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BERNARD OGILVIE DODGE

1872—1960

A Biographical Memoir by

WILLIAM J. ROBBINS

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B. O. Dodge

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April 18, 1872–August 9, 1960

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BERNARD OGILVIE DODGE was born April 18, 1872 on his father's farm near Mauston, Wisconsin. He died at the age of eighty-eight on August 9, 1960 in St. Luke's Hospital, New York City.

His ancestors[1] were old New England stock, mainly of English origin. They were hard-working farmers, blacksmiths, tanners, wheelwrights, who paid their debts, avoided law suits, had enough means to insure for themselves and their families a decent burial and to pass on to their heirs something material, as well as a good name. There were no plutocrats and no paupers—no great scholars, divines, physicians, or lawyers.

His paternal grandfather, Levi Dodge, was descended from Richard Dodge, who came from Somersetshire, England, to Salem, Massachusetts, in 1638. The Dodges lived in Massachusetts until they moved to New Hampshire in 1799 and to Vermont in 1833. Dr. Dodge's father settled in Wisconsin in 1856. It is worth recording that there were in Dr. Dodge's direct line five successive Richard Dodges, and that his great-great-grandfather (Richard Dodge No. 5) and five brothers were privates in the American Revolutionary Army.

His paternal grandmother, Olive (Blanchard) Dodge, was the granddaughter of John Blanchard, born in Connecticut in 1737. The ancestry of John Blanchard is uncertain; he may have been descended from Thomas Blanchard, William Blanchard, or the widow Blancher

[1] Most of this family history is based on an unpublished manuscript by Dr. Dodge entitled "My Four Grandparents, Their Forebears and Descendants."

[Blanchard], all of whom were in Massachusetts between 1637 and 1639. It is said that Olive (Blanchard) Dodge was accustomed in her old age to sit on her porch, rocking back and forth in time with the lively tunes she whistled with considerable skill, although she had long since lost all her teeth. Dr. Dodge, too, was an inveterate whistler, a habit he was inclined to assign to the genes from his grandmother, who died when he was two years old.

Dr. Dodge's maternal grandmother, Mary Ann (Rogers) Nourse, was descended from Robert Rogers who came from England to The Massachusetts Bay Colony in 1634. The Rogers family remained in Massachusetts for five generations until Mary Ann's father, Micajah Rogers, moved to New Hampshire in the 1790's and from there to Vermont. Mary Ann and her husband, William Nourse, went to Wisconsin in 1856.

Dr. Dodge's maternal grandfather, William Nourse, was a lineal descendant of Francis Nourse, who came from England and was in Salem, Massachusetts, in 1639. Francis Nourse's wife, Rebecca (Towne) Nourse, had come to The Massachusetts Bay Colony from Great Yarmouth, England, about 1638. Francis and his wife prospered and became one of the most highly honored and respected couples in the community. Rebecca bore her husband nine children and is reported to have been "a model of every virtue." In part because of their prosperity and popularity, jealousies were aroused, and in the hysteria which swept Salem in 1692 Rebecca Nourse was accused of witchcraft. On July 19, 1692, at the age of seventy-one, she was hanged on Gallows Hill. The Salem witchcraft madness soon passed away, and today Rebecca's old home is a shrine. A granite monument in her memory stands in the Nourse cemetery nearby. In spite of this tragedy, the Nourses remained in Massachusetts (they left Salem, a natural reaction under the circumstances) until Rebecca's great-grandson, Nathaniel, moved to New Hampshire in 1779, and his grandson, William (grandfather of B. O. Dodge) traveled on to Vermont in 1830 and to Wisconsin in 1856.

Dr. Dodge's mother, Mary Ann (Nourse) Dodge and father,

Eldridge Gerry Dodge, were unusual people. His father was a carpenter, a trade he followed along with teaching and farming until he was thirty-nine, but his real interests were in music, literature, and schoolteaching. He began his career as a schoolteacher in Vermont, where at the age of eighteen he taught in the district school, one term of three months at $8.00 per month; a second year brought $10.00 per month, and by the third year his compensation had risen to $12.00 per month. Since he was under twenty-one all this income was turned over to his father—a custom which, I believe, has long disappeared, along with $8.00, $10.00, and $12.00 per month teachers. Eldridge Gerry Dodge left Vermont in 1853 and after periods in Ohio, Minnesota, and Wisconsin, settled near Mauston, Wisconsin, in 1858, where he married Mary Ann Nourse in 1861. Save for a period of service in the Northern Army during the Civil War, he devoted the rest of his active life to farming, to teaching in the district schools, and to his family. A vegetable and fruit garden, an apple orchard, and a mill for making sorghum syrup, plus some cash from teaching, gave him and his family their living.

A man with no high school or college education, he had a great interest in and a wide acquaintance with the writings of Shakespeare, Byron, Chaucer, Spenser, Pope, Dryden, Tennyson, and others, and would recite entire scenes from Shakespeare to his attentive family. At the same time, he enjoyed reading about Peck's Bad Boy or Josiah Allen at Saratoga, and on one occasion, Dr. Dodge remembered, called his sons from the fields to hear him read dramatically an account of the Sullivan-Kilrain prize fight.

Dr. Dodge's mother, Mary Ann (Nourse) Dodge, was sixteen when she accompanied her family from Vermont to Wisconsin in 1856. The Dodge and Nourse families had been neighbors in Vermont, and the Nourses probably moved to Wisconsin in order to separate Mary Ann and Eldridge Gerry Dodge. In any event, the presence of Mary Ann in Wisconsin was responsible for Eldridge Dodge's deciding to settle there after his wanderings in the Middle West. Mary Ann's father was very religious and bitterly opposed his

daughter's interest in a carpenter and schoolteacher who sang songs and recited poetry, and in a man whose only religion was to "follow the Golden Rule seven days a week." Mary Ann taught school, the romance prospered in spite of her father's disapproval, and on July 4, 1861, she married Eldridge Gerry Dodge before a Justice of the Peace in the village of Mauston, Wisconsin. Arriving at the Nourse home after an hour and a half's drive from Mauston in a spring wagon through the rain, the young couple were barred from the family home and forced to take refuge with sympathetic neighbors until their own home could be completed far enough to be lived in. It was many years before any reconciliation occurred between the righteous father and his daughter and son-in-law.

Mary Ann Dodge bore her husband five sons and two daughters and lived the life of a pioneer's wife. Dr. Dodge thought of her as a poet and an artist who saw beauty in the common labor of the day, inspired in her children a love for music, and was able to express herself in rhyme, though she had never had more schooling than that provided by a district school. At sixty-nine, she undertook the translation into English of a 64-page Spanish story.

His mother and father were happy in their family life and though limited in their formal education were inspired by a love of music, literature, and learning—a circumstance which must have had much to do with molding the attitudes and determining the ambitions of their children.

The daughters married; one died at the age of thirty-three, the other lived to be eighty-three. Of the five sons, one graduated from Rush Medical School and practiced medicine at Franklin Park near Chicago for more than forty years; one became a musician and band leader; one with a musical and mechanical bent entered business; one graduated from Northwestern Dental College and practiced dentistry until his untimely death at the age of twenty-nine; and one was Bernard Ogilvie Dodge.

Bernard Dodge spent the first twenty years of his life working on his father's farm, a period which he describes as interesting and

eventful. He recalls that at the age of ten a bumper crop of sorghum required operation of the mill day and night during the rush period of syrup making. At such times his father, his two older brothers, and he worked eighteen hours at a stretch. His job was to stand on the circling horse-power platform and drive the horses, walking sideways to avoid dizziness. Dodge's stretch began at midnight—you can imagine the reluctance with which a boy of ten came from his warm bed to drive the horses of the sorghum mill, but a silver fifty-cent piece from his father quickly overcame it. That winter, he had his first regular job—walking over a mile to the schoolhouse each morning, with temperatures far below zero at times—to sweep out the schoolhouse and build the fire, at five cents each school day.

Perhaps because his help was needed on the farm, Bernard Dodge did not complete his high school education until he was twenty years of age. He taught school and then in 1896 entered the University of Wisconsin as a special student, but before the college year was completed his funds were exhausted, and he found it necessary to return to teaching, first in the district schools and then as High School Principal in Greenwood, Wisconsin. By the time he was twenty-eight he could afford to resume his formal education. Alternating periods as teacher (as High School Principal at Greenwood and at Algoma, Wisconsin) and student, he was graduated from the Milwaukee Normal School in 1901 and from the University of Wisconsin (Bachelor of Philosophy) in 1909. To complete the requirement for the degree from the University he attended that institution for a semester in 1895–96, five summer sessions, and finally the school year 1908–9. Few would have had the persistence then (or now) to pursue a college education for interrupted intervals over a period of fourteen years.

As I write I have before me a photograph of the schoolhouse in which Dodge received his first instruction. It is a plain frame building of one, perhaps two rooms, with a belfry on the roof at one end. There is also a photograph of the one-room country school with outside conveniences where Dodge taught in 1893. I look in vain for

evidences of laboratories, an auditorium, gymnasium, athletic field, facilities for a uniformed marching band, and the other paraphernalia now regarded as essential for proper elementary, grammar school, and high school education. I wonder how any of the Dodge generation obtained an education. Can it be that the individual and his attitudes as well as those of his family and the community are more important than the physical educational plant, or are modern boys and girls and their needs so different from those of seventy years ago?

In 1906 he married Jennie S. Perry, with whom he lived happily for more than fifty years. There were no children.

At the University of Wisconsin, he came under the influence of R. A. Harper, at that time one of the great figures in American botany, who was soon to transfer from Wisconsin to Columbia University. At Harper's suggestion, Dodge decided to undertake graduate work and accepted a minor position as Assistant in Botany at Columbia. He and Mrs. Dodge moved to New York City in the summer of 1909 and three years later Dr. Dodge received the degree, Doctor of Philosophy.

He remained at Columbia as Instructor of Botany until 1920. These eleven years in poorly paid minor positions must have been difficult indeed for a married man approaching fifty before they ended, not to mention his wife, whose whole-hearted support and sympathetic interest were major factors in making it possible for Dr. Dodge to pursue his chosen field during these years. In 1920 he accepted an appointment as Plant Pathologist (in fruit diseases) in the Bureau of Plant Industry of the United States Department of Agriculture and spent eight satisfying years in Washington. Years later I visited with him the house he built there and listened as he extolled the view and detailed the labors which settling this new home and its grounds had required. It was here that Dodge initiated his studies of *Neurospora*. By 1928 The New York Botanical Garden had decided that it required a plant pathologist to maintain the health of its living collections, and Dr. Dodge was appointed. He remained in this position until he retired in 1947 to become Plant Pathologist Emeritus

and Consultant in Mycology. At his request he was relieved of his duties as Consultant in 1957. After retirement, he continued to work in his laboratory until a few months before his final illness, making his way several times each week by subway, nearly an hour's journey from his apartment in the vicinity of Columbia University to The New York Botanical Garden.

How Dodge became sufficiently interested in plants, especially the fungi, to make their study his life's work can be told in his own words.

"In order to understand how I came to make botany my life's work, it should be recalled that in high school at Mauston, Wisconsin, our botany teacher, Mrs. Goetting (also English teacher, history teacher, etc.) knew very little about plants in general, but since it was only a three month course and this was nearly 70 years ago, botany consisted mostly in learning how to analyze plants and give them their specific names. Therefore, we had very little knowledge regarding the lower forms of plant life, such as fungi, algae or bacteria. Mrs. Goetting's plan was to have each student collect in the wild at least 75 species of plants and identify them, following Gray's *School and Field Botany* keys. We learned, along with other characters, about the arrangement of leaves but nothing about the spiral arrangements as related to the Fibonocci series of fractions and this with logarithmic spirals and the Golden mean! These collections were presumed to be dried and pressed botanical specimens. For each 25 species more than the minimum 75, we were given an additional higher grade. I ended up with a standing [of] 125!

"My interest in plants did not end with graduation day, June 10, 1892, but it was several years before I became interested in plants in general, especially the lower forms. One of the courses I took as an adult special student at the University of Wisconsin in 1896 was with Professor Charles R. Barnes, who later went to the University of Chicago as Professor of Plant Physiology. He had originally specialized in the mosses, liverworts, and other lower forms of plant life. Naturally, as teacher of botany at the Algoma High School, I became

interested in such types of plants as grew in swamps and woods. The liverworts were especially interesting, particularly after we had purchased a good microscope, one for the whole class of 20 or more students. The students were fascinated to see under the microscope such algae as *Spirogyra, Hydrodictyon,* and diatoms. The boys would often come up to the school house in the evening if they saw a light in our laboratory. They never knew what kind of a show I was putting on! At first, we had only a kerosene lamp for light for the microscope, one of the stage footlights loaned by the Perry 'Opera Hall.' Even so, I discovered something on one of those nights that I had never seen before and never have seen since. That was the process by which an antherozoid of a little leafy liverwort migrated down the neck of the archegonium and finally appeared to fuse with the egg.

"Some years later at Columbia University I studied *Marsilia* to show the class in botany stages in fertilization. The antherozoids would swim into the jelly-like mass but I could never find that they ever seemed to fuse with the egg in the archegonium. Later on I learned that the species was said to be apogamous!

"While Principal of the Algoma, Wis., High School, I taught botany and physics regularly, and sometimes geometry. One afternoon after school I went into a swamp to collect some *Spirogyra* and *Hydrodictyon* for classwork the next day. After getting a good collection I started walking back home on a narrow dirt road a mile or so from home. I met an old Bohemian tailor with his dog, his pipe, and a basket, leading his cow for the night milking. He had several curious things in that basket, so I asked him what he had. He said, 'Pilze.' 'What are you going to do with them?' I asked. 'Gut für essen.' Well, here I was teaching botany and I did not know what a common wild mushroom looked like! Actually, I found out later what he had were specimens of species often erroneously reported to be poisonous and not an edible mushroom at all—probably *Boletus luridus* or *B. satanus.* I never heard of any people in that region being poisoned by eating those *Boletus* 'toadstools.' The secret of this

is probably that they had a few species that they knew from experience were safe but they avoided the ones they did not know 'for sure.'

"We had no books on mushrooms in the high school library, so I sent specimens to the Department of Botany, University of Wisconsin. Their reply was to the effect that a mushroom that turned blue when broken open was poisonous and so was a 'toadstool.' I soon found out that when I would try to give some of my friends a basket of 'mushrooms' from the wild they would not touch them until they had shown them to Mr. Danek, Mr. Thiard, or Mr. Pauly—or some authority better than Dodge.

"Finally, I was able to buy a book on mushrooms written by Prof. George Atkinson of Cornell. Then Mrs. Dodge made me a Christmas present of MacIlvain's *One Thousand American Fungi.* With these two books, we were able to identify a number of the fleshy fungi. Some of the species had been dried and later brought to Columbia University.

"My contacts with such authorities, for example, as Heinrich Rehm of Munich, Bresadola of Austria, Charles Peck, the famous New York State Botanist, were all factors leading to future interest in the morphology, taxonomy, pathology, and genetics of the fungi. Certainly, the first meeting between that old Bohemian gentleman with his pipe, dog, basket, and cow, was one 'lucky accident' that led to my long and happy years investigating these fascinating groups of plant life."

Dodge published about 150 papers dealing with the life histories, cytology, morphology, pathology, and genetics of the fungi, and with insect and other animal pests of plants. He was forty-two when his first paper was published; his last appeared when he was eighty-five.

Dodge was a mycologist, intensely interested in the fungi as such, though for a substantial part of his active career he studied their relations to disease in plants and in animals also. He began with collections of fungi in Wisconsin (published 1914) and would probably

have continued as a taxonomist had Dr. Fred Seaver of The New York Botanical Garden not interested him in the development of species of Ascobolaceae, especially their reproduction, on which he published papers in 1912 and 1920. Papers on the formation of the capillitium in some Myxomycetes (1914), the morphological relationships between Florideae and Ascomycetes (1914), the life history of *Pilacre faginea* (1925), species of *Scophilariopsis* (1931), *Penicillium* (1933), *Peziza pustulata* (1937), *Stevensea wrightii* (1938), *Papulospora* (1915, 1941), *Pleurage anserina* (1936), *Gelasinospora tetrasperma* (1937), and de Bary "bubbles" in ascospores (1957) illustrate his insatiable curiosity about the fungi, as such, especially their reproduction. His researches on these organisms included anatomical and cytological observations which illuminated the details of their development, a procedure he followed also in his investigations of disease-producing fungi, since he was convinced of "the importance of a full knowledge of the life history, identity and synonymy of pathogenic fungi" (1921).

As a graduate student and Instructor at Columbia University, he investigated the taxonomy and reproduction of species of Ascobolaceae. One observation made during the course of these investigations proved to be of key importance later for his studies of *Neurospora.* Dodge found that the ascospores of several species of *Ascobolus* which rarely germinate under ordinary conditions on artificial media do so readily after being subjected to 50 to 70°C. for five or ten minutes.

This discovery was made by accident. He had encountered great difficulty in germinating the ascospores of *Ascobolus* but continued to try various methods which he hoped might prove effective. One day, in the crowded laboratory where he was working at Columbia University, he found no convenient place to set a batch of test plates while he taught one of his classes. He put them for temporary safekeeping in a hot-air sterilizer which was not running and left for his class. When he returned he was dismayed to find that someone had lit the oven in his absence and the temperature had reached 70

or 80°. He removed the plates, assumed the spores had been killed, and prepared to set up a fresh lot of plates the next day. But before discarding the old plates he examined them microscopically and was astounded to find that most of the spores had germinated. I believe it was Pasteur who said that accidents are fruitful only if they occur before a prepared mind. Dodge's long and unsuccessful efforts to germinate the ascospores of *Ascobolus* had prepared him to make this accident fruitful, and he proceeded to define the life cycle of *Ascobolus,* discovering its heterothallic character, the first example of this condition in the Ascomycetes.

Although Dodge was absorbed by the fungi as such, he became involved early in the study of them as causes of diseases in plants—especially the rusts. Over a period of twenty-two years Dodge published twenty-five papers on the plant rusts, the first in 1915 and the last in 1936.

The cedar rusts first engaged his attention, and interest in them persisted throughout his career, though other problems eventually occupied him and he discontinued (1934) active investigation of these intriguing obligate parasites. His concern with the cedar rusts ranged over all aspects. By field observations, infection experiments, and histological and cytological methods he studied their alternate hosts; the amount of damage done to the cedar host; the development of witches' brooms, galls, and tumors; the length of time required for each rust to mature; the particular host tissues invaded and the method of invasion; the mechanism by which the fungus when it came to fruit broke open the overlying bark, epidermis, or cuticle and thus developed its spores at the surface; and the cytological details of the formation of haustoria and spores.

At the Bureau of Plant Industry his attention turned to the rusts of the blackberry, dewberry, and raspberry. His observations on the systemic infection of these small fruits by the orange rusts illuminated the life cycles of these parasites and suggested methods of control. His investigations included also the stem blister-rust of pines which has species of oak as the alternate host, may apple rust, the

scrub pine needle rust, hollyhock rust, and rust of *Sempervivum*.

Everything about the rusts interested him. He speculated on their evolution and on their sexuality. He attempted to cultivate them in vitro, became characteristically excited when he heard a report that this had been accomplished and at once proceeded to set up experiments to test the validity of the report. He noted that systemic infections of the blackberry resulted in the formation of stomata on the upper surface of the leaves where normally few, if any, occur and was a bit disappointed when he found a similar situation did not exist for rust-infected may apple.

The forcible ejection of spores from aecidia aroused his curiosity. He wrote,

"On examining aecidia [of *Gymnosporangium myricatum*] on freshly gathered leaves with a microscope of fairly low power, it was noticed that the spores were being shot out of the cups one by one with considerable force like popped corn jumping out of the popper." Here was another of nature's wonders which always aroused Dodge. What made these tiny objects shoot out of the cup in which they were formed, and what happened with aecidiospores of other rusts? Were they, too, forcibly ejected? After careful microscopic examination including the study of sections of aecidial cups, Dodge concluded that forcible ejection of the spores was the result of a deformation of their elastic walls. This deformation is caused by crowding and by the presence of small plugs, which are formed during spore development and eventually indent the spore walls, as they lie free between the spores. As a spore was pushed upward by the growth of other spores below, it eventually reached a position where the pressures on its wall caused by plugs and neighboring cells were reduced. The wall of the spore sprang back to its undeformed position and the spore was shot out to distances of 100 or more times its diameter.

Dodge compared the dynamics of spore ejection to what happens if tennis balls with marbles tightly packed between them are compressed and one ball is pushed up to the surface. There, liberated

from the forces which deformed it, the elastic wall of the tennis ball snaps back to its original position and the ball shoots upward. Aecidiospores of some other rusts are not discharged with violence, though in a later paper (1924) Dodge found those of the may apple rust [*Puccinia podophylli*] were shot out by a system much like that he had reported for *Gymnosporangium myricatum,* and still later (1941) he discussed the forces concerned in the discharge of the sporangioles of bird's nest fungi.

His concern with plant diseases was not limited to the rusts. He published on the heart rot of apple trees; on several species of fungi responsible for rotting of strawberries, including morphological studies of the pycnidia of some of them; on blight of raspberry; and on diseases of *Iris,* Japanese cherries, roses, phlox, *Pachysandra,* gladiolus, geranium, marigold, *Opuntia, Delphinium,* and other ornamental plants. In 1953 he published with H. W. Rickett a textbook, *Diseases and Pests of Ornamental Plants,* which was revised by Dr. P. P. Pirone in 1948, and appeared in an enlarged edition in 1959 under the authorship of Pirone, Dodge, and Rickett.

For about twenty-five years he was responsible for the practical control of plant diseases and insect pests at The New York Botanical Garden. The rose garden was his special care and delight. He took his duties most seriously, frequently supervising spraying, dusting, and other control measures so personally that he would appear in his laboratory spotted with spray materials or covered with dust.

Dr. Dodge's concern with fungi as causes of disease extended from plants to animals, including man. Along with Dr. R. M. Harper he played an important advisory role in the organization by J. Gardner Hopkins in 1928 of the Department of Dermatology in the College of Physicians and Surgeons. From 1928 to 1939 he was Consultant in Mycology for the Vanderbilt Clinic of the Presbyterian Hospital of New York City, and from 1928 to 1951 Lecturer in Dermatology, College of Physicians and Surgeons of Columbia University.

Although Dodge considered himself primarily a mycologist and plant pathologist, his studies of *Neurospora* are regarded by many

as his major work. They were not begun until he was fifty-three. How they began may be told in his own words.

"I was, 1925–6, highly interested in studies on the blackberry rusts, short and long cycle forms (species). I had found that I could pass the rust on the Black Diamond blackberry (dewberry) by grafting to young shoots of this species, and was trying to culture the rust on cornmeal agar in plates and flasks. So I had several plates and flasks (250 cc) standing on shelves in the Arlington Farm greenhouses. I was much concerned to find that *Monilia sitophila* had gotten into some of the flasks as well as some plates. Practically all of them were soon bearing perithecia resembling those Dr. C. L. Shear had shown me and [had] asked me to try to germinate the ascospores. I was so much excited and interested in my rust studies that I could not leave those experiments to help my superior out. I did suggest trying to make these spores germinate by heating them. I loaned their office my young man assistant to try this heating aid for them. Later on he reported that my method did not work. Several weeks had passed when this red bread mold developed in my greenhouse flasks and plates. Just on a chance I inverted plates of corn [meal] agar on cultures of perithecia shooting spores so spores were shot upward and scattered about. I put some of these plates in a drying oven and heated them as I had done many times to make ascospores of *Ascobolus* to germinate. By next morning a high percentage of the mold had germinated."

The ability to germinate the ascospores of *Neurospora* made it possible to define its life cycle, distinguish species, make crosses between species, and grow the haploid offspring.

Shear and Dodge reported part of their findings before the Botanical Society of Washington in 1926. In 1927 they published a paper in which they described the life history of the *Monilia sitophila* group, established a new genus (*Neurospora*) and demonstrated that *N. sitophila* and *N. crassa* were heterothallic, while *N. tetrasperma* was homothallic ("pseudo-homothallic"). They crossed species and obtained ascospores which were stated to have hybrid characters. This

report was followed by one by Dodge (1927) on nuclear behavior in the formation of ascospores by *Neurospora* in which he showed how a binucleate spore of *N. tetrasperma* came to be bisexual, and in which he discussed the process by which half the spores in an ascus of *N. sitophila* were of one sex and half the other, and the significance of the arrangement of the spores in an ascus.

Over the next thirty years, Dodge published over forty papers on *Neurospora.* He clarified the mechanism of mating type inheritance and showed that this was determined by a single pair of alleles (1929). He investigated morphologically different mutations of spontaneous origin and demonstrated that sex linkage occurs in this fungus (1930). Using a sharpened sewing needle in a simple wooden handle to isolate all the ascospores in a single ascus, he showed for the first time the possibility of securing in linear order all the products of a single meiotic division, and demonstrated absolute Mendelian ratios (2:2) in the segregation of mating types and the character of albinism. He defined two types of asexual spores, the multinucleate macroconidia and the uninucleate microconidia (spermatia), and described how they were produced and how they functioned in fertilization (1932). The microconidia, because of their uninucleate condition, proved to be especially useful for studies of mutation, both spontaneous and induced. Between 1934 and 1956 he published thirteen papers on lethal factors for *Neurospora* in which he described and discussed a dominant lethal (*I*) responsible for the formation of indurated asci (1934), a recessive lethal (*d*) which caused ascospores to deliquesce, but under some conditions resulted in indurated asci capable of germination (1935), and a dominant lethal (*E*) which caused ascospores to abort but did not induce indurated asci (1939).

He reported on heterocaryotic vigor in *Neurospora* (1942, 1945), and with Butler and Robbins (1941) found biotin to be the only growth substance required for the development of *Neurospora* in a chemically defined medium, a condition essential for the successful use of *Neurospora* in biochemical studies. He discussed possible ways in which one species may have arisen from another.

Dodge recognized and emphasized the importance of the haploid condition of fungus mycelium for genetic studies, as is demonstrated by the following quotations:

"In genetic studies of higher plants and animals, the effects of factors of inheritance are usually manifest in connection with diploid structures where each nucleus carries two sets of genes, one set contributed by each parent (Dodge, 1939). The haploid mycelium of a fungus (*Neurospora*) shows many variations as to color, types of growth and in the production of propagating bodies of all sorts. The foundation structures of the fruit bodies themselves are haploid. One is, therefore, enabled to observe in the fungi the effects of a single set of genes without the complications which are bound to arise when two different sets are present, as in diploids. . . ."

Dodge was greatly intrigued also by the potential usefulness, scientifically, of an organism containing pairs of haploid nuclei (a dicaryophyte) as compared to one in which the two haploid nuclei had fused to form a diploid nucleus.

"Dicaryophytes," he said, "are among the most interesting and important phenomena in the whole biology of the fungi. . . . We have not yet opened our eyes to the beauty and significance of these dicaryophytic haploid partnerships."

And again, "We find (Dodge and Appel, 1944) in recent literature the statement that a dicaryotic cell of a rust or of a mushroom is in fact diploid because the genes carried in each of its two haploid nuclei operate just as they would if they were included in the same nuclear membrane. The authors of such statements should realize that when two haploid nuclei of a rust dicaryon fuse, so that their genes are included in the same membrane, a mature teliospore results. When two nuclei in the mushroom discaryon fuse so that their complementary genes are now within the same membrane, a basidium is formed. So it is clear that the manifestations of genes carried in haploid nuclei of a dicaryon cannot possibly be the same as they are when drawn together within the membrane of a diploid nucleus." So strongly did Dodge feel on this matter that he often said,

"I wish my epitaph to read, 'A dicaryon is not a diploid.'"

Dodge's investigations of *Neurospora* revealed principles of genetics in beautiful simplicity and dramatic fashion, in some respects more sharply than was possible with any other organism previously studied. *Neurospora* had several advantages. The linear arrangement of the ascospores in an ascus permitted all products of a single meiotic division to be examined in linear order. This is possible only in those Ascomycetes which produce asci of this type. A single pair of alleles control compatibility so that perfect 50:50 ratios are obtained when the spores of a single ascus are analyzed. The mycelial form is haploid, but through heterocaryosis the influence of two different nuclei on development can be studied. In addition, *Neurospora* is easy to grow and requires little space and only a few days to complete its life cycle. These characteristics made *Neurospora,* in many respects, an ideal organism for the investigation of genetics and of biochemical genetics.

Dodge fully appreciated the advantages of *Neurospora.* He convinced T. H. Morgan of its potentialities as a genetical tool and Morgan took cultures with him when he transferred from Columbia University to the California Institute of Technology in 1928. There C. C. Lindegren, on Morgan's advice, undertook the preparation of a dissertation on *Neurospora* in the course of which he spent the summer of 1930 in Dodge's laboratory, one of many who felt the impact of Dodge's ideas and enthusiasm.

Dodge himself wrote in 1929, "The three species of *Neurospora* furnish very desirable material for studies on inheritance in the fungi. Perhaps in no other place in the plant or animal kingdoms so far known can the progeny from a single mother cell be studied so readily and to such advantage."

George Beadle, in his Nobel lecture, refers to Dodge's enthusiasm for *Neurospora* as a tool for genetic research and traces the relation of Dodge's investigations to the selection of *Neurospora* by Beadle and Tatum for their investigations on biochemical genetics.

"As a graduate student at Cornell I [Beadle] had heard Dr. B. O.

Dodge of the New York Botanical Garden give a seminar [1930] on inheritance in the bread mold *Neurospora.* So-called second-division segregation of mating types and of albinism were a puzzle to him. Several of us who had just been reviewing the evidence for four-strand crossing over in *Drosophila* suggested that crossing over between the centromere and the segregating gene could well explain the result.

"Dodge was an enthusiastic supporter of *Neurospora* as an organism for genetic work. 'It's even better than *Drosophila,*' he insisted to Thomas Hunt Morgan, whose laboratory he often visited. He finally persuaded Morgan to take a collection of *Neurospora* cultures with him from Columbia [University] to the new Biology Division of the California Institute of Technology, which he established in 1928.

"Shortly thereafter when Carl C. Lindegren came to Morgan's laboratory to become a graduate student, it was suggested that he should work on the genetics of *Neurospora* as a basis for his thesis. This was a fortunate choice, for Lindegren had an abundance of imagination, enthusiasm and energy and at the same time had the advice of E. G. Anderson, C. B. Bridges, S. Emerson, A. H. Sturtevant, and others at the Institute who at that time were actively interested in problems of crossing over as a part of the mechanism of meiosis. In this favorable setting, Lindegren soon worked out much of the basic genetics of *Neurospora.* [Some of the "basic genetics" had already been worked out by Dodge.] New characters were found and a good start was made toward mapping the chromosomes.

"Thus, Tatum and I realized that *Neurospora* was genetically an almost ideal organism for use in our new approach."

One of the most dramatic applications of Dodge's results on segregation occurred on July 17, 1950, at a meeting of the International Botanical Congress at Stockholm, Sweden.[2] A special evening session of the Congress, with Professor Öjvind Winge of Copenhagen as Chairman, was devoted to Russian genetics—at that time dominated by the Michurian theory which rejected, under the leadership of

[2] This account is based on letters written to the author by Dr. L. M. Black and by Dr. A. Quintanhila, both of whom attended the meeting.

Lysenko, the chromosomal theory of heredity and the Mendelian interpretation of genetics. Lysenko did not attend the meeting. After Professor I. Y. Glushchenko had delivered the first paper, Professor Winge stated that questions submitted in writing would be answered after the second paper. Quite a few questions were submitted but the Russians announced that because there were so many, answers would be given the next day. This was greeted with some laughter, a response which naturally nettled the speakers, and Professor Glushchenko at once proceeded to answer a number of questions at great length. The audience grew restless after listening politely for some time, interrupted him and demanded that he answer certain questions dealing specifically with the Lysenko controversy.

Glushchenko attempted to dispose of Mendelian genetics by saying that the ratios obtained in segregation were merely statistical averages, but Prof. A. Quintanhila cited the results obtained by Dodge in isolating and cultivating all the spores in an ascus of heterozygous *Neurospora.* Here Dodge obtained an exact (2:2) ratio. Glushchenko raised various objections—there was difficulty in translating the question; the question was ambigous; Mendelian ratios were merely statistical averages. When his attention was again called to the results with *Neurospora* he claimed he knew nothing about the experiment (which was probably true), that it should be repeated (this caused some laughter), and that he should visit the laboratory where the work had been done. The scientist (Dodge) who did the work would give his interpretations and Glushchenko would give his. The audience rejected these evasions and the session adjourned, having sat from about seven o'clock until near midnight.

According to one report, Glushchenko finally said he would answer the questions "tomorrow." According to another, he admitted that chromosomes might be concerned in the transmission of characters, in the particular example mentioned (*Neurospora*), but that he nevertheless did not accept the chromosomal theory of heredity.

Dodge never developed a larger genetical program involving an extensive study of spontaneous and induced mutations, chromosome

mapping, and the like. In fact, his researches on *Neurospora* were an "extra" carried on while his official duties were those of a plant pathologist responsible (while he was in Washington) for a program of research on the diseases of small fruits and later in New York for the investigation and control of diseases in a botanical garden of more than 200 acres. His investigations on *Neurospora* were conducted with limitations of time, assistance, and facilities uncommon in these days of relatively massive support of basic research. Routine media were frequently prepared by Dodge himself; individual ascospores were isolated by a sharpened needle inserted in a wooden handle, and his equipment was limited to the simplest essentials in the way of incubators, autoclaves, and similar facilities.

Nevertheless, his investigations laid the foundation for the use of *Neurospora* in the investigation of genetics and biochemical genetics on a world-wide basis. Beadle, in a letter to Dodge (November 1, 1959) said, "Without your pioneer work, those of us who have made use of *Neurospora* never could have done what we did.... *Neurospora* has been good to many of us and it is your baby more than anyone else's. Thanks again for giving it to genetics." Edward L. Tatum (1959) wrote in his Nobel lecture, "I shall not enumerate the factors involved in our selection of this organism [*Neurospora*] for the production of chemical or nutritionally deficient mutants, but must take this opportunity of reiterating our indebtedness to the previous basic findings of a number of investigators. Foremost among these, to B. O. Dodge for this establishment of this ascomycete as a most suitable organism for genetic studies; and to C. C. Lindegren, who became interested in *Neurospora* through T. H. Morgan, a close friend of Dodge."

This is not the time nor place to attempt an evaluation of Dodge's contributions to the genetics of the fungi and to place them in their proper historical perspective.

By the early 1920's the genetics of the fungi was emerging from ignorance and confusion. Dr. Albert F. Blakeslee had demonstrated heterothallism in the Mucorales, and a number of investigators had

presented evidences of one kind and another for hybridization between races and even species of fungi. Mendelian concepts and terminology had been used by investigators of inheritance of sex in Hymenomycetes; but the data even for Hymenomycetes where the results were most clear cut were limited to mating types and often complex. The status of the genetics of the fungi was summarized by W. B. Brierley at the Fourth International Congress of Plant Sciences in 1926, the year in which Shear and Dodge first reported on *Neurospora* before the Botanical Society of Washington. Brierley said, "As one considers all this recent work on sexuality and hybridization in different fungal groups, and particularly if one keeps in mind results of similar work in other groups of organisms, the conviction grows strongly that the genetic relationships in fungi are of a perfectly definite and causal order and are to be formulated in terms of combination and segregation, although these often seem to be of a highly complicated order."

Brierley recognized, however, that this conviction of his was not generally held. In fact, he wrote in the same paper, "The fungi, and still more, the bacteria are usually regarded as exceptional or aberrant groups of plants to which the genetic concepts derived from the study of other groups of organisms do not apply."

Dodge (1940) gave H. Burgeff, whose papers appeared in 1914 and 1915, credit for first obtaining mutant races of a fungus (*Phycomyces nitens*), mating them, and proving that factors for sex and for different types of growth segregate according to Mendelian principles. However, Dodge's simple beautiful demonstration (1930) of segregation in the eight cultures grown from the eight spores of a single ascus obtained by crossing an albino with a wild type race of *Neurospora sitophila* was convincing for all but the most sceptical and placed the genetics of the fungi on the same Mendelian basis as that of higher plants and animals.

His concept of his own relation to the study of the genetics of the fungi can be stated in his own words (1936). "Mycologists who undertake the study of ascomycetes or other fungi genetically will al-

ways be handicapped by their lack of that training which is necessary for adequate analysis of any extensive breeding work. Our proper function is to remain mycologists. We must discover for the geneticist species which are well adapted not only morphologically but culturally for their type of work. We should develop the culture technique and work out the morphology and the details relating to propagation and reproduction and prove by our preliminary hybridization work that there is need for a further genetic study of the species. On the other hand, geneticists without mycological training undertaking to study the ascomycetes should realize that the ways of these fungi are devious and beset with pitfalls. Nevertheless, it is encouraging to know that fundamentally in their reproduction and inheritance the fungi follow exactly the same laws that govern these activities in the higher plants and animals."

Dodge's studies on *Neurospora* led also to the resolution of a controversy on the life cycle of Ascomycetes which had existed since 1908. One group of botanists held that in a typical Ascomycete, as in other organisms, there is only one nuclear fusion, which for Ascomycetes occurs in the ascus, and that there is, therefore, only one reduction division by which the double number of chromosomes is returned to the single number. Another group insisted that one nuclear fusion occurred before the asci were formed and a second in the ascus itself. This situation, in which the final fusion nucleus contained four sets of chromosomes, would require two reduction divisions, one of which it was asserted was completed in the first two nuclear divisions in the ascus and the other in the third division.

This difference in opinion arose because the evidence offered was based on counts of chromosomes and observations on nuclear fusions or absence of nuclear fusions in the young fruiting structure. However, the chromosomes of Ascomycetes are minute and difficult to count accurately, and two nuclei lying next to one another or a nucleus completing division may be interpreted as a stage in nuclear fusion.

Genetic studies on *Neurospora* first carried out by Dodge and

amply confirmed by later investigators proved that there was only one nuclear fusion and this occurred in the ascus. This was a source of some embarassment to Dodge because his loved and revered professor, R. A. Harper, had supported the "double fertilization" school.

In his later years Dodge was intrigued by phyllotaxy, the Fibonacci series, and other expressions of design in nature, and spent much of his spare time puzzling over them and directing the attention of his colleagues to their wonder.

Dodge, like his father before him, was a teacher and nothing pleased him more than to enthuse some beginner or a colleague for the subject in which he took so great an interest, and few could resist him.

He was very proud of his fourteen years of teaching in the public schools of Wisconsin and of the Unlimited Life Certificate for which he qualified in 1898. One of his great satisfactions was to receive, in 1958, a letter signed by eight members of the Algoma High School Class of 1908 sending him greetings and expressing their gratitude for his patience, guidance, and personal interest in them when he was their teacher, and acknowledging the effect on them of his ideas and ideals and of his intense desire to impart his knowledge.

Preserved among his papers is a letter of recommendation written in 1908 which may be quoted in full as it summarized qualities which persisted with Dodge throughout his life:

OFFICE OF JAMES H. McGOWAN, LAWYER
ALGOMA, WISCONSIN

Algoma, Wis., February 22, 1908

To Whom It May Concern:-

To school officials desirous of securing a principal for a modern up-to-date high school I heartily commend the candidacy of Mr. B. O. Dodge, of this city, for that position. Not only is he possessed of the necessary qualifications in the way of scholarship, but he is industrious, energetic and attentive to his work, and is impelled by that professional enthusiasm and interest in it which give him a high place among those who are in the van of the educational movement in Wisconsin. He is essentially a worker and

not guilty of any of those distractions that tend to divert attention from the work on hand, devoting to his labors more time than the contract really calls for. He is an excellent classroom instructor, and easily and naturally arouses the interest and cooperation of his students and holds them. As a citizen he is exemplary in both his habits and morals.

Very respectfully,

J. H. McGowan
Pres. Board of Education

It is impossible to list all those who felt his influence at Columbia University and later at The New York Botanical Garden. They include Carl Lindegren, I. H. Hirshkowitz, Chester W. Emmons, Myron Backus, S. F. Pady, Jesse Singleton, Esther Zimmer Lederberg, Marjorie Swift, Alice Aronescu, F. Li Tai, Thomas Laskaris, George Bistis, and many others.

Dodge participated willingly and effectively in the affairs of the various organizations to which he belonged. He served as Associate Editor of *Mycologia;* as Vice-President and Councilor of the Mycological Society of America; as Vice-President and Chairman of the Section on Botanical Sciences, American Association for the Advancement of Science. With the loyal support of Mrs. Dodge he held nearly every office of the Torrey Botanical Club. He was Secretary-Treasurer at a time when the position included a multitude of duties—recording the minutes, collecting money and keeping records of all receipts and expenditures, negotiating contracts with printers, writing letters, and last but not least, delving into the depths of the sub-basement at Columbia University, ferreting out from the dust-covered stock and debris back numbers of the *Bulletin,* and shipping them to all parts of the world. For several years he was Editor of the *Bulletin* and for many years collaborated with Mrs. Dodge who served as Bibliographer. He was elected President in 1940, and volume 47 of the *Bulletin* was dedicated to him in recognition of his services to the Torrey Botanical Club and his standing as a botanist.

Dodge's contributions to science were widely recognized in this country and abroad. He was a member of Sigma Xi; a member of

the National Academy of Sciences (elected in 1933), Fellow of the American Academy of Arts and Sciences, Fellow of the American Association for the Advancement of Science, Foreign Member of the Linnean Society of London, and Honorary Member of the British Mycological Society. He was honored by being chosen a Vice-President of the Seventh International Botanical Congress at Stockholm, President of the Mycological Society, and President of the Torrey Botanical Club. When he received the Golden Jubilee Award of Merit of The Botanical Society of America, the citation read:

"Bernard Ogilvie Dodge, whose perceptive researches into the taxonomy, evolution, and pathological relations of the fungi have not been surpassed, but only overshadowed, by his discovery and exploitation of *Neurospora* as a principal source of genetical truth."

Dr. Dodge was a big man physically as well as mentally. Blonde, blue-eyed, fine looking, he was over six feet in height and weighed 190 pounds in his prime. He was proud of his strength and recalled that in his youth he carried 120 pounds of wheat on his shoulder at threshing time. He was modest to an extreme, a bit shy, not agressive, friendly, cheerful, good-natured, never bitter—though some of his experiences might so have inclined a lesser man. I never heard him make a mean remark about anyone. His most marked characteristic was his enthusiasm, boyish one might almost call it, for any subject which interested him, and his intense desire to have everyone about him appreciate how wonderful the subject was.

He loved good music and one of his prized possessions was a Scott radio. In his youth he played the violin, mandolin, and clarinet, but he mastered none of these instruments and in his mature years limited himself to the appreciation of music played by others.

At one time he took pleasure in bridge, especially if it occurred with friends he knew well who would join in miscellaneous visiting before, during, and after the bridge sessions. A drive at night down the West Side Highway with all the lights aglow was an event in Dodge's life, and he always attempted to give a visitor to New York City the opportunity to enjoy this spectacle with him.

His quick, at times almost jerky motions made one a little reluctant to be a passenger in his automobile (in forty years of driving he never had an accident), and one was always a bit surprised that he could pick a single spore from an ascus with no more equipment than a sharpened sewing machine needle inserted in a simple home-made handle.

His mental processes were quick also—often to the confusion of others whose habit of thought was more pedestrain. At one time he enjoyed a good cigar and smoked a pipe, but he gave up smoking completely in the early 1930's as one individual's protest against what he considered to be an unjustified increase in the price of tobacco. He was inclined to be a conservative in politics. Formal religion does not appear to have been important in the lives of his parents—none of their seven children was baptized; he himself was affiliated with the Episcopal Church.

It is fruitless to speculate on what Dodge might have accomplished had he been less handicapped by lack of funds in his earlier years and had he been more generously supported in his research later. I can only express my astonishment and admiration for a man who was graduated from college at the age of thirty-seven, received his Ph.D. and published his first paper at forty, was still an Instructor at forty-eight, and began his most important researches (on *Neurospora*) when he was well past fifty.

He is survived by his wife, Jennie Perry Dodge, of New York City, and a brother, Roy Dodge, of La Jolla, California.

CHRONOLOGY

1872 Born at Mauston, Wisconsin, April 18.
1892 Graduated high school, Mauston, Wisconsin.
1893 Teacher, one-room country school, Lyndon Station, Wisconsin.
1894 Teacher, Tomah, Wisconsin.
1895–96 Special student, first semester, University of Wisconsin.
1897–1900 High school principal, Greenwood, Wisconsin.
1898 Unlimited Life Certificate to teach in the Educational System of the State of Wisconsin.
1899 Summer session, University of Wisconsin.
1900–01 Student, Milwaukee Normal School.
1900 Summer session, University of Wisconsin.
1901 Graduated, Milwaukee Normal School.
1902–03 High School Principal, Greenwood, Wisconsin.
1903–08 High School Principal, Algoma, Wisconsin.
1904 Summer session, University of Wisconsin.
1905 Summer session, University of Wisconsin.
1906 Married Jennie S. Perry, June 14.
1906 Summer Session, University of Wisconsin.
1908–09 Student, University of Wisconsin.
1909 Bachelor of Philosophy degree, University of Wisconsin.
1909–11 Assistant in Botany, Columbia University.
1911–13 Research Assistant in Botany, Columbia University.
1911–13 Secretary-Treasurer, Torrey Botanical Club.
1911 Collecting trip to Bermuda.
1911 Research scholarship, The New York Botanical Garden.
1912 Doctor of Philosophy (Botany, Physics), Columbia University.
1912 Research scholarship, The New York Botanical Garden.
1912 Published first paper on fungi.
1913–20 Instructor in Botany, Columbia University.
1920–28 Plant Pathologist (fruit diseases), Bureau of Plant Industry, U. S. D. A., Washington, D. C.
1923–32 Associate Editor, *Mycologia.*
1926 Fellow, American Association for the Advancement of Science.
1927 Published, with C. L. Shear, first paper on *Neurospora.*
1928–48 Plant Pathologist, The New York Botanical Garden.
1928–39 Consultant in Mycology, Vanderbilt Clinic, Presbyterian Hospital, New York City.

1928–51 Lecturer in Dermatology, College of Physicians and Surgeons, Columbia University.
1930 Delegate, International Botanical Congress, Cambridge, England.
1932–40 Editor, Bulletin of the Torrey Botanical Club.
1933 Member, National Academy of Sciences.
1934 Vice-president and Chairman, Section on Botanical Sciences, American Association for the Advancement of Science.
1934 Vice-president, The Mycological Society of America.
1935 President, The Mycological Society of America.
1935 Fellow, American Academy of Arts and Sciences.
1935 Delegate, International Botanical Congress, Amsterdam.
1936–37 Councilor, The Mycological Society of America.
1937 Exhibit, Genetics of Fungi, Paris World's Fair.
1938 Mycological Foray—Quebec.
1939 Convener, Section on Fungi and Fungus Diseases, Third International Microbiological Congress, New York City.
1940 President, Torrey Botanical Club.
1946 Honorary Member, British Mycological Society.
1947 Invitation Paper, *Neurospora,* Genetics Section, Fourth International Microbiological Congress, Copenhagen.
1948–57 Consultant in Mycology, The New York Botanical Garden.
1948–60 Emeritus Plant Pathologist, The New York Botanical Garden.
1950 Vice-president, Seventh International Botanical Congress, Stockholm.
1951 Distinguished Service Award, The New York Botanical Garden.
1951 Life Member, Wisconsin Academy of Sciences, Arts and Letters.
1952 Dinner honoring B. O. Dodge, B. M. Duggar, and Charles Thom, Ithaca, New York.
1955 Foreign Member, Linnean Society, London.
1956 Golden Jubilee Award of Merit, Botanical Society of America.
1960 Died, St. Luke's Hospital, New York City, August 9.

KEY TO ABBREVIATIONS

Am. J. Bot.=American Journal of Botany
Ann. Bot.=Annals of Botany
Bklyn. Bot. Gard. Mem.=Brooklyn Botanic Garden Memoirs
Bull. N. Y. Bot. Gard.=Bulletin of The New York Botanical Garden
Bull. N. Y. Acad. Med.=Bulletin of the New York Academy of Medicine
Bull. Torrey Bot. Club=Bulletin of the Torrey Botanical Club
Compt. Rend. Lab. Carlsberg Physiol.=Comptes rendus, Carlsberg Laboratoriet, Physiologique
Gard. J. N. Y. Bot. Gard.=Garden Journal of The New York Botanical Garden
J. Agr. Res.=Journal of Agricultural Research
J. Heredity=Journal of Heredity
J. N. Y. Bot. Gard.=Journal of The New York Botanical Garden
Mem. Torrey Bot. Club=Memoirs of the Torrey Botanical Club
N. C. Agr. Exp. Sta. Bull.=North Carolina Agricultural Experiment Station Bulletin
Phytopath.=Phytopathology
Proc. Am. Phil. Soc.=Proceedings of the American Philosophical Society
Proc. Eighth Am. Scientific Cong.=Proceedings of the Eighth American Scientific Congress
Proc. Fifth Internat. Bot. Cong.=Proceedings of the Fifth International Botanical Congress
Proc. Internat. Cong. Microbiol.=Proceedings of the International Congress of Microbiology
Proc. Internat. Cong. of Plant Sciences=Proceedings of the International Congress of Plant Sciences
Proc. Linnean Soc.=Proceedings of the Linnean Society of London
Proc. Ninth Nat. Tree Conf.=Proceedings, Ninth National Tree Conference
Trans. Wis. Acad. Sci. Arts Lett.=Transactions of the Wisconsin Academy of Sciences, Arts and Letters
U. S. Dept. Agr. Farmers' Bull.=U. S. Department of Agriculture Farmers' Bulletins
Year Book Am. Phil. Soc.=Year Book of the American Philosophical Society

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1912

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1914

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1915

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1916

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1917

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1918

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1920

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1921

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