This number of ECOLOGY is dedicated to the memory

of

BURTON EDWARD LIVINGSTON

FIG. 1. Burton E. Livingston in his garden at Riderwood, Maryland, August 21, 1944.

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SOME CONVERSATIONAL AUTOBIOGRAPHICAL NOTES ON INTELLECTUAL EXPERIENCES AND DEVELOPMENT: AN AUTO-OBITUARY

BURTON E. LIVINGSTON

INTRODUCTION

(By D. B. Lawrence)

This issue of ECOLOGY is dedicated to the memory of a very influential worker in the field of "physiological ecology," Dr. Burton E. Livingston, who died February 8th. Livingston was for many years in charge of the Laboratory of Plant Physiology and occupant of the Chair of Forest Ecology at The Johns Hopkins University. Since his retirement in 1940, he had carried on his research at his home in Riderwood, Maryland. He probably will be remembered best by ecologists for his contribution in atmometry,[1] "Atmometers of porous porcelain, and paper, their use in physiological ecology," published in ECOLOGY in 1935, and for his publication with Forrest Shreve "The distribution of vegetation in the United States, as related to climatic conditions," but his contribution to physiological ecology was spread widely over the field of water relations and mineral nutrition of plants. His viewpoint was outstandingly ecological and he conceived of the environment as something which had an inherent capacity to *supply* materials and energy to the dynamic organism, and also to *remove* materials and energy from an organism. He went to great lengths to devise instruments which would measure the rates at which the environment supplied materials such as water and oxygen, and rates at which the environment removed material such as water from the plant. He planned his research in mineral nutrition, always with an ecological background which included the fluctuating regimes of light, temperature, and humidity inherent in the greenhouse conditions at Baltimore. He was not satisfied with constant conditions of environment nor with conditions at any one season of the year so he would repeat experiments four times or more to cover the various seasons. Nor was he satisfied with standard greenhouse bench experimentation which introduced tremendous differences in microclimatic influences; nothing short of a tremendous rotating table with flowing solutions, and using very carefully selected plants, chosen not only for uniform genetic background but also uniform physiological response, made him feel content. Livingston was one of the first to carry physiological methods into the field, and to him ecology is greatly indebted for pointing the way to improvement in method and viewpoint. Probably Henrik Lundegardh is the only other person who can compete with him for top credit in welding physiology and ecology together. Livingston had few prejudices, but he had one that was striking; he had no use for statisticians or

[1] Readers will be glad to know that the atmometer business which had long been operated on a world-wide scale by Dr. Livingston from his home in Riderwood, will be continued at the same address by Mrs. Livingston.

their methods and, so far as I know, he never went beyond the mean and the extremes in his mathematical analysis of scientific data. He felt that if there were important differences to be detected, they could be seen from a table of figures, and he felt that statistical treatment was too often used as a substitute for adequate planning and careful technique. His lack of appreciation of this field was as striking as Clements' blind-spot for genetics, and yet this did not deter his students from interesting themselves in that line. Indeed his antipathy toward statistics probably contributed strongly to making a student of his one of the U. S. Air Forces' top statisticians today.

Livingston's finest attribute, so far as student training was concerned, was the limitless energy and effort he devoted to the criticism of a manuscript. Each page was gone over orally, across the table, paragraph by paragraph, line by line, word by word, critically examining each expression to be sure that no ambiguity was left, and that there was not some more accurate way of expressing the idea. He devoted the same energy to answering letters from fellow scientists, and would often answer a query with a 10-page letter and the apology that he was sorry he didn't have time to write a short letter.

Livingston will always occupy an important place in the history of ecology because he played a vital role in directing ecological research toward precise measurement of the natural environment, and experimentation, during the formative years of the development of our field. It will be remembered that until the late 1800's ecologists, with the exception of Bonnier in 1890 and 1895, were content with superficial observations and untested hypotheses, and as late as 1893 the statement was propounded at the Madison, Wisconsin, Convention of Botanists that "ecology was *observational* and physiology *experimental*." Nothing could have been more stimulating to ecologists, and their minds seemed to explode with indignation, though to be sure, a long time-fuse was very definitely attached which took 12 to 15 years to burn. Clements in 1905 was first in this country to respond. In the first chapter of his book "Research methods in ecology" he tried to show that there was essential identity of physiology and ecology and he thought that the two fields would become one and that the name would probably be physiology. Clements seems to have coined the term "experimental ecology" in the same chapter. He considered Bonnier (1890) the first to carry on ecological experimentation out of doors. In December 1908 at a Botanical Society symposium in Baltimore entitled "Present problems in plant ecology" Livingston was one of the five ecologists and physiologists who spoke their minds. All pointed out vigorously the need for precise measurement of environmental influences [2] and emphasized the fact that ecology could be experimental, and had to be if it was to advance to anything that scientists would mention without a smile. Almost immediately thereafter ecologists began applying experimental methods to the solution of ecological problems and this has brought forth fruit abundantly in the applied fields of forestry, range management, plant pathology, horticulture, and wildlife management. The year 1893 should therefore be considered the date of stimulus and the year 1905 (Clements) the beginning of the response that brought ecology close to the side of physiology, but it was the year 1908 and the ideas of Livingston, Cowles, Transeau, Spalding, and Shaw, that really set ecology and its applied phases off on a new tack with the recommended use of experimentation in the solution of problems.

It seems unnecessary here to list individually the publications that emanated from the Laboratory of Plant Physiology during Livingston's 31 years of active service as its chief, but a statement of the total production will reveal the energy

[2] Livingston would not use the word *factor* in this sense; he preferred to restrict its use to the mathematical sense.

of the man. In all, 269 articles were prepared for publication, and 30 students received their Ph.D.'s in that time. It is doubtful that any other person in the nation has ever demonstrated such a sustained high level of activity in this branch of science. It is not the purpose of this introduction to catalogue the scientific activities of this remarkable man. The intent here is to point out the unusual fortune of ecologists in having on record the following "auto-obituary" as Livingston jokingly called it when he submitted it to me at my request in November, 1937. It is important for us because it traces the ecological development of a very influential worker of our field of science during the critical early stages in its development.

THE AUTO-OBITUARY—NOVEMBER, 1937

My boyhood was spent in Grand Rapids, Michigan, where our home grounds were an unusually large town lot near the edge of the growing city, at the northeast corner of what are now Sheldon and First Streets. Across the street to the south was the shallow valley of a brook, which flowed through a treeless mucky stretch where grew mints and iris, Joe Pye weed, iron weed and milkweed, sedges and grasses. In other directions there were neighbors' homes and a few lots still vacant—remnants of upland meadows, where we "studied" bumble bees in one of my early summers, and red clover. The natural forest of this moraine edge was "oak opening," but there were few native trees excepting a patch of upland oak forest a block or two away. I think I was familiar with the burr oak and the white oak before I entered my fifth year. The other trees of my very early acquaintance had been planted, though most of them represented species native in that region. A few blocks to the west, in the broad valley of the Grand River and a tributary creek, there were railways and a variety of swampy stretches, where the river overflowed in spring. There were log jams (white and Norway pine) in the 80's. I afterwards mapped that glacial topography in the paper on Kent County and described its natural vegetation after a fashion (BOTANICAL GAZETTE, 1903).

Our home lot was largely in cultivation, with little lawn. There were beds of vegetables, rows of Mexican blue maize and potatoes, beds and patches of native "wild flowers," currant and gooseberry bushes, grape vines on the "back fence," sumacs, tall tree-like sunflowers (*H. annuus*) for the chickens. Around the margins of that lot were large trees of sugar maple, silver and Lombardy poplar, elm, white cedar, red cedar (*Juniperus virginiana*), tamarack, butternut, sassafras, horticultural cherries and apples—both eating and crab. Two of the crab-apple trees had been so grafted that each bore three or four kinds and sizes of fruits, which ripened one after another. A single peach tree died before I became well acquainted with it. Almost all of the old-fashioned ornamentals of that region and period were abundant—tulips, hyacinths, croci, candidum lily, daffodils, gladiolus, English violet, primula, columbines, various ferns, vinca, euphorbia, perennial pea, flowering currant, lilacs, snowball, euonymus, roses, German iris, hollyhock, hibiscus, and many annuals. I was something of a specialist in growing pansies in rock-paved beds when I was about fifteen. In spring there were small flats of seedlings in the house, where there were always numerous "house plants" before the windows. We had an oleander, but no rubber plant. I suppose I planted the seeds of my two potted trees of orange and lemon at the age of four or earlier. They were inconveniently large before I went away and left them to whatever fate finally overtook them.

Perhaps one of the most influential portions of my botanical environment in very early years was our haphazard plantings of native flowering plants—spring flowers such as two hepaticas, bloodroot, Claytonia and the rest, two

yellow cypripediums, and so on; also the common weeds of the garden and roadside, like stellaria, polygonums, pigweed, amaranth, the smaller ragweed and others. The last-mentioned was esteemed in midsummer as furnishing a convenient added weight for the end of a kite's tail. We called many of these plants by their Latin names; I am unable to recollect a time when I didn't recognize the two hepatica species of that region as triloba and acutiloba, but I think I had reached high-school age before I had any name at all for *Ambrosia artemisiaefolia,* and I have never called that kite-tail plant ragweed without feeling bookish and pedantic to a degree.

Perhaps I took naturally to plants and their ways, but parents and older brothers and sisters had been and were specially interested in plants, and to their interest was due the peculiar immediate environment into which I came, and those early surroundings gave what was going to become my mental and philosophical leanings an unusual bent towards the field of plant science. But there were other influences. My parents had grown up on farms and they had the usual knowledge and interests of farmers, but we lived in town and my father had become a street-grading and sewer contractor for the rapidly growing community. In his business he dealt with soil and gravel and paving stones, with sand traps, gutters and gravelled road surfaces, with pick-and-shovel workers and teamsters, with plows, scrapers, paving hammers and an iron road roller that was drawn by a team of horses. He paved several long stretches of streets with cedar blocks, with coal tar poured into the joints, but I am sure he never saw a street paved with brick or asphalt or concrete; those things came later. My boyhood surroundings therefore included the grading contractor's equipment; tool boxes, of shovels, picks, saws, hammers, canthooks and such, came home in the fall, to remain in our backyard till they went out onto the "jobs" the following spring. My earliest memories are framed with tools of rusty iron, plows, scrapers and the interesting contents of those old tool boxes. The red-chimneyed kerosene lanterns were of special interest.

In those days, in that region, street grading and paving was all accomplished in summer and my father was at home most of the time in winter. In that season the tools were repaired for use the following summer. My father and older brothers were experts at the hand woodwork of these operations, and I cannot remember when I was not familiar with saws and planes, with draw-shaves and spoke shaves, with bolts and screws and nails, with wrenches and files. In my time we never had a forge, but I played about an ingenious shaving horse, which held the work when an axe handle or pick handle was being shaped from selected pieces of hickory wood. It is thus seen that my boyhood surroundings extended into the realm of simple mechanical operations as well as into that of gardening and plant lore.

Still another feature of those early surroundings lay in a home atmosphere of books, reading and ideas. There were many books about the home and they continually increased in number. There were generally a number of books from the public library. Every one in the family circle read and read. We had the complete file of Harper's Magazine and the subscription was always continued; for a number of years we had the old Century Magazine. Beginning when I was seven I read the St. Nicholas magazine pretty thoroughly for the next four or five years. The "big dictionary" (Webster's unabridged) was in plain sight on a table in our living room, and it was used almost every day. I think my first use of it was to elevate a chair seat when I sat at that table to play with letter cards, author cards, jack-straws or checkers. I became acquainted with the names of many literary personages and their pictures, also with their main titles, from that old game of "authors." We were never much interested in ordinary playing

cards, whose peculiar attractiveness has always been far beyond my mental capacity. Longfellow and Bryant were my first poets. From having them read to me, I knew by heart many of their passages before I was able to read. I attended a Congregational Sunday school regularly for many years, finally ceasing when the teacher of my class left town. Though some of the older brothers and sisters attended that church, I never cared for that popular sort of recreation, and my reading of the Bible was generally confined to the preparation of each Sunday-school lesson. I never knew either of my parents to attend church. Life flowed smoothly in our group, without the common religious precepts. I remember a few experiments with prayers when I was very young, but they always gave negative results. Only when a ministerial relative—of whom there were many, it seems—came to our home did I hear grace said at mealtime. It may be interesting to note that I cannot remember to have ever "believed" in Santa Claus though we always celebrated Christmas with many presents and an excess of good things to eat. In the later teens, for interesting information about how various peoples have tried to set up their backgrounds for thought, I dipped now and then into the great religious books: The Koran, the Hindu books, the Bible, the Book of Mormon, the Meditations of Marcus Aurelius, Plato's Dialogues and the like, all in English. After dozens and dozens of boys' books had been read, including C. C. Coffin's volumes on the American Revolution and the Civil War and stories like Little Lord Fauntleroy, Stockton's Tinkham Brothers' Tide Mill, Roe's Driven back to Eden, Carrol's Davy and the Goblin, I think my first serious venture in grown-up reading was Thoreau's Week on the Concord and Merrimac Rivers. I must have read the latter when I was fourteen, the winter before I entered high school. In the next half decade I read much of Thoreau, Emerson, Carlyle, and so on. Thoreau's writings may have influenced my development more than those of any other in those early days.

From the time I could handle hammer and saw I was forever "building things," following more or less in the path of my father and older brothers. A treadle scroll saw was mine and was in almost daily use throughout four or five years before I entered the high school. In the high-school period I built two canvas canoes, but never used them because available water was too far away. In that period I built some glass-covered trays for our butterfly collection (my older brother Luther's and mine) and made presses and cases for dried plants. Whenever mechanics could be observed at work I followed their every move. I am still greatly impressed by memories of the operations seen in a cooper shop and in a wooden-shoe shop passed on the way to school when I was about ten. I watched the neighboring blacksmith. A relative in a nearby village, whom I sometimes visited, was a cooper as well as a farmer. In early grade-school days I played games with other boys—such as leap-frog and pullaway—and made and flew kites in summer, built and used bobsleds, skated and caught rides on sleighs in winter. For a number of years I split and stored our firewood, kept the snow about our home shoveled in winter and ours and a neighbor's lawn mowed in summer. I earned my first money by making toy furniture and by mowing that neighbor's lawn. I still have a set of Shakespeare's works bought with that lawn-mowing money—but the morocco-leather backs are now mostly gone.

Before I began to attend school I had seen flies' wings and the like under a small compound microscope. It was in that same pre-school period that I brought some pieces of woody stems—such as those of raspberry—into our living room, at a season when the leaves were off, and cut thin sections of them

to examine under that little microscope. I think I was familiar with the appearance of cell structure and with tracheae of such small woody stems before I was eight years old. I was eight when I first went to school, where they started me in the third grade. Somewhat later my brother Luther had a larger microscope for a year or more, through which I was allowed to look now and then, but that instrument went away soon—it may have been borrowed—and my real beginnings in microscopy were in high-school days.

I suppose I must have learned a great deal in the grade schools, which I think were very good in that Michigan town at that time, but I am unable to recall many particulars of what was gained there. Orderly development of my conscious life seems to have begun with the high-school period, which was definitely formative for later things. It was of course in that period mainly that botany (Gray's Lessons and Manual), zoology, physics, algebra, geometry, chemistry, rhetoric, grammar, drawing and the like came really into my conscious field of mental vision. Following an English course throughout, I awoke to the need for other languages at the end of the second high-school year and I worked by myself at elementary Latin that summer and at elementary Greek the next summer, completing in school, as "extra work," the first three years of Latin and the first year of Greek. I never had any instruction in these languages beyond the reading of Cicero's orations and a little of Xenophon's Anabasis. Modern languages came later.

Almost from the very first I, more or less unconsciously, studied nature as well as books and people. Two older brothers were much interested in wild plants in a sort of quasi-scientific way, and I began to accompany one or the other of them on their Sunday trips to the woods as soon as they thought me old enough to go along. There were wild areas within easy walking distance of the terminals of several street-car lines. I thus learned very early to name most of the conspicuous wild plants of that region and to know where one went to find each sort. We specialized a bit on native orchids. I remember buckets and pitchers of *Cypripedium spectabile, Habenaria psychodes, H. fimbriata,* Calopogon, and so on, which were arranged about the home on Sunday evenings after such trips.

In the high-school course in botany each student made an herbarium of one hundred or so plants. By that time I was botanizing on my own. I added to that herbarium for a number of years, till I went to Ann Arbor; and I received ten hours of advance credit in botany for my herbarium when I entered the University of Michigan. Without any arduous study I came to know many plants and their habitats, and the use of books like Gray's Manual, before I entered college. I never received any instruction in systematic or taxonomic botany anywhere excepting in the half-year course in high school.

After finishing high school I spent a week at the first Chicago World's Fair, a good deal of it in the horticultural building; then went to Short Hills, N. J., to work as laborer at Pitcher and Manda's great nurseries—at six dollars a week. My older brother Luther—a cataloger and bibliographer—had been making catalogs for that nursery and had spent a year in Colombia and Venezuela collecting Cattleyas and shipping them north. He and I lived together at Short Hills for a year, in a friend's barn, which we transformed into a passable cottage. We did our own cooking and were vegetarians. I worked in most of the various nursery departments, hardy perennials, palms and ferns, seeds and bulbs, orchids (there were several acres of orchid houses), packing room. I learned to do most of the things gardeners do; in the early autumn I got out the plants for orders for hardy perennials. The hardy grounds comprised about seven acres—just beds and beds and cold frames and

cold frames *ad lib.* Everything was labeled with the Latin name. My brother Luther was the first to use consistently Latin names and actual photographs for commercial florists' catalogs, at least in this country. There were many houses of ferns, palms, anthuriums, chrysanthemums—a great exhibition of chrysanthemums every fall. We were always interested in knowing the regions from which the plants had come. In my year there the firm was about the first to introduce Japanese iris in this country; we were propagating the first twenty varieties received from Japan. Traveling collectors sent in plants from various parts of the world and there were traveling salesmen who covered this country. One man did nothing but take photographs of plants. Foremen of the various departments and these traveling men became known later as heads of nurseries —Manda, Lager, Bobink and Atkins, *et al.* Their names are still familiar to gardeners. That firm, the United States Nurseries, did a great thing for garden and greenhouse botany and ornamental horticulture. They had a large exhibit at the World's Fair—some thirty enormous tree ferns among other things.

But that nursery never paid, and Mr. Pitcher had financial reverses about 1903–4 and ceased to pay the annual deficit. So they tried to curtail, and my year there was in the period of curtailment. I suppose there were thirty or more men employed in my time. After I left, to go to college, the firm went out of existence and their large and rare plants were sold at auction; the big tree ferns, which had come back from Chicago, were sold at five dollars each, for the roof of the old Madison Square Garden. In my time they were selling off the specimen plants and we were propagating cheap stuff, to try to "make some money"—greenhouse ferns in flats and 2–3-inch pots by the thousand, palm seedlings, etc. I remember working with one other chap for two or three weeks doing nothing but pot tiny fern plants from flats to 2-inch pots. There was a large field of *Lilium auratum* and *L. speciosum,* where, for several days, I cut tubs full of flowers —to go to New York and be auctioned for a cent or two a flower. With a Polish boy, I collected a thousand plants of Solomon's seal on the hill west of East Orange, to go to somebody's estate in England. One afternoon I collected one thousand stems of golden rod, for some girl's wedding in New York.

Saturday afternoons, Sundays and holidays, in this Short Hills period, I spent botanizing in the neighboring region—the hills around East Orange, the Newark marshes, etc. In early summer of 1894 I left the nursery and botanized every day till fall, when I went to Ann Arbor. The herbarium grew apace, but I never added to it considerably after that summer.

At the University of Michigan there was no one who cared especially for the taxonomic botany of higher plants, though Prof. V. M. Spalding was sympathetic with a plan I had proposed to catalog the plants of the Ann Arbor region according to habitat. We knew little or nothing of ecology, the word wasn't known to us, but Spalding knew natural vegetation very well. There was a fair local herbarium. I worked at this plan by myself for a year or two. In the meantime I "did courses" in plant morphology, physiology, animal embryology, chemistry, physics, German, French, Italian, and the "required" courses in mathematics, psychology, etc.—just one short and unsatisfactory course in English composition. I had studied Spanish with my brother at Short Hills—he had recently returned from South America and liked to speak that language. Because of my plan concerning plant distribution I asked Prof. F. C. Newcombe to let me take his lectures in plant physiology, omitting the laboratory work, but he said that would be only a half-year course and it would do me no harm to know a bit about physiological experimentation; furthermore, he would predict that after I had

completed that laboratory course I would find physiology more interesting than any other field of botany. How he reached that prediction I can't tell, but I became his laboratory assistant the next year and have stuck to physiology ever since. Newcombe was not interested in ecology, which was then just getting started under the leadership of Warming and Schimper. Their books were not available to me till I went to the University of Chicago in the summer of 1899. I was a confirmed enthusiast for physiology by then.

Luther became well known as a bibliographer and specialist in rare books. He was first with Dodd, Mead and Company and then—in partnership with one of the Messrs. Dodd—with Dodd and Livingston. The last named firm had a store on Fifth Avenue for several years at a later time, till Luther became librarian of the Widener Collection, which had been given to Harvard University after Harry Widener had been lost in the Titanic disaster. From a wheel-chair, Luther saw the famous Harry Widener Memorial Room in the great Harry Widener Memorial Library before the building was finished, but he died before he occupied it. At the present time the librarian in charge of that room is Luther's widow, who writes as Flora V. M. Livingston. My eldest brother, Lincoln, was a lawyer, first in Grand Rapids and then in Denver, but he died in middle age, after developing a law library in the last-named city.

The first summer vacation of my Ann Arbor period was spent in New York City, where my then widowed mother and I visited my brother Luther and a sister who was living with him, on 21st Street, near Ninth Avenue. Those months I devoted to a library study of plant geography. Prof. N. L. Britton gave me permission to study in the library room of the old botany building of Columbia University, on 49th Street, where I was to be found almost daily that summer, reading in many of the classic treatises on plant geography, De Candolle, Grisebach, and others. At that time I first became acquainted with N. L. Britton and John K. Small, both of whom exerted a good deal of influence on my development. Columbia University soon moved to Morningside Heights. In that connection, I remember well a rainy Saturday afternoon in the summer of 1894, when Luther and I explored the woods and fields where the great university was going to make its home, as had recently been announced in the newspapers.

After leaving Ann Arbor, in June, 1898, I got me a job teaching an array of sciences (physics, chemistry, human physiology, physical geography, botany and zoology) in the high school at Freeport, Illinois. I found time to explore the neighboring country (with a bicycle), but did little with botany as such. I kept my alga growing in cultures in my bedroom window; the alga had been brought from Ann Arbor (discussed more fully below). In the late winter of 1898 I made applications for fellowships in about ten universities and received three appointments that spring—at Harvard University, University of Wisconsin, and University of Chicago. I became a fellow and assistant in the last-named institution, going there about the end of June. Prof. C. R. Barnes had been called from the University of Wisconsin the year before, to take charge of plant physiology and to edit the BOTANICAL GAZETTE. He gave very excellent lectures for elementary and advanced students; he prepared a set of laboratory outlines on the Detmer plan, which have become classic through his students, "grand-students," and "great grand-students." The conducting of the laboratory work was my own responsibility; on arrival, I was given the necessary keys and some outlines and was told that "the laboratory class would appear next Tuesday." Throughout the next four years, with some vacations, I conducted that laboratory, on the fourth floor of the Hull Botany Building near 57th Street, consulting Professor Barnes at times, but in all that period I think my

chief was never in the laboratory rooms more than perhaps twenty times. When he did appear he showed quite wonderful facility in handling experimental apparatus, but he always hurried away and he never interfered. He left the laboratory to his assistant but he always seemed to be aware of all that was going on there. On an early occasion, when I had asked him whether things were going forward as he wished, he replied, "You may rest assured that I will let you know if I think anything is going wrong." It was through his lectures and personal conversations that Barnes's students received guidance and inspiration from him, and through the sometimes peppery reviews that he prepared for the BOTANICAL GAZETTE. I think Barnes's mental processes and his facility in presenting his thoughts were more nearly perfect in precision, clearness and completeness than those of any other person with whom I have ever worked. I was indeed fortunate to be his first active assistant at Chicago. His publications in plant physiology were few, but his mind and personality exerted a profound and lasting influence on the development of this science.

At that time Goodale held a professorship in plant physiology at Harvard University, but Newcombe, MacDougal and Barnes were, I think, the first with that title to devote themselves primarily to physiology in this country. In these early years of the present century, plant physiology was in the incunabula or even embryonic stage of its development here, although it had already become a distinct science in Germany, especially at the hands of men like de Vries, Sachs, Detmer, and Pfeffer. Newcombe had been inspired by Pfeffer and it seemed to give the fine old Geheimrat some pleasure when, in 1908, in his Leipzig laboratory, I remarked to him that I was his "intellectual grandchild." The three volumes of Ewart's classic English translation of Pfeffer's Pflanzenphysiologie appeared in the very earliest years of this century. In those times American botany had, in most places, already relegated plant taxonomy to the basement, and its main quarters were occupied by Strasburger morphology and the beginnings of cytology; physiology was still generally struggling in attic rooms, and ecology—a sort of regenerating outgrowth of taxonomy, floristics and plant geography—was just beginning to be considered. It was notable that three professors of plant physiology were appointed about 1909, at Washington University (St. Louis), at Harvard University and at The Johns Hopkins University. A number of assistant professorships and instructorships in the new science were already in existence in this country and interest was turning in that direction.

Throughout my Chicago period I was greatly attracted toward the field of ecology, which was being rapidly developed there under the general leadership of Professor J. M. Coulter and at the hands of the young H. C. Cowles. Like Asa Gray, Coulter had realized the possibilities of the newer phases of botanical study that were attracting so much attention in Germany. A taxonomist himself, he encouraged Chamberlain and Cowles—who were among his earliest Chicago students—to devote themselves respectively to the morphology, histology and cytology of reproduction and to ecology. Cowles was coming into ecology from geology and plant taxonomy (see ECOLOGY for July, 1935) and his main interests lay in the details of local plant distribution. That was exactly the field towards which I had been groping, with the encouragement of Spalding, in the early part of my Ann Arbor period and it inevitably appealed strongly to me when I became intimate with Cowles at the time of his classic sand-dune studies. Cowles's great contribution was toward what he called physiographic ecology, which dealt with local features of distribution in terms of the physiographic characteristics of the corresponding plant habitats; he and his students studied vegetation rather than

plant individuals or species, in bogs and swamps, on beaches, flood plains and bluffs, on moraines, in ravines. Glacial topography was just as important in this as was plant taxonomy, and the concepts of plant physiology always entered into these studies to a considerable degree. For that period I add the name of Cowles to those of Newcombe and Barnes as representing the personal contacts that most influenced my own scientific development.

In my last year at Ann Arbor I had carried out a somewhat elaborate experimental physiological study on the influence of the osmotic characteristics of liquid culture media on the growth and development of a small polymorphic freshwater alga, and that study was continued throughout my Chicago period. (See "The rôle of diffusion and osmotic pressure," Chicago, 1903.) That was also, of course, an experimental study in the ecology of that alga, and it turned out that the polymorphism of the latter might be controlled—as we used to say—not only by the osmotic characteristics of its environment but also by environmental characteristics that influence water absorption and water loss; a relatively strong solution of non-toxic solutes acted on the cells as a drying agent and similar effects might be produced by exposure to the air (evaporation).

It was thus natural and easy to try to find for ordinary plants relations of vegetational differences to the drying influences and water-supplying influences of air and soil. I had the thought that the physiographic conditions emphasized by Cowles might be effective largely in that way. For the pine plains of Michigan, which I studied in the summer of 1901 (see BOTANICAL GAZETTE, 1905), it turned out that upland vegetation types were rather nicely correlated to the water-holding capacity (size of particles) of the surface soil, which appeared to determine water supply and soil aeration, for the macro-climate is essentially the same for the whole of the region considered. This seemed to mean that upland vegetation differences in those plains might be mainly due to differences in soil-moisture content in summer; sandy uplands were generally dry unless the soil was much modified by humus, and clayey uplands were generally moist, the more so as the humus content or covering of the soil was greater. Lowlands were generally moist or wet whether the mineral constituents of the soil were sandy or clayey—because there the subterranean water table was at the soil surface or not far beneath it.

That bog water might perhaps differ from the water of drained swamps by having a much higher osmotic value had been deduced from Schimper's brilliant but cryptic observation that bogs are "physiologically dry." That deduction was easily shown to be untrue—by cryoscopic tests carried out on samples of water from various bogs and swamps of Michigan and New Jersey. These waters were then tested by employing my alga as an indicator and it emerged that water from about the roots of typically bog plants acted on the alga as if it contained stimulating or slightly toxic solutes. Bog waters were generally more acid than the others (by titration tests; H-ion concentration and the now familiar pH did not appear on the physiological and ecological stage till somewhat later), but their stimulating or toxic influence was apparently not directly related to total acidity. Perhaps I was dealing to some extent with some cryptotrophic or cryptotoxic solutes in bog water, presumably of organic nature, and my accounts of these studies now seem to suggest substances more or less resembling some of the hormone-like substances now being studied so vigorously in both animal and plant physiology.

While at the University of Chicago I listened to several series of lectures on plant morphology given by Coulter, an inspiring teacher whose talks and writings are fine examples of excellent presentation. What I remember of Coulter centers about his fine personality and his remarkable facility of expression. Of course his fundamental philosophy or out-

look on things in general was not physiological. He always tried to avoid the implications of teleology but one felt that he was perhaps only half-hearted in that. Barnes's attitude towards the problems of causation—in plant activity, development and evolution, and apparently in everything—was similar to that shown in Pfeffer's later writings and in those of Loeb; light on problems of determination and causation was to be sought in terms of the properties or characteristics of matter and energy, and teleology and anthropomorphism were to be avoided wherever the nature of language would permit. I was afterwards much impressed and encouraged in my attempts at clear thinking and writing by Verworn's little essay on Kausale und Konditionale Weltanschauung, but encouragement came to me mainly in those years from Barnes and Loeb.

Loeb's general course in physiology broadened my field of intellectual and philosophical vision very greatly. My *Weltanschauung* seems always to have been of the etiological rather than of the teleological sort, but most writers whom I read continually indulged in teleology or even in out-and-out mysticism. Overenthusiastic as he often was, Loeb's characteristic approach to physiology from the standpoint of physical and chemical concepts represented a remarkable forward step in the fundamental biological thought of that time. It soon became a sort of vogue to point out where his interpretations and generalizations were far two simple to find full support in the results of subsequent experimentation, but what we call his mistakes of overenthusiasm turned out to be valuable contributions; where he went too far he led others to proceed more slowly in the same direction. I think I perhaps received somewhat more edification from Loeb than from Barnes, but both men exerted enormous influence in my formative period. There were times when I decided that animal physiology should be my field, but Loeb always advised against a change. Although he was always enthusiastically sympathetic with my problems and findings, he did not offer me a fellowship in his laboratory, and so I remained in the plant physiological field.

From chemical studies of a quasi-research nature under the guidance of Gomberg (at the University of Michigan) and of Lengfeld and Stieglitz (at the University of Chicago) I gained much in appreciation of the physical and chemical bases of vital phenomena. As is generally true for any group of young men studying in a university—or elsewhere, for that matter—the graduate students and instructors at the University of Chicago exerted great influence on the mental development of one another. I gained much from intimate friendship with men like H. N. Whitford, W. B. McCallum, H. Hasselbring, J. B. Overton, O. W. Caldwell, J. P. Goode, A. W. Greeley, and the rest.

My native instincts were perhaps as much bent toward literary art as toward the intellectual field of science. Language and its use have always claimed much of my attention by the way but, aside from elementary high-school and college courses in Latin, Greek, German, French, Italian and English, my education in this general field was informal and self-conducted. I have always been wont to devote a good deal of time each week to general reading of eminent and less eminent writers. Never becoming expert with any foreign language, I dabbled superficially in many, being able to read easily only German and French, however. It has always been pleasant to study the dictionary, especially for etymologies and the finer shades of word meaning.

A part of one summer of my Chicago period was devoted to teaching botany in the Eastern Illinois State Normal School, at Charleston, where I had many very pleasant and inspiring conversations with the president of that school, L. E. Lord.

Toward the end of my Chicago period I spent an autumn at the New York

Botanical Garden, in an experimental study of the influence of a number of inorganic salts on the polymorphic green alga mentioned above. It was at that time that I became acquainted with MacDougal, Lloyd, H. M. Richards and C. C. Curtis, all of whom were very helpful and encouraging. The following summer, and a grant from the Carnegie Institution, were spent in study at the new Desert Laboratory, with Cannon and Lloyd. From the study of desert plants came many new ideas, especially with respect to water supply and water loss. My interest in osmotic pressure and soil dryness as environmental features was broadened to include evaporativity and the drying influence of sunshine. The porous-porcelain atmometer was devised and first used by me at that time—but I afterwards learned that it had been independently devised at least twice before, by Babinet and by Mitscherlich. After the experiences of that first summer at Tucson transpiration and water supply were among the topics that I found most interesting. They enticed in many directions.

Returning from Tucson in September, I spent the following autumn at the U. S. Bureau of Soils, in Washington, where I gained much from conversations with Whitney, Cameron, Schreiner and a number of other members of the bureau staff. We were studying the possibility that some soils might contain chemicals that acted to retard plant growth therein. I returned to Chicago for the winter quarter at the University and then received my first appointment to a permanent position, in the Bureau of Soils. That position was not very permanent, however, for I resigned and entered the staff of the Desert Laboratory the following January. MacDougal was the newly named director of that laboratory and my studies in physiological ecology—especially on relations of plants to soil moisture and to evaporation—were continued with his encouragement. I remained at Tucson till the next December, spent that winter and the early spring in eastern institutions, partly in library studies but mostly in experimentation at the Missouri Botanical Garden, at St. Louis, where I gained much from pleasant contacts with William Trelease, H. von Schrenck and J. A. Harris. That St. Louis period was devoted to a study of transpiration in greeenhouse-grown cacti. The year 1908 was spent in Europe, mainly at Munich, where I learned a great deal from von Goebel's lectures and field trips and from Hegi's botanical-geological excursions. Renner, who was studying the physics of plant transpiration at that time, was my frequent companion and I owe much to his keen mind. At Munich I was engaged in studies on soil moisture and it was there that I first devised and tested the porous-porcelain soil-moisture "meter," which has recently been independently developed by Rogers and others. In the autumn I spent a few weeks at Pfeffer's institute at Leipzig and had the good fortune to have some conversation with the Geheimrat almost daily. I also visited and became well acquainted with Klebs and Glück, at Heidelberg, and made tourist excursions through Germany and Switzerland. In the early winter I spent a few days at the Rothamsted Station in England, where Hall was then director. At the end of 1908 I attended the Baltimore meeting of the American Association and associated societies on my way back to Tucson. It was at the time of that meeting that I first saw the late Professor D. S. Johnson's recently established garden, on the newly acquired Homewood tract which was to become the home of The Johns Hopkins University. At the close of that meeting Johnson led a day's excursion through the snowy woods and fields north of Cockeysville, Maryland, in which Lloyd and I took part. That was a memorable occasion; we were almost cold and became more than almost hungry, but were withal exceedingly enthusiastic

over many different sorts of botanical observation and discussion.

Another memorable excursion from which I gained a great deal was from Tucson to the Salton Sea, in the early summer of 1909. It was arranged by MacDougal partly for several visitors. We spent a day or two around Yuma, a day at Yuma Junction and a week in camp near the U. S. Weather Bureau's evaporation station, where we became acquainted with the field studies, in evaporation, of Bigelow and his associates, who were very busy there with pans and tanks, anemometers, thermometers, and psychrometers. We operated atmometers, examined the neighboring desert hills and the newly-formed beach, visited Pelican Island and the place on the shore from which pumice fragments, worn to rounded pebbles and boulders, were floating away. I afterwards referred to those floating stones as notable examples of a remarkably perfect "adaptation" in the realm of the non-living.

While on that trip I received a forwarded telegram asking if I would consider an appointment as professor of plant physiology at The Johns Hopkins University, and my affirmative reply was sent, on a Sunday evening, from the railway office at Yuma Junction (where the branch for El Central leaves the main railway line), while the west-bound train halted for a quarter-hour; the appointment was authorized in Baltimore on the following day.

In the early fall of 1909 I attended the Winnipeg meeting of the British Association, after which Blakeslee and I made a short trip by boat, north from Kenora, on the Lake of the Woods, and I then became a resident of Baltimore. At the Winnipeg meeting I became well acquainted with Yapp, who had been studying evaporation influence on a spiraea and other plants, also with Gwinne-Vaughan, Wager and others from abroad.

The first year at The Johns Hopkins University I lectured in the old Biology Building on Eutaw Street (the place is now a parking lot) and conducted a laboratory course and research in the small building and greenhouse which already stood on the Homewood grounds, at the south side of the new botanical garden. Among those who studied with me that first year were F. H. Blodgett, L. A. Hawkins, W. D. Hoyt, W. R. Jones, L. W. Sharp, H. H. York and W. H. Brown. Brown [3] is about to come from Manila to be lecturer in botany here. Our group was an ideal one, working in an ideal atmosphere, and I gained as much from those students as they gained from me.

With the help of Hawkins and others, I soon devised and constructed the first rotating table for use in the standardization of atmometers. That table, which is eight feet in diameter, first stood in the attic of the small building just mentioned; it is now on the upper floor of the Laboratory of Plant Physiology (fig. 2), where it is still occasionally used. Two or three years later Shive and I introduced rotating tables for series of simultaneous plant cultures, and such tables for equalizing aerial conditions in experiments with plants soon came into general use in many laboratories. After Sam F. Trelease and I had introduced the employment of continuously flowing solutions in solution-culture experimentation it became desirable to arrange for continuous flow in cultures on rotating tables, but that was not actually accomplished until 1934, at the hands of Ch. Zinzadze and Karl A. Grossenbacher.

The present laboratory was completed in the early winter of 1911–1912. My personal study throughout much of the preceding year had been devoted to the interior details of this building; all cases, tables, sinks, the electric wiring, and so on, had been specially designed, to produce a small, compact and very efficient working unit. In a great many of the

[3] Brown died at the end of that academic year.

FIG. 2. The Johns Hopkins University Laboratory of Plant Physiology at Homewood, Baltimore, Maryland.

arrangements I was aided by students, most of whom were good mechanics. The laboratory has served its purpose excellently for a quarter of a century.

For several years after coming to this University I returned to the Desert Laboratory each summer for a period of three or four months, continuing along lines of study that had been opened in my Tucson period. Each of these summers I had, through small grants of funds from the Carnegie Institution of Washington, the help of one or more assistants —young men who were about to receive, or had just received, their Ph.D. degrees, either here or elsewhere. Following is a list of these men, to all of whom I owe a great deal: W. H. Brown, J. S. Caldwell, E. M. Harvey, H. E. Pulling, J. W. Shive, A. L. Bakke, H. C. Sampson, E. S. Johnston, J. D. Wilson, L. J. Pessin, M. Fraps.

The summer of 1915 was spent in the region between The Pas and Split Lake, Manitoba, on a canoe trip, in company with H. E. Pulling. The new Hudson Bay railroad was partly constructed—as far as the bridge, at Manitou Rapids, on the Nelson River—and we had the benefit of transportation aid from the men in charge of construction and fire inspection. Without any guide, we traveled slowly on lakes and rivers from Picotinay to Split Lake Post and then ascended the Nelson past Grand Rapids to Manitou Rapids, whence we returned by railway. On that trip we gained an excellent general knowledge of the vegetational conditions of that general region.

After the new railway had been completed to Churchill, Mrs. Livingston and I visited that place, in the summer of 1935, where we lived for a week in a room in Mrs. Riddock's woodshed and explored the nearby region, with help from the officials of the new grain port. Although I had for years a desire to visit the country beyond the northern limit of forest, I never really passed beyond that limit, which is apparently not far north of Churchill. My "farthest north" is the old Hudson Bay Post at Fort Churchill, across the river from the new town of Churchill. I was much impressed by

seeing a large plant of the small white lady's-slipper in full bloom (about July 6, 1935) at the side of the age-old trail on the north side of the Churchill River; we also were interested in the gardens of residents of The Pas, where, in early July, we saw the yellow cypripedium and *Cypripedium spectabile* in full bloom, also the most vigorous plants of the perennial delphinium I have ever seen anywhere.

On that second trip to the North we motored from Baltimore to Winnipeg, attending the Minneapolis meeting of the science societies on our way, and, returning from The Pas, we motored from Winnipeg to Duluth, took a lake steamer to Sault Ste. Marie and then motored back to Baltimore from that point, stopping at Mackinac Island and other places.

With Mrs. Livingston I spent the summer of 1921 at the Desert Laboratory, continuing earlier studies with Forrest Shreve. The book on climates and vegetational types of the United States, which had occupied Shreve and me for a period of ten years, was published in that year.

The summers of 1922 and 1926 were spent at the Desert Laboratory, but I have not been there since.

To complete this survey I add below a list of those who have received the Doctor's degree with me at The Johns Hopkins University.

DOCTORS OF PHILOSOPHY IN PLANT PHYSIOLOGY FROM THE JOHNS HOPKINS UNIVERSITY, 1909–1939

(*Date of degree in parentheses*)

Coggeshall, Mary	(1931)
Darrow, G. M.	(1927)
Edwards, T. I.	(1932)
Espino, R. B.	(1919)
Fawcett, H. S.	(1918)
Free, E. E.	(1917)
Gericke, W. F.	(1922)
Haasis, F. W.	(1928)
Hawkins, L. A.	(1913)
Hildebrandt, F. M.	(1917)
Hutchins, L. M.	(1924)
Johnson, E. S.	(1917)
Lawrence, D. B.	(1936)
LeCompte, S. B., Jr.	(1939)
Mack, W. B.	(1929)
Marshall, Robert	(1930)
Matz, Julius	(1932)
McCall, A. G.	(1916)
McLean, F. T.	(1915)
Norem, W. L.	(1936)
Pardo, J. H.	(1932)
Shive, J. W.	(1915)
Swingle, Charles F.	(1927)
Tang, Pei-Sung	(1930)
Tottingham, William E.	(1917)
Trelease, S. F.	(1917)
Veerhoff, O. L.	(1937)
Veihmeyer, F. J.	(1927)
Verner, Leif	(1934)
Wilson, J. D.	(1926)