tions should now be made by educators and scientists. Paperbacks have been absorbed into teaching and educational programs on a haphazard, individualistic basis. More considered plans and recommendations for their use could be helpful, along with carefully evaluated reading lists. (Some efforts have been made along these lines in the *Inexpensive Science Library* booklet, put out by the American Association for the Advancement of Science.) Suggestions for further paperback republication of monographs and treatises are not lacking, and all that is needed in that category is continued support through purchase by individuals, schools, and libraries. Encouragement of a more active nature is needed if the many gaps in popular scientific literature are to be filled.

Although the paperback is only a slight modification of the traditional book it may very well be a more powerful tool in scientific education than recent developments which assume a more modern and complex form. The book, like the wheel, is a magnificent contribution of the past which still has a great potential and which is not likely to be superseded in the foreseeable future. Paperbacks have reminded us not to take books for granted but to exploit them with enthusiasm and imagination.

Note

1. During the past 50 years there have been similar developments in European literature, such as Ostwald's *Klassiker, Sammlung Göschen*, and the *Que Sais-Je* series. These parallel developments did not appear directly to affect the trends in America and England, except perhaps to afford a stimulus to individual editors and publishers.

Potpourri and Gallimaufry

Being a Random Assortment of Anecdotes about Greater and Lesser Scientists

Paul E. Klopsteg

During the years which have passed since my becoming identified with science in general and physics in particular, many events of a sort not usually publicized have come within my experience and are stored in my memory. I remember them, probably, because of the unusual personalities with whom they are associated, and because the events themselves are not commonplace.

Memory is not a dependable source of facts amenable to proof. In such a recital as this, however, unswerving adherence to facts is not demanded. Anecdotal material does not call for a notarized statement attesting to its complete factuality, but this does not imply that the stories are inventions. I have endeavored to state forthrightly what I have been able to draw from my recollections. These word pictures may help to humanize notable personages in a way which could not be done in research papers or other more formal presentations.

The author was a member of the Governing Board of the American Institute of Physics for 16 years, and its chairman for 7. He is a past president of the AAAS.

Robert A. Millikan

My first story is about Robert Andrews Millikan. At the time, he was a demigod to a beginning graduate student, although his image became more terrestrial with growing acquaintance. It was in Minneapolis, at the 1910 meeting of the American Association for the Advancement of Science, that Millikan was scheduled to give his first paper on the oil-drop experiment for measuring the charge of an electron. My studies were in part financed with money I earned as lecture assistant to John and Anthony Zeleny, professors of physics at the University of Minnesota.

Millikan's talk was to be given in the main lecture room of the physics building, and it was to be illustrated with lantern slides. Because the 750- or 1000-watt bulb with concentrated filament had not yet been invented, the carbon arc was the universal light source for projection lanterns. The arc was controllable in position, if not in steadiness, by concentric adjusting knobs at the rear of the projector. My job made me ex-officio projectionist, for which my undergraduate work in electrical engineering helped to qualify me as an expert.

In his talks Millikan was somewhat wordy and rambling, and no time table for showing the slides had been provided. Since the arc could not be turned on and stabilized quickly, I kept it ready for instant response to the speaker's signal. Millikan's discussion continued at such length that the rapid consumption of the carbons gave me increasing anxiety. By perverse coincidence, the signal for the first slide came just as the arc failed. Changing carbons without getting one's skin seared by contact with hot metal required most deliberate care, and a minimum period of perhaps a minute, with another quarter of a minute added before the arc could be adjusted properly. During this nervous interval Millikan made some uncomplimentary comments while waiting impatiently for the first slide. I am sure that his remarks, in relation to what he felt, were restrained, and represented magnificent self-control; for Millikan was a religious man.

A. Wilmer Duff

During World War I Millikan was a lieutenant colonel in the Signal Corps, in charge of a research and development group which, in miniature, was a precursor in purpose if not organization of the National Defense Research Committee of World War II. The group worked under the auspices of the newly created National Research Council. Among its members were F. C. Brown, A. Wilmer Duff, L. P. Sieg, John T.

Tate, and David L. Webster. One of the projects, carried on at Langley Field, was bombsighting from aircraft. Webster was the pilot who, in addition to contributing to excellence of data by his skill in flying the experimental craft, also gave his scientist-observers many a thrill if not cataleptic reaction by stunting after a run for obtaining data.

Duff, a genial, delightful Scotsman, frequently came to Washington, where I was working for the Ordnance Department at the Bureau of Standards. He is remembered, among other things, for Duff's *Physics,* a text I had used in teaching. During one of his visits we went to lunch at the University Club, then located in the building which was later acquired by the United Mine Workers as headquarters. In the dining room we were shown to the one unoccupied table, set for four, and presently two young men were shown to the other two places. There were perfunctory greetings, but no introduction. The newcomers began exchanging ideas about their experiences in college, and one of them commented that he supposed physics was all right, but for him it was rough because of "that damned Duff." Duff chuckled inwardly, arose from his chair, bowed, and said, "Gentlemen, I'm Duff!" Thus began a warm camaraderie which continued through the luncheon period.

Anthony Zeleny

Anthony Zeleny has been mentioned. He was a man of much charm and great earnestness about things in which he believed; and his convictions were powerful. His principal aversions were tobacco, liquor, and social dancing. Students quickly discovered that a well-contrived question could elicit a lecture on morals which might occupy a large part of the class period.

When Jack Tate came to Minnesota, he brought with him the pipe-smoking habit, which to Zeleny was hardly less "filthy" than the cigarette habit. Tate soon learned that the aroma of burning tobacco in the physics building, however slight, was quickly detected and traced to its origin, and that a mild reprimand was administered by the professor to the instructor. Tate's ingenuity was equal to the predicament. For his research, he promoted the use of a laboratory with a ventilating shaft having a grille. He located his desk in such relation to the latter that the fumes from the pipe were completely aspirated and carried up the shaft.

Zeleny's devotion to his concept of moral principles seemed to bear an inverse relation to his sense of humor. After one of his demonstration lectures in which a comment of his had stirred the risibilities of the class into loud guffaws, he asked me to explain why the remark had precipitated such hilarity. I did so, and he indicated comprehension, with thanks for my effort. His understanding of the matter must have faded, however, for he used the same remark in the same lecture each succeeding year, with the same results. It came in connection with a demonstration of electrostatic shielding, the so-called ice-pail experiment.

A gold-leaf electroscope is enclosed in a metal can with apertures on opposite sides to permit the image of the electroscope leaves to be projected. The demonstrator then discharges miniature lightning flashes from an electrostatic machine to the can. The electroscope shows not the slightest charge, showing that the inside of the can is completely screened against external fields. The comment was, "Ladies and gentlemen, when you perceive a thunderstorm approaching, you must rush into a can."

Lee DeForest

About 1919, while living in Philadelphia, I attended a meeting of the Franklin Institute, at which Lee DeForest was the speaker. DeForest related the history of the vacuum tube, tracing it from the Edison effect through the Fleming valve to his audion with the third electrode, and described his invention of the regenerative circuit. After he had concluded and questions were invited, a man in the audience arose, went forward to the lecture desk, and said, " All the things that Mr. DeForest has mentioned are very interesting, but . . .," then launched into a vehement discussion to persuade the audience that it was not DeForest, but he, who had invented the regenerative circuit. The discussant was Edwin H. Armstrong, pioneer in radio and later the inventor of frequency modulation; he was engaged in patent litigation with DeForest over the feedback principle. After the dispute had gone on for some time, the chairman, searching for a way out of a less than amiable situation, recognized a teen-age boy. The boy's question was, "Mr. DeForest, how much would it cost to build a set that would receive over a hundred miles?" DeForest commented that he couldn't say, because that depended also on the signal strength of the transmitter. To this the lad replied, "What would you say, Mr. DeForest, if I told you that me and my pal built a set like that for eleven dollars?" The laughter which followed relieved the mounting tension and gave the opportunity for the chairman to adjourn the meeting *sine die.*

National Academy of Sciences

When the National Research Council was established in 1916, there were some scientists who looked upon it as an agency for the regimentation of research. One of these men, less inhibited than some of his fellows in making his views known, wrote them down and submitted the piece to *Science,* where it was published. The author was William Morton Wheeler, professor of economic entomology at Harvard. Although I have not looked up the article in the *Science* volumes of 45 years ago, I have never forgotten his asperitive comments. He referred to the National Research Council as "that superorganization of superorganizers," and to its parent, the National Academy, as "a distinguished organization of distinguished scientists whose principal occupation is writing each others' obituaries, and it's a pity they don't have more to do."

My recollection of Academy meetings includes one immediately following World War I. One of the papers was a demonstration, with a report of mechanical, thermal and acoustical characteristics, of a tuning fork made of fused quartz. In the audience was Alexander Graham Bell, at that time about 72 years old and quite hard of hearing. Also present was Dayton C. Miller, who had done noteworthy experimental work on the quality of tone from flutes as affected by the materials of their construction. Miller, being solicitous about Bell, and rightly assuming that he had been unable to hear the fork during the demonstration, borrowed it from the speaker and brought it down to show him. Hopefully, he would try to have Bell listen to it at close range. Although decibels had, at that time, not come into general use, Miller realized that quite a few of them would be needed to make the fork audible to Bell, which led him to strike it with so much vigor that one prong was broken off. Although the private demonstration failed, Miller's intentions were of the best.

Captain DeKhotinsky

At the turn of the century, there was a coterie of young men in Chicago who occasionally "went out on the town" for a good time. Most prominent among these was A. A. Michelson, who seemed to have an affinity for individuals with great skill in optical and mechanical craftsmanship. The other four were such craftsmen: Edward Petididier, an optical technician; Albert Porter, constructor of fine apparatus; William Gaertner, founder of the Gaertner Scientific Corporation, who had been the mechanical "right hand" of Samuel P. Langley, and who had built the first powered aircraft for Langley which should have flown but failed to do so for other than aerodynamic reasons; and Achilles DeKhotinsky, a former captain in the Russian navy, who had come to America and become instrument maker at Ryerson Laboratory during the gay nineties.

When I joined Central Scientific Company in 1921 to take over responsibility for its research and development along with production, I found that I had "inherited" as a development engineer none other than Captain DeKhotinsky, then 71 years old. He was a man of aristocratic bearing and pride almost to the point of arrogance, completely disdainful of workmanship which failed to measure up to his exacting standards. He was himself highly skillful in all the known laboratory arts, and had ruled some of the first six-inch diffraction gratings. "The Captain," as he was known around the organization, had many other accomplishments about which he liked to talk when I had time to listen. He showed me a Belgian patent issued to him on a pasted storage battery plate which predated the Planté process. Among scientists his name was probably best known for the laboratory cement which he had invented, and to which his name was attached. His speech and diction bore a strong flavor of his native tongue. Among his irreverent associates in the organization the cement was known as "Tsementski." Its composition and production were still secret some 40 years after its invention, shared only with a stepson who produced it, with CENCO the exclusive distributor.

According to The Captain, the cement was invented because of necessity. He needed a sealing material for making electric light bulbs which he had used in street lighting in Vienna several years before the invention of the carbon filament lamp in Edison's laboratory. DeKhotinsky's bulbs used a thin graphite rod as the incandescent element, hermetically sealed in a vacuum with the cement. Later the cement proved itself so useful in various applications that it became a standard supply item in many laboratories.

As captain in the Russian navy in the late 1870's, DeKhotinsky had been assigned to supervising and expediting the construction of a couple of battleships for which the Tsar's government had contracted with an American firm in the New York area. This tour of duty gave him his first contact with the United States, and, being observant and inquisitive, he used his spare time to visit places and see things that interested him. One visit was to Menlo Park, N.J., to Edison's laboratory. Edison was absent, but an assistant showed the visitor around, and described what was being done in trying to develop means for producing light by incandescence produced by an electric current. Platinum wire was being used. DeKhotinsky, having in mind his bulbs with graphite rod elements, said, "Vy don't you use car-r-rbon?" He told me that several weeks later patent application was filed on the carbon filament lamp, in which the filament consisted of carbonized organic material such as thin bamboo strips. He liked to believe that his remark about carbon gave Edison the idea of a carbon filament. Perhaps it did.

A sequel to this story is that the Edison patent or patents were sold to the General Electric Company which improved the product and developed the production process for making the bulbs. DeKhotinsky had by this time completed his navy assignment, had received commendation and a jewel-encrusted watch from the Tsar for his good services, and decided that his future lay in America. He immigrated in the early 1880's and started an incandescent lamp factory at Marblehead, Mass. After he had begun production, economic lightning struck in the form a patent infringement suit, and his manufacturing venture had to be closed out. The experience, as well it might, seemed to have left him a permanent scar of embitterment.

When I first knew DeKhotinsky, in 1921, he was very much "the cat that walked by his lone." He shunned those in the organization whose status he regarded as inferior to his, which included most of the personnel; but he seemed to enjoy visiting with those whose authority he recognized. He made designs and sketches of experimental models, and the shops constructed them, but never to his satisfaction. His stock comment was that they "bugger-r-red" his ideas. He was obviously not too happy in his work.

About 1924 the physics department at the University of Michigan, under the leadership of my old friend H. M. Randall, began a research program in fine structure in the far red and infrared. This created a need for precision-ruled diffraction gratings to produce maximum intensities in the regions selected for study. Randall inquired whether I could put him in touch with an instrument maker who could build a ruling engine and produce the gratings needed.

Although The Captain was already 74 years old, it seemed a made-to-order position for him; it would give him release from surroundings which were obviously not congenial, and challenge his skill in a way which could warm his enthusiasm and stir his pride in accomplishment. I recommended him to Randall. He was hired, went to Ann Arbor, and there spent a number of years in great contentment, doing precisely what he liked to do, and giving satisfaction to the group for which he worked.

R. W. Wood

The mention of gratings brings to mind Johns Hopkins, and Hopkins brings to mind R. W. Wood. His office at the university contained the usual desk, table, chairs and shelves. All horizontal surfaces were piled high with papers, unopened journals, notebooks and whatnot. On each of these storage areas the limiting angle of repose for the materials in question was established, though not by design or intent. One more of the items thus stored, if placed on any one of the piles, would slide off to the floor.

Probably no scientist has had more stories told about him, and probably none has told more stories about himself than Wood. Some 15 years ago a book was published with the title, *Dr. Wood,* by William Seagrave, who had written two earlier books: *Asylum* and *Voodoo.* The unkind comment was made at the time that this completed the trilogy. Many of the anecdotes in the book were told exactly as they had been related many times by Wood himself. They were accounts of unusual and mostly amusing events. One which Wood told me was not included.

He expressed the view that if he were remembered after his death it would be not for his accomplishments in physical optics, but for his authorship of a small book with board covers, published in 1907 with the title, *How to Tell the Birds from the Flowers.* Each page had whimsical sketches of a bird and a plant which resembled the bird, with doggerel verses pointing out the differences. Wood had done both the sketches and the rhymes and was proud of his authorship.

Sometime in the early 1920's he had a Japanese graduate student at Hopkins. One day the student, with some diffidence, said, "I understand, Dr. Wood, that you wrote book about how to tell birds from flowers." Wood acknowledged having done this, and went on to say, "The little book has long been out of print, and if you should locate one in a bookshop, you would probably have to pay as much as ten dollars for it." The student thanked him. A few days later he returned and said, "Dr. Wood, you told me about your book the other day." Wood replied, "Did you locate one?" "Yes, Professor, I did." "You were lucky to find one. How much did you have to pay for it?" "Twenty-five cent." After a pause the student resumed, "Professor Wood!" "Yes, what is it?" "I find two copy."

Another unpublished story about Wood merits telling. In 1931 a committee was appointed to help plan the science exhibits at the Chicago Century of Progress. One meeting was held at the Loomis Laboratory at Tuxedo Park, where Wood was doing research as a guest of the laboratory. While the committee was deliberating, Wood was busy in the same room with his work. He was using a spectrograph which had been brought there from a barn in Long Island where he had previously used it. At the Long Island barn laboratory there had been extended "off" seasons, during which busy spiders spun webs across the instrument's long cylindrical tube. In one of his papers in the *Philosophical Magazine,* about 1915, Wood reported that, to brush out the spider webs, he put a live cat of proper size into the tube and closed the end behind it. The phototropic impulse of the cat toward the opposite open end rendered the experiment completely successful.

While Wood was quietly at work, there came a moment when some matter engaged the committee's concentrated attention, and the silence of deep thought was profound. At that moment Wood entered the circle and probably derailed the committee's train of thought entirely with the conundrum, "Why is an historic novel like a bustle?" No one had a ready answer, which was what the questioner expected and wanted, for he was eager to supply the answer. When someone asked for it, it came: "It's a fictitious tale based on stern reality." Whether this was original with Wood, I cannot say; it might have been. The relish he displayed in giving the answer left a lasting impression.

C. E. Mendenhall

One of Wood's friends was C. E. Mendenhall, head of physics at the University of Wisconsin. Mendenhall never enjoyed writing or dictating letters. As many non-correspondents do, he discovered that an answer to a letter if sufficiently deferred is no longer pressing, and eventually not required. He told me that he had systematically applied this discovery. The back of his flat-topped desk was cleared for six letter-sized spaces, one for each successive day of the 6-day week, occupied by letters which had arrived on the consecutive days. Each day's new crop was placed in the space at the left end of the row after all six had been moved one space to the right. On the floor, at the end of the desk, stood a wastepaper basket which received number six pile when the six were moved. He called it "the Sunday file."

Frank Jewett

Frank Jewett, late president and chairman of Bell Telephone Laboratories, was president of the National Academy of Sciences during World War II. One evening at the old Cosmos Club, he regaled a small group of us with an autobiographical note which had a flavor of biophysics.

During his childhood in Pasadena he and some of his friends, aged about ten, became interested not only in bird-watching, but also in studying the habits and life histories of birds. Jewett chose hummingbirds for his study, many species of which congregated in the Pasadena area during the winter months. With the onset of migration, each species sorted itself out from the others and took off for its summer habitat. One fact of particular interest to him was the cleanliness of the humming bird's nest as compared with that of any other kind of bird. Careful observation disclosed the reason. The earliest training given a chick by the mother hummingbird was "toilet training" of sorts. It consisted of teaching the chick, immediately upon emergence from its shell, to elevate its posterior above the edge of the nest when defecating.

Jewett's interest in physics suggested the possibility of a simple experiment based on this observation. He measured the height of the nest above the ground, and the horizontal distance of the droppings from the vertical to the nest. These data enabled him to determine the initial velocity, assuming horizontal propulsion. It proved astounding that a hummingbird chick, weighing only a few grams, could muster such propulsive energy. Jewett speculated on the validity of extrapolating from the velocity-weight relation for a few grams of body weight to velocities for greater body weights, say up to 75 kilograms. The reader's imagination can readily supply the discussion about these speculations.

Robert James Wallace

A man relatively less prominent in science, but a rich source of anecdotal material related to science, was Robert James Wallace. For many years the replica diffraction gratings which he personally made were the only such gratings generally available. He was skillful in the use of instruments and had special aptitude in sketching and painting. He developed an intense interest in photography during the early nineties, and was probably the only available consulting expert on photosensitive emulsions when Yerkes Observatory was established. What follows is a record of a few of the interesting episodes related to me by Wallace, not once, but several times, during successive annual visits at his home.

Robert James was the eldest son of a Glasgow wine and spirits merchant. His father was eager, in the Scottish tradition, to see his eldest son succeed him in the family business, but his father's outspoken desire was Robert's sufficient reason for deciding against this step. Instead, at age 18, he took a ship for America without notice or forwarding address. He was first employed selling artists' supplies in Chicago, and through his consuming interest in photography eventually became the production expert on photosensitive materials for a manufacturer of photographic dry plates.

My acquaintance with him derived from his desire, in his advancing years, to be relieved of the task of producing the grating replicas. These had been sold for many years by the Central Scientific Company, with which I was then connected. About 1932 I made my first trip to Asheville, where he lived, to take over the production. Although he was almost a recluse, he seemed to welcome the opportunity to tell me about some of his early experiences.

His expert knowledge of photographic materials was the reason, some 35 years earlier, for his having been sought out by George Ellery Hale, first director of Yerkes Observatory, to become the observatory's photophysicist. His responsibility was to assure that photography with the new 40-inch refractor yielded optimum results. He stipulated two conditions of employment: one, that he be permitted to continue as consultant to the dry plate company which had employed him, and two, that he might use the 40-inch telescope for photographing celestial objects of interest to him when its use was not otherwise scheduled. In his consulting, a surplus of dry plates was provided which he could use as he saw fit. One of his most successful projects was a sequence of photographs of the moon on successive days throughout the lunar cycle. At the time, these were the best such photographs which had ever been made.

The staff of the observatory included Barnard, his sister Miss Barnard, Ritchey, and Frost. Under Hale's directorship things went along tolerably except for the feuding which might be expected in an isolated group including Wallace. In particular, he and Frost didn't get along too well. When Hale announced in 1904 that he was leaving in a few months to establish a new observatory on Mount Wilson, he indicated that Frost would be his successor. Shortly thereafter, Frost appeared in Wallace's laboratory-office and said, "I suppose you have heard that when Dr. Hale leaves, I shall be the director?" "Yus-s-s". "I came to talk about the moon photographs. The unusual series which you have, in my opinion, belongs to the observatory, and I shall require you to turn them over when I take office."

His ire aroused, and without inhibitions on his Scottish burr, Wallace exclaimed, "Those photographs do not belong to the observatory. I exposed them on my own plates and developed them on my own time with my own developer. They belong to me and I shall not turn them over!" Frost replied, "We shall see; I shall talk to the director about this." A few days later Wallace was summoned to the director's office. "Please be seated, Mr. Wallace. Mr. Frost tells me that you and he had some conversation about photographs of the moon which you made." "Yus-s-s." A long pause. "Well, Mr. Wallace, I am inclined to agree with you." End of the interview.

Years later, on one of my visits with Wallace at Asheville, he showed me, in his attic, the box containing the moon negatives. These he had transported from domicile to domicile, recognizing all the while that they had little value for him. Among his former associates at Yerkes only Miss Barnard survived. As we looked at the box, he said, "Now I know what I shall do with the moon photographs; I shall ship them to Miss Barnard." So far as I know this was done forthwith, and the pictures, by indirection, came into the possession of Yerkes Observatory.

Wallace's skill at sketching caused his eventual dismissal from the observatory. He was fond of cartooning events and caricaturing people and then posting his graphic productions on the bulletin board. The final sketch thus displayed, depicted the director sitting on a throne as a puppet with his wife behind the throne manipulating the strings. Wallace's exit was preemptory. However, when the director retired, and he and Mrs. Frost drove to Florida for the winter, they stopped at Asheville for a visit with Wallace, and a friendship was restored which continued throughout their remaining years.

A personal experience gave me a warm feeling for Frost. In 1923 my family and I went on a Model T camping trip through Wisconsin and Minnesota. We stopped at Williams Bay to visit the observatory. Frost had become blind, but he insisted on being our guide through the laboratories and the domes. We marveled at his unfailing mastery in orientation, going up and down stairs, unlocking doors, and explaining the telescopes, their mountings and use in a way that made us unaware of his handicap. We could not fail to remember our visit with the blind astronomer.