington for some work at the Hygienic laboratory. He made use of the facilities at the Army Medical Museum; and his last action at his own laboratory on May 19, before leaving for Philadelphia, was to consult with Dr. Robert Ervin Coker of the United States Bureau of Fisheries on the habits of egg-laying sea turtles and the feasibility of his working during July and August at the Bureau's laboratory at Beaufort, North Carolina. He did

not go to North Carolina that year. We do not know definitely what research plans he had in mind, but, as an extension of his study of chemically induced crown galls, he perhaps wanted to investigate chemically induced growth in unfertilized eggs of animals. He had read and written of Dr. Jacques Loeb's study of compounds which start growth in unfertilized eggs of the sea urchin.⁶⁰ This had definite significance, he thought, in his study of the chemical excretions of *Bacterium tumefaciens*.

Yet before him was the task of finally completing for publication his paper on "Appositional Growth in Crown Gall and in Cancer." As early as January he had finished a final reading of this and begun to seek a publisher. Three or four figures in the text and many photomicrographs were to illustrate it. In March he was invited to address the cancer research association, and his address became a condensed version, or abstract, of the paper. Still, he was in doubt about some points of medical pathology. So he began a journey to more cancer research centers.

He first went to Philadelphia and consulted with Dr. True of the department of botany at the University of Pennsylvania. He read all of one day in the medical library and then went on to New York.

Five days were spent in New York consulting with cancer research specialists at the Crocker Institute of Columbia University and at several laboratories of Cornell University Medical College. Extracts from his diary May 22-May 25 reveal the important research work he found being performed there: At the Crocker Institute he

met Mr. Kegerreis and saw 200,000 volt x-ray machine and Dr. Woglom showed me around. I was most interested in the new Sarcoma of white rats produced in six months to eighteen months by feeding *Cysticercus* eggs from cat's dung. Some strains of rats are more susceptible than others and young rats than old ones, in fact old rats are free from liver cysts due to the worm. Not all of the rats develop the Sarcoma by so far [as] 4% to 36% and not all cysts in the same rat show it. It is a malignant disease metastasizing freely. I saw one rat dissected (dead at end of 18 months) and many in formalin. They have over 5000 inoculated. They feed the eggs by washing them out with water strained through cloth. . . . They have promised me a set of slides.

⁶⁰ Chemically induced crown galls, *op. cit.*, 313; Mechanism of overgrowth in crown gall, *op. cit.*, 439-440.

May 23: Spent most of the day with Drs. Stockard and Chambers at Cornell Medical School and worked on Chambers papers and ms [Appositional Growth in Crown Gall and in Cancer] till 9 p. m. He is to teach me how to manipulate his improved Barber apparatus. I wish to use it to inject cells with crown-gall bacteria and watch their behavior under the microscope. I had my first lesson today.

May 24: All day at Cornell Medical School. . . . Talked with Charles Stockard. Talked and lunched with James Ewing and worked all afternoon in Chambers' Laboratory. He has made substantial improvements in Barber's apparatus and I am learning how to use it.

May 25: At the Crocker Laboratory. . . . Saw Dr. R[ohdenburg] feed Norwegian rats with cockroaches infested with Fibiger's *Gongylonema* nematodes. Later saw eggs and cysts and a worm escape from a cyst after repeated trials. Dr. R. also showed me slides showing stomach and tongue carcinoma due to the nematode.

The slides, Smith believed, were Fibiger's.

Spiroptera neoplastica is the scientific name now applied to the Dane Johannes Fibiger's carcinogenic worm, *Gongylonema neoplasticum*. This, and *Cysticercus fasciolarus* (the larval form of a cat tapeworm, *Taenia crassicolis*), are malignant tumor-inducing agents in animals,⁶¹ and Smith, in his study of *Bacterium tumefaciens*, had been investigating what he believed to be a malignant tumor-inducing agent in plants. He was familiar with Borrel's position which, to quote Oberling,⁶² was that "neither the Spiroptera nor any other parasite can provoke a malignant tumor unless contaminated with a carcinogenic virus." His diary comments, however, made no reference to Borrel's hypothesis, whatever more he was to say on this subject later that year in his address before the Radiological Society of North America on "Twentieth Century Advances in Cancer Research."⁶³

On May 25 Smith observed Miss M. R. Curtis of the Crocker laboratory dissect two rats fed with *Taenia* eggs from cat feces. Borrel had seen and attempted artificial infestation to study the rat-liver cyst sarcoma. But, his work having been interrupted,⁶⁴ the further work of Bullock and Curtis became very important. Smith wrote into his diary a memorandum on the dissections, believed it "undoubtedly sarcoma and tremendously interesting,"

⁶¹ Chas. Oberling, *The riddle of cancer, op. cit.*, 67, 69.

⁶² *Idem*, 70.

⁶³ *Op. cit.*, Worm cancers, 303-311.

⁶⁴ Oberling, *op. cit.*, 73; Smith, Twentieth century advances in cancer research, *op. cit.*, 309.

and added, "It seems as if a parasite might be cultivated from it. . . ." That day Dr. William H. Woglom accepted for publication in the *Journal of Cancer Research* Smith's manuscript, "Appositional Growth in Crown-Gall Tumors and in Cancers."

Later that year, on December 8 at Detroit, Dr. Francis Carter Wood of the Crocker Institute told Smith that they now had "over 800 rat liver sarcomas from feeding eggs of the cat tapeworm." He added to his diary memoranda that these had "come along very rapidly in the last six months and they have had 100% of cases in descendants of Sarcomatous rats bred together, *but none without the irritation of the worm.*" This last fact was to Smith significant since it tended to minimize, without dispelling, the hereditary aspect of cancer and did not disturb the susceptibility aspect.

He regretted that time did not permit him to visit the botanists and zoologists of his acquaintance at the Rockefeller Institute. The end of the week had arrived and by the beginning of the next he had to be in Washington. Reaching there, he on May 31 assembled his photographs, and again re-studied parts of his paper on appositional growth. He was yet to go to some research centers and libraries in the middle west to settle a few final points. But, before doing this, Dr. Brandes, now with the Department at Washington, brought him a copy of a paper by L. O. Kunkel on corn mosaic in which were figured certain intracellular bodies associated with mosaic disease in *Hippeastrum equestre* and corn in Hawaii. After their interview, Smith prepared another entry for his diary: "Iwanowski in 1903 in *Zeit[schrift] f[ür] Pfl[anzen]-kr[ankheiten]* has a long paper on Tobacco mosaic in which he figures the Kunkel bodies. [D.] Prain has recently named the small bodies which Iwanowski saw, as a protozoan . . . and says it is the parasite but offers no proof."

In the early 1920's comparatively little was known of the chemical composition of viruses and their physical mechanisms. The stronger optical instruments with vastly improved lighting facilities of today were then not available, at least not in quantity. Superior instrumentation and special apparatus for various purposes of investigations were being invented and perfected. Techniques of filtration, as distinguished from ultrafiltration, and centrifugalization, as distinguished from ultracentrifugation, in cancer

research, were being explored, but were not raised to the present-day level of efficiency and effectiveness. A new era of technical research was in the making. But as yet little more than the groundwork had been laid. In cancer research, Borrel's theory of the carcinogenic virus was taking on more importance, because of the discoveries and proof of parasites as causes of animal cancer. His insistence was that "parasites take part in the spreading of viruses, and" he, Oberling says,⁶⁵ "suspected from the first that in the so-called hereditary cancer of mice something other than genetic factors may be transmitted. More than once I have heard him suggest the passage of some agent by way of the milk." Virus study in cancer was almost completely, if not entirely, dissociated from what study there was of virus diseases of plants, and rightly, it would seem. Since these years, however, scientific knowledge of viruses, their chemical composition and physical mechanisms, has been greatly advanced, and a not inconsiderable portion of the credit has been due to research in the plant sciences. "Most of this work," Oberling explains,⁶⁶ "has been carried out on the plant viruses because they are easily handled, but above all because they are available in such large amounts; in certain plants with mosaic disease 80 per cent of the protein is virus."

Smith to the last years of his life believed that scientists were "far from having sounded all the depths of parasitism."⁶⁷ Extraordinarily he wished to see research in parasitology advanced. He realized that, in animal cancer research, the parasitic hypothesis was gaining favor. Borrel in France had maintained the virus theory at a time when the "greatest courage"⁶⁸ was required to do so. This year Smith, in his address, "Twentieth Century Advances in Cancer Research," was to mention Borrel's work on several points. In plant pathology, moreover, the study of parasites as causes of disease was being extended, especially in two comparatively new research fields, virus diseases and diseases believed to be caused by flagellate protozoa.

This year, on December 28, the American Phytopathological Society would join with the Physiological Section of the Botanical

⁶⁵ *Op. cit.*, vi.

⁶⁶ *Op. cit.*, 121.

⁶⁷ Twentieth century advances in cancer research, *op. cit.*, 317.

⁶⁸ Chas. Oberling, *op. cit.*, vi.

Society of America in a symposium on mosaic diseases of plants. Four of the fourteen papers—one by Kunkel—reported finding “definite bodies of organisms in the tissues of plants affected with mosaic.” H. H. McKinney, Sophie Eckerson, and R. W. Webb described finding intracellular bodies in mosaic of *Hippeastrum johnsonii* and rosette disease of wheat. These “many interesting papers” made Smith “live a month in this day. . . . Fourteen papers on Mosaic diseases,” he exclaimed, “including a magnificent one by Ray Nelson on Trypanosomes and related forms in four plants—bean mosaic, clover mosaic, tomato mosaic and potato leaf roll.” Illustrated by beautiful photomicrographs, Nelson’s paper received an ovation. Two papers by Dr. A. J. Riker on crown gall were of much more than usual interest, but these will be considered at a later point in our narrative.

The symposium on mosaic diseases of plants was regarded as “an important milestone in the progress of plant pathology.”⁶⁹ During the next year, in *Phytopathology*, would appear an abstract of Nelson’s paper on “The occurrence of protozoa in plants affected with mosaic and related diseases.”⁷⁰ Following the publication of his bulletin⁷¹ from the Michigan Agricultural Experiment Station, other studies were made and published.⁷² Charles Atwood Kofoid, eminent protozoologist, and some co-workers at the University of California, however, were unable to agree that the organisms believed to be “a flagellate of new generic rank” were either trypanosomes or of “a protozoan nature.” Nor were such “as might be expected in an organism with trypanosome affinities, or, indeed, of any known protozoan type.”⁷³

Smith was aware that, emerging more or less from the recent study of mosaic diseases of plants, a “great revival of interest in ‘virus’ diseases of plants,”⁷⁴ was taking place. In fact, in his own laboratory during the first half of the year 1922, he and his co-workers had been studying the tomato streak virus, so-called. His intention was presented in an address⁷⁵ of that year:

⁶⁹ *Phytopathology* 13: 33-60, 188-198, 1923.

⁷⁰ *Phytopathology* 13(1): 41, Jan. 1923.

⁷¹ *Mich. Agr. Exp. Sta. Tech. Bull.* 58, 1922, 30 pp.

⁷² *Phytopathology* 13(7): 324-325, 326-329, July 1923.

⁷³ *Idem*, 330-331. See also, Harold Kirby, Charles Atwood Kofoid 1865-1947, *Science* 106(2759): 462-463, Nov. 14, 1947.

⁷⁴ Fifty years of pathology, *op. cit.*, 35.

⁷⁵ Twentieth century advances in cancer research, *op. cit.*, 317.

We must expect to continue to find animal and plant parasites with peculiar methods of propagation difficult to discover and to find parasites much smaller than any microorganisms now known to us. There are plant viruses, the particles of which are so small that an ordinary bacterium swimming among them would be almost like a whale among minnows, or a zeppelin among cockchafers. There is an immense lee-way for living things between the size of the largest molecules and that of the smallest known organisms. There are many filterable virus diseases in plants and more are being discovered every year. The virus of the tomato streak is exceedingly infectious and sometimes kills in three or four weeks yet we have not been able to isolate any organism. The same is true of the tobacco mosaic and yet the least particle of the juice of a diseased tobacco plant will infect a healthy one, and some of the particles of this virus will pass through a filter with pores only $3/100$ micron in diameter (B. M. Duggar).

In March and April of that year, he had tried to isolate, by the use of culture media, a parasitic organism from tomato streak. At one time he had what he described as a "gray white bacterium." "Parasite?" he asked himself. He described his culture media thus: "Various agars, Cohn, Fermic, steamed potato, milk, litmus milk and bouillon of juice and tissue fragments from the interior of the stem under the stripes taking the utmost care to keep out surface contaminations." Other agar plates with various preparations were used. Miss Brown continued the study when other researches required most of his time.

In 1926 Smith expressed⁷⁶ some of his final views on this subject. He believed that "possibly, in the end," the "flagellate protozoan diseases," (concerning which there was by that time "quite a literature"), and virus diseases in plants would be shown to be "one." But he still referred to them as "two groups." Plant virus diseases were to him very "interesting," and he recognized that, while the cause or causes yet remained in doubt, much had been "learned respecting signs of these diseases, host plants, and methods of transmission. Scepticism," he said,

will not down until these assumed flagellates and amocba can be cultivated on artificial media and reinoculated with production of the disease. Allard (1914-1918) showed that tobacco mosaic is spread by aphides, and we now know that many of these diseases are introduced by the bites or punctures of insects, that some of them have several host plants and that in some of these plants they produce no signs. . . . So far, trypanosome-like flagellates have been found principally in plants with a milky juice. Atten-

⁷⁶ Fifty years of pathology, *op. cit.*, 35-36.

tion was first called to these parasites by Lafont in Mauritius in 1909. He discovered them in Euphorbiaceae and pointed out that they were transmitted by insects, but most of the work has been done in the last six years in Portugal, Italy, Russia, Dahomey, and the United States.

Smith said more but, it must be remembered, this was a period of scientific knowledge in the making on these subjects. Modern knowledge has greatly refined and extended our learning of both virus and protozoan diseases of plants.

He had been consulted by many of the early students of plant virus diseases. Since 1892 Iwanowski's demonstration of the filterability of the tobacco mosaic virus had overshadowed in importance his description of cell inclusion corpuscles found associated with the disease. While his filtration experiments had been confirmed, his work on the cell inclusion bodies was not corroborated really until the early 1920's. In 1910 Dr. Lyon in Hawaii described inclusion corpuscles in Fiji disease of sugar cane.⁷⁷ Kunkel's work and the symposium of American plant pathologists brought the discovery and its significance into real notice, and new data followed. On July 19, 1920, F. S. Earle, then located in Puerto Rico and studying sugar cane varieties, had called on Smith at his laboratory and, during their visit, he told Smith⁷⁸ that the "pathologist of the station [there had] found a slime mould constantly present in the cells" of stripe disease of cane. In January Smith mentioned in his diary a "new Dutch paper from Quanjér on mosaic disease of potato. He thinks," he wrote, "it is in many respects like peach yellows and peach rosette to which he refers. I gave him alcoholic material of both diseases last year when he was in this country." May 24 appeared this entry in his diary: "Dr. Iodidi explained to me results of his analysis of spinach mosaic. He finds decrease of total nitrogen and increase of proteid nitrogen and also presence of nitrous nitrogen. All indicating that a *denitrification* occurs in plants attacked by this disease," and on July 1 Smith read the "manuscript of Dr. Iodidi's paper on cabbage mosaic to be published in September" in the journal of the American Chemical Society. Work at Dr. Harper's department of botany, Columbia University, for some

⁷⁷ See, Gustav Seiffert, *Virus diseases in man, animal and plant*, *op. cit.*, 265. Also, L. O. Kunkel, Virus diseases of plants: twenty-five years of progress, 1910-1935, *op. cit.*, 53.

⁷⁸ Quotation Smith's, written into his diary.

years had included research in mosaic plant diseases. On August 24, Dr. Makoto Nishimura, one of his students on this subject, called on Smith at his laboratory. Frederick V. Rand's doctoral thesis, published March 20, 1922 as bulletin 1038 of the Bureau of Plant Industry, "Pecan rosette: its histology, cytology, and relation to other chlorotic diseases," had been begun under Dr. Harper's direction and completed under Smith in his laboratory. November 15-17, 1920, Smith read a revision of this and commented, "He thinks [pecan rosette] resembles the infectious mosaics." Important research in mosaic diseases was being done at the University of Wisconsin where Rand was also a graduate. At Cornell University, Michigan Agricultural College, and probably elsewhere, valuable studies of mosaic diseases and their transmission in cucumber, bean, and other crops, were being made.

In the west curly top of sugar beet was being studied by Eubanks Carsner and others. Carsner had studied at the University of Texas under F. D. Heald and at the University of Wisconsin under Jones. On a few occasions he conferred with Smith, and Smith regarded him as a good and able scientist. On January 6, 1921, after an interview, Smith wrote in his diary:

He has been working for three years in California on the cause and methods of transmission of curly top of the sugar beet and has found a new kink which makes the etiology still more complex. The insect which transmits it from beet to beet transmits it also to *Erodium* but if fed on *Chenopodium murale* the second generation of the insect will not transmit it to beets if transferred directly, but if first fed on *Erodium* and then transferred to sugar beet *will* do so. He has done it over three times, he says, in insect proof cages! I am to give him letters to Dr. H. Noguchi, McCoy, Hitchens and Theobald Smith.

During these years, very little was known of variant strains of viruses in plant diseases, and still less of the origin and nature of variant strains. In 1924 Carsner and C. F. Stahl published in *Phytopathology*⁷⁹ on "The relation of *Chenopodium murale* to curly top of the sugar beet," and announced that an attenuated strain of curly top of beets had been obtained by passing the curly-top virus through the resistant weed host, *Chenopodium murale*. Since these years it has been shown that strains of low and strong

⁷⁹ 14: 57, 1924 (abstract). See also Carsner's study, Attenuation of the virus of sugar beet curly-top, *Phytopathology* 15(12): 745 f., Dec. 1925.

virulence exist in the curly-top virus, and the discoveries of this and other factors have given rise to new interpretations placed on the same or similar fundamental phenomena.

In 1923, when Dr. L. O. Kunkel returned to the United States after several years spent in Hawaii studying diseases of sugar cane and other plants, he accepted a position with the Boyce Thompson Institute for Plant Research at Yonkers, New York, and there performed his valuable studies on the aster yellows. Smith had told him that when he solved the problem of aster yellows he would find the way to solve the peach yellows problem. In 1925 in *Science*⁸⁰ appeared preliminary pronouncements on "Insect transmission and host range of aster yellows," and in 1926, in the *American Journal of Botany*,⁸¹ the article, by Kunkel, which Smith read the following January. This was entitled, "Studies on aster yellows," and of it Smith wrote in his diary:

It is a fine piece of work. He has transmitted the disease to more than 50 plants belonging to 23 families, but not to Rosaceae or Leguminosae. In many ways it is like Peach Yellows and Curly Top of beets. It is transmitted by a leafhopper (*Cicadula sexnotata*) but only after it has fed on diseased plants and then a period of 10 to 14 days must elapse before the hopper can infect plants.

In 1926 Dr. Simon Flexner was directed by the board of the Rockefeller Institute for Medical Research to go to Europe and investigate the subjects of plant pathology and physiology with a view possibly to establishing a department within the Institute. About 1932 a department was created and Dr. Kunkel was placed in charge of the work which since has been combined with that of animal pathology. One of the great laboratories of the world for the study of virus diseases of plants has been developed under his administration, and the work, of course, has included the study of all plant diseases. The late Dr. William Crocker, former managing director of the Boyce Thompson Institute and himself one of the greatest authorities on plant physiology this country has produced, has this to say in his recent book *Growth of Plants*⁸² of the work of Dr. Kunkel at the Thompson Institute:

⁸⁰ 62: 524.

⁸¹ 13: 646-705, Dec. 1926. The leaf hopper *Cicadula sexnotata* is now described as *Macrostoteles divinus* Uhler. See *Phytopathology* 31(2): 120-135, Feb. 1941, and literature cited.

⁸² *Twenty years' research at Boyce Thompson Institute*, 15, N. Y., Reinhold Pub. Corp., 1948. Dr. Kunkel's work is reviewed, pp. 10-15.

During the decade Dr. Kunkel and his associates . . . made the Institute outstanding headquarters for contributions to the knowledge on virus and yellows diseases of plants. Rockefeller Institute for Medical Research, including the branch at Princeton, were, of course, interested in virus diseases as well as other diseases of both humans and animals. They concluded that researches on human, animal, and plant diseases could be carried on profitably in close association, the advance in each contributing to and benefiting by advances in the others. This is especially true of virus diseases. Consequently, Rockefeller Institute employed Dr. Kunkel to head a new division of plant pathology at Princeton, New Jersey, as a part of their now newly named Department of Animal and Plant Pathology.

The Department recently was transferred to the main buildings of the Institute in New York.

In 1926 Dr. Smith spoke of "living carriers of disease [as a] subject [which had] grown to vast proportions and . . . become one of the most important considerations of preventive medicine."⁸³ He said,

We now know definitely that there are human carriers of disease, that is, persons who appear to be well and yet are able to transmit deadly disease to others. . . . Of animal carriers there are many! The trypanosome of rats (*T. lewisi*) is transmitted by a louse; the deadly African Nagana disease of horses and cattle is transmitted by a fly; the surra disease of domestic animals in East Indies and the Philippines is transmitted by a fly; the fatal African sleeping sickness, by the bite of another fly; trench fever, by a louse; the typhus fever of jails, ships, and camps, by a louse; the bubonic plague, from rats, marmots, and ground squirrels to man, often by fleas; stomach cancer of rats, by nematodes carried in the muscles of a cockroach; trichinosis in man, by another nematode living in the muscles of rats and hogs; the sarcoma of rat livers, by the larvae of a tapeworm of the cat; Texas fever of cattle, by a tick; the Rocky Mountain spotted fever of man, by another tick; malarial fever of man and birds by mosquitoes; Dengue fever, by a mosquito; yellow fever, by the same mosquito (*Aedes egypti*); the embryos of the worm *Filaria*, the cause of the enormous human overgrowth known as Elephantiasis, are also taken up and distributed by several kinds of mosquitoes; rat-bite fever is caused by a rat microbe; the Tularemia fever of man is caused by a microbe living in rabbits and it is transmitted by the bites of a deer fly and of a tick, and directly by handling dead wild rabbits; typhoid fever and cholera are transmitted by house flies; purulent ophthalmia, by flies; the Egyptian fluke disease, Bilharzia, by water snails; a similar disease of the Japanese, by another snail and by crabs; a third fluke disease of dogs, cats, and man, by eating raw fish; a tumor of frogs, by a nematode living in earthworms; anaemia with dropsy in salamanders is associated with another nematode; Malta fever in

⁸³ Fifty years of pathology, *op. cit.*, 42-43.

man is caused by a coccus conveyed in milk from diseased goats; the spirochaete of African relapsing fever is transmitted by a tick; the cause of undulant fever, another spirochaete, is carried in the body of a louse and is transmitted by scratching; and so on. The list is a long one. Referring again to the Rocky Mountain spotted fever, transmitted by infected ticks, Spencer and Parker of the Hygienic Laboratory, have discovered a protective vaccine. . . .

Smith concluded his elaboration of this subject by the paragraph on "carriers of plant parasites," quoted in the last chapter.

His proximity to the Hygienic Laboratory of Washington enabled him to keep in close contact with the progress made there in medical studies. Practically all of our knowledge of tularemia came from this laboratory,⁸⁴ and at least once Dr. Edward Francis, surgeon of the United States Public Health Service, consulted with Smith. On January 24, 1921, Smith in his diary described their conference:

He brought slides of a pathogenic organism (schizomycete) for me to photomicrograph. He has obtained it from the Deer fly fever of Utah. This also attacks jack rabbits and can be inoculated into guinea pigs. It attacks the spleen of jack rabbits and if the cut spleen of a diseased rabbit is rubbed on the shared skin of the pig it contracts the disease through the lymphatics. He himself contracted the disease during his two year study. 550 rabbits were shot and dissected and the disease found in 18.

According to Smith, Dr. Francis, during 1919-1920, had isolated in Utah *Bacterium tularense* from seven human cases and seventeen jackrabbits, and called the disease "Tularemia." In 1921 he and Dr. Mayne "proved the deer-fly (*Chrysops discalis*) to be a transmitter of the infection of Tularemia to laboratory animals" and by 1926 this fever had been found in man in the District of Columbia and thirty-one states.⁸⁵ Among the list of animal disease carriers, the other transmitters were mentioned; and in Chapter X a reference to Director McCoy's discovery in 1911 of *Bacterium tularense* in ground squirrels dead of an epidemic disease resembling plague appeared.

The foregoing paragraphs indicate what Smith believed. The reader is cautioned not to regard their factual content as complete condensations of the knowledge to the present time. Since those

⁸⁴ The National Institute of Health, *Science* 107: 615, June 11, 1948, in which many other scientific achievements of the Institute are listed.

⁸⁵ Fifty years of pathology, *op. cit.*, 40-41.

years, new extensions of learning have been brought about, and new interpretations have been placed on some of the factual data. The important points to remember are his studious interest and abundant enthusiasm to know as much as possible concerning diseases of man, animals, and plants. A biography of him can only indicate the wide scope of his knowledge. Dealing with so many differing problems in plant pathology, he had to acquire much learning of animal and human pathology. For instance, when he and Miss Brown studied the tomato streak virus disease and discovered "a curious body (30 and 40 μ in diameter) in cortex cell of a tomato petiole close to a streak spot," he carefully described their findings and illustrated these by drawing six differing positions within range of the nucleus taken by the organism at various times. Then, five days later, Kunkel, recently arrived in Washington from Hawaii, brought with him "stained slides of maize and *Hippeastrum* mosaic," and Smith learned from him his technique of studying the "problematic bodies" there found "usually attached to the nucleus." These seemed to have the "structure of protoplasm and often contain[ed] small normal dark staining bodies *in pairs*." In his diary memorandum, he wrote, "They are most abundant in parts most badly diseased and look to me as if they might be parasites, yet they do not disorganize the nucleus, though sometimes they make it stain differently." Kunkel told him that "in corn mosaic the diseased cells have very active streaming movement on plasma strands." Again he drew a picture of a "vacuolate problematic body," and asked himself, "Is it a protozoan?" But no answer or further opinion was suggested.

Today, these "x-bodies" found in cells of certain plant virus diseases are presumptively regarded as concentrations of virus particles. During the 1920's, research in virus diseases of plants began to take on definitely the stature of a highly specialized branch of experimental science, inquiring into the nature of virus infection, the various viruses and the species and families of plants susceptible to each virus, the ways and vector agents of transmission of plant virus diseases including study of hereditary transmission of diseases from one generation to another; in other words, full-scale research in another class of plant maladies. Cytological and histological investigations became the rule, not

the exception. Dr. Smith was well along in years now and pre-eminently a plant bacteriologist. But he read everything he could find, and among the last articles studied by him was one on the cytology of tobacco mosaic.⁸⁶ That "the x-bodies" were found "constantly present in the cells and not in normal cells" was the fact interesting to him. The year before, Miss Eckerson had described and figured "small flagellate protozoans as the cause of tomato mosaic."⁸⁷ His reading on the "ultra-microscopic viruses of animals" extended to the British *Journal of Experimental Pathology*.⁸⁸ Later, we shall see that in 1926 he especially examined at Cornell University scientific exhibits on the point of protozoa and viruses in plant diseases.

In December of 1921 President M. L. Burton of the University of Michigan had advised Smith that on June 19, 1922, his alma mater wished to confer on him the honorary degree of Doctor of Laws. On the journey to this happy event, he planned to complete his final study of the unfinished points of his manuscript on "Appositional Growth in Crown-Gall Tumors and in Cancers." The ceremonies took place at the annual spring commencement, the graduating address being given by the Secretary of State, the Honorable Charles Evans Hughes. When Smith replied to President Burton's letter and accepted the "great honor," he modestly qualified his acceptance by saying, "it seems to me that I am hardly worthy of it. I believe I could have written acceptable literature if I could have devoted myself to it rather than to science. How much I have accomplished in science is for others to judge. To myself it seems very little for the years I have put upon it."

The University of Michigan had an unbroken tradition against degrees conferred in absentia. Dr. and Mrs. Smith journeyed to Ann Arbor, and on June 19 he received his second honorary doctorate. Professor Arthur Lyon Cross of the Department of History read the characterization, a copy of which was sent after the ceremonies by President Burton to Smith. He was characterized as an

⁸⁶ Bessie Goldstein, A cytological study of the leaves and growing points of healthy and mosaic diseased tobacco plants, *Bull. Torrey Bot. Club* 53(8): 499-599, 29 pls., with bibliography. Smith found the plates "interesting."

⁸⁷ Sophie H. Eckerson, An organism of tomato mosaic, *Botanical Gazette* 81: 204-209, pl. 19-22, Apr. 1926.

⁸⁸ Fifty years of pathology, *op. cit.*, 36, fn. 4.

investigator, a scholar, and a writer of varied gifts and interests. Distinguished for his research in plant pathology and bacteriology presented to the world in notable publications. Furthermore, an extensive series of investigations on plant tumors produced by parasitic organisms led him to a critical interest in the progress of cancer research to which he has made serviceable suggestions. Loyal and self sacrificing public servant. The most productive worker in his particular field, a man exceptionally honored and loved by a large and diverse group of colleagues, the University of Michigan proudly claims him as one of the most eminent of her alumni.

On August 14 Chief of the Bureau of Plant Industry W. A. Taylor transmitted to Dr. Smith Secretary Henry C. Wallace's "personal congratulations on the recognition of [his] scientific service by the University of Michigan. . . . I assume," wrote Dr. Taylor, "that expression of our personal gratification is not necessary. We all trust that you may long continue in the important work you are prosecuting." Secretary Wallace's letter read: "I trust that you will extend my personal congratulations to Dr. Smith, not for the recognition that has been granted to him but for the splendid achievement which has made such recognition inevitable." The Secretary's expressed trust that future conditions in the scientific work of government service would enable the Department to "show more appreciation of such exceptional talents as Dr. Smith has displayed" was to be realized within a few years. He would arrange a trip to Europe for Dr. Smith.

En route to the University of Michigan ceremonies, Smith stopped at Buffalo and spent two days at the Gratwick laboratory. There he examined cancer research slides and experiments, and "talked shop" with Drs. Gaylord and Simpson, and a young Austrian chemist, Carl F. Cori. He read several of Cohnheim's papers, and on the afternoon of the second day was driven by Dr. Gaylord to Springville where he "saw Dr. Marsh and some of his mouse carcinoma experiments. Among other things," Smith said in his diary, "I saw the nematodes that are lodged in the colon of the cancerous strain in great numbers and in much less numbers in the colon of the strain that is less subject to cancer. He is breeding mice free from the nematode and they will soon reach cancerous age."

At Detroit Smith continued his study of cancer slides, this time in the laboratory of pathology at Grace Hospital. He had lunch and in his offices and at the hospital conferred with Dr. Rollin

H. Stevens. After two more days of study, he went on to Ann Arbor and there found more cancer slide materials. Dr. Warthin, he found on June 22, "has thousands of cancer slides and it would pay me to come here for two months to study them." Dr. Warthin disagreed with him that "cancer grows by apposition." But when at Chicago he became acquainted with Dr. H. Gideon Wells, formerly dean of medical work at the University of Chicago and then director of medical research at the Otho A. Sprague Memorial Institute, he found a medical scientist in partial agreement with him, at least. "Wells," he wrote in his diary, "says he thinks cancer of the liver grows by apposition from his own observation on primary liver tumors. He gave me very helpful references to half a dozen papers. He says all the pathologists who have worked on pr[imary] liver tumors are agreed on this." At Chicago, Smith first called on Professor E. O. Jordan. Wells, he said, "who is a delightful man to meet, studied under [Ludvig] Hektoen, and Jordan at Mass[achusetts] Tech. under Sedgwick about whom we talked. He studied under Duclaux and is pleased with the Pasteur book. He says D[uclaux] was a very genial man."

Smith spent the next several days reading in the John Crerar Library on primary carcinoma of the liver. On June 29 he happily wrote in his diary: "I have found very interesting confirmation of my views, and have read about a dozen papers in English and German and abstracted them. I have now the best part of the recent literature and am ready to see Dr. MacCallum's slides." At this library he renewed his acquaintance with J. Christian Bay, who was then manager of the medical part and who during many recent years has since been librarian there. In 1948 Dr. Bay, in an article in *Science*⁸⁹ on "Some vital books in science: 1848-1947," characterized Dr. Smith's three volume work, *Bacteria in Relation to Plant Diseases*, as "a peer in its field."

While in Chicago, Smith called also on Miss Maude Slye, whose researches in cancer (especially on heredity) he alluded to appreciatively in addresses several times and whose work on breeding races of mice formed part of not only his diary account of his visit to her laboratory but also of the discussion of his address that year on "Twentieth Century Advances in Cancer Research."

Cleveland was the final destination of Dr. and Mrs. Smith's

⁸⁹ 107(2785): 490, May 14, 1948.

journey. They had attended several pleasant social affairs. At Ann Arbor Dr. Smith sat several times for his portrait, an honor arranged for by Dr. Harley Harris Bartlett, professor of botany at the university and a close and good friend for many years. At Cleveland they spent some time with Norman P. Buffett, brother of Charlotte May Buffett Smith. From a research point of view their Chicago stay had been "very profitable," and so immediately Smith began to go forward with his studies. He first tried to consult with Dr. George W. Crile of the famous Cleveland Clinic at Euclid Avenue and 93rd Street. He was out of the city, so, until his return, Smith spent his time consulting other doctors of the Clinic and the Lakeside Hospital. On July 5 he had luncheon with Dr. Crile who introduced him to more doctors and to Miss Amy F. Rowland who was making some electrical conductivity experiments with animal tissues in the laboratories. So interested was Dr. Crile in Dr. Smith's work that he offered him "all the facilities of the laboratory" if Smith wished to make conductivity tests on crown gall. Dr. and Mrs. Smith arrived in Washington July 6, he highly elated and with a prepared insert on primary liver tumors, and literature, for his paper on appositional growth in crown gall tumors and in cancers.⁹⁰

The following year, on March 8, he wrote to Dr. Bloodgood:

I am in high feather this afternoon because I have discovered a paper by Dr. H. T. Deelman of Amsterdam, in which he says that tar cancer grows by apposition as well as invasively. His exact expression is that there are two ways of growth, first, an invasive growth "aus sich heraus," and second, a horizontal growth in all directions by cancerous conversion of neighboring cells. What a pity I didn't know of this in time to make a reference to it in my Journal of Cancer Research paper. He thinks Ribbert and his followers are all wrong. He says that the neighboring cells of the skin become first enlarged and then cancerous, just as I find in the crown gall.⁹¹

In December 1922 Dr. and Mrs. Smith visited again in Ann Arbor, and also this time at Lansing. To botanists and bacteriologists, about seventy-five persons in the audience at Ann Arbor and at least twenty botanists at Lansing, he repeated his lecture

⁹⁰ Appositional growth in crown-gall tumors and in cancers, *Jour. Cancer Research* 7: 1-105, 28 pls., 1922, Literature ref., pp. 47-49.

⁹¹ Smith told Bloodgood that Dr. Deelman's paper was found in *Klinische Wochenschrift*: 1. Jahrg. Nr. 29, 15 July, 1922, pp. 1455-1457.

on "Twentieth Century Advances in Cancer Research,"⁹² presented first a few days previously at Detroit before the Radiological Society of North America. On the afternoon of December 7, at the society's annual meeting held at the Hotel Statler, he had spoken before an audience of four hundred and fifty persons who vigorously applauded him. At all three places the lecture evoked interest. Dr. Bloodgood made Smith promise to give it before his medical students in January. Dr. Albert John Ochsner, noted surgeon of Chicago, who spoke also before a joint meeting of the society and the county medical organization on "Cancer from the Surgical Standpoint," alluded to Smith's work during his address. Smith became acquainted with him and that night told in his diary: "He believes cancer is due to a parasite. . . . [H]e said alluding to the sceptics, 'You have got them.' 'Next time you go to Chicago let us know and we will be at home.'"

Smith's exhibits attracted "very little attention." Ten photomicrographic illustrations of rat Spiroptera cancer mounted together on a white card board was one, and this will be mentioned again later. Similarly mounted crown gall photographs of dwarfings and killings of plants due to crown gall, solid embryomas due to crown gall, tumor strands in crown gall, and other carefully chosen charts and illustrations comprised another exhibit. When his paper was published and reprints sent out, however, several cancer research workers thanked him. Dr. Alexis Carrel of the Rockefeller Institute, without criticism, found the contents "most interesting." Peyton Rous sent "good wishes for a continuance of your significant work. [Smith had] disclosed to [him] many new things for which I," Rous said, "have to thank you even more than for the just exposition of my own work." Dr. J. M. T. Finney of Johns Hopkins wrote that he had read the address "with the greatest interest and profit." He quoted from it in a lecture to medical men from the northwest and told them to visit Smith in his laboratory and see "the excellent work" being done. He thought the reprint "most interesting and scholarly," and he told Smith he believed he had "thrown a good deal of light on the cancer question; your work," he said, "has certainly given very interesting sidelights. . . ." Dr. M. C.

⁹² *Jour. Radiology* 4(9): 295-317, Sept. 1923.

Marsh of the New York State Institute for Study of Malignant Disease congratulated Smith on presenting "what we need. With Fibiger's two summaries of experimental cancer research in French and German and yours in English, workers can take stock in three languages."

On December 2, en route to Detroit, Smith spent another day at Buffalo with Drs. Gaylord, Simpson, Marsh, Cori, and Carey. He saw some of C. A. Kofoid's slides of intestinal amoebae which he thought looked "astonishingly like some of the problematic cell inclusions found in breast cancer in man." While Dr. Kofoid was director of the parasitological division of the California State Board of Health, he maintained a laboratory to make systematic examinations of persons for intestinal parasites. An extensive series of papers on flagellates, amoebae, and ciliates of man and other animals was published. The following December 27 Smith found Kofoid at the annual meeting of the American Association for the Advancement of Science and discussed with him his recent article on Hodgkins' disease in the *Journal of the American Medical Association*.⁹³ A theory that the origin of this disease may be associated with cells of the protozoan *Endameba dysenteriae* had been advanced, and Smith, after receiving Kofoid's explanation, believed he had "demonstrated it. He wants," Smith wrote in his diary, "a large sum of money to head a drive on Cancer with a parasite in view."

That month, in his address on twentieth-century cancer research, he had described under "Worm Cancers"⁹⁴ three main series of specific experiments: "Fibiger's rat carcinoma due to cockroach-nematodes," "Kopsch's frog tumors due to angleworm nematodes," and "the rat liver-cyst sarcoma due to tapeworm of the cat." He introduced these more important experimental investigations by listing instances where repeated attention had been drawn to "the suspicious close association of various parasitic worms" with tumors of animals and plants. "We have," he said,

destructive nematode tumors in plants. One is due to *Heterodera radicolica*. It occurs on the roots of a great variety of plants and is more injurious than crown-gall. Structurally, the tumor is a soft, perishable, small-celled connective-tissue hyperplasia containing a great many large multinucleate

⁹³ See, Twentieth century advances in cancer research, *op. cit.*, 303.

⁹⁴ *Ibid*, 303-311. Quotation of paragraph, p. 303.

giant cells. Many plants are killed by this tumor, and many others are dwarfed so badly as to be worthless. It occurs on a multitude of hosts in many parts of the world and has not received the attention it deserves. Other destructive above-ground plant tumors are due to nematodes of the genus *Tylenchus*. These occur on wheat, clover and other plants.

While in Ann Arbor, Dr. and Mrs. Smith visited in the home of Dr. and Mrs. Bradley Moore Davis. Dr. Davis was now a professor of botany at the university and on December 11, the two men conversed on the "possibility of destruction or modification of some chromosome as [a] basis for the cell modification in cancer." Smith asked Davis "if he accepted bud variation" in plants, and his reply was "yes."⁹⁵ In his address,⁹⁶ Smith had suggested that "some element of the cell or of its environment which ordinarily acts as a break on cell division is destroyed or weakened by the action of certain physical and chemical activities (x-ray burns, common burns, arsenic, cobalt, tar products, products of worms and of bacteria) and so we get a cancer, that is, a cell-multiplication passing beyond physiological control." In 1922-1923 he was planning some new experiments, and by 1926 he was prepared to suggest in a formal paper on "Recent Cancer Research"⁹⁷ the possibility of chromosomic destruction with a subsequent abnormal affection of all descendants of all the cell or cell-complex.

There was another point in crown gall research about which Smith was unwilling to venture a final opinion until later. He did not disagree with C. O. Jensen that, in crown gall, *Bacterium tumefaciens* converts normal cells into tumor cells. But, on the basis of a lack of scientific evidence at that time, he questioned the conclusion that if the bacteria die out the cells still retain and transmit their acquired characters. He awaited more corroborative evidence for the present view that once the genes within the chromosomes have been changed from normal to tumor cells, in crown gall the tumor cells without continued stimulus from the bacteria will continue to grow indefinitely and autonomously. Yet, before the end of his life, he candidly conceded that experimental proof might be produced to show that cells could be so "ab-

⁹⁵ Smith's diary, Dec. 11, 1922, p. 345.

⁹⁶ Twentieth century advances in cancer research, *op. cit.*, 315.

⁹⁷ *Amer. Naturalist* 60(668): 247, May-June.

normally changed that they will go on indefinitely" growing and multiplying without the primary cause subsisting.⁹⁸

In 1925, at the eighteenth meeting of the American Association for Cancer Research, Dr. Ewing would ask him whether he maintained that a parasitic organism exists in connection with those experimental cancers produced by repeated coal tar paintings, a subject we shall discuss further very soon. Dr. Ewing would ask Dr. Smith, "Does the microorganism get in with the tar? . . . Or are we required to assume that there are a great many factors in the production of cancer?" Dr. Smith would reply:

Direct irritation certainly cannot be the cause of tar-cancer. It remains to be explained why some spots of the painted area respond while most do not. We do not know just how chemical substances act on the cells to produce a malignant process, and whether a parasite is necessary after the change once occurs for it to go on. It may be that several types of chemical stimulus will give the same result, and that we might have cells so abnormally changed that they will go on indefinitely without the primary cause continuing to act.

Since his earliest publications on crown gall, Smith had believed that the bacteria were located intracellularly. In bulletin 255, "The structure and development of crown gall: a plant cancer,"⁹⁹ he had pronounced the situs of the bacteria "*must occur* within the cells," since the microscope did not show their occurrence between the cells or in the vessels. Because of the proof that bacteria had been found in the tumor, this conclusion had seemed to him inescapable unless the causal organism was to be supposed as "ultramicroscopic, i. e., totally unlike their form on culture-media, and also unlike the rods and Y-shaped bodies that diffuse out of the sections. Then, of course," he said, "they might occur anywhere." In 1911 his belief was that the stimulus of the mechanism of the controlling cell division came from within the cell, and in a footnote he had said:¹⁰⁰ "It is inconceivable to the writer that a foreign organism, by any localized and brief presence in the tissues, should so modify cell inheritance that, after the organism and its products have disappeared, the cells should continue to develop abnormally rather than return to their normal habit." No later than 1922 he confessed as to crown gall "an

⁹⁸ *Jour. Cancer Research* 9: 497, 1925.

⁹⁹ *Op. cit.*, 17.

¹⁰⁰ *Idem*, fn. 1.

entire revision of views as to the nature of the disease.”¹⁰¹ In his textbook (1920)¹⁰² he had said that Jensen’s belief in an autonomous growth of parasitized cells without the aid of the bacteria, that is, in their absence, was “a subject for further consideration.”

Biologist M. C. Marsh of the New York State Institute for Study of Malignant Disease answered on February 3, 1924, an inquiry from Smith about some of his experiments made with mice along this line. “Yes,” said Marsh, “we are getting interesting results from mice. That quite expresses it. They add to, rather than subtract from, the difficulties, and indicate the need of long drawn out replanned experiments. The absence of worms does not stop the tumors from developing.” Marsh’s studies on breeding and the hereditary aspects of cancer etiology interested Smith for years; he mentioned them in several addresses, and Marsh was now planning experiments to “continue generations of worm-free mice indefinitely.”

At the 1925 cancer research association meeting, Dr. Smith entered into the discussion which followed the reading of a paper by Dr. Leonell C. Strong on “Genetic deviations within the transplantable tumor cell.” He urged:¹⁰³

It would be interesting if it could be made out definitely that through irritation certain factors or structures inside the cell are destroyed or weakened. I have for a long time thought it might lead to a solution of the cancer problem, if the cytologist could make out changes in the cell which would give to its progeny an aberrant course. Boveri called attention to such changes.

Irritations, as a factor leading to cancer in susceptible individuals, were believed by Smith to be of numerous kinds.¹⁰⁴ Repeated tar paintings fell in this category, and these are yet to be considered more fully. Woundings due to repeated applications of arsenic, anilin, paraffine, and other substances were another. The latter has significance in crown gall study as well as cancer. In 1938, in a study of “Growth Substance and the Development of Crown Gall,”¹⁰⁵ Dr. Riker, Dr. B. M. Duggar,

¹⁰¹ Synopsis of researches of Erwin F. Smith, *op. cit.*, 39-40.

¹⁰² *Intro. bact. dis. of plants, op. cit.*, 569.

¹⁰³ *Jour. Cancer Research* 9: 509, 510, 1925.

¹⁰⁴ *Jour. Heredity* (Organ of the American Genetic Assoc.), 16(2): 61-62, Feb. 1925.

¹⁰⁵ *Jour. Agric. Res.* 57(1): 21-39, July 1, 1938.

and Dr. S. B. Locke explained the association of several hormone effects with crown gall infection and, in the course of their discussion, cited another study¹⁰⁶ by Riker and T. O. Berge to the effect that, "following Smith," they have listed "numerous other chemical agents capable of stimulating cell division locally." A conclusion reached was: "The great diversity of materials reported to cause stimulation suggests that irritation or injury induced by them, which is apparently a common characteristic, is more important than the nature of the substance." This would seem to add credence to Smith's earlier beliefs about irritants and injuries as causal factors in the processes of gall formation.

Smith's studies of the processes of tumor formation included examining the nature, amount, and the continuity of chemical-physical stimuli required to induce structural modification and tissue reactions of variously aged plants under differing environmental conditions. After publishing his paper, "Appositional Growth in Crown-Gall Tumors and in Cancers," he wrote Brierley,

My last paper I think throws considerable light on the early development of the tumor and ought to be of more than a little interest to cancer research workers. I am planning another paper on what goes on inside the cell, a subject which so far I have not published much upon. I would like very much also to see the tumor develop under the microscope from single cells, and I believe that I can bring it about with enough patience. This I am hoping to try the coming year.

His diary reveals that on August 30, 1922, he began new experiments to demonstrate bacteria in the cells. Nothing in his diary or letters discloses that he had read in *Phytopathology*¹⁰⁷ a preliminary announcement that year that Dr. Riker, from "evidence secured from microscopic study of the tissues at different stages of gall formation and from the development of the original and secondary galls," was securing evidence on the location of the bacteria in crown gall. Dr. Riker reported their location to be intercellular, rather than intracellular, as Dr. Smith had said they must be. According to the new pronouncements, *Bacterium tumefaciens* lives in the gall tissue of the host plant "between the cells," rather than within the cells, "Its intercellular position and

¹⁰⁶ Atypical and pathological multiplication of cells approached through studies on crown gall, *Amer. Jour. Cancer* 25: 310-357, 1935.

¹⁰⁷ Studies of crown gall, *Phytopath.*, 12(1): 55, Jan. 1922.

its scarcity," said Riker, "apparently contribute to the difficulty of demonstrating it in the tissues. Under certain conditions, the organism was found to travel through the vascular bundles."

In November of 1922, Dr. L. R. Jones read in the Medical Library of the University of Wisconsin a copy of Smith's article, "Appositional Growth in Crown-Gall Tumors and in Cancers." On December 1 he sent Smith a letter to congratulate him on his paper and to extend him the courtesy of advance knowledge of two papers on crown gall to be given by Riker at the forthcoming Boston meeting of the American Phytopathological Society. Your "various lecture calls," wrote Jones,

show that your missionary work is arousing interest among medical men in the search for a parasitic factor in animal cancer. Congratulations as well as sympathy for the distractions. I regret to possibly add another by urging your attention to a matter which may distract you for the moment but which, unless I mistake, may then bring more rapid progress. I note that in this Journal article you again call attention to the intracellular position of *B. tumefaciens*. One of our keenest young men, Dr. A. J. Riker, published some months ago (*Phytopathology* 12: 55-56., Jan. 1922) his judgment that the crown-gall organism lives between the cells of the host rather than intracellularly. He has since advanced the demonstrations of this to our satisfaction as well as his own, and has sent in two notes (abstracts) for the Boston program, of which I enclose carbons. One MS. (corresponding to Abstract No. 1) would have been released before this if I had done my part editorially as promptly as he has his, and the other is nearly ready for submission (developing abstract No. 2). I am sure you will accept his evidence as convincing and we would like you to be the first to see it. He is going to take his materials (microscopic mounts and lantern slides) to Boston.

Jones invited Smith to visit them at Madison:

It offers a fortunate possibility that you are coming west next week. I am . . . writing in hope that you can come from Detroit to Madison next week and spend at least a day or two as our guest, going over this most interesting and important matter. I am sure you will be delighted to know Riker personally as well as to see his fine workmanship and its results, and he, of course, wishes you above all others to see and judge of the evidence.

After presenting at Detroit, Ann Arbor, and Lansing his address on twentieth-century cancer research, Smith tried to go on to Madison. But he got not further than Lansing where, because of heavy snows, he and Mrs. Smith had to change their plans and

return to Washington. At Lansing he examined "Ray Nelson's photos of Trypanosomes in phloem of Bean mosaic, Tomato mosaic, Clover mosaic and Potato leaf curl. . . . It is a great discovery," he wrote in his diary, "and Nelson or someone will be able to complete the proof of pathogenesis, beyond a doubt." As it happened, when he arrived in Washington, he learned at the Department of Agriculture that Dr. Jones was in the city. The next evening he and Dr. R. A. Harper called on Dr. and Mrs. Smith at their home. They "discussed Academy matters and other things." A few days later, Dr. Smith and Dr. Jones "drew up a preliminary report on Nomenclature of bacteria for the Boston meeting of the Phytopathologists. I," Smith wrote in his diary, "am the chairman of a committee they appointed for that purpose last year." Later that day, Dr. H. H. Bartlett and he spent an hour together in his laboratory. Recently Bartlett had been appointed head of the department of botany at the University of Michigan.

The fourteenth meeting of the American Phytopathological Society was held in the main building of the Massachusetts Institute of Technology at Cambridge. President E. C. Stakman presided, and seventy-five papers were read. On December 27 Smith listened to a paper by Melville T. Cook on galls, and the next afternoon Dr. Riker's two papers, "The location of the crown-gall organism in its host tissues" and "Some morphological responses of the host tissue to the crown-gall organism,"¹⁰⁸ were read. Smith's diary account will be of interest to every botanist:

In the p. m. two papers on crown-gall which may require us to revise all our notions as to the location of the bacteria. He thinks they are all between the cells. Botanists' dinner at Youngs Hotel in evening 225 present. Address by Dr. [Charles Elmer] Allen of Uni[versity] of Wisconsin on the potentialities of the cell—very good. Dinner also very good. I sat with Riker and his new wife. They were married on the 26[th] in Phila[delphia]. She is a Smith College woman specializing in Botany and Chemistry in U[niversity] of Wisc[onsin] where she met Riker. I like her and him. He is an overseas soldier.

When Dr. Smith met Dr. and Mrs. Riker for the first time, he said, "Oh you are the young people on your honeymoon. Let

¹⁰⁸ *Phytopathology* 13(1): 43 (abstracts), Jan. 1923. See also, Some relations of the crown-gall organism to its host tissue, by Riker, *Jour. Agric. Res.* 25: 119-132, 1923; Some morphological responses of the host tissue to the crown-gall organism, *Jour. Agric. Res.* 26(9): 425-435, Dec. 1923.

me give you a word of advice. It always takes two to make a quarrel.”¹⁰⁹

The next day Smith attended a conference of fifty-five plant pathologists on extension work. He thought them “splendid fellows, mostly young. Very eager to help the farmers.” He himself was approaching the retirement age. Under the provisions of the Retirement Act of May 22, 1920, he in 1924, after thirty-seven years with the Department of Agriculture and at the age of seventy, could retire from active scientific service. He would choose, however, to apply for a two-years extension, and, at a salary of \$5,500 a year, the extension would be granted. His laboratory of plant pathology possessed a personnel of eighteen employees and he, as its chief, supervised an annual expenditure of approximately \$52,460. In July 1922 he had explained one of his main reasons for his perseverance: “if I,” he said, “succeed in turning the tide of cancer research into biological channels I shall be more than repaid.” In 1923 Smith was unable to get at his crown gall researches again until late in January. He tried to begin work on a “revision and completion of [his] ms. for Volume IV of [his] Carnegie monograph,” *Bacteria in Relation to Plant Diseases*. “I shall never complete it if I don’t get going,” he commented in his diary. “There is a good half-year’s work on it, and perhaps more.”

His attitude toward Riker’s papers was one of awaiting further scientific proof. On May 14, 1923, he acknowledged receiving from Professor Etienne Foëx of the Station de Pathologie Végétale, Paris, France, some specimens of diseased tobacco leaves, two letters, and a paper by Foëx which summarized Smith’s studies on crown gall. “Concerning Riker’s work,” Smith wrote,

it is as yet only in the “dough,” so to speak, and we must wait for a full paper before any decision can be come to and probably not then. I heard his paper . . . and at that time pointed out that he had not shown anything which we did not already know, namely, in the needle wound and its vicinity, that is as far as the wound exudate is spurted when the needle is driven in, there, of course, the bacteria will be carried and found. This does not, I think, explain what we have had very often, namely, the occurrence of secondary tumors at a long distance from the primary tumor in leaves already fairly well developed when the needle prick was made in the stem below the leaf. Until we have ourselves gone over Riker’s work critically,

¹⁰⁹ Told author by Dr. Riker.

and by that I mean experimentally using his methods, I am not prepared to admit that he has shown anything except presence of the bacteria in the vicinity of the needle wound already known about. However, as I have worked year after year on the crown gall and have studied the cancer problem in its larger bearings as it behaves in animal tissues I believe that too much emphasis has been put on the supposed fundamental differences between cancers and granulomas. In other words, I believe the difference is much less than has been supposed and is still supposed by a majority of the workers.

The intercellular or intracellular location of the bacteria in crown gall is still, even today, under discussion among the most reputable scientific authorities.¹¹⁰ Workers do not agree entirely on fundamental points of how "tumor strands" and "secondary tumors" or "secondary galls" are formed in crown gall.¹¹¹ Crown gall research is still very much in the foreground of plant scientific research. But the early studies of Dr. Smith and his co-workers are still regarded as "classic" authority, even by those who, on some fundamentals, do not agree with his (or their) conclusions and interpretations. As research facilities have become more adequate for the purposes of studying these fundamental phenomena, newly discovered truths and factual data were inevitable. In all branches of science, from generation to generation, this has been so. But no one may deny that Dr. Smith was first to observe these phenomena, to work out proper methods for their study, and to foresee the possible correlations with animal cancer research. A very considerable proportion of his findings as to crown gall has withstood the test of time. Surely his place as the leading pioneer student of this disease in plant pathology is secure.

During his lifetime, Smith knew that some revisions of his views on crown gall might become necessary. In April 1923 was published from England a paper by W. Robinson and H. Walkden entitled "A Critical Study of Crown Gall."¹¹² Smith read this on June 18, and that night wrote in his diary:

There is a paper in April Annals of Botany attacking, and perhaps overthrowing, most of my crown-gall hypotheses, which makes me feel very

¹¹⁰ Riker, Spoerl and Gutsche, Some comparisons of bacterial plant galls and of their causal agents, *op. cit.*, 69-74.

¹¹¹ Armin C. Braun, Development of secondary tumors and tumor strands in the crown gall of sunflowers (review of literature), *Phytopathology* 31(2): 135-137 f., Feb. 1941; Riker, Spoerl and Gutsche, *op. cit.*, *idem*.

¹¹² *Annals of Botany* (London), 37: 299-324.

humble; but if I could not make the discoveries they report (which confirm Riker's) I have at least stirred up others to do so, and there are some things I must see for myself before I shall be convinced. Probably my desire to help in the solution of the cancer problem has led me to over-stress phases of the interpretations, and anyway the hundreds of clear-cut experiments remain whatever the conclusion drawn from them. Next I am going to study the Potentiometer reactions of the crown-gall tumors and after that I will try to produce tumors out of single cells by means of the Barber apparatus.

It may be added now that Dr. Smith did not change his views on crown gall research during the balance of his life, although, as we shall see, he continued to study points concerning the "tumor strand," the "secondary tumor" and location of the bacteria in crown gall throughout his remaining years. For the time being, however, this must end our consideration of this subject. In February 1925, while visiting at the Pasteur Institute, he would defend Riker's skill as a careful scientific workman, and more, he would recommend Riker to the International Education Board for a year's study abroad when, among other places, he and Mrs. Riker would study at the Pasteur Institute.

The new papers of, first, Dr. Riker, and then, Robinson and Walkden, were published after Dr. Smith presented his address on "Twentieth Century Advances in Cancer Research" but before the address was published. He did not delete his quotation from Dr. E. J. Butler's *London Lancet*¹¹³ paper, "Some Relations between Vegetable and Human Pathology," in which was mentioned the "extreme difficulty of detecting the parasite in the cells." To his parenthetical comment, "it is certainly *in*, not between the cells," Smith, presumably, suffixed a footnote: "Recently Riker in the United States and Robinson and Walkden in England have denied this, maintaining that the organism is always between the cells. . . ." In August of 1921, Dr. Butler, then in America, had spent four hours with Smith at his laboratory, and had been shown "many specimens and many stained slides." For nineteen years he had served as a plant pathologist in India, at Pusa, and was now Director of the Imperial Bureau of Mycology or plant pathology at Kew. Smith first saw his paper on May 18, 1922, and said of it in his diary:

¹¹³ Jan. 21, 1922, p. 160.

Dr. E. J. Butler has written the best account of our work on Crown Gall that I have seen. He has got in it all my notions about its relations to Cancer and has made no mistakes. It forms the first part of a paper he read on relation of botanical research to human diseases before an English organization devoted to Tropical Medicine.

The location of the parasite was significant, Smith believed, from the standpoint of cancer research. The analogy between crown gall and animal cancer was admittedly not complete. But his persistent experiments to demonstrate bacteria within the cells had extended over many years and were planned as studies in cellular pathology. The first paragraph of his conclusions as to "Twentieth Century Advances in Cancer Research"¹¹⁴ began:

What goes on inside a cancer cell that does not go on inside a normal cell? If we knew that we should be very near the solution of the cancer problem, and it is not beyond hope that eventually we shall know just that—chemically and structurally. We must conclude that the cell or its progenitors has been under a foreign stimulus of some sort. Everything we know about cancer points to this conclusion. In case of the crown-gall bacteria, acid and alkaline by-products are given off in simple culture media and we have a right to assume that they are also given off inside the plant where similar proteids and sugars are at their disposal. When applied to the surface of the plant these substances set up chemical and physical changes which lead to an excessive, disordered, hyperplasia growth and they probably do this in the tissues where they are produced by the parasite, but here our knowledge ends.

This was so for crown gall. But, alike for tumors in animals and perhaps in man, physical and chemical activities from many causes—parasites among them—were known to bring about the "cell-multiplication passing beyond physiological control" which was one of the phrases used by Smith to explain cancer. Later he spoke of "multiplying extraneous organisms" as activating the origin of malignant tumors.

Since Dr. Smith's years of research, in cancer laboratories of the world, the transformation of normal mammalian cells into cancer cells has been observed in vitro. A vast amount of environmental and genetic learning about cancer induction has been accumulated. Knowledge of the modes of cancer transmission is superior, especially in the direction of cytoplasmic inheritance and transplacental transmission. Indeed, the knowledge of chemically in-

¹¹⁴ *Op. cit.*, 315.

duced sarcomata has been extended.¹¹⁵ Such a discovery as that made by John J. Bittner in 1936 that a cancer virus may be transmitted by a susceptible mother to her offspring through her milk, a conclusion still very much in doubt,¹¹⁶ finds adherents of the virus theory of cancer etiology defending the hypothesis and as many, perhaps more, equally qualified cancer authorities opposing it. Smith contributed knowledge to a research period from which much of the modern, more advanced knowledge has emerged, or on which in considerable part it has been based. When we consider the subject of tar cancers, more will be said about carcinogenic agents. But it may be suggested now that, had not the chemical compounds been made available, *Bacterium tumefaciens*, as such an agent, likely would have found more of a place in cancer research laboratories as well as in laboratories of plant pathology.

Several times Dr. Smith in writings or utterances had expressed confidence that scientific proof would settle the question whether a parasitized cell would continue to grow if the parasite died out, lost virulence, or was removed. In recent years, from research in crown gall, an answer has been made available. Dr. Philip R. White of the Lankenau Hospital of Philadelphia and Dr. Armin C. Braun of the department of animal and plant pathology of the Rockefeller Institute, with the use of a plant tissue culture technique, and taking advantage of the native sterility of secondary crown gall tumors on sunflower, have isolated and cultivated *in vitro* tissues of varying degrees of malignancy. Some of these have been shown to be "capable of producing bacteria-free crown-gall tumors when grafted back into sunflower or artichoke after prolonged multiplication *in vitro*. The tumor-forming capacity has thus been segregated from the bacteria and the tumor can be studied and its metabolic processes followed in the absence of the original inciting agent."¹¹⁷ In 1942, while Dr. White was yet with the department of animal and plant pathology of the Rockefeller Institute, he and Dr. Braun published in *Cancer Research*¹¹⁸ an important study, the product of their researches and

¹¹⁵ See, Leonell C. Strong, A new influence on chemically induced sarcomata, *Science* 108(2816): 688-689, Dec. 17, 1948.

¹¹⁶ Charles Oberling, *The riddle of cancer*, *op. cit.*, 82, 162.

¹¹⁷ Philip R. White, Plant tissue cultures, *Ann. Rev. of Biochemistry* 11: 624, 1942. See also *Science* 94(2436): 239-241, Sept. 5, 1941.

¹¹⁸ 2(9): 597-617, Sept. 1942.

collaboration, entitled "A cancerous neoplasm of plants. Autonomous bacteria-free crown-gall tissue." Their summary reads:

Secondary or metastatic tumors frequently arise on sunflower plants inoculated with *Phytomonas* [*Bacterium*] *tumefaciens* at considerable distances from the primary tumefactions. These have been shown by cultural and serological methods to be characteristically bacteria-free. Tissue cultures from these tumors show a rapid disorganized type of growth contrasting sharply with the slow, moderately organized growth of those isolated from healthy tissues. Upon implantation into uninfected plants of the same or related species, these tissues produce typical crown-gall tumors. This capacity for unrestrained, invasive, potentially malignant growth both *in vivo* and *in vitro* in the absence of the original excitant differentiates these tissues from any other plant materials reported to date and places them in a category comparable to cancerous growths in animals.

Smith's unvarying aim had been to enlist medical research men to study "the symbiotic action of organisms . . . an almost untouched field in medicine and yet . . . one of the commonest and most striking things in Nature, witness the lichen," he said.¹¹⁹ But he cautioned not to expect as many parasites as there are forms. The ability of experimental scientists to produce "cancer in plants by means of bacteria, in chickens by means of a filterable virus (Rous), in rats by means of a nematode (Fibiger), in rats by means of a tapeworm (Bullock and Curtis), and in rabbits and mice by means of tar (Yamagiwa, Tsutsui, Fibiger, and many others)" was of utmost significance to him. "The cause of cancer, so far as we now understand it," he urged,¹²⁰

may be defined as an irritation acting on an organ or organs unable to withstand it owing to a transmitted or an acquired weakness. Heredity alone cannot cause cancer, but irritation (parasitic and possibly non-parasitic) plus heredity can and does cause it. No such conclusions could possibly have been drawn twenty years ago. They are the measure of the progress we have made.

In January of 1924, when he sent Dr. Gaylord a reprint, he spoke of this address as "a popular paper" prepared "with reference to etiology." Smith realized that Dr. Rous and his associates (chiefly Dr. James B. Murphy) had not isolated the "agent or virus" and determined the cause of the chicken sarcomas but, said he, "experimenting through a series of years

¹¹⁹ Twentieth century advances in cancer research, *op. cit.*, 317.

¹²⁰ *Idem*, 316-317.

they demonstrated indubitably that the filtrate of the crushed cells will cause the disease, and a little later that the virus persists in cells killed by freezing, by heat, by drying, and by glycerin." ¹²¹ Even today, this presumably "'living organism,'" ultramicroscopic in some, perhaps all, of its forms, an organism having specific attributes or properties, distinguishable from ferments, and practically as capable of inducing chicken tumors as the carcinogenic hydrocarbons, can survive physical cell destruction, and yet it has not been isolated and cultivated in pure cultures *in vitro* as well as *in vivo*. Very important research has been performed in recent years on this virus-like agent of chicken tumor. At a meeting of the American Philosophical Society on April 19, 1912, Rous read a paper on "An avian tumor in its relation to the tumor problem." ¹²² Smith may have attended this meeting, although not yet a member of the Society, and, much enthused, sent Rous his letter of May 16 quoted in Chapter IX. In his address "Twentieth Century Advances in Cancer Research," he quoted from Rous' paper several times. Much more could be said about these investigations started at the Rockefeller Institute for Medical Research but one more point has special pertinence. With the use of cell-free filtrates and under definite, highly artificial conditions, the disease can be transmitted from one animal to another, although under natural conditions, it has not been found transmissible; that is, healthy fowls may be kept with diseased chicken without danger of infection.

Fibiger supplied Smith with the ten photomicrographs of rat *Spiroptera* cancer used to illustrate his discussion of the Dane's "'rat carcinoma due to cockroach-nematodes.'" Later Fibiger mentioned Smith's "renowned researches" in a lecture before the Danish Medical Association. Several letters passed between the two scientists during 1922-1924. On November 1, 1922, Fibiger wrote:

I have published during the last years no special papers concerning the nematode-carcinoma, but in various papers I have mentioned the summary of the results. I am sending you these papers accompanied with some papers concerning the tar carcinoma. Having given away during these last years an exceedingly great number of preparations, I am sorry to say that just

¹²¹ *Idem*, 301.

¹²² *Proc. Amer. Philos. Soc.* 51: 201-205; also, xi, minutes of the meeting.

at this moment I possess only a very restricted number of good stained slides showing the nematode-carcinoma, but I think I may be able to give you some good photos, and I shall send these as soon as they have been finished. If, in the next time, I should get some beautiful specimens, I shall send them to you. . . .

Smith in 1924 sent him a copy of his address and called his attention to the fact that he had given "a good deal of space to your beautiful researches which I considered to have advanced cancer research very materially. I mounted up the photographs which you sent me on a big sheet and exhibited those also at the X-ray Congress." Fibiger became president of the committee for cancer research in his country and, as such, was congratulated by Smith who by that time was president of the American Association for Cancer Research. He cordially responded on behalf of the Danish Association and the cancerologists of his native land. A year or so thereafter, he, because of his discovery of the Spiroptera carcinoma, won the Nobel prize in physiology and medicine.

While preparing his address, Smith had abundantly read the literature of many cancer research workers—Rous, Fibiger, Kopsch, Borrel, Haaland, and others. Both his text and his diaries show this, and included among them were works from students of experimental coal tar cancers. Early in 1923 Koitchi Itchikawa sent him from Japan "a whole bundle of cancer papers." Furthermore, in 1925, he was to meet Dr. Itchikawa in Dr. G. Roussy's laboratory at the Ecole de Medicin of Paris and be given by him eight more reprints on tar cancer. Smith outlined the earlier work in his address. Dr. Katsusaburo Yamagiwa, erstwhile student of Virchow, founder of the Japanese cancer journal, and professor of pathology at the University of Tokyo, and Itchikawa, doctor of veterinary medicine and also of the Tokyo pathological institute, chose rabbits as their experimental animals and, on the theory that rabbits' ears are peculiarly free from any suggestion of carcinoma growths, they painted their inner or outer surface every second or third day more or less for a year with the coal tar itself until at last papillomas and a few keratinizing carcinomas of a mild type resulted. Other experiments with mice followed, performed by Dr. Hidejiro Tsutsui, professor of general pathology and pathological anatomy in the medical high school in Chiba. Menetrier and Surmont in Paris confirmed K. Yamagiwa's and

Itchikawa's results. It had been said that the incentive for their experiments had been the parasitic action on rats obtained by Fibiger when he fed rats with nematodes taken from muscles and the thorax of roaches. Fibiger also made a series of coal-tar paintings on white mice, and in 1921 his results as well as those of Deelman of the Leeuwenhoek laboratory of Amsterdam and some English experiments by Murray and Woglom were announced. Smith described still other experiments from England and Denmark.

His purpose in the study of tar cancer experiments was to strengthen his crown gall analogy. In March 1923, after reading Dr. Deelman's recent paper on tar cancer in mice, he called Dr. Cullen's as well as Dr. Bloodgood's attention to the article. In his letter to Dr. Cullen he underscored Deelman's finding that in the youngest tar cancers a horizontal growth "*in all directions by cancerous conversion of neighboring cells*" takes place plus an invasive growth, and attached special significance to the Dutch pathologist's view that "the cells are hypertrophied before they become cancer cells." The disclosures were believed to "throw much light on the origin of cancer."

In his address on twentieth-century cancer research,¹²³ Smith predicted that great advances would result from the use of "coal tar distillates" and "soot extracts." Since then, from the coal tar, carcinogenic hydrocarbons have been derived, and from the coal tar hydrocarbons extracts and by-products. He illustrated his point by examples. One was:

Recently an attempt has been made by Bloch and Dreifuss (Sch. Med. Wochenschr. Nov. 10, 1921) to isolate the active principle from coal tar and these efforts if we may believe the preliminary report have proved remarkably successful and will greatly advance our knowledge. They began work in 1920 using rabbits, with the same results as Yamagiwa (number not stated); guinea pigs, which proved resistant; and white mice which yielded results "exceeding anything hitherto obtained." Raw tar was used only on a small part of the animals. All the fine results were obtained with a definite fraction of the coal tar, produced by distillation. Corresponding to English observations on the variable number of cancers in men who handle various kinds of coal tar products (H. C. Ross, J. Cancer Research, 1918) the bases, phenols and hydrocarbons, which come over at low temperatures, were found to be negligible. With the first two

¹²³ *Op. cit.*, 313-314.

nothing was obtained and with the low-boiling hydro-carbons only mild results which first appeared after many days. On the contrary with a product which distills over above 300°C. and is soluble in benzol, astonishing results were obtained. With such a distillate they state they were able to produce skin cancers on mice in four months' time in 100 per cent of the cases (number not stated nor strains or age of mice employed). Their technique was the same as that used by Yamagiwa. . . .

This does not imply that Smith was first, or among the first, to realize the value of, and recommend to cancer research workers, the use of phenols, hydrocarbons, and various chemicals for tumor induction. He was, however, among the first, if not the first, of the students of crown gall and cancer, to recognize the value of, and apply, chemicals and chemical influences in experimentally inducing tumors in plants. Utilizing techniques of his own making and techniques devised by others to study tumors of animals and man, he obtained in plants, through less than killing doses, types of cell-growth varying between what he regarded as hyperplasias and hypertrophies or a combination of the two. He produced overgrowths in plants with at least a half-dozen and more substances which readily pass through protoplasmic membranes, each of which are products of *Bacterium tumefaciens*: ammonia, amines, aldehyd, alcohol, acetone, acetic acid, formic acid, and carbonic acid(?). Many more effective compounds have been reported since his years, each active in gall production in plants. During his last years this subject became one of his special research interests.

CHAPTER XII

THIRD EUROPEAN JOURNEY. COORDINATE STUDY OF CANCER RESEARCH. INTERNATIONAL CONGRESS OF PLANT SCIENCES OF 1926. LAST WORK. FINAL HONORS.

IF SMITH knew at this time of the studies of Otto Warburg of Berlin, Germany, on cell metabolism and cell chemistry, he did not, so far as this author knows, say so. In 1925 he was to become acquainted with Dr. Warburg and, being given reprints, one¹ especially was to capture his interest and prompt him to write in his diary March 12:

what I thought out respecting oxygen hunger as the cause of cell division in cancer and published in 1920 in my Textbook he and his assistants have verified experimentally by most delicate and ingenious methods. They show clearly as I have always maintained that the cancer cell is chemically different from a normal cell, stores more acid under anaerobic conditions and cannot get rid of it under aerobic conditions. In this respect it is quite unlike normal embryonic tissue. He says the acid is lactic acid. Query: Does the crown gall organism excrete lactic acid and not formic? Ask the chemists to do over again flask cultures of various strains of *Bacterium tumefaciens* for the presence of lactic acid. Query: Can Warburg be wrong in his conclusions and a part of the acid of cancer cells be *formic acid*? It is important also for us to make a lot of experiments in 1925 on Ricinus and other plants using dilute lactic acid 1: 100, 1: 300, 1: 500, etc. Carry these along with experiments using ammonium lactate, and others using monobasic ammonium phosphate on Ricinus plants out of doors as well as others in the hot house.

Smith was to find Warburg believing as he did that "cancer cells behave unlike normal cells and contain unlike substance." How Warburg and his associates determined "the respiration of tissues and how the acid content of cancer cells" proved "very interesting." Smith took two pages of notes on the demonstrations made for him. But he reserved judgment on a few points. He thought that small quantities of acids other than lactic acid might be present and serve to stimulate cell division. Warburg had shown that if grape sugar was present, the cancer cells might

¹Ueber den Stoffwechsel der Carcinomzelle, *Die Naturwissenschaften* 12(50): 1131-1137, 1924.

live without oxygen for a considerable length of time, and that, owing to the splitting of sugar during their anaerobic growth, an excess of irritant acids was produced in cancer tissue. We shall have cause later again to mention Warburg's suggestion that, to quote Smith,² the "normal resting epithelium is a mosaic in which a few cells are strongly glycolytic, while most are oxidizing cells, and that when for any reason (pressure, sclerosis of vessels, presence of bacteria, etc.) there is a lack of oxygen the non-glycolysing cells perish and the others grow." In rats results had been obtained which pointed, furthermore, to a "glycolytic action of cancerous tissue on grape sugar with production of a great excess of lactic acid."

"Warburg, in Berlin, has shown," Smith would say in 1926,³ both for carcinoma and sarcoma that the malignant cell breathes anaerobically, even in the presence of air, whereby sugar is split and irritant lactic acid is produced in the tissues continuously. In all these ways the tumor cell behaves unlike a normal cell, but we can not decide from this whether the stimulus to growth is due to a parasite; to some derangement of the internal mechanism of the cell due to an irritant; or, finally, to some change in the environment of the cell. We might suppose some of the chromosomes destroyed, so that all descendants of the given cell or cell complex would necessarily be abnormal. . . . Warburg believes that anaerobic conditions are the explanation of tumor growth. According to his idea, our bodies and those of all animals are a mosaic of two kinds of cells, the great majority of the cells being aerobic. Under normal conditions the few anaerobic cells are controlled and kept in abeyance by the great mass of aerobic ones. Under certain abnormal condition, such, for example, as the influx into an ulcer of aerobic organisms, avid of oxygen and the destruction of the aerating blood vessels, the aerobic cells are killed off and under cover the few anaerobic cells begin to grow rapidly as a tumor.

Whether malignant tumors have their origin this way or are due to the direct action of parasites, Smith believed that in each the "multiplying extraneous organisms must be the activating thing in most cancers." By 1925 he had read a study⁴ by Dr. Montrose T. Burrows, director of laboratories of the Barnard Free Skin and Cancer Hospital of St. Louis, Missouri, in which

² Some newer aspects of cancer research, *Science* 61(1589): 596, 597, June 12, 1925.

³ Recent cancer research, *Amer. Naturalist* 60: 247, 255, May-June 1926.

⁴ *Archives of Internal Medicine*, Sept. 15, 1925.

was advanced a theory of cell and tissue heaping and stagnation as a cause of cancer. This theory Smith discussed with Warburg's.

Since 1917 in his address, "Mechanism of Overgrowth in Plants,"⁵ Smith had urged that in tumor etiology much might be learned from following a line of inquiry represented by Dr. Bernhard Fischer's paper (1906) on overgrowths of epithelium due to the injection of scarlet red and indophenol into rabbit's ears. He said, "Fischer's paper in particular pointed the way clearly toward the solution of the cancer problem, but it was received very coldly and he became discouraged, and no one else took up the suggested clue." The point to remember was not Fischer's discouragement when the "invading epithelial cells subsequently ceased to grow, with disappearance of the stimulus, and were finally absorbed." Smith stressed that "if specialists had then assumed that quite other substances than scarlet red and indophenol can cause overgrowths, as we now know, and that some of the substances may be the products of tumor-producing parasites, as also we now know, how suddenly luminous the whole subject would have become and what an incentive it would have given, and *still gives*, to further research!"

When, therefore, five and one-half years later, Smith examined the facts concerning experimental cancers reported from injections with anilin compounds—"Anilin-Dye Cancers,"⁶ he called them—he explained more about Fischer's "remarkable results . . . obtained by injecting rabbits' ears with olive oil saturated with scarlet red." Describing why subsequent experiments, which followed the "good deal of interest" created by Fischer's publication, failed, he suggested, "Here apparently, as in case of the coal-tar treatments, the failures were due to lack of persistency on the part of the experimenters, since in 1918 Yamagiwa and Ohno reported success with scarlet red dissolved in olive oil when injected into the wall of the ovary in hens (Gann, Bd. XII,

⁵ *Op. cit.*, 444.

⁶ Twentieth century advances in cancer research, *op. cit.*, 315. By 1945 the substances of known tumor-inducing effectiveness, chemical, mechanical, physical, etc., were believed to be "at last accounting . . . more than 290" in number. See Peyton Rous, "Concerning the Cancer Problem," *American Scientist* 34: 329-358, at 336, 1946. This paper, presented that year in the Sigma Xi national lectureships, is especially valuable for its discussion of "tumor-producing viruses," and mentioned among other carcinogenic agents the hydrocarbons and "fat soluble dyes Sudan III and Scharlach R."

S. 3). Their experiments began in December, 1914," and, after presenting what data concerning these he had been able to find, Smith urged that the work be repeated for the purpose of verification. A tumor obtained with a one per cent solution of Sudan III in olive oil by Dr. Nobumasa Umehara, professor of general pathology and pathological anatomy in the medical high school in Kyoto, Japan, was also described. Smith's interests embraced the whole range of environmental and genetic influences in tumor production. Each way to break "new ground" to solve the problems of tumors was important.

Since his years, belief in his general theory of chemically induced plant galls has been consistently furthered. Many of his conclusions, and those of other workers of his generation, have undergone modification, revision, and refinement. But consistently there has been an extension of many of the principles which he first elaborated. There is an "extensive literature" ⁷ about auxins, which have been shown to be "particularly active" in gall production. Mitchell, Burris, and Riker ⁸ have listed other important more or less recent studies. They say:

The use of lanolin as a carrier was a great improvement in experimental procedure. With it chemicals could be released in continuing sublethal concentrations over relatively long periods. Brown and Gardner (1936) ⁹ grew crown-gall cultures in media containing peptone with some tryptophan added. An ether extract of the culture was dried, incorporated into lanolin and applied to bean plants. Excellent galls developed. No control with the unfermented medium was reported. However, Kraus (1941) ¹⁰ showed that 2 per cent tryptophan in lanolin was active. Also Brown and Gardner used 2 per cent indole-3-acetic acid in lanolin and secured excellent galls. The concept has developed that a chemical cause of crown gall has been found.

In 1935 Miss Brown, still with the Department of Agriculture and continuing her studies of *Bacterium tumefaciens*, thought of producing a plant gall with indoleacetic acid and, in so doing, made an important discovery. A growth substance was extracted

⁷ F. Skoog, Growth substances in higher plants, *Ann. Rev. Biochem.* 16: 529-564, 1947.

⁸ Inhibition of respiration in plant tissues by callus stimulating substances and related chemicals, *op. cit.*, 368-369.

⁹ Nellie A. Brown and F. E. Gardner, Galls produced by plant hormones, including a hormone extracted from *Bacterium tumefaciens*, *Phytopathology* 26: 708-713, 1936.

¹⁰ Ezra Jacob Kraus, Histological reactions of bean plants to L-tryptophane, *Botanical Gazette* 102: 602-622, 1941.

from cultures of *Bacterium tumefaciens*; and in 1936, she and Dr. Gardner announced "a hormone extracted" from the crown gall bacterium. "In a theory of gall formation," they said, "it is not the mere presence of the organism that leads to overgrowths but rather the stimulus of certain products of the organism's metabolism." At that time they obtained "no definite information" as to "the chemical nature of the growth-promoting substance from *Bacterium tumefaciens* or to the best method of its extraction." But the work was continued and new studies were suggested.

Dr. Kraus, Miss Brown, and Dr. K. C. Hamner began a new set of investigations, results of which were announced in December.¹¹ Many similarities in phenomena were found to exist between bean plant tissues inoculated with *Bacterium tumefaciens* and those tissues to which indoleacetic acid was applied. That Smith's work, a decade after his death, was still having influence is indicated by the following quotation from their published results: "The histological responses of the cells of the bean plant to indoleacetic acid," they said, "show many developmental patterns which are almost exact duplicates of those Smith, Brown, and McCulloch have shown for crown gall. Those which follow the application of the indoleacetic acid are somewhat more pronounced."

Not every research-phase of this vast subject can be treated here. The various investigation-aspects of the many problems, as in Smith's time, are very much in the current scientific literature. Recently, R. S. de Ropp of the New York Botanical Garden has evaluated the important work done on "Plant tumours and Animal Cancer."¹² He points out that "the work on plant tumours has brought to light three factors—virus, hormone and genetic—which may act singly or in combination to produce pathological growth. This trio of etiological agents," he adds, "is already familiar to students of animal cancer." Three forms of plant tumors which have "certain properties in common with those neoplasms in animals which are collectively referred to as cancers" are described, and he says that the "resemblance is

¹¹ E. J. Kraus, N. A. Brown, and Hamner, Histological reactions of bean plants to indoleacetic acid, *Bot. Gaz.* 98(2): 370-420, Dec. 1936; also, *Bot. Gaz.* 98(4): 735-807, June 1937. Quotation at 414.

¹² *Nature* 160(4075): 780-782, Dec. 6, 1947. See also by de Ropp, Some new plant growth inhibitors, *Science* 112(2913): 500-501, Oct. 27, 1950.

closest in bacteria-free crown-gall tissue," and in L. M. Black's "virus-tumour tissue." The "virus wound tumour" (*American Journal of Botany* 32: 408, 1945) is described as follows:

Susceptible plants can be affected with the wound tumour virus either through the agency of agallian leaf hoppers or by being grafted to infected plants. Infection is systematic and symptoms include enlargement of the veins as well as the formation of tumours on roots and stems in many species of infected plants. Most of these tumours appear to arise at points where root or stem has been wounded. Black (*Nature* 158: 56, 1946) showed that the tumour tissue from sorrel could be cultured for as long as twenty months on an artificial medium without the production of roots, stems or leaves. On being grafted back to healthy plants, tissues from these cultures induced a systemic infection in the stocks, showing that the virus was still present. Such a tumour-producing virus has obvious resemblances to entities such as the Rous sarcoma or the Bittner milk factor.

De Ropp's discussion of the work on bacteria-free crown gall tissue included that of many workers since Smith's time: his own, Braun and White, Riker, T. Laskaris, E. M. Hildebrand, A. C. Hildebrandt, and others. The explanation of a tumefacient factor and a growth hormone seems indicated from most of the studies. However, it is said:

If a virus is responsible for the formation of induced tumours, it is probably of the type transmissible only by grafting, as extracts of tumour tissue are without effect on normal tissue [White and Braun, *Cancer Research*, *op. cit.*; de Ropp, *Bull. Torrey Bot. Club* 75(1): 45-50, 1948]. The existence of such a bacteria-borne tumefacient factor has still not been conclusively demonstrated.

At the 1949 meeting of the American Association for the Advancement of Science, Dr. Braun presented a paper on the development and proliferation of plant cancers caused by crown gall bacteria. In this he described experiments, the results of which pointed to the conclusion that "it is before and during early stages of active wound healing that normal cells are converted into tumor cells." Two factors (among the other important conclusions reached) were elaborated: first, a wound by which the bacteria gain entrance to the plant and, second, a time factor during which the "tumour-inducing principle," probably a hormone, transforms the "normal plant cells to tumor cells." Because of his studies "so well designed that they chart a basic course

in man's understanding of the greatest human malady," he was given the association's annual one thousand dollar prize.¹³

According to de Ropp, the third form of plant tumors—on hybrids of *Nicotiana Langsdorfi* and *N. glauca*—are of interest because, to quote him, "the abnormal growth results¹⁴ from a cytoplasmic disturbance occasioned by the introduction of the chromosome complement of *N. Langsdorfi* into the cytoplasm of *N. glauca*. . . . Such tumours," de Ropp says,

develop only in hybrids in which *N. Langsdorfi* is used as the pollen parent. The tumour tissue was cultured *in vitro* by White (*Amer. Jour. Bot.* 26: 59, 1939), who showed that it would continue to grow as a tumour when grafted back on to normal plants. He showed also that this tumour tissue was capable of differentiating organs *in vitro*, in which respect it differs from crown-gall tissue and virus wound-tumour tissue.

That Smith adhered to a parasitic theory of animal cancer etiology seems undoubted. But what form, or the nature and mode of action, of parasite or parasitic agency was stated on the basis only of what authentic experimental evidence allowed. He believed that "the products of parasites or symbionts [are] the probable cause of most cancers or at least of their initial stages."¹⁵ A preponderance of utterances seems to have favored a virus interpretation, as yet experimentally undetermined: analogous to the bacterium of tumors in plants. In practically the last working years of his life, 1926, he would still be urging more research on the cancer cell: "Is the stimulus to this malignant overgrowth intrinsic or extrinsic?" he would ask. "Does it come from within the cell, due to some change in its internal mechanism brought about by a parasite or by some non-living irritant, or is it due to a changed environment acting on an otherwise normal cell? Here is the problem that lies just ahead of us."¹⁶

Since 1922 he had given a large share of his attention to an important series of experimental tests to determine with the use of the potentiometer and electrometric titration the hydrogen-ion concentration and total acidity of various crown gall tumors. The

¹³ *Science* 111(2871): 21, Jan. 6, 1950; abstract of Dr. Braun's paper, Thermal inactivation studies on the tumor-inducing principle in crown gall, *Phytopathology* 40(1): 3, Jan. 1950.

¹⁴ T. W. Whitaker, *Jour. Arnold Arboretum* 15: 144, 1934.

¹⁵ Twen. cent. adv's in cancer research, *op. cit.*, 316.

¹⁶ Recent cancer research, *op. cit.*, 246.

purpose was to ascertain the exact amount of acidity in tumor juices over and above normal plant juices scaled down to phenolphthalein neutrality, to determine the ranges of tolerance of various plants to acids and alkalis, in other words, to fix for each set of phenomena the so-called pH factor at which, among other points, bacterial growth and gall development will be inhibited. In 1923, at the annual meeting of the American Association for Cancer Research, Dr. Smith had been elected vice president of the organization, and in 1924 he had been elevated by the association's council to the office of president. This took place at its seventeenth annual meeting which was held that year at Buffalo and at this meeting Dr. Smith (and Miss Agnes J. Quirk) presented a paper on "Hydrogen-Ion Content and Total Acidity in Crown-Gall Tumors."¹⁷ Since 1917 Smith had associated ammonia production with the pathogenicity of *Bacterium tumefaciens*. It may be added, furthermore, that today ammonia production is believed to be the factor most common among the various gall-stimulating bacteria. In this paper he again indicated his belief that a "combined acid," (in part, ammonia) is fundamental as a factor in the origin of crown gall tumors. Some interesting observations on oxidation phenomena were also revealed. But one theory appears to have been quite definitely shaping in his mind, a theory based on his belief as to the inhibitory action by acids in gall development. This would have a bearing on his later explanation of immunity manifest in some varieties of plants to crown gall. Much future research would be done by him and other workers along these lines.

On April 1, 1924, he had "completed [his] abstract of Potentiometer work on crown gall as far as [they had] gone—170 experiments," he wrote in his diary. "Tests are yet to be made on 6 other groups of inoculated plants, making in all about 200 experiments." On April 9, he participated in a symposium on cancer of the District of Columbia medical society. Approximately one hundred and twenty doctors were present, and he spoke on "Biological Aspects of Cancer." In his address before the American Association for Cancer Research he announced many

¹⁷ *Jour. Cancer Research* 8: 515-516, 537 f., 1924. Entries in Smith's diaries for the summer of 1923 are interesting in this connection, as, for instance, that of July 17.

interesting conclusions, all building to the final point that "Apparently the acid produced by *Bacterium tumefaciens* is locked up in the tumors nearly or quite as fast as it is produced, but it may nevertheless be the active agent in inciting the cells to abnormal division." More than 250 potentiometer tests of juices from normal and diseased tissues of sunflower, sugar beet, and other plants, had been made, and most of the tumors studied varied in age from three to six weeks, thorough tests of the youngest tumors and the youngest, outermost parts of older tumors not being included. "The general trend of the experiments," he suggested, "indicates that the apical, actively-growing parts of a normal vegetative plant, especially *Helianthus annuus*, are more alkaline than the middle or lower parts where growth is less active." From sugar beet, comparisons were drawn between the juice alkalinity of youngest and full-grown leaves, and between the juice alkalinity of blades of full-grown leaves and their petioles. In general, the tumor juice, especially for sugar beet, was found to be "more alkaline than the juice of corresponding normal parts, and the older the tumors, up to a certain stage, i. e. until secondary invasions have set in, the more alkaline" was found "their juice—with some few exceptions." Electrometric titrations followed the determinations of pH, and further investigations were promised to decide "whether the tumor juice is ever more acid than the normal juice, as we believed," said Smith, "would prove to be the case when we began these experiments. . . . It seems to be so in some instances. It is less alkaline in early stages of growth and in a few instances it has been more acid than normal juice and in other instances it has been of the same pH value, but such readings have not been very numerous." Smith illustrated his paper with twenty-two lantern slides which showed some of the results of their fifteen months of work.

Immediately on returning to Washington, he and his co-workers continued potentiometer tests of crown galls on sugar beet and sunflower, and on May 2 he started study of "galled Ricinus plants inoculated April 9th." By June 6 the work included "crown gall on Cauliflower. Doubtful," however, he said, "if pH can be determined electrically. Something seems to interfere." Crown galls on other plants were tested. But not until mid-summer did his diary contain any important memorandum of conclusions. July 31 he wrote:

Did on the potentiometer the last of the stem crown-galls on the Garden sunflowers of the second planting. They consistently have had a higher acidity (pH) and a greater total acid (Fuller scale reading) than any of the normal parts of any age; even the youngest. The youngest part have almost as little acidity as pure water. In these particulars the sunflower is unlike the sugar beet. That plant is more acid and the crown-galls on it are less acid than the tissues out of which they are developed whereas just the reverse is the case in the sunflower, i. e. tumors more acid than the tissues out of which they have developed.

The following September, when Dr. and Mrs. Smith went to Europe, he took with him his notes of that summer's potentiometer crown gall studies, and planned to add these results to those announced at the Buffalo meeting of the cancer research association. Some of the conclusions furnished a basis for addresses given by him in Europe. Further, the prepared program of the Association's eighteenth annual meeting, held in Washington May 4, 1925, soon after his return, announced that he would present a paper, "Additional notes on the pH of crown-gall tumors." Evidently, however, regarding the new research matters which he found while in Europe more important, he substituted another address, "Some Newer Aspects of Cancer Research." Medical men had confidence in him. In 1923, when Dr. Willy Meyer, former president of the association's council,¹⁸ telegraphed his congratulations because of Smith's election as vice president, the latter wrote in his diary of Dr. Meyer: "He is a New York surgeon greatly interested in some of my work. He perfected a major operation for breast cancer. I saw him operated on for gall stones by Dr. W. J. Mayo in 1913 at Rochester, Minnesota. He said once in my Laboratory, 'Doctor, may you live a thousand years!'" Dr. Bloodgood once said¹⁹ of Smith to someone else, "I have picked him to find the cause of cancer." Remember, furthermore, that Smith had been elected president of the cancer research association by a council composed of president William Duane, secretary-treasurer W. H. Woglom, Dr. Robert B. Greenough, Dr. Meyer, Dr. Ewing, Dr. H. G. Wells, Dr. Frederick Prime, and Dr. Francis Carter Wood. These men were all medical doctors of the highest order.

Dr. Smith appraised highly the value of pH, total-acid, and

¹⁸ *Jour. Cancer Research* 8: 137.

¹⁹ Smith's diary, December 8, 1922.

oxidation determinations, as bases for comparative study of normal and diseased tissue juice from various plants. That he gave fully one-half of his working hours to this type of research in his laboratory, especially as the work began to elaborate growth accelerating and growth inhibiting formulas for the study of bacterial organisms, indicates his interest and belief in its possibilities. After his cancer research association address, within less than two months, he had completed 325 experiments, and several more series, including some on *Bryophyllum* and *Ricinus*, were planned.

But this work was but one department of his laboratory. Many papers on other subjects had been, or were being, completed by his co-workers. November 6, 1923, Miss Bryan was working on a paper on a disease of delphinium; Miss Brown, on a paper on limb knot of apple; Miss McCulloch, a paper on a spot disease of gladiolus; Miss Fawcett, one on *B. tracheiphilus* in media curves of growth; Dr. Braun, one on pythium disease of *Pelargonium*; Miss Elliott, on a paper with Smith on *Bacterium andropogoni* and another paper by herself on several cereal spots; Miss Hedges, a paper on bean wilt, and from 1922 to 1926 she discovered and described a new and destructive bean disease caused by *Bacterium flaccumfaciens*; Dr. Rand and Miss Cash, a paper on Stewart's disease of maize transmitted by *Diabrotica duodecempunctata*; Dr. McKenney, a paper on field treatment of soil for *Bacterium solanacearum*; and Miss Quirk, with Smith, a paper on hydrogen-ion concentration of crown gall fluids versus normal juice of sugar beet and sunflower. In 1923 Miss Quirk and Miss Fawcett, with Dr. Smith's aid, had published studies on the hydrogen-ion concentration of culture media as compared with Fuller scale readings. In this had been elaborated "the approximate ranges of growth (acid-alkaline)" of more than two dozen bacteria pathogenic to plants. An abundant, steady increase in learning about plant bacteria and bacterial diseases of plants was being published from this laboratory.

Late in the year 1922 and early in 1923 the first centennial of Pasteur's birth had been celebrated; and in at least two commemorative events in this country Dr. Smith had participated. At a celebration held at Rutgers College on December 20, 1922, he had spoken on "Pasteur—the man,"²⁰ and on January 10,

²⁰ *Scientific Monthly* 16(3): 269-279, March 1923.

1923, at the more elaborate Pasteur Centenary of the New York Academy of Medicine, on "Pasteur's study of silkworm diseases." On these programs were other celebrities: at Rutgers, Drs. R. G. Wright, J. G. Lipman, and J. F. Anderson, at the New York Academy, Drs. R. H. Chittenden, Hermann Biggs, Theobald Smith, Simon Flexner, W. H. Welch, and W. W. Keen. He could have spoken at other centenary celebrations, but refused because he had so often given addresses on the man who he thought had "moved the world."²¹ He would have accepted the invitation from the American Museum of Natural History to speak on Pasteur's contribution to agriculture, but, being away from Washington at the time, he did not receive the invitation soon enough. Smith was a collector of Pasteuriana, and he lent some of his photographs and a bronze figure fifteen inches high of the great Frenchman to the New York Academy of Medicine occasion. His new studies added to his high rating as an authority on the life and work of Pasteur. Especially appreciated was his translation of a paper by Émile Roux on "The Medical Work of Pasteur."²² His and Miss Hedges' translation of Duclaux's *Pasteur, The History of a Mind* had been received as a "history of two minds" because of the senior translator's excellent appraisal of the life and work of Duclaux. As a scientist, Smith had been recognized many times by workers and officials of the Pasteur Institute. Brazilian O. da Fonseca, who had studied in Smith's laboratory, found when he went to Paris to study that Smith's crown gall specimens, left at the Institute in 1913, were still there, and mycologist Magrou had much to say about his work and books. That year another former student from Smith's laboratory, K. Nakata, wrote from the Rothamsted Experiment Station of England and expressed similar appreciation of the recognition he found being accorded an American's work. Etienne Burnet of the Institut Pasteur in Tunis, Director Roux, Dr. A. Berthelot, chief of the laboratories, Dr. Calmette, and Dr. Magrou of the Pasteur Institute of Paris each congratulated and commended Smith for various of his writings about "the maker of new pathways in half a dozen fields of science."²³

In 1923, moreover, Dr. Smith represented the Bureau of Plant

²¹ E. F. Smith, Louis Pasteur, *Nature* 8(6): 346-347, Dec. 1926.

²² *Scientific Monthly* 21(4): 364-389, Oct. 1925.

²³ E. F. Smith, Louis Pasteur, *op. cit.*, 346.

Industry as a delegate to the centenary observance which honored Joseph Leidy, whose "chief contributions," Smith believed, "were in comparative anatomy, palaeontology (fossil vertebrates of the Cretaceous era) and protozoology." He did not give an address at this occasion. But, at many other times, he had been called upon for oral as well as written disquisitions. His interests in civic and literary matters brought forth as many requests as his work in science. In 1916 he had been asked to prepare an "in memoriam" tribute to Thomas J. Burrill,²⁴ and in 1918 he wrote an appreciative sketch of the unique and productive career as a plant explorer of Frank N. Meyer.²⁵ Time and the years had given to Smith many, many enjoyable friendships, and the opportunities were now arriving for his co-workers and friends to honor him. Perhaps the first of such recognitions was the annual dinner of the Washington Botanical Society, held March 4, 1924, at the Ebbitt House, when Dr. Smith, now seventy years of age, was guest of honor. At this occasion, he chose to present a character portrayal of himself, "Some thoughts on old age."²⁶ Early in 1923 he had charted a work-plan for the rest of his life. "I have been so interested in my work," he said,²⁷

that the years have slipped away unheeded and now I observe by various signs that I am growing old with not half of my work accomplished. It is always the way, I suppose, with men who are absorbed in what they are doing. I should like (1) to finish [translating the sonnets of José-Maria de] Heredia,²⁸ (2) publish a new volume of sonnets, (3) write an autobiography, (4) do a book on a scientific man's religion, (5) complete the unfinished volume of the Carnegie monograph. Alas, there is little time left! But I am not sad, only a little melancholy.

A discovery of the following mid-summer caused him to revise these plans. While working at pH determinations of crown gall juice from sunflower, he noticed one day that "four of the sunflowers which were inoculated in the young flower head with crown-gall bacteria" almost a month before had "developed abnormally, giving rise in the center of the heads to strop-shaped

²⁴ *Jour. Bacteriology* 1(3): 269-271, May 1916.

²⁵ *Science* 48(1240): 335-336, Oct. 4, 1918.

²⁶ *Jour. Washington Acad. of Sci.* 14(11): 231-238, 1924.

²⁷ *Diary*, Feb. 1, 1923.

²⁸ Work never completed, but many of the sonnets published in *For Her Friends and Mine*, *op. cit.*, 341 ff.

flowers.”²⁹ He photographed one, and added to his memorandum: “14 of these yellow flowers and 18 subtending green bracts. Some small tumors in the vicinity but most in stem under the flower head.” His potentiometer tests of sugar beet crown galls had yielded results “consistent with all that have preceded. The tumors, even these relatively young ones none of which show any necroses, [were] 2-4/10 pH more alkaline than the normal tissues of the sound sides of same beets.” Most of the month had been given, therefore, to testing the sunflower crown galls for pH and total titratable acidity, and the work had brought forth a discovery of sterile ray flowers in the middle of sunflower heads. The full significance of this was not realized until the following years, 1924 and 1925. But, during 1923, he did not forget what he had observed, and later, when we consider his work of these years, we shall find this discovery supplying the basis for months of research.

During August of that year, Dr. and Mrs. Smith attended a reunion of old residents of Gilberts Mills, New York, and then took a journey into Canada and the New England states.

When he returned to his laboratory, his attention was at once called to the “good growth” some Begonia plants had made during the passed six weeks. In April he had begun experiments, evidently injuring leaves in one way or another to study any resulting proliferations. But he was disturbed to find the variations in proliferation graded from “very slight” to abundant, and so he started some new experiments to restudy the results. At some time either that year or early in the next, nematode galls due to *Heterodera radicolica* were found on the crowns of a tall-growing begonia. The same cultures of *Bacterium tumefaciens* which produced tumors in sunflowers, sugar beets, and Ricinus plants failed to produce tumors on *Begonia lucerna*, although the experiments were three times repeated. Dr. Smith and Miss Quirk, therefore, began a series of potentiometer studies on this plant, *Begonia phyllomaniaca* which had been the subject of the spring to autumn 1923 experiments, and other begonias.³⁰ Eventually the studies were extended to onions, garlic, banana, sugar cane, and many other plants to determine not only their acidity

²⁹ Diary, 1923, July 23; quotation concerning sugar beet crown galls, July 2.

³⁰ A “B. P. I. Hybrid Begonia” was another variety especially studied.

but also their immunity or semi-immunity to crown gall. These tests and examinations also were to last through the years 1924 and 1925; and from their results they would publish their paper on *Begonia lucerna*, "A Begonia Immune to Crown-gall: with observations on other immune or semi-immune plants."³¹ All of the begonias were found to contain oxalic acid and to be "rather resistant to crown-gall inoculations," but their final conclusion read:

We started out with the assumption that the pH controlled tumor formation and that no tumors could be produced on plants having a greater acidity than pH 5.00, but the final experiments show that we have been able to produce crown-gall tumors on three begonias having a very much more acid juice reaction than any the organism will tolerate in fluid cultures. The only conclusion we have come to is that mass action must control infection, *i. e.*, if you can once get a tumor started in a tissue, even in a very acid tissue, it will make its own milieu and continue to grow in spite of the inhibiting pH. This probably would be true even on *Begonia lucerna*. Considering these results, it is premature to state that no crown-gall tumors can be produced on the plants mentioned and supposed hitherto to be immune owing to their acid juice. Further experiments should be made, but it is still believed that crown-gall tumors on such plants are non-existent in a state of nature. Other factors than acidity of course may determine resistance in some of these plants.

Smith's idea that bacterial growth may be inhibited by pH alone is now known to be sound. Under definite conditions, bacterial growth and gall development in tissue culture are inhibited at about pH 4.5. Oxalic acid, furthermore, is now high on the list of organic acids known to be inhibiting and bactericidal at high concentrations.³²

Another discovery made in September of 1923 was "some very astonishing fasciations in flower head of a Dahlia."³³ Immediately he had this photographed, and poured plates for their study. In 1921 he had prepared an explanatory note on "Fasciation of plants due to bacterial infection."³⁴ This was to accompany the publication of four or five photographs of crown gall inoculations on nasturtium. Early in 1922 he had studied at the microscope sections

³¹ *Phytopathology* 16(8): 491-508, Aug. 1926, and reprint.

³² A. J. Riker, Studies on the influence of some environmental factors on the development of crown gall, *Jour. Agr. Res.* 32(1): 83-96, 1926; Hildebrandt and Riker, *op. cit.*, 83; memorandum to author by Dr. Riker.

³³ Diary, Sept. 12.

³⁴ Diary, Dec. 23.

of *Tropaeolum* fasciation due to crown gall, and prepared a short paper which was afterward revised with new material from further study added, and published by *Phytopathology*³⁵ under the title "Fasciation and prolepsis due to crown gall." At one time, most of the crown gall inoculated nasturtiums showed "shoots developing from the inoculated axil and from no other." He photographed one plant which had "three sets of branches," the main axis bearing secondary, tertiary, and quaternary shoots, all within a period of two months. "Strangely suggestive," he commented, "of what occurs in peach yellows."³⁶ Within a few months he photographed a nasturtium shoot which showed that the translocation of starch had been hindered by a crown gall. His memoranda read:³⁷ "There is much more in two swollen shoots from this axil than in the main stem. The main stem is long and has many leaves. These shoots have lost almost all leaves and the larger ones have begun to shrivel at apex (5 of the 12 inches). These stems have a diameter twice that of the main axis below the tumor. There is more sugar in the juice of the main axis." After dissecting more shoots, he found there was a "damming back of the starch."

In his paper on the subject he suggested that in this disease we may have all varieties of stimulating secondary effects on normal tissues, from prolepsis of uninjured leaf and flower bud and root anlage in the vicinity of the tumors, through simple fusions or divisions (fasciations), to the breaking up of the dormant bud, or of a cambium, into dozens and even hundreds of small vegetative fragments which may either grow as roots or shoots on the surface of the tumor or be buried in its depths.

As interesting as any suggestion was his assumption that the initial disturbance may have begun in the embryonic or dormant bud stage—an observation not unlike the embryonal hypothesis of the origin of cancer, possibly. Smith pointed out that similar phenomena were to be found in the animal world—"many interesting figures of trunk and limb duplications or fusions, especially of parts of the human body." Recently, he said, Charles Rupert Stockard, professor of anatomy of Cornell Medical College and an investigator for the Huntington Fund for Cancer Research, and others, had started experimental studies, and these Smith

³⁵ 12: 265-270, 1922.

³⁶ Diary, Jan. 14, 1922.

³⁷ Diary, April 13, 14, 1922.

commended, since he was convinced that the problem would have to be solved by experimental methods.

His study of the problem with plants did not end here. In September of 1923, when agar poured plates taken from the base of the fasciated Dahlia revealed "dendritic strongly iridescent colonies, white, brownish, blue, or purple, according to the lighting," he began taking some field notes and his memorandum reads:³⁸ "It is a big double pink and the only fasciated pl[ant] in 36 plates. I counted more than 50 fasciated blossom shoots and no good flowers. Two fasciations have given 3-4 strop-shaped pink petioles among the many green bracts." He photographed these along with perfect flowers from the next plants in the row. In April 1926 the *Journal of Heredity*³⁹ published as its frontispiece of that issue two photographs entitled, "Fasciation of Dahlia," with the following description by Smith:

A full double pink Dahlia, one plant of which was fasciated as here shown. This was a large much branched very leafy plant. There were many floral axes stretched three to six inches and covered with green bracts. There were no normal flowers on this plant, but occasionally a few pink petals appeared, as on these two shoots. In addition to its excessive vegetative growth the plant differed visibly from its fellows only in having an injured main axis into which rainwater had penetrated. This cavity extending through several internodes was full of a mixed bacterial growth, and the fasciation is attributed to the action of ammonia and other bacterial by-products. Some of these fasciations were much more striking. One of them had seven centers of growth at its apex. Types of the colonies obtained on poured plates are also shown. There were at least three sorts, yellow colonies, round white colonies and iridescent branched ones. Unfortunately the cultures were lost before experimental inoculations could be made.

No later than 1918,⁴⁰ he had stated his belief that "thyloses, fasciations, distortions of tissues, and various duplications, simplifications and inverse tissue differentiations are caused by the excretions of feeble parasites although in nature probably all are not so caused."

In 1923 Smith received some specimens of "early stages of aerial galls on apple" which showed "dead buds as though the swelling which is a root anlage might be started by death of the

³⁸ Diary, Sept. 14, 18, 1923.

³⁹ 17(4): opposite p. 113, Apr. 1926.

⁴⁰ The relations of crown-gall to other overgrowths in plants, *op. cit.*, 453.

buds. They suggest," he wrote in his diary May 22, "the tumefactions I got on Bryophyllum by moving from cool to warm house." Since 1908 he and Miss Brown had recognized that the apple strain of the crown gall organism possessed a tendency under definite conditions to produce roots in conjunction with tumors. In his textbook of 1920⁴¹ he had said:

In 1916 the writer discovered crown galls bearing leafy shoots and subsequently produced many by needle-puncture inoculation on stems and leaves of tobacco, Pelargonium and other plants. Earlier than this by some years, Miss Brown and myself had demonstrated that crown galls may bear roots (hairy root of apple, etc.) but I did not then perceive the full meaning and trend of this discovery, *viz.*, that because the type of a crown gall depends on the kind of tissues inoculated it should be just as easy to produce tumors bearing leafy shoots or flower buds as roots. This had to be stumbled upon to be seen, like many another perfectly obvious thing. Its discovery, however, it seems to me, adds very considerably to our knowledge of the nature of crown gall and throws a flood of light also on the origin of animal teratomas.

Crown galls occur very often on what gardeners call the "crown" of the plant. They may develop on any part of the root or shoot, and are "very common above ground on the branches of the daisy, grape, quince, apple, rose, willow and poplar. They are also common on the roots of a variety of plants but must not be confused," Smith pointed out,⁴² "with root galls due to nematodes." In 1908 he expressed "little doubt"⁴³ that hairy root of the apple is of bacterial origin and caused by a "similar if not identical organism" as in the case of crown galls. The crown gall and hairy root bacteria have since been differentiated.⁴⁴ Infectious hairy root of apple is now believed to be caused by *Phytomonas* [*Bacterium*] *rhizogenes*, an organism distinguishable from *Bacterium tumefaciens*.

In 1925, in the *Journal of Heredity*,⁴⁵ Dr. Smith published

⁴¹ *Intro. to bact. dis. of plants, op. cit.*, 419, 421.

⁴² *Idem*, 421.

⁴³ *The etiology of plant tumors, op. cit.*, chap. VIII. See also, N. A. Brown, The tendency of the crown-gall organism to produce roots in conjunction with tumors, *Jour. Agric. Res.* 39(10): 747 ff. (G. G. Hedgcock accredited with establishing in 1905 the pathological effect of crown gall on orchard trees. Crown gall and infectious hairy root of apple considered also as the same disease).

⁴⁴ A. J. Riker, Studies on infectious hairy root of nursery apple trees, *Jour. Agric. Res.* 41(7): 507 ff., Oct. 1, 1930.

⁴⁵ 16(8): opposite p. 273, Aug. 1925.

another photograph of a "Tumor formation in *Bryophyllum*." This was also a frontispiece, and was published to show that "one successful crown gall inoculation on *Bryophyllum calycinum* does not protect from a second inoculation." His explanation read:

The base of the plant was inoculated with the hop strain of *Bacterium tumefaciens* March 22, 1920. The top of the plant was inoculated with the same strain of the organism on October 3, 1921, after the lower tumor had become of large size. Photographed March 2, 1922, about one-third natural size, i. e., the height of the plant was 20 inches. At this date both tumors were still free from necrosis, the lower tumor being nearly two years old and the upper one over five months.

Smith's two most important publications of this period were communicated in 1924 and 1926 to the National Academy of Sciences: the first, "Tumors, cysts, pith bundles, and floral proliferations in *Helianthus*,"⁴⁶ and the second, an abstract of which was published also in *Science*,⁴⁷ "Changes of structure due to a modified environment. A study of labile protoplasm in *Helianthus annuus* L." Following his discovery in 1923 of four sunflower heads with strop-shaped flowers located in the middle of the head and among tubular disk flowers, he found on July 25, 1924, "tumor strands" in the pith of sunflowers inoculated twenty-two days previously. He fixed one in carnoy for sections, and this, when examined on August 29, was seen to be "a perfect vascular cylinder in the center of the pith and in its center [were] tumor cells." Immediately he wanted to know whether these phenomena could be obtained by simple wounding and in the absence of tumor irritation. He had studied the effects of simple wounds on other plants. On about sixty undeveloped sunflower heads, he punched out the middle of the torus, but three⁴⁸ weeks later he examined them in blossom and could find "no evidence of any central strop-shaped flowers." Believing that "the wounding and inoculations" had not been made early enough, he decided to repeat the experiments the next year.

From the beginning of his study of these phenomena, it appears, he attributed their occurrence to some "mechanical traumatic

⁴⁶ *Mem. Nat'l Acad. Sci.* 22(4): 1-50, 43 pl., 1924.

⁴⁷ 63(1637): 505, May 14, 1926.

⁴⁸ Twenty-five days for one set. Eleven days for another. Diary, Aug. 18, for conclusions.

displacements occurring in the early life"⁴⁹ of the plants. His final explanation of the "cysts" or "cavities in the pith, lined by a well-defined small celled membrane," which he later discovered, was that these were "the result of implantation of tiny fragments of torus into the tissues under the torus." This was the simplest explanation, he said, but also in his mind was the question, "may they not be the product of very young pith cells abnormally stimulated? Their regularity would suggest this. If the ray flowers out of the place were fertile," he commented, "we might infer that they got an excess of nutrient substances, but as they are sterile like the normal rays, we may suppose that they are tubular flowers converted into sterile ray flowers through some defect in nutrition."

A theory of parasitism was also advanced. Along with his potentiometer tests of inoculated plants that year to determine at what day-and-night hours the maxima and minima readings for pH and total acidity fall, he had been studying the influence of light, among other environmental features, as a factor in the stored acid content of tumors. He studied in other plants bud germination and the interference of functions subserved in the process by sugars and starch in various parts of young and old plants. His memoir, "Tumors, cysts, pith bundles, and floral proliferations in *Helianthus*,"⁵⁰ covered special investigations which lasted from 1924 through 1926; but the summary of his results in his paper, "Changes of structure due to a modified environment," reads:

These results were first obtained by inoculating the crown gall organism (*Bacterium tumefaciens*) into young flower heads and so far this method has given the most numerous and striking examples, but the same results, exclusive of the supernumerary capitula, were obtained in a smaller number of instances from simply dislocating tiny fragments of tissue of the receptacle by means of repeated needle punctures. The results show that cysts are independent of tumor formation, but in case of the bacterial inoculations the walls of the cysts bore numerous tumors which were either discrete or fused over long distances. These crown galls frequently filled the cyst cavity and stretched it, often rupturing its walls to appear on the surface of the stem. My experiments were made in 1923-1924, and were repeated

⁴⁹ Tumors, cysts, pith bundles, and floral proliferations, *op. cit.*, summary.

⁵⁰ Smith's diary shows that he wrote the memoir in 1925. He may have begun to report his discoveries of 1924 to the Academy that year. Evidently a delay in publishing his paper enabled him to include his confirmatory results of 1926.

on a larger scale in 1925 with the same results. It is the first time I have observed cysts in connection with crown-galls.

On June 18, 1925, Smith inoculated "about 90 sunflowers . . . in the small undeveloped flower head hoping to get again strop-flowers in center of the heads." By July 15 the five or six heads "smallest when inoculated" showed "large green bracts in the center" and would, he believed, later "show central strop shaped flowers." Two days passed, and twenty of the inoculated plants showed "large central green bracts and the beginnings" of the phenomenon. He wounded eighty side buds of small heads in the center with a scalpel and in each case cut out a central piece of the torus to see whether again he could get any result. By July 18 one of the "strop shaped yellow flowers" had opened and, on dissecting it, he found "a little crown-gall tissue in the extreme top of the stem under [the] bracts and also one small central white lobe distorted with tumors but," wrote he, "there seems hardly enough to account for the results. Can it be due solely to the wounding? The distorted tumefied piece" was then "fixed in alcohol." July 22 he "got very striking results in one head, viz. a second small head developed in the middle of the large one." He dissected two inoculated sunflower heads that showed no central proliferation, were not split open, and had no tumors on or in the stem, but did have tiny ones on the torus. By July 25 his memorandum read: "There are many long tumor strands in the stems of the proliferated sunflowers. It seems hardly possible the 14 to 19 inches of strand is all due to stretching of stem under the young head and no invasion, but most of it may be." Within three more days twenty-four of the eighty-eight sunflowers had proliferated in the head and he saw "in the pith of stems under the head long cyst-like cavities lined like the torus with a soft hairy membrane. These cysts," he noticed, "often bear tumors on their walls and are subtended by a ring of vascular bundles, a dozen or more with xylem outermost and phloem turned toward the cyst cavity."

What he had learned from these experiments he itemized in a memorandum of July 31:

- (1) have found many stems in the pith extending down long distances,
- (2) have found pith cysts lined by a hairy membrane like that of the torus

and giving off tumors at intervals, (3) the pith stems have reversed bundles i. e. phloem in the center and xylem on the periphery, (4) occasionally roots developed in the center of the flower head, (5) in a few cases complete secondary small capitula formed in the head as a rose sometimes proliferates out of a rose, most of these were sessile but four of them were pedicellate, (6) the development of big leafy bracts and expanded yellow ray flowers in the middle of the disk flowers was often very striking.

He planned to repeat on a large scale some experiments to decide whether by wounding very young buds he could not get the same results as to proliferation without using the crown gall organism. Already he had about one hundred wounded side flowers on big plants and these, together with seedlings coming on for autumn, would provide, he thought, sufficient material. On August 11 he counted one hundred and fifty-seven wounded sunflower heads and, finding not a proliferation, cyst, or abnormal development of vascular bundles in the pith, he concluded, "Woundings without crown gall inoculation will not cause green bracts and ray flowers." But by September 28 his conclusion was changed: "All but 5 of the 108 needle pricked sunflower heads are now in blossom," he wrote. "3 of the youngest heads (of those in bloom) show ray flowers (not yet open) among the tubular flowers. Wounding alone will do it! If done early enough." He now had enough material for "an interesting paper" and the general conclusion he drew from the experiments was that "many of the common teratological forms in plants and animals are due to parasitic or mechanical traumatic displacement occurring in early life." More specifically, he said:

The results of these experiments confirm and extend earlier ones on other plants—daisy, pelargonium, sugar beet, tobacco, ricinus, tropaeolum, etc. They show that what young cells will become depends to a very considerable extent on how they are treated. A changed stimulus, if not too strong and if applied early enough, leads inevitably to a changed structure."⁵¹

While his experiments on sunflower plants were being pursued, he had been studying also crown gall inoculated Ricinus plants. In the first weeks of August 1924, three of his diary memoranda read:

⁵¹ Tumors, cysts, pith bundles, and floral proliferations in *Helianthus*, *op. cit.*, summary and discussion.

August 2: As indicating the long continued virulence of certain strains of *Bacterium tumefaciens*, witness the Hop strain through col. 1 (1915) i. e. on agar since 1915. It was isolated from a hop tumor in 1906 or 1907. We obtained our first infections with it in 1907, on hop et cetera. Now, of the 190 young Ricinus plants which I inoculated July 24th (1924), every one but 3 have given fine tumors in nearly or quite every needle prick! One of these three shows no pricks and may have been overlooked and of the other 2, one shows a tiny tumor. I consider this very remarkable because it is 18 years since first isolations and 9 years since the sunflower col. 1 re-isolation was first cultivated on agar and in all this time it has not been reinforced by passage through plants! Of the 2 exceptions (which I will do over) one shows nothing and the other very slight tumors.

August 9: Reinoculated the 3 Ricinus plants (see Aug. 2) with 4 day whey agar, also as checks two younger Ricinus plants—all from one tube.

August 12: Examined inoculated Ricinus plants and made 12 photos (each 2/3 natural size). I see no evidence of tumors with roots or leaves, although they cover a large part of the top of the stems and rim out on many leaves (petioles and blades).

Two days later he happened to show his Ricinus inoculations to Secretary of Agriculture Henry C. Wallace. On the previous April 23 the Secretary had seen some crown galls, at a time when Smith was working at potentiometer hydrogen-ion determinations of crown gall of sugar beet, sunflower, ricinus, tropaeolum, and other plants. His main reason for seeing the Secretary was to protest against a proposed removal of some of the Department's facilities to government-owned land on the Potomac twenty miles distant. He had recently been elected president of the American Association for Cancer Research, and his enthusiasm to continue his work without any change in working conditions was evident. In 1922 the Secretary had been given an account of Smith's research achievements, and his already quoted response showed that he recognized the value to the Department of such a world known scientist in its employ.

During their conference of August 14, Secretary Wallace suggested to the veteran scientist a journey to Europe. They "got to talking about agricultural research in the United States and in Europe, and he said, 'How long is it since you were over there?'" Smith answered,⁵² "'Eleven years.' 'Don't you think it would be a good idea to go over there and see what they are doing?'" the Secretary asked. The response was, "'I should like to, would

⁵² Diary, Aug. 14, 1924.

you approve it?' He said, 'yes.'" Smith explained, "'This is the only year I could go, for one of my assistants is on leave of absence, and next year I shall [be] hard up again.' He said 'yes. Go this year.'" Smith was very happy. He had "met the Secretary only a few times and in no way [had] tried to 'cultivate' him. I like him, however," he wrote, "and, evidently, he likes me." Immediately beginning to prepare for his journey, he booked passage for the steamship *George Washington*, of the United States Line. He gathered together his notes made during the summer from his potentiometer work, and arranged to take more than one hundred mounted photographs illustrating his crown gall studies. On-September 3, Dr. and Mrs. Smith left for Europe.

On the day of their departure, he wrote further: "It has been a great wrench to pull loose. . . . However, the summer's experiments are pretty well over and the autumn ones I had planned must wait." He planned to "make some further experiments on *Ricinus* with monobasic ammonium phosphate for tumors in young stems." When he returned, he inoculated *Ricinus* plants with ammonium formate, ammonium lactate, and ammonium carbonate.⁵³ These experiments were started early in the summer of 1925 and by October, "Each one [had] given proliferations, but in most cases not very extensive ones."

Dr. Smith kept a journal of his journey, and he purposed, while in Europe, to write up the "potentiometer crown-gall studies" of the past two years. He had also planned to "complete a paper on proliferations in *Bryophyllum*," to "do one more on *Begonia phyllomanica* proliferation," to "complete a paper on night and day variations in pH of leaves of sunflower," and to prepare with various of laboratory co-workers papers on *Begonia lucerna*, on a bacterial disease of sorghum and broom corn, on the Puerto Rico bacterial disease of sugar cane, on tuberculosis of sugar beet (*Bacterium beticolum*) and, among other things, he had not forgotten the final revision of some chapters of the fourth volume of *Bacteria in Relation to Plant Diseases* and the experiments with Chambers' improved form of Barber's apparatus "to produce crown galls in single cells and see them grow in nutrient solutions under the microscope." But each of these tasks would have to await his return.

⁵³ Diary, June 20, July 3, Oct. 26, indicating ammonium carbamide a type used.

On the *George Washington*, he became acquainted with several distinguished passengers: Dr. Oswald S. Lowsley, urologist of New York and consulting surgeon of the steamship company, one Dr. Young of Montreal who had been at Johns Hopkins and was now going to Paris to study anatomy of the brain, and others. One afternoon he showed his crown gall photographs to the two doctors.

Straightway after arriving in Paris, Dr. Smith went to the Pasteur Institute where he met Dr. A. Besredka. To him and one of his students, Dr. Harry Platz, a few days later he showed his crown gall photographs. Dr. Besredka, at the time, was preparing a book on "Local Immunity," a subject which Smith believed opened "a vast field for study."⁵⁴ In an address he later spoke of this as an important advance of the decade and described Besredka's theory as showing that "bacteria deadly in certain tissues are harmless when placed in other tissues." For instance, the anthrax organism "kills when placed in the skin but is harmless when placed in wounded muscles, liver, lungs or other internal organs. This tissue resistance [is] called 'local immunity.'"

Dr. Besredka presented Smith with a copy of his *Histoire d'une Idée L'œuvre de Metchnikoff* (1921). The great Élie Metchnikoff, "founder and defender of the phagocytic theory,"⁵⁵ the discoverer of phagocytosis, and former vice-director of the Pasteur Institute, had died in 1916. He had written a book on the subject of Immunity, and Émile Duclaux, director of the Institute until 1904, in his book on Pasteur,⁵⁶ had devoted fully one-half of his discussion of viruses and vaccines to immunological problems.

On September 17 Smith learned that one Dr. S. Metalnikoff, now five years with the Institute, had "been making experiments with *Bacterium tumefaciens* on the larvae of the wax-moth," and the next day he saw "smears made from worms injected" with the crown gall organism. He examined also "smears and beautifully stained sections of larvae infected with the human tubercle organism. In a few hours," he noted,

the bacteria are all picked up by phagocytes and destroyed. I never before saw phagocytes so literally stuffed full of bacteria. In a few days they are

⁵⁴ Fifty years of pathology, *op. cit.*, 41. Also, Journal, Feb. 10, 1925.

⁵⁵ V. C. Vaughan, *A doctor's memories*, *op. cit.*, 139.

⁵⁶ Pasteur, *The history of a mind*, *op. cit.*, 8th part, 299-321.

all dissolved into a brown debris and around this there is formed a thin fibrous wall. In this stage it is impossible to kill guinea pigs by injecting bits of these parts of the worm previously containing enormous quantities of a very virulent culture. All are dead.

The *Bacterium tumefaciens* experiments had produced no tumors. "Those larvae kept at 37°C," Smith was told, "were not injured by the organism; those kept at room temperature (20°C) were killed." His journal memoranda read:

He used very large doses, too many organisms I think. [September 18] In smears made from worms injected with *Bact. tumefaciens* four hours previously, the rods are capsulate and very short and in smears made after 48 hours the rods are larger and inclined to be in chains and non-capsulate and the worms are killed, if kept at room temperature. M. thinks a new strain has been developed. I am in doubt. He has not tried out the "new strain" on any plants.

Director Roux was "most cordial" to Smith. On the second day of his visits, he had an hour with the famous French physician, bacteriologist, pathologist, pupil of Duclaux, collaborator with Pasteur and Chamberland, and whom he had heard believed cancer of parasitic origin. When Smith showed him his crown gall photographs, he sent for Dr. Calmette; and they both "asked many questions and showed great interest." In the Director's office, he met Dr. Régaud, said by Besredka to be "the most expert radiologist in France," and "famous for his studies of mitochondria in animal cells."⁵⁷ Later, at the Madame Curie radium laboratories in Paris, Professor Régaud and his associates would show Smith "many interesting things, including numerous photographs of remarkable cures effected, some by X-ray, others by radium." In 1923, at a clinic of the New York State Institute for Study of Malignant Disease, he had learned of "similar encouraging results" from treatments by x-ray for special types of cancer. On this European journey Smith was truly the president of the American Association for Cancer Research and would study the subject from the standpoints of diagnosis, treatment, palliatives, biology, and etiology.⁵⁸

On September 23 he began to divide his visits between the Pasteur Institute and the Laboratory of Plant Pathology, number

⁵⁷ Some newer aspects of cancer research, *Science* 61(1589): 596, June 12, 1925.

⁵⁸ *Idem*, 595-601.

11 bis rue d'Alesia. There he found the laboratory and garden looking "much as they did in 1913." There he met the Japanese professor Yendo, who was interested in silkworm culture and mulberry diseases and to whom Smith had given a letter of introduction the summer previous to Etienne Foëx. Foëx showed Smith a new potato disease from Morocco, which "begins as numerous surface specks" and which he concluded to be "probably a lenticel infection" which "ends in an interior soft rot." He was shown also "various interesting tobacco diseases," and he met Miss Sara Bache-Wiig, Norwegian by birth but then a professor and graduate of Smith College who had spent a year in study with Whetzel at Cornell and a year with Foëx in his laboratory. Smith remembered an assistant and student of mulberry diseases, M. Arnaud, from his work at the laboratory in 1913. He showed them all his crown gall photographs and, in turn, Foëx let him see "stained slides of Morocco potato tuber disease. Under the lenticels were disintegrating tissues and many bacteria (short rods and filaments)."

Several more times during this stay in Paris, Smith went to the laboratory. Once he had a "confab" with the Marquis le Normand de Bretteville, whose family owned large sugar beet interests which had been attacked by the curly top disease⁵⁹ and, still later, Foëx showed him "a new dwarfing disease of fodder beets in which no parasite [was] evident. He said," Smith wrote in his journal,⁶⁰ "it was not very common and had appeared only in the beets from one lot of seed. The leaves are thickened and die irregularly. The roots I saw were large. Mosaic?" Tumors on *Pinus cembra* from Nice, France, were also shown him; and Dr. Dufrenoy there promised to write the Forester at Nice to help Smith, when he went to southern France, to procure some specimens of pine tumors. An Egyptian brought out "sections of *Psycotma* tumors stained with hematoxylin" and, while he saw short and very abundant rods, they were not "sharply outlined."

Soon after arriving in Paris, Smith had begun to work on "a revision of [his and Miss Quirk's] potentiometer crown-gall paper." The completion of this was interrupted, however, by an invitation to give an address on crown gall. On October 3 he presented his first principal address to an audience of scientists while on this tour. He had but a few days in which to prepare it,

⁵⁹ Journal, Sept. 29, 1924.

⁶⁰ Journal, Oct. 9, 1924.

in fact, he finished his seventeen pages of manuscript during the morning of the day it was given. He expected to find only eight or ten persons in attendance. But when he got to the Jardin des Plantes he found "about 75 persons, and some very distinguished ones" awaiting his lecture. The occasion was the first meeting that year of the Société de Pathologie Végétale et d'Entomologie Agricole de France, and among those present were Dr. Roux, Dr. Calmette, Dr. Magrou who when Smith first called at the Pasteur Institute was away on vacation, Dr. Mangin, director of the Museum, Dr. Molliard of the Faculty of Sciences, Dr. Foëx, Dr. Maréchal or-Marchál (Smith spelled his name both ways), Dr. Bertrand of the Academy of Science, President Radais of the college of pharmacy, Dr. Vayssière, Dr. Patouillard, Dr. Loubet, Drs. Fron and Maublance, Dr. Mesnil, Professor Bouvier, and others of "the best known men in Paris." Mme Lemoine algologist, was also there. Dr. Smith exhibited his crown gall photographs, read his address slowly, received the "minutest attention," and was "applauded liberally." Jubilantly he wrote his laboratory co-workers: "'On Crown gall' ⁶¹ is to be printed and I have been asked to talk again on our return from Italy, next time on the general subject of bacterial diseases of plants."

Dr. Magrou and Dr. Pinoy of the Pasteur Institute asked to see again his crown gall photographs. He had met Pinoy when on his 1913 journey. In the conference which soon took place were also Foëx, C. R. Hursh, assistant to Professor Elvin Charles Stakman of the University of Minnesota and now studying for a year with Magrou, and Dr. Jean Dufrenoy who spoke English and occasionally served as an interpreter. Pinoy showed Smith "sections of a crown gall tumor on Pelargonium made with the hop strain of *Bacterium tumefaciens*" obtained from Smith's laboratory. He wrote in his journal: ⁶² "These were stained in Riker's way with very dilute stain, and it really looks as though he had succeeded in staining the bacteria *inside the cells*. He finds none in the intercellular spaces, nor in most of the proliferating cells, but a few cells of rather large size are *full* of short rods which may be the bacteria, in which case surely there is action at a

⁶¹ E. F. Smith, Le crown-gall, *Revue de Pathologie végétale et d'Entomologie agricole*, 11(4): 10 pp., 1924.

⁶² Oct. 6, 1924.

distance." In his address he had mentioned the question of the location of the bacteria, intracellular or within the cells, as a point still under discussion: "Les bactéries du Crown-gall sont-elles dans les espaces intercellulaires ou bien dans les cellules? Cette question est jusqu'à présent un sujet de discussion. En quelque lieu que soient logés ces micro-organismes, leur action est, je pense, toujours de nature physico-chimique et non mécanique."⁶³

Before Dr. Smith had left the United States, Dr. Riker had again reported observing the bacteria in the intercellular spaces. The bacteria, seen between cells in living uncontaminated sections placed in agar, had been observed to grow, had been isolated, and identified.⁶⁴ Nothing from Dr. Smith's diary intimates that he knew this. But, after he had gone to southern France and Italy and returned to Paris, he read "Pinoy's note on Plant Cancer or Crown Gall"⁶⁵ and wrote the following in his journal February 6, 1925:

Pinoy and Magrou were unable to find bacteria in crown-galls on *Pelargonium* by Riker's method. Pinoy found in large peripheral tannin cells short rods often paired which he takes to be the bacteria. He did not find them either in or between the small cells of the tumor (Magrou's statement to me and my own observation of Pinoy's slide). His sections were fixed in [-] with iron sulphate, washed free, and stained in indigo blue and counter-stained with fuchsin. The bacteria are red and the tannin granules black. There were fewer of these in the cells containing the bacteria. He emits the hypothesis that in crown-gall, as in the nitrogen tubercles of Leguminosae, the bacteria are not in the cells that form the tumor. This I doubt. They are not, it is true, in the fine hyperplasia of the Legume nodules, but the swollen cells in which they occur are so numerous that they make up the bulk of the tumor. He thinks Riker mistook for crown-gall bacteria *B. fluorescens*, very frequent on tomato and tobacco and always intercellular. This I also doubt. Riker is I think too good a worker for that.

On October 14, Dr. and Mrs. Smith left Paris and, after stopping a few days at Avignon and Nîmes, journeyed on to Montpellier where Dr. Ravaz escorted them about the agricultural college and botanist Boyer took them through the Arboretum.

⁶³ Le Crown-gall, *op. cit.*, reprint, 7.

⁶⁴ Some rel. of the crown-gall org. to its host tissue, *Jour. Agr. Res.* 25(3): 119-132, at p. 130, July 21, 1923; also *Bot. Rev.*, *op. cit.*, 12: 70.

⁶⁵ P. E. Pinoy, à propos du cancer des plantes ou crown gall, *C. R. Acad. des Sci.* 180: 311-313, Jan. 26, 1925.

By October 20 they were in Marseilles where, as at Montpellier, they enjoyed the botanic garden. Before Smith left Paris, he had been told by Dufrenoy about the tumors to be found on *Pinus halepensis* and *Pinus strobus*. Since 1913 he had been trying to get suitable materials for a more or less complete study of pine tumors. What materials he had received, however, had proven old or otherwise unsatisfactory. He purposed now himself to collect proper specimens and send them to his laboratory in Washington. They visited Arles near Marseilles, then Nice, and, among other points, Monte Carlo, where north into the mountains near La Turbie, he, accompanied by a forest guard, climbed down a steep and very rough mountain-side to a "stragglng plantation of *Pinus halepensis*."⁶⁶ There he found an abundance of tumors of all sizes and "collected tumors from 7 or 8 trees all living, and all the younger ones up to $\frac{3}{4}$ inch in diameter covered with a smooth unbroken bark, the larger ones fissured." He saw none on wood of the season's growth; and, inquiring as to other species, he learned "there is a similar tumor on *Pinus cembra* in the high mountains back from the coast, but none . . . on *Pinus maritima* which grows on the mountains near *P. halepensis*. . . . Strange," he said, "that this species should be wholly immune! I wonder if it is." He remembered having seen one tree of *Pinus maritima* where they collected the tumors. So he mailed six of the pine tumors to Miss Brown of his laboratory and planned to send other specimens later from Florence, Italy, evidently after he had consulted with Dr. Petri of the School of Forestry in the Cascine there.

Dr. and Mrs. Smith spent one day at Genoa and, soon after arriving at Florence, he learned at the botanic garden where Dr. Petri could be located. Finding him, he discovered that he recently had published

a well illustrated paper on the pine tumor from material obtained near the sea between Leghorn and Quercianella and in the latter place. He [had] isolated and described a yellow organism, a polar flagellate gram positive organism, as the cause of the tumor, but he [had] not reproduced the disease by his pure culture inoculations. He [thought] the disease is introduced into the pine by aphides, *Eulachnus agilis* (Kalt.) Del Guercio.

Smith read his paper, next day examined his "interesting slides,"

⁶⁶ Journal, Oct. 22-24, 1924.

saw his "very good . . . photomicrographic apparatus." promised him a copy of his text book, but still believed that another paper on the subject could be prepared. His reasons were that the "failure to reproduce the disease [made him] suspect [Petri] may have isolated and described the wrong organism. . . . His drawings and photomicrographs [were] very attractive. He [had] stained bacteria inside of certain cells as well as in the intercellular spaces and the cavities." But Smith still wanted to re-study the pine tumor and Petri gave him three of his slides.⁶⁷

He learned that Dr. Victor Peglion was now in Rome as Under Secretary of Agriculture. On November 5, in Rome, he called at the Department of Agriculture and Peglion immediately presented him with a copy of his book, *Le Bonifiche in Italia, Problemi e Finalità Agricole*. He met many interesting officials, saw the laboratories and museum, and promised to return the next day to study specimens and examine more closely an exposition of the Italian universities then being held. These were troublous times in Italy, politically. Premier Mussolini had established himself in complete control of the government. This pleased Smith no more than the prospect of the English elections and the possibility that the Labor party "with its Socialistic program" might not be beaten. What Smith objected to was the then aspect of "Communitistic domination. . . . The Communitistic tail is wagging the Dog," he said.⁶⁸ "The rougher elements in the party cannot tolerate free speech when it is not on their side and are breaking up meetings by catcalling and throwing things. It is a shameful side of the Labor movement in all countries that when they are short of arguments they resort to violence." He had been recently shocked to read in the *London Times* of the sudden death of Secretary Wallace. He admired the great men of both political parties, Republican and Democratic. At one time he had been a Republican, but right now he was in sympathy with the views of Woodrow Wilson. In politics, he was, as he said, "an onlooker rather than a worker." Science was to him "the key wherewith [to] unlock all mystery of time and sense, of life and death, of whence and whither borne." His plea was: "Let me the strong sweet life of Nature find, the God of earth and heaven whom Science bares." In Italy, he found that, owing to the war's after-

⁶⁷ Journal, Oct. 28, 29, 1924.

⁶⁸ Journal, Oct. 27, 1924.

much, almost no money was available for plant pathology and young men were going into commerce. The same is true," he wrote, "all over Europe, and for the next twenty years the United States, if it can keep out of another great and wasteful war, will have scientific research (outside of the operation of munitions) very much to itself, and ought to lead the world and *more!*"

Dr. and Mrs. Smith visited the International Institute of Agriculture, founded by David Lubin. There they met, among other important persons, Madame Olivia Rossetti Agreva. When he had sent her a reprint of his address on twentieth-century advances in cancer research, she had thought its contents "so valuable" that she had given her copy to Dr. W. S. Gray of the Institute who had written Smith that "this survey should be studied by all those who are working on the subject. Years ago," he wrote, "when studying for my first degree, I did some work on 'Crown Gall of Lucerne'—but no-one then connected it in any way with cancer." Peglion and Dr. Beniamino Peyronel, vice director della R. Stazione di Patologia Vegetale, took Smith on motor tours and field trips around Rome, and Peyronel called to his attention a recent article on crown gall by Dr. Magrou. The political tension and disturbances accompanying the celebration of Armistice Day in Italy persisted. He, nevertheless, got to a library, read a paper by Petri on olive diseases, examined his photos and photomicrographs in his exhibit at the university exposition of pine tumors and olive and ash fasciations, and at last located Magrou's article in the *Annales de l'Institut Pasteur*.¹⁰ He found it "based on his own studies, but referring very frequently to my work. He brushed aside the criticisms of a certain objector by saying they amount to this only: 'Crown gall is due to a parasite, and cancer by definition is non-parasitic, therefore crown gall cannot be a cancer.'" He read other literature on cancer causation and before he left Rome met "Professor Grassi of mosquito fame. He discovered," he said, "the carrier of the human malaria parasite to be *Anopheles*."

On this journey while in Italy and Sicily, Dr. Smith especially enjoyed renewing acquaintance with three of his older friends in

¹⁰ Journal, Nov. 5, 1914.

¹¹ J. Magrou, Recherches expérimentales sur le cancer des plantes. *Ann. Inst. Pasteur* 33(1914): 857-871, 1914.

science: Peglion at Rome, Cavara at Naples, and Savastano at Acireale. Cavara was director of the botanic garden, and Savastano, "an indefatigable student of literature," had "the best library in Italy on trees and plant pathology and experiment station publications." He told Smith that the carob tree tumors were not due to bacteria. He had described and figured, as not due to fungi, the hard burls of the olive. Mal de Spacco, a wood disease of grafted oranges, due to bacteria, interested Smith, the more since the malady did not appear on wild stock.⁷¹ At Palermo, he discussed tumors and showed his crown gall photographs to Director Luigi Buscalioni of the botanic garden there. His talks with scientists about tumors and pathology were many, and he was flattered at the interest which Dr. W. T. Councilman, whom he chanced to meet in Rome, took in his photographs: Together they examined a tumor on live oak (*Quercus ilex*). "The trees," Smith wrote,⁷²

are parasitized by a beetle and some of them are enormously deformed. The leaves on the sprouts from the tumors are sharply serrate while the healthy parts bear leaves wholly or almost free from serrations. This is only a young form of the leaf i. e. such as occurs normally on young trees, as I convinced him. After lunch I showed him the crown gall photographs. He expressed great interest and said he was convinced I am right in claiming analogies to malignant human and animal tumors.

Dr. and Mrs. Smith's return to Paris by way of Pisa, Genoa, Nice, Lyon, and other points preceded the New Year by a day or two. On the same day, January 5, that he sent out letters to fourteen members regarding the next annual meeting of the American Association for Cancer Research he received a report from Miss Brown of his laboratory that "successful inoculations on other plants" had been made "with colonies plated from the pine tumors. She thinks it crown gall," he added, and consulted Dufrenoy who promised to "try to find pines and ashes for [him]. He showed [him] small tumors on *Abies*" from which he got bacteria. A tumor on *Sequoia sempervirens* was next brought to Smith's attention. He thought this looked like crown gall and, given specimens, sent them to his laboratory. On January 19 Dufrenoy showed him "under the microscope a pure culture of a coccus he [had] obtained (single or in doublets) from the tumor

⁷¹ Journal, Dec. 11, 1924.

⁷² Journal, Dec. 18, 1924.

on *Sequoia sempervirens*. He also showed [Smith] the ms. of his thesis for the Doctorate on Tumors of Conifers and asked [him] to read it," which he did that night. Next day the two scientists went to Bois Vincennes and found not only a *Sequoia* with tumors resembling crown gall but also, about forty rods away, another tree of the same species free of tumors. Small tumors on an *Abies* on the same grounds were discovered. Dufrenoy took specimens of each for further study and Smith noticed "a long row of *Berberis* (many species)."

En route to the Jardin des Plantes from the Ecole d'Agriculture et d'Horticulture de la Ville de Paris, they called on Professor Guilliermond to whom Smith some two weeks before had been introduced at a dinner at Dufrenoy's home. Smith, during the next summer, would try to demonstrate by special fixations and dilute stains crown gall bacteria in large peripheral cells of tobacco stem tumors, and to differentiate the bacteria from mitochondria.⁷³ Guilliermond was believed to be "the best cytologist in France,"⁷⁴ and, in his laboratory, he showed them slides of stained mitochondria of *Elodea*, *Iris*, *Lilium*, *Pisum*, *Saprolegnia*, and other plants. Smith thought him "a superb technician." He showed him his crown gall photographs and photomicrographs. Guilliermond told him that "by his methods we ought easily to distinguish between mitochondria and bacteria in crown-gall cells — in five minutes, he said," and he presented Smith with a copy of his last important paper, "Nouvelles recherches sur les constituants morphologiques du cytoplasme de la cellule végétale." Together the next day they went to the Madame Curie laboratories where Professor Régaud and Dr. Antoine Lacassagne and other men whom Smith met took him through the new building, their laboratories, and the examination, lecture, and work rooms.

As time permitted, since his return to Paris, Dr. Smith had been preparing his conference address on bacterial diseases of plants. Before regarding it finished, he submitted it to Dr. Foëx, and on January 27 he and Mrs. Smith drove to the "Lecture room of the Museum of the Jardin des Plantes where [he] lectured to about 65 persons. Heavy rain all day discouraged some and others are reported ill, but I," Smith wrote of the occasion,

⁷³ Diary, June 18, July 30, 1925.

⁷⁴ Journal, Jan. 6, Jan. 20-21' 1925.

had many of the most distinguished biologists of Paris—Mangin, Mesnil, Viala, Bloëringhem, Costantin, Guilliermond, Magrou, Foëx, etc. They listened very intently and several took notes feverishly and when done I received a good round of applause. Professor Mangin, director of the garden, who introduced me in Professor Molliard's absence, also closed with some highly flattering remarks and asked to have the paper for *Revue générale des Sciences pures et appliquées*.⁷⁵

That evening, the first of two elaborate dinners given in Dr. Smith's honor, took place at the home of Jean Magrou, sculptor and brother of Dr. Magrou of the Pasteur Institute, with twelve guests seated. The other, given at Dr. Calmette's home for eighteen persons of great distinction in literary and scientific circles of Paris, occurred two evenings later. Madame Mary Duclaux and Dr. Pasteur Vallery-Radot were there, and among the scientists were Mesnil, Besredka, Weinberg, Magrou, Bertrand, Delezenne, Nègre, and Calmette.

While in Paris, Smith had many conferences with research men about diseases of plants: pear blight; mosaic, white-spot, and chancre of tobacco; crown gall; and others. At the Hôtel Dieu, the great hospital near Notre Dame, Professor Menetrier welcomed him and he became acquainted with Dr. C. Botelho, chief of the cancer laboratory, and Dr. Hartmann, surgeon. To them he showed his crown gall photographs, discussed whether cancer is caused by a parasite and whether its spread in plant and animal bodies is by apposition, and diagnostic methods by serum or radium emanations. Dr. Hartmann was especially interested in Smith's "photos showing results of chemical stimulation" in crown gall.⁷⁶ Dr. Menetrier showed Smith "a slide of stomach carcinoma in a rat produced by injecting tar into the loin of the rat at intervals (about 20 times) in course of a few months. After six months he obtained a stomach carcinoma very definite with no tar touching it. The tar is all on the outside of the stomach muscles plainly to be seen. Contrary to statements made in the first edition of his book Dr. Menetrier now says there is *no growth by apposition* in cancer. He thinks there is no parasite in cancer. Dr. Botelho, whose laboratory I next visited, believes there is a parasite. He tells me," Smith wrote,⁷⁷

⁷⁵ Les maladies bactériennes des plantes, 134-138, 193, Mar. 15, 1925.

⁷⁶ Journal, Jan. 24, 1925.

⁷⁷ Journal, Jan. 24, 1925.

that someone has found that in tar cancer in mice all the tripanosomes migrate to these sore spots. He says Dr. Magrou and the Surgeon with whom he is working have discovered that the growth of crown-galls can be prevented by exposing them to a certain kind of vibratory machine which gives very long waves. He himself has discovered a diagnostic method for cancer.

On January 31, Smith went again to Dr. Botelho's laboratory and this time met Dr. K. Itchikawa who had spent a year and half in Paris studying tar cancer at various laboratories including that of Professor G. Roussy in the École de Médecine. To Smith's delight he found that Itchikawa planned to visit in May his laboratory of plant pathology. "He seemed pleased to meet me and I certainly was to see him, because," wrote Smith,⁷⁸ "he has done splendid work on Tar Cancer. In Botelho's laboratory I read his serum charts. He claims, with serum from cancer patients, to obtain a definite diagnostic reaction," which he later demonstrated for Smith. Dr. Smith took pages of notes on the diagnostic tests, why some failed to give the reaction, the reagents used, and the mixtures which gave the best results of various purposes, including palliatives for hopeless cases abandoned by surgeons. Itchikawa confided to Smith that, in tar cancers of rabbits, he had secured confirmatory results of Botelho's method of diagnosing "invisible cancers" and believed in it. He was told also that Dr. Hartmann used the method in his surgical work.⁷⁹

Dr. Weinberg of the Pasteur Institute invited Smith to see his "many drawings and paintings showing result of the activities of intestinal worms in various experimental animals. He [had] proved that they can penetrate the musculars of the stomach and intestine in some cases allowing bacteria to enter causing ulcers and in one case a fibrous tumor." Since the war, he had been publishing and lecturing on anaërobic bacteria and gangrene.⁸⁰

February 3 was also an important day since in the morning Smith visited Dr. Magrou's tumor laboratory and the surgical laboratories of the famous surgeon, Dr. Gosset, at the great Salpêtrière Hospital. His journal reads:

Dr. Magrou showed me again the slide of Pelargonium tumor stained

⁷⁸ Journal, Jan. 31, 1925.

⁷⁹ Some newer aspects of cancer research, *op. cit.*, 595-596; Journal, Feb. 3, 4, 1925, Feb. 11, 1925.

⁸⁰ Journal, Feb. 2, 1925, Feb. 10, 1925.

The first part of the book is devoted to a general history of the United States from its discovery to the present time. It is written in a simple and plain style, and is intended for the use of schools and families.

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2. The second part of the book is devoted to a detailed history of the United States from the discovery to the present time. It is written in a simple and plain style, and is intended for the use of schools and families.

drawings and water color paintings of algae," Borner's library, Thuret's herbarium, and "photos or engravings of many prominent algologists, including Dr. Farlow." He visited the laboratory of Madame Lemoine, "an expert on calcareous algae."⁸⁵ He spent another day at the Pasteur Institute with Dr. Besredka and Dr. Weinberg.⁸⁶ When calling on Dr. Itchikawa in Dr. Roussy's laboratory again, he became acquainted with Dr. A. Kotzareff who had discovered "an interesting test for cancer."⁸⁷ His more complete estimate of this was later told to Dr. Sofia A. Nordhoff-Jung of Washington:⁸⁸

Dr. Kotzareff has found that if a few millicuries of radium emanations are added to auto serum and injected intravenously into cancer-bearing animals, they are not injured by it and the emanations are absorbed by the tumors which are killed. Undiscovered internal tumors may also be diagnosed in this way by exposing a photographic dry plate over the suspected spot. The only other tissues of the animal body capable of absorbing the radium are embryonic tissues. The method has been tried in a few cases of human cancer with apparently favorable results, but much remains to be done. . . . His method of attack on cancer seems to me at least very hopeful and *every method* of approach offering any least likelihood of success ought to be tried out thoroughly. It is at least very suggestive that Dr. Blair Bell's treatment in Liverpool, now attracting so much attention, acts in the same selective way, the lead salts killing the tumor cells more readily than they do the normal cells. It is interesting also that Dr. Blair Bell was induced to try lead salts in cancer by observing that in abortion in rabbits, induced by means of lead, it is the actively-growing chorionic cells that are killed.⁸⁹

On the way from Paris to Berlin, Dr. and Mrs. Smith stopped for a few days at Strasbourg where Dr. Amédée Borrel was in charge of the Pasteur Museum and the Institute for Bacteriology and Hygiene, one of the best in France. Before leaving America, he had written Borrel who told Smith to "Come over at once," and so they went immediately to the Musée Pasteur where they were shown through "the magnificent laboratory building" and the museum. Research work was going on in tuberculosis, cancer, syphilis, vaccine virus, and "all sorts of human and animal

⁸⁵ Journal, Feb. 6, 1925.

⁸⁶ Journal, Feb. 10, 1925.

⁸⁷ Journal, Feb. 11, 1925; Some newer aspects of cancer research, *op. cit.*, 595.

⁸⁸ Feb. 20, 1926, letter.

⁸⁹ Smith mentioned also an article by G. Tizzoni, E. Cetanni, and G. de Angelis, *Jour. Tropical Med. and Hygiene*, 438-441, Dec. 15, 1925.

diseases," even plant diseases, too, "for I saw," wrote Smith,⁹⁰ "plants inoculated with crown gall." They were introduced to "numerous assistants" and to Dr. Masson, professor of pathological anatomy, whose book on cancer Smith had been reading. In Strasbourg were located the botanical laboratories founded by Anton de Bary, which, when Smith visited them, seemed to him "like sacred ground, so much I was influenced by his writings many years ago!"⁹¹

Their first visit to the laboratories of the Institute for Bacteriology and Hygiene had been in the late afternoon. Arrangements were made that Dr. Smith should show Dr. Borrel his crown gall photographs the next morning, and in the late afternoon show them to Dr. Masson. Smith culled valuable information the first day, however: he wrote⁹² that Dr. Borrel

has found the cornea of rabbits an excellent place for propagation of vaccine virus and here he has demonstrated by staining methods the presence of great numbers of cocci, sharply stained (he showed them to me under the microscope), and closely resembling similar bodies he has found in *Molluscum contagiosum*, but from neither disease can they be cultivated. There also I saw numerous very large tubes of bacterial and fungous cultures prepared for museum exhibit. Dr. Borrel has a superb collection of cancer slides which I should like to study if only there were time. In the Pasteur museum are also many exhibits it would be profitable to study.

Of his appointment with Dr. Borrel the next day, he said:⁹³

He was lecturing when I got to the Laboratory. . . . He came in to see me in about fifteen minutes and took me to another room where we placed all the sheets of photographs on a large table. He then asked if his class might also see them. I said, yes, and the result was I had to explain each one to a class of 70 medical students. They listened very intently, and when I finished in thirty or forty minutes, they gave me round after round of applause. I don't know what Dr. Borrel said to them about me in the amphitheatre, but certainly all that clapping and shouting could hardly have been in honor of my bad French. Afterward Dr. B. took me to another part of the big building and gave me the Pasteur centenary medal, silvered, in a silk-lined leather case.

His appointment with Dr. Masson also became more or less of a formal occasion. "I expected," Smith said,⁹⁴ "only two or three

⁹⁰ Journal, Feb. 16, 1925.

⁹¹ Letter to W. A. Taylor, Mar. 5, 1925; Journal, Feb. 20, 1925.

⁹² Journal, Feb. 16, 1925.

⁹³ Journal, Feb. 17, 1925.

⁹⁴ Journal, Feb. 18, 1925.

other persons to be with him to examine my photographs but more than fifty persons came, and I explained each one of the 128 photos to the crowd, as best I could, in their own tongue." Professors and students from the university as well as the institute were there, and several branches of research were represented, biology, botany, internal medicine, and others. Hideo Komuro, a Jap, became Smith's guide one day, Professor Charles Killian, botanist, another day, when either Dr. Borrel or Dr. Masson could not be with him. Smith was especially pleased to find a biophysicist [Professor Vlès?] "doing a great deal of work on normal and diseased tissues, using the potentiometer."⁹⁵ He wished that he might accept more of the invitations offered him. A tea "with half a dozen other research men" present, and another tea "with three research women present" were enjoyable. The great hospital of 2,500 beds in connection with the medical school and the institute, where annually a thousand operations were performed, provided Professor Masson, he knew, with "a wealth of pathological material." At the university botanical laboratories he saw "many interesting cultures and drawings of Ascomycetes, hepaticae, etc.," and he was given a flask culture of one hepatic which he hoped to cultivate, when again at his laboratory, to "try to obtain crown galls on it." Other work on an ascomycete and cultures of "the fungus element of lichens free from algae"⁹⁶ interested him. But the time had arrived for Dr. and Mrs. Smith to leave the city, interesting for many reasons, but especially so because of the points of association with such lives as DeBary's, Solms Laubach's, Pasteur's and Goethe's. From there they went to Frankfurt, and then to Berlin.

On the morning of February 25, Dr. Smith became acquainted with Director Blumenthal, Dr. Auler, Miss Meyer, Dr. Kraus, head of the hospital, Dr. Lewin, Dr. Hirschfeld, and Professor Brugsch. To his delight he found that Dr. Auler and Miss Meyer had been students of tumors, including crown gall and other plant tumors, for several years. Dr. Auler had been "definitely working on animal inoculations and transplants of the bacterially induced sarcomas and carcinomas since September, 1923." Miss Meyer, who often had been in England and spoke English, had worked

⁹⁵ Letter to W. A. Taylor, Mar. 5, 1925.

⁹⁶ Journal, Feb. 20, 1925.

with Friedländer, Magnus, and other eminent pathologists. She had "been working on tumors in this Laboratory since 1918, definitely on human and animal tumors due to bacteria since May, 1923." Her's was the discovery of the bacteria in the breast cancer, PM, so-called because of her initials. She had inoculated the sunflowers and obtained the results about which Smith had read in Paris. At once she showed Smith a diagram of her experimental plots. He saw on

one plot she inoculated *Bact. tumefaciens*. On the other plot, separated by a roadway (width 3 to 4 feet) she inoculated PM and obtained many tumors on sunflowers and tomatoes. She is quite certain there was no "verwechlung" of the cultures or cross-inoculations. Dr. Blumenthal has given me photographs of three PM tumors on plants (All on sunflowers. The one on cactus is due to *Bacterium tumefaciens*). I also saw many rats inoculated with PM. 12th transplant, also transplants of Hu tumors as big almost as English walnuts and several with good metastases. I saw also histological preparations of the animal grafts (sarcomas and carcinomas). Dr. Blumenthal assured me that they held controls, i. e. animals inoculated only with cancer serum and kieselgur and with these two they were not able to cause tumors. I also saw stained sections of a mouse tumor said to have been produced without addition of either Kieselgur or cancer serum. It contained epithelial pearls and appeared to be a beginning skin cancer. . . . I examined stained slides of the bacteria from PM, Be, Ze, Hu(2nd) and Rhode and one other. They all look much like *Bacterium tumefaciens*. All were well stained and sharp. They are short rods single or paired. Fuchsin stain, I think. I saw also some of the sunflowers first inoculated with PM (out of doors on large plants). The tumors were as large and as typical looking as any we have obtained in Washington with *Bact. tumefaciens*. Dr. Auler is now trying to grow *Bact. tumefaciens* in cancer serum at blood temperature and if he succeeds he will then inoculate animals with it. The one strain they now have come from Jensen in Copenhagen, it is said. It our hop strain, I think.

Smith inspected the cancer clinic, and was told of vaccines being used to relieve extreme suffering. These vaccines were experimental and in some cases improvements had taken place. Dr. Auler, among other things, was trying "histidin." He later went through the main building and another known as the Polyclinic. Various doctors accompanied him, and on the first evening Dr. Blumenthal took him to a meeting of the Berliner Medicinische Gesellschaft held in the Hygienic Laboratory. Three hundred of the two thousand members were present, and Dr. Kraus, the organization's president, introduced Smith as their "distinguished

guest . . . from America, the discoverer of the cause of cancer in plants." After the meeting, Dr. Blumenthal entertained him.

Next afternoon Dr. Smith again worked with Dr. Auler and Miss Meyer studying sections of various rat tumors from *PM* and *L*. The *PM* tumor, he concluded, "is a curious one. In the mouse it produced horncelled cancer. In the rat mostly mixed tumors but sometimes carcinoma (not alveolar) and at other times (in other transplants) typical sarcoma. Whatever the type it is a malignant tumor metastasizing freely. This morning I selected a white rat to take with me. . . ." ⁹⁷ He also picked out five stained slides of *L* and *PM*. After he had examined the results of some dissections they had made, he exclaimed, "I must work longer in this laboratory!" He had been urged to stay. That morning he had shown, and explained, his crown gall photographs to Dr. Blumenthal, Dr. Auler, Miss Meyer, and two visitors, Dr. Lewin and Dr. Paul Lazarus. Dr. Lazarus had studied some time in Paris and knew Dr. Gosset. Late that afternoon, he received notice that, because of his "important services to cancer research," he had been made a foreign associate of the Deutsches Zentralkomitee zur Erforschung und Bekämpfung der Krebskrankheit E. V.

From a young American doctor he learned there was much skepticism concerning the merit of the discoveries made by Blumenthal, Auler, and Meyer. One objection being raised, "a perfectly foolish one," Smith said, "since I have never tried it," was, "How should they be able to find *Bact. tumefaciens* in cancer, if Smith, the discoverer of the organism, could not find it there?" ⁹⁸

The same American doctor told him of a Dr. Otto Warburg who, at the Kaiser Wilhelm Institut für Biologie, had "worked a great deal on the cancer cell," and that Smith "might find it of interest to talk with him." He agreed to visit Dr. Warburg and Dr. Carl Posener in their laboratory, and did so on March 10, just before leaving Berlin to go to Copenhagen. From them he was to learn "how they determine the respiration of tissues and how the acid content of cancer cells, all very interesting. . . . They have much delicate and beautiful apparatus for their physiological determinations," he said, ⁹⁹ "and after seeing how they work I

⁹⁷ Journal, Feb. 26, 1925.

⁹⁸ Journal, Feb. 27, 1925.

⁹⁹ Journal, Mar. 11, 1925.

have great confidence in their results. They think, just as I do, that the cancer cells behave unlike normal cells, and contain unlike substance." Smith's technical account of what he saw and learned at Dr. Warburg's laboratory on March 11 filled two pages of closely written material in his journal. Furthermore, in two addresses¹⁰⁰ given after his arrival home, he was to describe this work and estimate its value for the future. He knew he had met a very able, great scientist. In 1931 Dr. Warburg would win the Nobel prize in physiology and medicine for his discovery of the character and mode of action of the respiratory ferment. Famous today for his work on oxidation in the cell and the nature of the flavine enzyme, he is known, too, for his discovery, with Dr. Christian, of the secondary respiration ferment. Recently he has been a visiting professor at the University of Illinois. There a special laboratory was established for his work.

From February 27 until March 2, every working day was given by Smith to re-checking the research methods and results of Miss Meyer, Dr. Auler, and Dr. Blumenthal. From March 2 through March 4 Dr. and Mrs. Smith, accompanied by Dr. Blumenthal, took a "pleasant and profitable" trip to Dresden. There the two scientists went to the Sächsisches Serumwerk Aktiengesellschaft where Smith met two high officials, Drs. Behlke and P. Galewsky, and various assistants. He was shown their chemical laboratories and work, and in the evening attended the Dresdener Medicinische Gesellschaft meeting to hear a lantern slide illustrated lecture on rat cancers due to *PM* given by Privat Docent Dr. Reichert of the Serum Laboratory. After this event, Dr. Blumenthal entertained eight guests, and the scientists were "greatly interested" and asked many questions of Dr. Smith about his crown gall work. By this time he was much more convinced of the validity of the Blumenthal *PM* malignancies. On February 27 practically every doubt as to whether *PM* would produce tumors in plants had been removed. But he had yet to be convinced that the rat-tumors were really caused by *PM*. The possibility of "some unknown virus or other irritation introduced along with *PM* and since propagated by grafting" was still in his mind. "Once started," he said, "grafting to any number of generations is no evidence as to the cause of a tumor."¹⁰¹ In a letter to Miss Hedges of his laboratory,

¹⁰⁰ Some newer aspects of cancer research, *op. cit.*, 596-597; Recent cancer research, *op. cit.*, 247, 255.

¹⁰¹ Journal, Feb. 27, 1925.

he suggested that the virus might have been "introduced with the heated cancer serum they used along with the *PM*. The more so," he added, "because, except in the first-generation tumor, they have not succeeded in re-isolating *PM*." But he admitted that "an independent investigator in a great institution for Serum Cultures where the greatest exactitude is habitual" had gone far toward confirming the Berlin work. This investigator had "injected into rats pure cultures of *PM* (*without any cancer serum*, but with Kieselgur) and . . . obtained the same malignant tumor as they did in Berlin." Two malignancies, transplanted through five and six generations, were important, even though fifty rats were used in the experiment, in view of the difficulty of transplanting a tumor successfully from "any animal to one of another species" and since it was not likely that the rats were of "any specially susceptible race." Smith was learning of other new experiments on animals in which in one way or another *Bacterium tumefaciens* was involved. The experience was "highly exciting," but he insisted "we must wait for more work to be done and for confirmation from other laboratories."¹⁰²

He had not seen "plant tumors due to *L* but it [was] possible [he might] see some on [their] return to Berlin," since seedling sunflowers had been inoculated with *L* seven days previously. When he got there, he found that the last set of 29 inoculations on sunflowers, made twenty-one days before, with *PM* had developed "28 good tumors and one small tumor."¹⁰³ These were "more than double the size of those [he had obtained] before [he] went to Dresden," and he was given two specimens by Miss Meyer. He looked also at the young sunflowers inoculated with *Hu*, *L*, and three other strains four days before he went to Dresden. On one of *Hu* there was a slight tumor; on *L* there was "nothing very definite but some callous tissue in the lancet cleft of the stem that may later develop as a tumor. Judged by this," he wrote,¹⁰⁴ "*L* is losing its power to produce tumors in plants. I am eager to see these plants on Monday, probably our last day here." But nothing further was said in his journal about the *L* inoculations or those of any other strain except *PM*.

As president of the American Association for Cancer Research,

¹⁰² Written at Dresden, Mar. 3, 1925.

¹⁰³ Letter to Dr. W. A. Taylor, written at Berlin, Mar. 5, 1925.

¹⁰⁴ Journal, Mar. 6, 1925.

he prepared and sent a note to *Science*¹⁰⁵ in the hope of interesting research laboratories in the work he had seen. At Berlin and Dresden, investigators had convinced him of the possibility, contrary to his former beliefs, that some strain or strains of *Bacterium tumefaciens*, as yet undiscovered, might prove to be the cause of some of the cancers of man. In a letter to Dr. Taylor he explained:¹⁰⁶

I have believed for a long time, as you know, that crown gall has many analogies to malignant human and animal tumors and that its study in many laboratories was bound to throw more or less light on the terrible unsolved human tumor problem, but at the same time I thought only of similarly acting bacteria as probably the cause of cancers in men and animals and never of *Bact. tumefaciens* as the cause chiefly because the few strains we have tested out for temperature relations did not grow in our thermostats at the temperature of warm-blooded animals, although always in the back of my head was the suspicion that some strains might grow at such temperatures, and so I have been always careful in handling the tumors and have generally sterilized my hands afterwards.

After he returned to the United States, he determined to "try to discover strains able to grow at 38°C," and he still doubted that "the crown gall organism could be the cause of cancer in man." His real belief was that "cancer might be due to some parasite endowed with similar chemical activities."¹⁰⁷

The Blumenthal work made him want to consult Jensen, Fibiger, Deelman, and other cancer research experts. He had not forgotten that "Friedman and his associates claimed to get *Bact. tumefaciens* from non-tumorous diseased conditions in man, e. g. from the human intestine, several times." Miss Meyer claimed to have got *PM* from breast cancer and "closely resembling strains from 15 other human cancers"; on agar streaks *PM* looked "exactly like *Bact. tumefaciens*."¹⁰⁸ Possibly his objection that an irritant "unknown virus" was introduced with the cancer serum had been overcome. He, nevertheless, reiterated this objection after his return to America:¹⁰⁹ and, more important, he still thought that some indiscernible or unknown virus might have been taken in with the *PM* culture "from the surface of the rat."¹¹⁰ He thought

¹⁰⁵ Cancer in plants and in man, *Science* 61(1581): 419-420, Apr. 17, 1925.

¹⁰⁶ Berlin, Mar. 5, 1925.

¹⁰⁷ Some newer aspects of cancer research, *op. cit.*, 601.

¹⁰⁸ Letter to Miss Hedges, Mar. 3, 1925.

¹⁰⁹ Some newer aspects of cancer research, *op. cit.*, 600-601.

¹¹⁰ Letter to W. A. Taylor, Mar. 5, 1925.

still other errors were possible: unknown natural tumors in the rat "already in process of development" or the pyramiding of a "number of successful malignant tumor transplants upon a very slight basis, perhaps on only one primary tumor" due perhaps to the virus. At least he was skeptical, and his skepticism persisted.

Before leaving Berlin for Copenhagen, he called on Dr. Otto Appel, director of the Biologische Anstalt, and found the Anstalt "greatly enlarged" since 1906 when he had first conferred with Director Rudolph Aderhold and when Dr. Appel was first assistant. Now a great agricultural research institution, with more than fifty branches of work and five substations, an expanded museum, and many other improvements, had been developed. Dr. Appel questioned Smith closely on many points and introduced him to a bright, young bacteriologist, Dr. Carl Stapp, who was working on crown gall and various other plant diseases. He showed Smith "good tumors of crown gall on potato tubers and stems, obtained by dipping the tubers for a moment into the culture and then planting them. [Smith] promised to send [him] various strains of *Bact. tumefaciens*. [He] saw tumors of this [Smith's laboratory's hop strain] growing on slices of carrot floating in water in sterile Petri dishes."¹¹¹

On March 9 he said goodbye to Dr. Blumenthal and his assistants and was told of a third rat tumor, *Beta*, from another breast cancer; but it was not yet known whether the transplants would take.¹¹² Miss Meyer told him that "she had tried in vain to cultivate bacteria from this breast-tumor resembling *Bact. tumefaciens* but she will continue to try. The difficulties," Smith wrote,¹¹³ "are great because there are many saprophytes and they grow rapidly and overgrow the plates and swamp out all slow-growing bacteria like *Bact. tumefaciens*. This raises again the question whether some virus carried over with *Bact. tumefaciens* may not after all be the cause of the rat tumors. Miss Meyer evidently thinks the bacteria are there."

Dr. and Mrs. Smith left Berlin for Copenhagen on March 12, he possessed of the valuable reprint given him by Dr. Otto Warburg, "Ueber den Stoffwechsel der Carcinomzelle,"¹¹⁴ which

¹¹¹ Journal, Mar. 7, 1925.

¹¹² See, Some newer aspects of cancer research, *op. cit.*, 601.

¹¹³ Journal, Mar. 9, 1925.

¹¹⁴ *Op. cit.*, at p. 578 of this book.

he read on the train. Dr. Warburg had been "very pleasant and expressed great interest in [Smith's] crown gall studies." He found Warburg's paper "exceedingly interesting," since it verified experimentally what Smith had thought out about "oxygen hunger as the cause of cell division."¹¹⁵

On the afternoon of their first day in Copenhagen, Smith set out to locate Dr. C. O. Jensen and, after arranging a future appointment with Dr. Johannes Fibiger, found Dr. Jensen in the Serum Laboratory of the Royal Veterinary and Agricultural High School. Jensen asked to hear of the Blumenthal work, and Smith showed him the ten slides which Miss Meyer had given him. One slide the distinguished Dane pronounced a sarcoma, another from the same rat "possibly an endothelioma," but he said generally of the slides he "had never seen any tumor like it." They discussed foot-and-mouth disease of cattle which recently had made its way into Denmark and for which a serum had been prepared. Smith met his son who was studying axolotls, North American tailed amphibians. They showed him through the laboratories and he promised to return later.

The next day Smith had no special appointment. But he went to Professor Albert Fischer, pathologist of the university institute for general pathology in connection with Rigs Hospital, and spent two hours with him. In exchange for a promise to send crown gall cultures and papers, Fischer gave Smith a set of his publications. This became "another superb day." Fischer had studied in New York and Smith saw some of his work. He wrote,¹¹⁶

He has been able to grow cells of Rous' chicken sarcoma in serum, something Rous was not able to do, by adding to the drop a bit of muscle and a trace of embryo juice to harden the serum. The sarcoma cells invade the muscle, destroy the protoplasm and liquefy the serum, something normal cells do not do. In this way he has by transfers every second day kept the chicken tumor cells alive out of the body for *two years*. I saw such cultures under the microscope. These two year old cultures when introduced into fowls cause virulent tumors, some of which I saw. In the lungs of one such fowl, there must be a *thousand* metastases. . . . He tells me Dr. Alexis Carrel is now studying cancer intensely in New York, and has found that he can convert the big mononuclear leucocytes into tumor cell, by treating them with the Berkefeld filtrate from Rous' chicken sarcoma. Dr. Fischer introduced me to his chief, Professor Dr. Thomsen, and together we went to an

¹¹⁵ Journal, Mar. 10, 11, 12, 1924.

¹¹⁶ Journal, Mar. 14, 1925.

amphitheatre where they had a lantern operator throw upon the screen a series of interesting slides of tissue cultures of normal fibroblasts and epithelial cells which behave quite differently, the former never changing into the latter and when cultivated together the normal epithelial cells grow as gland-like nests in the midst of the fibroblasts. He also showed slides of the chicken sarcoma cells invading small pieces of muscle.¹¹⁷

That afternoon Smith tried to consult "the great surgeon Dr. Rovsing, almost," he believed, "the only man in Copenhagen who has insisted that cancer must be due to a parasite." The Doctor was ill, but he enjoyed meeting his son Christian, first assistant surgeon, and Dr. Rovsing senior wrote Smith cordial letters after his return to America.

Professor Fibiger called on Dr. and Mrs. Smith late that afternoon, and Dr. Fischer requested that he and other doctors of his laboratory be shown the Blumenthal rat-tumor slides and crown gall photographs. Accordingly, two days later,¹¹⁸ Smith went to Fischer's laboratory in the morning and to Dr. Jensen's laboratory in the afternoon. He showed his photographs and ten slides of the rat-tumors; four sunflower *PM* tumors; and three whole rats, one inoculated with *PM* and two with *L*. Dr. Thomsen, a bacteriologist Dr. Jensen, and a young doctor were also present at the morning conference. Dr. Fibiger, at his "patologisk-anatomiske" laboratory on March 17, saw these materials, or most of them, during a two-hours' interview with Smith. He inquired whether either Dr. Jensen or Professor Thomsen planned to "repeat" the experiments. He wished to do so, and for the purpose arranged with Smith to send him cultures of "all the strains" of *Bacterium tumefaciens* his laboratory had. Fibiger thought that the slides of the *L* inoculations indicated "malignancy more than do those of *PM*." He promised to send "a series of his cancer slides" and told Smith some points from his own work which prompted the latter to conclude that, perhaps, what "Blumenthal has observed in his rat-tumors occurs also . . . in tar-cancer of mice." Smith wrote in his journal:

In mice attacked by cancer due to tar-paintings he said he got both sarcoma and carcinoma, and mixed carcino-sarcoma in the same animal, and this he demonstrated to me in well-stained preparations. In one and the same mouse I saw (1) good invasive carcinoma in the muscles, (2) in the

¹¹⁷ See, Some newer aspects of cancer research, *op. cit.*, 598.

¹¹⁸ Journal, Mar. 16, 1925.

heart-muscle, a metastasis that was a plain sarcoma, not a trace of carcinoma in it. In the lungs of the same mouse a good sized clump of carcinoma cells, and in another place in the same lungs, a metastasis that contained only sarcoma cells. . . . Dr. Fibiger does not believe with Roussy [of Paris] any more than I do, that one form of cell under pressure can change into the other. Sarcoma remains sarcoma, and carcinoma remains carcinoma.

Fibiger told Smith he now had a tar that would produce cancer in 95 per cent of animals, and Smith's concluding notation was: "He does not think that tar cancer can possibly be due to any parasite." He tried to consult with Dr. Fibiger once more before leaving Copenhagen but, unable to arrange a time convenient to both, an exchange of letters had to take the place of the second interview. Both he and Dr. Fridtjof Bang¹¹⁹ of the Rigs Hospitalet wrote Smith: on March 19 Fibiger expressed his indebtedness "for all you have shown and told me about your wonderful experiments [and for] the opportunity of seeing the preparations and specimens of Dr. Blumenthal. I," he said, "am not able to judge about the significance of these last experiments, and although I cannot deny that two of the microscopical preparations demonstrated by you were closely resembling malignant and especially sarcomatous tumors, I dare not exclude the possibility of all the disease being an infectious one and not at all tumoral. It seems to me that great series of control experiments are required. . . ." These he hoped to do, if and when the duties of his university professorship and other work permitted. Dr. Bang had been "very glad to see [Smith] in Copenhagen, and to learn more exactly and from first hand your famous investigations of cancer in plants, giving so beautiful results." On the evening of March 18, Dr. Bang had called on Smith at his hotel and, after demonstrating his cancer slides, examined the Blumenthal rats and slides and the crown gall photographs.

Dr. and Mrs. Smith left Copenhagen for Amsterdam on March 20. Part of his last day had been spent with Professor August Krogh,¹²⁰ zoophysiologicalist, Nobel prize winner, and Silliman lecturer in 1922 at Yale University on the anatomy and physiology of the capillaries. Reaching Amsterdam the next day, Smith, at his first opportunity, went to locate Dr. Deelman only to find he was now professor of pathology in the medical school at Gronin-

¹¹⁹ Some newer aspects of cancer research, *op. cit.*, 598.

¹²⁰ Cecil K. Drinker, August Krogh: 1874-1949, *Science* 112(2900): 105 f., 1950.

gen. Slides of Deelman's "very early stages of tar cancer in mice" were, nevertheless, at the Leeuwenhoek Huis, and he studied these on March 24, finding that they showed what Deelman "claimed, *i. e.* appositional growth in early stages of the tumor."¹²¹ Of the Blumenthal "schizomycete isolated from human breast cancer," Smith wrote Miss Hedges of his laboratory.¹²² "I have come to the conclusion that they have isolated *something* . . . that will produce tumors in rats and crown galls in plants — either bacteria (the visible thing) or some invisible virus carried over along with the bacteria must cause the rat tumors." He was meeting "the cancer specialists of Amsterdam"—Dr. Bonne, Dr. W. F. Wassink, Dr. W. M. de Vries, and Dr. Nicolas Waterman, among others. Dr. Wassink told him of "remarkably successful" cancer treatments by radium and x-ray. He discussed with Dr. Bonne some questions about parasites¹²³ believed found in tar-treated mice. He showed these doctors his crown gall photographs and Blumenthal rat-materials,¹²⁴ and then he and Dr. Waterman, a biologist, may have discussed Dr. Otto Warburg's work.¹²⁵ Smith told his first American audience that Waterman had "verified Warburg's results obtained on rats, *i. e.*, the glycolytic action of cancerous tissue on grape sugar with production of a great excess of lactic acid, using, *in vitro*, a variety of human cancers . . . I suspect," he said, "as does also Waterman, that small quantities of other acids may be present and act as stimulants to cell-division."

On March 23 Professor Johanna Westerdijk of the Centraal-Bureau voor Schimmelcultures at Baarn telephoned and sent a letter to Dr. Smith:

I am so glad to see you here to-morrow, both of you! I do not know what other plans you have in Holland, but on Friday from four to five I have my university lecture in Utrecht. I should care very much that all my students at Utrecht should meet you and *bear* you. I wonder whether you (instead of myself) would care to give them that lecture. Utrecht is a nice old town, where I might show you things. Please think of it. I was just going to treat "Vegetable galls" in my lecture; think of just having you here at the time!

Dr. and Mrs. Smith went to Baarn and were taken by Miss

¹²¹ Journal, Mar. 24, 1925.

¹²² Letter written at Baarn, Mar. 26, 1925.

¹²³ See, Recent cancer research, *op. cit.*, 243.

¹²⁴ Journal, Mar. 24, 1925.

¹²⁵ See, Some newer aspects of cancer research, *op. cit.*, 597.

Westerdijk to her home and laboratory. The Willie Commelin Scholten Laboratory, formerly at Amsterdam, had recently acquired an estate, and Miss Westerdijk divided her work in plant pathology thus, her research at Baarn and her teaching at Utrecht. Dr. Smith revelled in the glorious gardens, good greenhouse facilities, and the laboratories where plant diseases were studied. By frequent transfers 1650 kinds of fungi were kept alive. He saw many interesting plants, the good library of books on science and literature; and in the evening Miss Westerdijk invited eight of her assistants and special students to meet her American guests. Dr. Quanjer was away from Wageningen that week. When reached, he promised to be at Utrecht the next day. Miss Wilbrink, discoverer of the hot water treatment against Sereh disease of sugar cane, had recently returned from Java and Smith, "pleased with her."¹²⁶ learned of her efforts "to infect sound cane . . . to find the parasite."

At four o'clock the next afternoon, Dr. Smith addressed fifty students in the University of Utrecht botanical laboratory "on crown gall and its relation to other galls and tumors. [H]e used English, and strange to tell," he said,¹²⁷ "all understood me. Students here are taught to use three languages. All can use at will English or German and some also know French. They seemed very much interested and at the close gave me three tremendous rounds of applause."

Professor Quanjer and two other scientists from Wageningen were in the audience. Smith enjoyed talking with them, and with a professor of plant physiology from the Imperial University at Sendai, Japan, who also heard the lecture. Dr. Went took them around the "new and commodious laboratory," and invited Dr. Smith to attend with him a meeting of the Dutch Academy of Science. He did not accept this invitation, however, since on that day at Amsterdam he planned to re-study "Dr. Deelman's slides of early stages of tar cancer in the skin of mice."

Dr. and Mrs. Smith returned to Amsterdam and he became more convinced than ever that Deelman was "right beyond doubt. The precancerous stage," he wrote in his journal,¹²⁸ "is cell-hypertrophy followed by cancerous invasion. The hypertrophy gradually

¹²⁶ Journal, Mar. 26, 1925.

¹²⁷ Journal, Mar. 27, 1925.

¹²⁸ Journal, Mar. 28, 1925. Also, Mar. 24.

extends farther and farther into the skin in all directions and new invasions follow. His sections show all this very clearly." The "very early stages" meant "before the muscles have been invaded."

One of their last days in Holland was spent at the International Flower Show at Heemstede and Lisse. They were invited to this event by Dr. E. van Slogteren who met them at Haarlem and drove them to the exposition grounds. There, among the important exhibits on plant pathology, Smith saw demonstrated the "marvellous effect of [Dr. van Slogteren's] hot water treatment in (1) killing eelworms and (2) stimulating growth. By this treatment, at small cost, the Narcissus [could] be made free from its worst parasite."¹²⁹ This former student of Dr. Moll at Groningen, who had been offered Moll's place, had tried out the method against other plant diseases, among them, the still serious yellows disease of hyacinths. He took Smith to his laboratory at Lisse and told him "he was not able to control the yellows disease of hyacinths by direct heat, but accomplished the same thing indirectly, *i. e.* by exposing the bulbs for a considerable time at 33°C. This temperature greatly favors the growth of the bacteria in the diseased bulbs and two months later at planting time he can then easily distinguish and throw out the sick bulbs."

One more week in Holland would have satisfied Dr. Smith since he had wanted to see "Deelman, Beyerinck, Moll, and Wakker." But to study the English work with the thoroughness he wished, he and Mrs. Smith had to leave Amsterdam on March 30. His work was known of now the world over. Scholars from South America, South Africa, British India, the Philippine and Hawaiian Islands, Japan, Russia, the Balkan states, Austria, Hungary, Scandinavia, and other world regions, had visited his laboratory. Many had applied to study with him; and many were his correspondents in all quarters of the globe. In 1925 he was made a "membre honoraire" of the mycological section of the Russian Botanical Society; and his "splendid work in the study of Bacterial diseases of plants" and his rank as an authority on this subject were regarded as "incontestable."¹³⁰ His influence on the plant scientific research work of Japan was especially pronounced.

¹²⁹ Journal, Mar. 29, 1925.

¹³⁰ In 1921 Dr. N. I. Vavilov and Dr. A. Jaczewski, Russian plant scientists, had been entertained at a botanists' dinner in Washington and by Smith in his home.

Recognitions from scientists of the British Isles had not been few. On February 28, 1921, Director A. E. Boycott of the department of pathology of the medical school of the University of London, while visiting leading schools in the United States, had called on Smith at his laboratory and been given photomicrographs of the tobacco cortex crown gall tumor. He was later sent slides and, thanking Smith, "frankly confess[ed] that a sight of the actual specimens altogether changed my ideas on the subject." Brierley of the Institute of Plant Pathology of the Rothamsted Experiment Station at Harpenden wrote: "The hard convincing evidence of your wonderful series of crown gall specimens made a great impression upon [Boycott] and he said to me what I have often said to others that he 'had never seen so magnificent a demonstration in his life.'" The next year, on September 2, Brierley, as editor for botany of the *Annals of Applied Biology*, asked Smith to prepare the first of a series of articles by leading scientists in the fields of economic botany — Johannsen, Nilsson Ehle, Berlese, Chevalier, among them — summaries of investigations with special biological applications, such as Smith's work on crown gall. Smith agreed to submit a 5,000 word article. But evidently it proved to be more than was wanted and, when the second request arrived, he was unable to promise a revision in the near future.

Late in 1922, Dr. George Adami, now vice chancellor of the University of Liverpool, had written:

Ever since I have been in England I have been crying up your work on crown gall, and people here are beginning to realize its huge importance. I confess it has been a rather interesting demonstration to me of the difficulty there is in one country in getting sound work in another country acknowledged unless a man comes over to the other country and actually demonstrates his methods to the assembled men of science.

On April 21, 1923, Sir Harold J. Stiles of Edinburgh, accompanied by Drs. Finney and Miller of Baltimore, made a laboratory call on Smith, and soon Dr. Harvey Cushing wrote to say that the noted Scotsman had written him "most enthusiastically in regard to the visit." In January of that year Sir Clifford Allbutt, Regius Professor of Physic at Cambridge, had thanked Smith for his "three

Jaczewski of the Institut Jaczewski de Mycologie et Pathologie végétale wrote Smith and told him of his new honor. They collaborated in studying the black chaff disease of wheat. *Science* 63(1629): 305-307, 1926.

most interesting and important research records. You may be aware," he wrote, "that for forty years I have been preaching the need of Comparative Pathology," and he requested that Smith send him more of his reprints. German Sims Woodhead of the pathological laboratory at Cambridge also had been interested in Smith's work, especially his study of "Production of tumors in the absence of parasites" (1920). "You have taken up an interesting subject," he wrote, "one that has very wide bearing."

Dr. and Mrs. Smith arrived in London on the evening of March 31, and soon he visited the Middlesex Hospital cancer laboratories. There he met several of the doctors, and with one, Dr. Helen Chambers, a cancer research specialist, he discussed questions of parasitism and immunity by x-ray, showed her his Blumenthal rat-cancer slides, and she showed him "some of her rats inoculated with Jensen's rat sarcoma." In sixty per cent of human breast cancer cases, the parasite Demodex, he learned, was found associated with the disease, and an appointment was arranged the next day with "Surgeon Handley of breast cancer fame." Eight of the doctors and surgeons saw his Blumenthal rat specimens. Unable to locate Dr. Sambon at the Tropical School of Medicine, he went on to officials and members of the famous staff of the Imperial Cancer Research Fund. He wanted to find its director, Dr. James Alexander Murray, but, he being out of the city, Smith consulted with Dr. Findlay who showed him about the laboratories and arranged an appointment with Dr. Murray for April 15. Dr. Findlay gave him a copy of the Fund's 1923 report, and Smith learned that he had "a note in *The Lancet* (April 4) reporting three cases of skin cancer in mice from a single application of hot tar (70°C)." Dr. Findlay then gave Dr. Smith some important information: that "one of their research men (at a substation) has found the virus of Rous's chicken sarcoma. This man's name [was] Dr. Gye."¹³¹

Dr. Smith was not to meet Dr. Gye until after his conference with Dr. Murray. Soon after their arrival in London, he and Mrs. Smith had had dinner with Dr. and Mrs. Brierley, and on April 6 they went to Harpenden to enjoy two days at the Rothamsted Experiment Station. At almost every point of their journey, they had been entertained with teas and dinners, and this was no

¹³¹ Journal, Apr. 2, 3, 4, 1925.

exception. The first day the Brierleys gave a tea and a dinner in their honor, and the next evening they had dinner at the home of Sir John Russell, the director. On one occasion Dr. Smith spoke to a company of about twenty men and women. They stayed at the home of Dr. Cutler, a Cambridge University protozoologist who was studying the micro-fauna and micro-flora of the region.

In the morning Smith, Brierley, and a young student from Geneva, Switzerland, strolled around the experiment station grounds and the village. Along with the hundreds of acres under cultivation, Smith saw the manor house of Sir John Lawes who had given the land on which the great English institution had been established. Leading to the manor house was a beautiful avenue of lime trees, and they passed through woodland to the famous wheat fields which, though cultivated for eighty years or more, were still yielding good harvests. Brierley and he talked some of plant diseases, among the topics being a canker disease prevalent in England and the results of preventive experiments against potato wart.

In the afternoon Smith visited the laboratories and saw several things of greater or less definite interest: laboratory cultures of soil algae and soil protozoa; a method of staining bacteria in soil without staining the humus compounds, "Winogradsky's new method"; slides of soil showing a stained coccus form of the nitrogen-fixing organism which was believed to change developmentally (or culturally) into other forms; a simple and rapid device for testing lethal doses of various insecticides; and, among still other points, he learned of ¹³² "Colpodium colpoda . . . a single celled mononeucleate organism" which he believed "might be good for some crown-gall experiments." Dr. Cutler had experimented with these organisms which "feed on bacteria in the soil [and] accustomed them to take their carbon from glycerine and their nitrogen from ammonium sulphate."

On April 8 Dr. and Mrs. Smith returned to London to attend another dinner given in his honor by Dr. Brierley, this time at the Criterion Restaurant in Piccadilly Circus. Those present were Dr. E. J. Butler, English pathologist; Dr. J. Ramsbottom, mycologist at the Kew Gardens; Dr. F. T. Brooks, plant pathologist of Cambridge University; Fernand Chodat, the young botanist from Switzerland; Dr. Ernest S. Salmon, Smith, and Brierley.

¹³² Journal, Apr. 7, 1925.

Dr. Salmon was a mycologist at the University of London, South-Eastern Agricultural College, located at Wye, Kent. H. Wormald from there, working in 1921 on crown gall of apple stocks, mulberry blight, and soft rot organisms, had corresponded with Smith. He and Mrs. Smith could not go to Wye until April 11 because for April 9 Sir John Russell had arranged that they be admitted to a session of the House of Commons. They went two days later, however, and, under the guidance of Dr. Salmon, saw the college and his work on hop and apple diseases. He showed them various experiments: grafted hops for mosaic studies; fungicidal treatments for apple scab; a new hop mildew; and sections of *Venturia perithecia* "attacked by some Protozoan-like parasite."¹³³

The next day was Easter Sunday. So they visited Canterbury and attended cathedral services and, after another day, during which time they saw a hospital founded by John Smith in 1657, they journeyed back to London. On April 15 Dr. Smith went to his appointment with Dr. Murray.

At the Imperial Cancer Research Laboratories Dr. Murray very carefully examined the slides from Dr. Blumenthal's laboratory, and Smith quoted¹³⁴ him as saying, "I should be greatly disturbed if they were mine.' This because the tumor is so variable from rat to rat. He said it looked like an infection there are so many polymorphs in it." He introduced Dr. Smith to Sir George Lenthal Cheatle, a noted surgeon and doctor who became so interested in his crown gall work that the following September while on a journey in the United States he visited Smith's laboratory and spent a whole day studying specimens.

Dr. Murray then arranged for the American visitor to go ten miles northwest of London to meet Dr. W. E. Gye at his substation laboratory, and in him Smith found the "most interesting" man he had met in London. Of his work Smith wrote:¹³⁵

He has been working on Cancer for fifteen years and on Rous' chicken sarcoma for the last two years, and has obtained wonderful results but has published nothing as yet. He has established two things: 1. That the filterable virus of the chicken sarcoma is particulate. This he has done by centrifuging it and showing by inoculations that the precipitate is infectious and that the supernatant fluid has lost all power to infect. The centrifuge must

¹³³ Journal, Apr. 11, 1925.

¹³⁴ Journal, Apr. 15, 1925.

¹³⁵ Journal, Apr. 15, 1925.

be run at 9,000 revolutions a minute and the fluid must be pH 6.5. It cannot be done if the fluid is 7.4 pH. 2. He has shown that the virus multiplies and must therefore be a living thing. This he has done by taking 0.02cc of the filtered virus and putting it into 5cc of his culture fluid. After three days from the first tube he transfers 0.02cc to a second 5cc. After another three days 0.02cc from the second tube is transferred to a third tube of 5cc and so on for seven transfers, the last of which is infections. This is Pasteur's dilution method and it leaves no doubt that something has grown. He has obtained about forty cases of the chicken sarcoma in this way, but what disturbs him is that only about 50% of his inoculations take, whereas the filtered virus derived directly from the chicken tumor will infect *every time*. Dr. Gye finds also that when his transfers become contaminated with cocci and other organisms they cease to be virulent. He formerly believed malignant tumors non-parasitic but now he believes very firmly that they are all or most of them of parasitic origin.

Dr. Smith learned that Dr. Gye had "not been able to demonstrate any organism [in vitro] by stains. . . . His tubes," he said, "do not cloud even faintly unless they are contaminated."¹³⁶

Dr. Gye examined the Blumenthal rat-tumor slides and said he thought "they were infections of some sort." In Amsterdam, Smith had read in *Surgery, Gynecology and Obstetrics*¹³⁷ a paper on "The Experimental Production of Metastasizing Carcinoma in the Breast of the Dog and primary Epithelioma in Man by repeated Inoculations of a micrococcus isolated from human breast cancer." They discussed this subject, and Smith left the laboratory to return for a second visit two days later.

On April 16 he went to the cancer hospital at Kensington to consult another doctor but, not finding him, he spent some time with Dr. Cheatle and Dr. Murray. On April 17 he went to Rhodes Farm, Mill Hill, and "saw Dr. Gye inoculate chickens with the Rous sarcoma virus." Smith's memorandum read:

He put into some, also, virus treated with chloroform for two hours which he says destroys the virus. Before injecting it he put the tube under an air pump and carefully extracted all the chloroform, otherwise the chickens would die. I suggested to him that his 50% of failures with the cultures, must be due to the fact that he has attenuated his virus (by excessive heat or otherwise) and consequently only the weaker chickens are infected. In that case the others should be *vaccinated*, and resist the fresh virus. Evidently he is aware of this interesting possibility and has not told me all.

¹³⁶ See, Some newer aspects of cancer research, *op. cit.*, 599; Recent cancer research, *op. cit.*, 251-253.

¹³⁷ Mar. 1925, 343-352.

Dr. Smith believed him "a very clever and a very interesting man, on the eve of great discoveries."¹³⁸ He was "a well-balanced, industrious and quiet man thirty-five or forty years old, with an excellent training for the work he [was] doing. He impressed [Smith] as a keen critic, cautious and well aware of the pitfalls on every hand, and more anxious to be right than to be famous."

Dr. Smith regarded his three conferences with Dr. Gye as "as interesting as any [he had had] anywhere in Europe." The third took place on April 18 at the National Institute for Medical Research located on Windmill Hill near London. There, Dr. Gye "introduced [him] to his colleague, the physicist and optical expert, J. Edwin Barnard, who [had] devised very clever new apparatus for seeing the ultramicroscopic filterable viruses. He examines them," Smith wrote,¹³⁹ "by monochromatic light through a green screen using a mercury lamp. This screen cuts out practically all the lines except the green and a mere trace of the orange. With this he uses an objective of 1.3 N. A. and eye piece which magnifies 1800 diameters. By this means he can see down to 0.1μ ."

Barnard had "been working on the problem of seeing and photographing the organism causing Rous's chicken sarcoma for two years" and Smith believed that he had "succeeded in both. I saw it under the microscope," he said, "and I saw photomicrographs of it. He first showed me the pleuro-pneumonia germ, fastened to a thin film of gelatin and afterwards the living organism bobbing about in a rabbit serum culture." Smith described and drew various developmental "stages" of the organism which he saw. He saw also "a four day, fifth transfer" of the Rous sarcoma organism. He "examined the pleuropneumonia culture for a long time and [was] satisfied it is a living thing. . . . If this filterable virus is an organism, then," he concluded, "I don't see how one can refuse the same name to the sarcoma virus."¹⁴⁰

He added:¹⁴¹ "I got more out of Dr. Gye today. He is trying all sorts of incubator temperatures on his virus cultures from 35°C to 42°C with the idea of attenuation and vaccination."

¹³⁸ Journal, Apr. 17, 1925; Recent cancer research, *op. cit.*, 252.

¹³⁹ Journal, Apr. 18; Some newer asp. of canc. res., *op. cit.*, 599.

¹⁴⁰ *Idem*; also, Fifty years of pathology, *op. cit.*, 45.

¹⁴¹ Journal, Apr. 18; Recent cancer research, *op. cit.*, 252.

Whether a vaccine had been or would be discovered awaited more definite completion of the studies.¹⁴² But at this time Smith commented, "What a wonderful discovery it would be!"

On April 22 he and Mrs. Smith sailed on the U. S. Line ship, "President Harding," and arrived in New York eight days later.

On May 3 and 4, the eighteenth annual meeting of the American Association for Cancer Research convened at Washington. At its council meeting Dr. Channing C. Simmons, vice president during Dr. Smith's regime, was elected the organization's new president. Dr. Burton T. Simpson, director of the New York State Institute for the Study of Malignant Disease, was elected vice president, and Dr. Woglom was continued in the office of secretary treasurer. At the Navy Medical School, morning and afternoon sessions were held, and in the presence of about forty members Dr. Smith presented his address, "Some Newer Aspects of Cancer Research." He wrote of the meeting:¹⁴³ "I exhibited Blumenthal's rats and slides. Drs. Ewing, Wood and Woglom examined them microscopically. . . . At the meeting Dr. Cori of [the New York State Institute for the Study of Malignant Disease] read a paper¹⁴⁴ stating that he had verified Otto Warburg's excess of acid in cancer cells, using man and *living* animals; and Dr. Warthin continued his study of a cancerous family." Smith tabulated some of Dr. Warthin's conclusions. He knew that his address had been well received. Members asked for cultures and slides to study the Blumenthal work further.

Dr. and Mrs. Simpson and Dr. and Mrs. Shrader of the Gratwick Laboratory called on Smith and saw his laboratory and the rat-tumor slides. Soon Miss Fox of his laboratory was cutting one rat specimen brought from Berlin, the "one of the big axillary metastases in *PM VIII* cell transplant."¹⁴⁵ By August he had received a "letter from Dr. Blumenthal"¹⁴⁶ which told that "another rat tumor now in 4th transplant, structurally like *L*, [had been obtained] with a bacillus cultivated from a cancer of the uterus plus kieselgur," and by early the next year,¹⁴⁷ after

¹⁴² Some newer asp. of canc. res., *op. cit.*, 599.

¹⁴³ Diary, May 4, 1925.

¹⁴⁴ Drs. Carl F. and Gerty Cori, The carbohydrate metabolism of malignant tumors, *Jour. Cancer Research* 9: 510 f., 1925.

¹⁴⁵ Diary, May 14, 1925.

¹⁴⁶ Diary, Aug. 7, 1925.

¹⁴⁷ Mar. 10, 1926. Letter, Feb. 9, 1926.

receiving another letter and reprint from him, Smith was writing in his diary, "They have obtained the bacillus *PM* from another human cancer." On August 20, 1926,¹⁴⁸ before the International Congress of Plant Sciences, Smith would say of this work:

Blumenthal and his associates in Berlin have . . . isolated what appear to be strains of [*Bacterium tumefaciens*] from human tumors and with them have produced tumors in plants and malignant tumors in rats. Very recently, Dr. Fritz Kaufmann, of Koch's Institute for Infectious Diseases in Berlin, has isolated an organism, said by him to belong to the *B. tumefaciens* group, from a carcinoma of the mouse (Ehrlich strain) and with it has produced tumors in plants.

In this address, Dr. Smith admitted that the "bulk of medical opinion at present may be said to favor the view that malignant tumors in man are due to a variety of long-continued local irritations rather than to any specific parasites. Favoring this view, which I do not share," he continued, "is the fact that cancers may be produced in experimental animals by tar-painting, by extracts of soot, and by other chemical substances applied repeatedly."¹⁴⁹

In July 1925, a year before, almost to the month, he had ordered a copy of the London *Lancet*¹⁵⁰ and, after studying the substance of a recent paper by Drs. Gye and Barnard, exclaimed in his diary:¹⁵¹ "It marks even a greater advance forward than I expected. [Gye] says there are two factors concerned in tumor production (1) the living virus which is non-specific and (2) a chemical stimulant derived from the tumor tissue which is narrowly specific." Late that year Dr. Smith was invited to participate in a symposium on the Cancer Problem, held in conjunction with the meeting of the American Society of Zoologists. He could not be present. But he sent Dr. Stockard his address, "Recent Cancer Research,"¹⁵² and a telegram the substance of which became its final paragraph. In November, Dr. Murray's Twenty-third Annual Report of the Imperial Cancer Research Fund had been published, and Dr. Smith quoted an excerpt which began:¹⁵³

The essence of Dr. Gye's conception is that malignant growth results

¹⁴⁸ Fifty years of pathology, *op. cit.*, 45.

¹⁴⁹ *Idem.*

¹⁵⁰ July 18, 1925.

¹⁵¹ Diary, July 30, 1925.

¹⁵² *Amer. Naturalist* 60: 240 f., May-June 1926.

¹⁵³ *Idem*, 254.

from the concurrence of two factors—an ultra microscopic microbe and an unstable chemical factor derived in his experiments from propagated malignant tumours of animals, and it would be erroneous to regard either of these factors, singly, as the cause of cancer. The direct evidence of this dual origin of new growths has so far only been furnished for the Rous fowl sarcoma and for a transplantable sarcoma of the mouse. For other tumours the evidence is indirect, cultures of these supplying the ultra-microscopic microbe, the specific unstable chemical factor being supplied by an extract from the Rous fowl sarcoma, in which species of animal the two injected simultaneously gave rise to progressively growing sarcoma. Injected singly, they are inert. The delicate racial and tissue specificity governing the transmission of malignant new growths therefore attaches to the labile chemical factor, and not to the microbe. One of the gravest objections to previous forms of the parasitic hypothesis of cancer is thereby met.

On December 29, Dr. Gye wrote Smith expressing confidence that the defenders of the "cell theory" would "in the very near future . . . find it hard to hold their ground." He confided that he had "a good deal of information on the subject of immunisation against new growths" but the materials were "only partial" and, requiring a "hundred and one qualifications," too difficult to put in a letter. His next letter¹⁵⁴ told of their "new laboratories" which, though small, were "as near perfection as one can ever hope for in this world. Barnard and his assistants," he wrote,

are on the ground floor; I have, with one colleague, the upper floor. It has taken several months to settle down, to arrange work and so on but now we are in full swing. Barnard of course is doing the microscopy and happily has already made many improvements. . . . The experimental work is of course in my hands and I am glad to say that we have pushed along pretty satisfactorily. I have repeated my published work with newly devised technique and I have nothing to withdraw or recant. If I had of course I should do it at once. I am now more satisfied than I was that a filterable virus is the true cause of the fundamental character of animal tumours. The parasitic theory has been a little battered lately but that is in the natural order of things. Scientific orthodoxy is against it and of course with reason. But then in science the orthodoxy of one decade is based on the best available evidence of the previous decade. And fortunately so or there would be no stability. I am pretty confident that with the better methods now devised there will be less difficulty in repeating my results and I am optimistic enough to believe that theory will change slowly. In any case let truth prevail.

¹⁵⁴ Nov. 7, 1926, Medical Research Council, National Institute for Medical Research, Farm Laboratories, Mill Hill.

Dr. Gye is today the director of the Imperial Cancer Research Fund. In a recent lecture, "The Propagation of Mouse Tumours by means of Dried Tissue,"¹⁵⁵ given before the Royal College of Surgeons on March 22, 1949, he has described three sarcomas propagated in mice with the use of a complicated technical procedure. His conclusion is still that experimental evidence points to cancer having a "continuing cause, and that this, in mammals as in birds, is a virus. . . . At present the only conclusion from the whole work which is of vital and abiding interest," Dr. Gye says, is that it is no longer justifiable to put the normally easily filterable tumours of chickens in a separate class and turn a rather scornful eye on them. This work has now brought tumours of mice—average tumours found in all well-equipped cancer laboratories—into the same class. They have, as was to be expected, a continuing cause, and, since the continuing cause of chicken sarcoma is a virus, probably it is viral in nature.

In his letter to Dr. Smith of November 1926 he had said:

The technique of filtration is of course one of the most obscure and difficult jobs in filterable virus work. . . . We are using collodion filters which can be graded with some approach to precision. And the results are now being standardised *if it is possible* against the "depositing power" of centrifugalisation. For the purposes of centrifugalisation we have now a better machine—a real engineering job—which will spin 15,000 rev[olu-]tion[s] a min[ute], record the speed and enable us to repeat with accuracy any given experiment.

His address of more than two decades later evaluates much of the modern knowledge of Dr. Rous's chicken sarcoma. "The agent," he said,

which multiplies with the growth of the tumour is now recognized as a virus. Electron-microscope photographs have been taken, and the sizes revealed by such photographs are in fair harmony with sizes deduced from experiments with collodion filters. The virus is more or less spherical and occurs in clumps in the tumour cells. Now we know of several hundred filterable tumours of the domestic hen. They are of different structures: some, but very few, are simple spindle-cell tumours resembling the Rous sarcoma; others include chondromas, osteochondromas, fibrosarcomas, myxosarcomas, muscle-cell tumours, and so on. . . . [I]t has universally been found that the inoculated virus can start a tumour only of the kind from which it is derived. . . . The virus is so specialized in its effects that it represents in itself all the qualities and properties of the cell from which it is derived. Nevertheless, an immune serum made by injecting virus

¹⁵⁵ (With colleagues), *British Med. Jour.*, 511-515, Mar. 26, 1949.

from one tumour will neutralize the viruses from other types of tumour; which indicates a common antigenic element.

Chemical, physical, or parasitic causes have usually been described by Dr. Gye as "remote or adjuvant." But these "lead to cancer and the intracellular continuing cause. This continuing cause is related to the pathological nature of cancer and is, in the sense in which we say that the tubercle bacillus is the cause of tuberculosis, the cause of cancer."

In September 1926 Dr. Smith attended the International Symposium on Cancer held at Lake Mohonk, New York, Mountain House under the auspices of the American Society for the Control of Cancer. He was not a member of this organization and not on the program of speakers. But, on the morning of September 23, his extemporaneous remarks "on the question of parasitism in cancer" stirred up "a lively discussion."¹⁵⁶ With him he had his crown gall photographs, and, in his room, he showed them to many important men of the conference: Dr. Henri Hartmann and Dr. Gustav Roussy of France, the first of whom he had met while in Paris; Dr. Raffaele Bastianelli, associate professor of surgery at the University of Rome and vice president of the Italian League for the Control of Cancer; Sir John Bland-Sutton, president of the Royal College of Surgeons of England; and many other European and American scientists. At the conference Dr. Blumenthal told him of the Kaufmann work at Koch's Institute. On *Pelargonium* and sunflowers five large tumors had been produced with bacteria (resembling *Bacterium tumefaciens*) from mouse cancer (Ehrlich strain). He told Smith also of other workers who claimed to have obtained one plant tumor on a *Pelargonium* with "a yellow schizomycete growing only at 30°."¹⁵⁷ Dr. Blumenthal was later Smith's guest in Washington and spent a morning in his laboratory.¹⁵⁸ As the concluding event of the symposium, the foreign guests were honored with an elaborate dinner at the Hotel Astor in New York City. Dr. Smith attended this occasion, and also a cancer research program held at Johns Hopkins Hospital on September 27. He returned the next day to his laboratory and set out his crown gall material for any guests who might visit his

¹⁵⁶ Diary, Sept. 23, 1926. See *Cancer Control*, 179, Chicago, Surgical Publishing Co., 1927.

¹⁵⁷ Diary, Sept. 20, 1926.

¹⁵⁸ Diary, Sept. 25-26, 1926.

display. Among those who arrived were "the Dutch cancer specialists, DeVries and Deelman. They spent about three hours looking over crown gall material, specimens and slides."¹⁵⁹

On October 1 he applied for an additional \$40,000 research appropriation for his laboratory during the approaching year. For the crown gall work he listed four problems which he purposed to have studied:

1. Can tumors be produced in cold-blooded animals with *Bacterium tumefaciens*? . . .
2. Can the process of tumor formation be observed under the microscope by introduction of *Bacterium tumefaciens* into a single cell by means of Chambers' apparatus?
3. Can the strains of the crown gall organism we now possess be gradually adapted to growth at blood temperature and thereby made to produce tumors in rats and mice? Can strains of *Bacterium tumefaciens* be found in nature which naturally grow at 38°C., at temperature of man's blood?

The appropriation was to be divided equally between four branches of investigation: crown gall; bacterial soft rots of potatoes, field and laboratory studies; bacterial diseases of wheat in the middle west beyond the Mississippi River, field and laboratory studies; and bacterial diseases of beans and related legumes, and bacterial diseases of tobacco, field and laboratory studies from Massachusetts to Florida.¹⁶⁰

In March, Dr. B. M. Duggar, chairman of the executive committee of the fourth International Botanical Congress, had called on Smith and told him that he had been selected to give one of two main addresses for the sessions of the congress meeting as a whole. "This is a great honor," he replied. "I am hardly equal to it or worthy of it."¹⁶¹ Dr. Went was the European chosen to speak on the evening of August 18 and Dr. Smith's address was scheduled for August 20. He took for his topic, "Fifty Years of Pathology," and, during its preparation, he found that to summarize a half-century of pathology and "make it interesting [was] no easy task."¹⁶² More than once, while writing, re-writing, and condensing his address, he was unsure that he would succeed. The International Congress of Plant Sciences, as the congress became known, was held at Cornell University during the week of

¹⁵⁹ Diary, Sept. 28, 29, 1926.

¹⁶⁰ Letter addressed to Dr. W. A. Taylor, chief of the Bureau of Plant Industry.

¹⁶¹ Diary, Mar. 29, 1926.

¹⁶² Diary, Aug. 5, 1926.

August 16 of that year. There were between eight and nine hundred registrants, and, of these, fully one-eighth were foreign delegates. Dr. Smith found the occasion "the most delightful scientific gathering [he had] ever attended."¹⁶³ His great friend, Dr. Liberty Hyde Bailey, was president and presiding chairman of the congress. He introduced Smith on the night he gave his address to an audience of about three hundred persons. The address, well received, required two hours in presentation and was illustrated by forty-five lantern slides.

Smith one day attended a luncheon of the nine members present of the National Academy of Sciences. He spoke on "Dr. Jones, the man," at a dinner given in honor of Professor L. R. Jones and at which the honored guest's portrait was presented to the University of Wisconsin and a set of twenty-nine bound volumes of the collected works of his students given him. Among several other important events was a luncheon, which Smith also attended, honoring twenty-two foreign botanists at the congress. He believed that "great credit" for the congress' effectiveness should go to Dr. Whetzel, chairman of the committee on local arrangements, and he visited his laboratories of plant pathology on the last day. After the congress, Dr. and Mrs. Smith took a short journey through points in New York State to Toronto where he enjoyed again visiting with Professor Leslie C. Coleman who was teaching at his alma mater after having returned from Mysore, India, where he had been director of agriculture.

At the congress, Dr. Smith became acquainted with Professor B. Nemeč from Czechoslovakia who showed him "very beautiful stained slides of root-nodules of Legumes and leaf teeth of *Ardisia*" and who claimed that he had "stained the bacteria in crown gall [which] like *B. radicum* are both between the cells and inside of them." While in London he had been told of a method of staining, used by Dr. Murray, which might be applied for crown gall bacteria.¹⁶⁴ He wrote down Professor Nemeč's formula.¹⁶⁵ The papers on pathology at the congress which appear to have interested him the most were on "Filterable virus diseases,

¹⁶³ Diary, Aug. 19, 1926.

¹⁶⁴ Journal, Apr. 17, 1925.

¹⁶⁵ Diary, Aug. 21, 1926. Nemeč had read a paper before section C, Cytology, on The mechanism of mitotic division. Another paper which Smith marked for hearing at this section meeting was Dr. Harper's, The structure and functions of plastids, particularly elaioplasts.

yellows, mosaic and related diseases": three "very instructive papers from different angles on Mosaic diseases by B. M. Duggar, [H.] Klebahn, and Kunkel." He quoted Duggar as saying that "poke weed juice in sufficient amount 'kills' tobacco mosaic virus." A full half-page memorandum from Dr. Kunkel's "Studies of the aster yellows disease" read:

Aster yellows cannot be given to Rosaceae but it is inoculable into plants of many other families, can be budded like peach yellows, dwarfs the plant and causes it to send out numerous chlorotic shoots of erect habit, and often causes the flowers to become green. It is not transmitted by the juice, but by a leaf hopper. . . . One day's feeding on a diseased plant is enough to infect the hopper but he cannot transmit the disease to healthy plants until the virus passes through an incubation period in his body the shortest time for which is ten days.¹⁶⁶

More than this, while examining an exhibit of "Opuntias inoculated with an exceedingly destructive bacterial soft rot obtained in Bermuda," Smith saw "various mosaic preparations including those of Ray Nelson in squash petiole where were *numerous bodies* stained with iron haematoxylin and looking like protozoans." He saw also G. P. Clinton's "amoeba-like bodies in living trichomes of mosaic tobacco and . . . also in many other cells. Clinton," he said, "can get mosaic, he claims, from dried mosaic tobacco twenty years old (done by finger rubbing)," and he noticed that the Connecticut plant pathologist's "stain 1% iodine green in 1% acetic acid differentiates the amoeboid bodies from the nucleus." He examined also Miss Eckerson's "stained smear preparations of tomato mosaic," and Miss Goldstein's "striking slides of tobacco mosaic stained with 1% iodine green in 1% acetic acid. The amoeboid bodies pink in contrast to the blue nucleus."¹⁶⁷

These exhibits were seen on the day before he gave his address, "Fifty Years of Pathology." Into this he had written several paragraphs on the current study of plant viruses and protozoa. He knew something of Duggar's work. At the spring meeting of the National Academy of Sciences, Smith had introduced him when he read a paper on "Indications respecting the colloidal behavior of the agency of virus inducing mosaic disease of tobacco."¹⁶⁸ By

¹⁶⁶ Diary, Aug. 18, 1926.

¹⁶⁷ Diary, Aug. 19, 1926.

¹⁶⁸ Diary, Apr. 27, 1926; and program of the meeting. At this meeting Smith also presented his paper, Changes of structure due to a modified environment: a study of labile protoplasm in *Helianthus annuus* L.

the time Smith's address was published, Kunkel's work on the aster yellows was published in the *American Journal of Botany*, Miss Goldstein's study of tobacco mosaic had appeared in the *Bulletin of the Torrey Botanical Club*, and Miss Eckerson had a paper on tomato mosaic in the *Botanical Gazette*. Smith read each, and inserted a footnote reference to those, as well as other, studies not mentioned in the main body of his address.¹⁶⁰

Toward the end of the year 1925, Dr. Smith had begun to suffer at times from insomnia. More and more he reflected on the possibility that his work might be nearing completion. "Whatever happens," he wrote, "life is good and I have had a long and interesting stretch of it. In a month I shall be 72 [years of age] and if I could sleep I should go on into the 80's, I believe. How I should like to do it. There are so many interesting problems almost within my grasp but then if I lived another 70 years the same would be true. The arcana of the Universe are endless."¹⁷⁰ On New Year's Eve, his thoughts had been retrospective: "For us," he said, "it has been an eventful and happy year. In France, in Germany, in Denmark, in Holland, in England, and then in our home making it over inside and making a garden out of the disreputable old side-hill back yard. May we have some joy in it in the new year!"

The year 1926 did bring far more than he hoped, a corresponding membership in the Academy of Natural Sciences of Philadelphia, among other honors, near its close, an event replete with lasting satisfaction took place. On December 29, at the eighteenth annual meeting of the American Phytopathological Society, a dinner of 195 covers at the Hotel Normandy in Philadelphia was given in his honor. He was presented with a leather-covered brochure, hand-engraved on parchment paper, containing 172 signatures of members of the society and visiting pathologists. Its dedicatory statement read: "To Erwin Frink Smith scientist, linguist, poet, friend, who for forty years has devoted his life's service to the broad field of pathology, in grateful appreciation we the members of the American Phytopathological Society dedicate this testimonial."

Dr. L. R. Jones spoke for the plant pathologists and he extolled Smith

¹⁶⁰ Fifty years of Pathology, *op. cit.*, 36.

¹⁷⁰ Diary, Dec. 19, 1925.

For leadership in early study of peach yellows, most stimulating example of dogged work upon a baffling problem, with prophetic assurance that knowledge of tobacco mosaic and aster yellows was pertinent to the solution; for leadership in pioneer studies of bacterial plant pathogens, with classic publications exacting models for all who followed; again with prophetic vision of the boundless extent of this field; for zealous devotion in defense of truth; for assembled contributions to knowledge of bacteria in relation to disease in plants; for epochal researches on crown gall; for sympathetic counsel to eager younger scientists, from far and near; for thus exemplifying the Pasteurian characteristics: clear vision, instant action, intuitive judgment, precise method, tireless endeavor, sympathetic patience, self-sacrificing devotion in service through science; for these things we delight to honor you: pioneer, prophet, exemplar, dean of our science.

Dr. William H. Welch spoke next to Dr. Smith. "No one in our day," he said,

has done more to bring the two great divisions of pathology into close relation to their mutual advantage. Your studies of plant tumors have brought you into the field of onkology in its broadest aspect. Here you take your place in national and international congresses and associations devoted to medical research and here your work is recognized as of the greatest interest and importance. While your name is associated especially with the championship of the parasitic theory of the origin of tumors, your studies of the mechanism of tumor formation, of problems of histogenesis, of formative stimuli and inhibitions of growth are scarcely of less importance. We too on the medical side have learned to admire you as a man inspired with the highest ideals of the searcher for truth, and devoted to this search, with the heart, the methods and the loyalty of the ideal man of science.

Dr. Rand, Miss Hedges, Miss Brown, Miss McCulloch, Miss Bryan, Miss Elliott, and a former member, Miss Ames, were present from Dr. Smith's laboratory. Dr. Rand presented "the memento."¹⁷¹ "What Robert Koch was to the early days of human and animal bacteriology," he said,

that and more have you meant to the bacteriology of plant diseases. Almost single handed you saw it through those first years of speculation and scepticism to its present broad and solid position among the sister sciences. In your scientific work and in your influence you have made an indelible impression not alone upon plant science or upon animal science, but upon the whole field of experimental biology. And what is to me most vital and reassuring, through it all you have never for a moment lost sight of the humanities nor of the beautiful things of the mind and of the world

¹⁷¹ Diary, Dec. 29, 1926.

without. May I therefore be permitted to add the personal tribute of one who for over fifteen years has worked under the inspiration of your guiding hand.

Dr. Smith's response reviewed "how [he] came to be a pathologist" and his work of forty years as "a research pathologist of the United States Department of Agriculture."¹⁷² There he was regarded as "undoubtedly the most eminent research man ever connected with the Department."¹⁷³

This was to be his last appearance at a large gathering of scientists outside of Washington. On December 17, 1925, he had addressed forty members of the Washington Academy of Sciences on "Recent Cancer Research." On October 6 of the year he had spoken before an audience of sixty persons, including Secretary of Agriculture W. M. Jardine, on "Things seen and heard in Europe." This was a meeting of the Washington Botanical Society, and on March 1, 1927, he would address the 200th meeting of this organization and review its origins and history since 1893 when it was known as the "Botanical Seminar." Other addresses were made by him: as instances, one on May 6, 1926, before the Women's Club of Rockville, Maryland, on "Modern advances in medicine and surgery," and many for the National Carillon Association. Twice he had been asked by Dr. Bloodgood to speak at important dinners or meetings of scientists in Baltimore.

During the years 1925 and 1926 many well known scientists had visited his laboratory. On September 17, 1925, Sir George Lenthal Cheatele told him of new accomplishments at the Imperial Cancer Research Fund laboratories since he had been there. Dr. van Slogteren visited this country, called on Smith at his laboratory November 21, and while here journeyed to California, spoke at the Ithaca International Congress, and lectured at the National Museum on bulb diseases and their treatment. On November 30, Dr. F. O. Bower, emeritus professor of botany at the University of Glasgow, spent an hour with Smith and was given a dozen crown gall slides and some recent publications. Earlier that month, Dr. Giulio Savastano, had arrived from Italy to study "some months" in the laboratory. In August, William Brown, plant

¹⁷² *Idem.*

¹⁷³ Letter from Dr. L. O. Howard of the Bureau of Entomology to Mrs. Erwin F. Smith, Apr. 13, 1927.

pathologist of the London Imperial University, called while on his way to the American cotton belt regions. American botanists, too many to list all, continued to visit the laboratory, some to examine scientific materials and some for consultations. During these years, a noticeable increase of correspondence with Canadian scientists took place: mostly with botanists, however, interested in the crown gall-animal tumor work.

Dr. Charles Mayo, in whose company Dr. Smith had gone to the cancer symposium at Lake Mohonk, spent an hour or two at the laboratory on January 14, 1927. So interested was he still in Smith's work that, like other doctors at other hospitals, he had sent both surgeons and medical practitioners to inspect his materials. The importance of the laboratory's work was particularly recognized in other scientific laboratories of Washington. For instance, within a little more than a month's time in 1926, three pathologists from Garfield Hospital in the company of Dr. Garner, and Professor Treat Baldwin Johnson of Yale University with Dr. William Charles White of the Hygienic Laboratory, visited Smith's laboratory. Dr. Johnson was planning to do some work in organic chemistry on crown gall. As promised, Dr. Koitchi Itchikawa on May 25, 1925, made a trip from New York to Washington for the sole purpose of examining Dr. Smith's plant tumor collections, and was given slides and specimens, and cultures later. On January 25, 1926, Jensen of the experiment station at Copenhagen, Professor Hurdon of the Rothamsted station, and Dr. B. M. Duggar, called. March 16 Miss Wilbrink arrived from Holland after having been since September in the Dutch and English West Indies, and was entertained by Dr. Smith and his laboratory assistants. Visitors from all parts of the world made a point of seeing the laboratory before returning to their native lands. At times one countryman would tell a fellow scientist of work seen. For example, from Sweden in 1922 had come Professor Barthel, director of the central agricultural stations and department of agricultural bacteriology, and three years later, on November 27, Dr. Lundegardh, pathologist interested in destructive soil parasites including *Fusariums*, spent a good part of the day with Dr. Smith. Before and after the Ithaca congress, many foreign scientists visited his laboratory, Nils Svedelius of Uppsala, student of marine algae, among them. Olof Arrhenius, son of the great

physical and bio-chemist Dr. Svante August Arrhenius, of Sweden, also consulted Smith and in 1921 hoped some five years later to work in his Laboratory of Plant Pathology "for some months." At the Ithaca congress O. Arrhenius submitted an invitation paper on "Soil acidity, plant growth, and its practical application." On November 19, 1921, after a conference with him, Smith had written in his diary: "He has been for six months in Java and Sumatra and has found that the tobacco soils on which tobacco wilt is bad *are acid soils*, which agrees very well with our findings in the United States. I sent him cultures of Bact[erium] sol[anacearum] . . .-six weeks ago . . . and he says it behaved in his tests the same as the Sumatra organism."

In April 1926 Dr. Smith received a letter from Dr. Alexis Carrel of the Rockefeller Institute for Medical Research. Dr. Carrel regretted that he was unable to be in Washington on April 27 to hear Dr. Smith read his paper, "Changes of structure due to a modified environment," before the National Academy. The subject interested both scientists, and he arrived on April 30 and they discussed tumors and examined materials.¹⁷⁴ Smith, in his address, "Recent Cancer Research,"¹⁷⁵ had twice mentioned Dr. Carrel's work.

On February 25 of that year, Dr. John W. Churchman of the laboratory of experimental therapeutics of Cornell medical school and known for his studies of selective bactericidal properties of certain aniline dyes had thanked Smith for cultures of the crown gall organism and for reprints: "I have read with much interest the reprints you sent me," he said. "Of course, I have followed closely the extremely valuable work you have done on galls and hope that I may have the pleasure of visiting your laboratory in Washington and seeing some more of your work."

Dr. Smith's laboratory remained one of plant pathology and plant bacteriology to the very last year of his life. In 1927, evidently still skeptical of the genuineness of the Blumenthal rat tumor, he sent slides or sections for neoplastic diagnosis to several specialists: Dr. MacCarty of the section of bio-pathology and diagnosis of the Mayo Clinic, Dr. Wells of the department of pathology of the University of Chicago, Dr. Lewis of the depart-

¹⁷⁴ Letter, Apr. 26, 1926; Diary, Apr. 30, 1926.

¹⁷⁵ *Op. cit.*, 247, 254.

ment of embryology of the Carnegie Institution of Washington, Dr. Burrows of the Barnard Free Skin and Cancer Hospital of St. Louis, and perhaps others. Each one denied the true neoplastic nature of the tumor. After February of that year nothing further on this subject appeared in any of Dr. Smith's writing or memoranda of his diary. Late in 1926 and early 1927, furthermore, he gave much time to studying the so-called "cancer germ" and method of treating cancers by antitoxin of Dr. T. J. Glover of New York City. Dr. William B. Coley arranged on November 16¹⁷⁶ for his first appointment with Dr. Glover who had been seven years at work on his antitoxin and had never sold it or made money with its use. Dr. Glover showed him young and old cultures of his organism, his collection of tumor-bearing animals, stained sections of various types of cancers, explained his work, and gave Smith cultures of his organism to study in his laboratory at Washington. Dr. Smith spent parts of three months studying the "schizomycete,"¹⁷⁷ and his conclusions,¹⁷⁸ some of which were suggested, found, or verified by Dr. Thom, were that the organism was not a "schizomycete," as Dr. Glover had told him he believed, but contained several organisms. Smith wrote: "I have found one 'spore sack,' that of Glover's 'cancer germ,' to be the conidia of *Penicillium brevicaulis*, a contaminating fungus. The rest of the 'germ' was made up of several types of sporulating bacteria." On March 1, 1927, Dr. Woglom acknowledged receiving from Smith a paper entitled, "Glover's 'cancer germ,'" to be read at the Rochester meeting of the American Association for Cancer Research. This paper was never presented, and for reasons soon to be disclosed.

Practically all of Dr. Smith's last publications had been based directly upon, or built around, his work in crown gall. He had continued to direct and supervise all of the work of his laboratory. Much important work had been done by his co-workers. Miss Hedges' study of the bean disease (*Bacterium flaccumfaciens*) had been extended to Europe and it was known that its cause is transmitted "from year to year on and in the seeds." In 1923-

¹⁷⁶ Diary, Nov. 16, 1926; interview with Dr. Glover, Nov. 17; interviews with other New York doctors concerning Dr. Glover's work, Nov. 18-19.

¹⁷⁷ Diary, Dec. 20, 21, 22, 23, 24, 28, 1926; work continued, Diary, Jan. 14, 1927; conference with Dr. Thom, Diary, Feb. 7, 1927.

¹⁷⁸ Fifty years of pathology, *op. cit.*, 35.

1924 Fred Reuel Jones of Wisconsin had discovered and established the etiology of a serious vascular disease of alfalfa, and in 1925 Miss McCulloch had described the cultural characters of its cause, *Aplanobacter insidiosum*.¹⁷⁹ That year¹⁸⁰ Miss Bryan had obtained "interesting root infections on tomatoes with *Aplanobacter michiganense* and also demonstrations that the disease is carried on the seeds, as [Smith had] long suspected." In 1924 Dr. Smith and Miss Elliott had presented at the meeting of the American Phytopathological Society a paper on "A bacterial disease of broomcorn and sorghum."¹⁸¹ These were but a few examples; and on March 5, 1927, he summarized the laboratory work which was going on satisfactorily: "Miss Fawcett," he wrote,¹⁸²

has interesting studies of distilled water well along. Agnes Quirk and Miss Brown are working on crown gall oxidation phenomena. Miss Brown has a note with two plates on sweet pea fasciation due to *Bact. tumefaciens* in *Phytopathology*. Miss Elliott has written a paper on stripe disease of oats. Miss Hedges is working diseases of beans. Miss Bryan on several things—one of which is *Aplanobacter* on tomato. Lucia McCulloch is working on bulk diseases. Miss Cash on miscellaneous things. Miss Fox the same. I am trying to complete the illustrations for my Ithaca paper, [Fifty Years of Pathology]. [J. F.] Brewer is helping.

For many years Brewer, botanical artist of the Department, had helped Dr. Smith valuably in preparing illustrations for publications from his laboratory, i. e., by drawings and prints for half-tone plates and other special features.

In January, Dr. Rand had begun work to "complete a bulletin on bacterial wilt of sweet corn."¹⁸³ During the first week in March a "tumor-like disease on nodes of sugar cane [had been] received," and on March 8 Dr. Smith began to examine these for protozoans or cell inclusions in the tumors. Some root tumors of lilac from France were also studied, and he believed these crown gall.¹⁸⁴ That night he suffered the first of several recurrent heart attacks and was never again the strong, virile man he had been. But

¹⁷⁹ *Idem*, 37.

¹⁸⁰ Diary, May 9, 1925.

¹⁸¹ *Phytopathology* 14(1): 48, Jan. 1924; A bacterial stripe disease of sorghum, *Jour. Agric. Res.* 38(1): 1-22, Jan. 1, 1929 (extension of paper of 1924 meeting, Amer. Phytop. Soc.).

¹⁸² Diary, Mar. 5, 1927.

¹⁸³ Diary, Jan. 3, 1927.

¹⁸⁴ Diary, Mar. 7, 8, 1927.

he went to his laboratory almost every day for the next three weeks. His comments on the work grew fewer and fewer, although on March 23 he wrote in his diary a paragraph concerning Miss Bryan's studies of the "American and European lilac blight" due to *Bacterium syringae*.

Dr. Smith's strength had given out. On March 25 he welcomed Dr. Jaroslav Peklo, professor of applied botany in the Czech Technical University, Prague, to his laboratory and gave him the work-table which Dr. Nakata had had. Dr. Peklo wanted to "get general information on bacteriology, mycology, plant breeding, etc. Very likely," wrote Smith,¹⁸⁵ "we can do little for him in our laboratory. He has studied with Pfeffer in Germany and Blackman in London." Other well qualified plant pathologists had studied in the laboratory during recent years.

Among them had been one of America's leading plant scientists today, Dr. Elvin Charles Stakman, a recent president of the American Association for the Advancement of Science. On January 26 he had called at the laboratory and furnished Dr. Smith with some conclusions about "sulfur dusting for rust of wheat." This was a subject which interested him. In his address, "Fifty Years of Pathology,"¹⁸⁶ he had spoken of the "great attempt [that had] been made in the United States to control stem rust of wheat (our worst disease)," he said, "in a dozen or more of our western states by a united effort to eradicate all the barberry bushes over a territory a thousand miles square. It is too early to know what the final results will be, but thus far they appear to be favorable. Plant breeding would seem to offer a more effective remedy. This also is being tried. Possibly dusting by airplane with colloidal sulphur is the coming remedy." Smith wrote¹⁸⁷ of what Stakman told him:

They saved twelve bushels per acre on one field. He thinks more is to be expected from resistant varieties and from barberry eradication than from dusting. If rust were equally bad all years, it would pay but there is only now and then a bad year. It costs six dollars per acre and sometimes the crop is injured and sometimes the rust is not prevented. The sulphur prevents the germination of the spores, but if they have entered the plant the mischief is done.

¹⁸⁵ Diary, Mar. 25, 1927.

¹⁸⁶ *Op. cit.*, 39.

¹⁸⁷ Diary, Jan. 26, 1927.

On March 28, Dr. Smith knew that he was "not well. In everything I do I have now to be very careful of my heart, which is deuced inconvenient," he complained.¹⁸⁸ His last day of studious activity, March 29, was spent reading all day in the Surgeon General's library. For almost a week after that day of pleasure every page of his diary was a blank. On April 6 he passed beyond the world of living forms as earth-born creatures know them. He had said: And since God "must be like His universe — Because things made reflect the maker's mind — And most of all like man, at least not worse, I think that we shall live again in Him, and nobler and diviner than shall find clear light from Him to read what now is dim." The last "of the deep secrets the universe conceals" had been unfolded. He had believed he would go to the "Divine . . . Great Love the one sole thing to satisfy a hungering world" and "become just earth and sky." He had written his own epitaph: "Be then my scroll: lies one beneath this sod to whom all nature voiced the living God."

The Reverend U. G. B. Pierce officiated at funeral services held at All Souls Unitarian Church in Washington, Dr. A. F. Woods gave the eulogy or address of appreciation. Dr. Smith's body was cremated and his ashes scattered over the waters of the "many-islanded and wide, blue sea, the winding shore" at Woods Hole, Massachusetts.

At the meeting a few days later of the American Association for Cancer Research, the place which Dr. Smith's paper was to have had on the program was "devoted to an appreciation of his character and his work."¹⁸⁹ Dr. Mortier F. Barrus, president of the American Phytopathological Society, telegraphed the "sincere sympathy of every member" and wrote that "Doctor Smith [had] long been regarded as the Dean of American plant pathologists and his death [brought] a sadness to us all." Every organization and publication with which Dr. Smith had been identified in his work, and many, many American and European scientists, prepared or sent memorials, memoirs, testimonials or tributes. Almost two dozen sketches on his life and work were published in current scientific literature of the world.¹⁹⁰ Dr. A. Besredka, on behalf of his colleagues of the Pasteur Institute, assured Mrs. Smith "that in

¹⁸⁸ Diary, Mar. 28, 1927.

¹⁸⁹ Letter, Apr. 16, 1927, from Dr. W. H. Woglom to Mrs. Smith.

¹⁹⁰ *Biog. Mem. Erwin Frink Smith 1854-1927*, Nat'l Acad. of Sci., *op. cit.*, 21: 69-70 (list compiled by F. V. Rand).

him we have lost a great savant who honored microbiology." Secretary of Agriculture Jardine wrote:

He has been so closely identified with the research work of the Department of Agriculture and so outstanding in the quality and magnitude of his personal achievements in research in his chosen field, that the Department and the country suffer with you in his departure at a time when, while long mature, he was in possession of the full intellectual power with which he was so richly endowed and which he applied so enthusiastically and effectively in his researches and so unselfishly for the general good. The record of his life and service will long continue to inspire those of his associates who remain and those who follow, to exert their full power in the pursuit of truth, and at the same time developing strong human friendships and relationships. The memory of these hundreds within and many outside the Department service share with you.

Dr. Smith's greatest monument, of course, was his scientific work. But the genius of his example is still alive in the memories of many a working scientist today. Where else, in the history of American science, may be found a scientist who, more than establishing a fame and reputation unexcelled in his chosen work, could attain to such sublimity of expression in poetry as the following sonnet on "God and the Universe"?

His will alone that rhythmic force explains,
 Which cell on cell of tiniest being shows—
 And star dust old not less than new-born rose,
 But His deep *Self* incognito remains:
 In all earth's realm, I find not Him who reigns
 And vain the search where old Antares throws
 A ruddy flame, or lone Arcturus glows.
 Alone His finger prints the clay retains.
 Man proud, some say: He dwells within the soul;
 And some: He liveth not; and mournful some:
 He *wills* but hath not yet to pity come;
 And some with me: He is our final goal,
 He hides within the deeps, withdrawn, world-vast,
 That man through search may come to Him at last.

Truly the "near and small" had foretold for him "the great and far": God had been "over all and in all" his life and work.

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