Physics and Physicists of the Past Fifty Years

By Raymond T. Birge

The address of the Retiring President of the American Physical Society, presented at the APS–AAPT Joint Ceremonial Session in New York City, February 2, 1956, during the 25th Anniversary Celebration of the American Institute of Physics.

DR. Wigner, Members of the American Physical Society and of other Societies associated with the American Institute of Physics, and Guests:

In the case of British organizations such as the Royal Society and the British Association, it is quite customary for the president to deliver an address of a general, nontechnical character. On the other hand, in the case at least of the American Physical Society, a purely technical paper is much more common. It seems to me, however, that at a meeting where several hundred technical papers appear on the program, at least one nontechnical paper may appropriately find a place.

I have some precedent in that at least half of the retiring presidential address of Dr. Bridgman in 1943 and of Dr. Van Vleck ten years later was devoted to certain general problems then confronting physicists. Accordingly I should like today to speak first of physics as I found it in my younger days and then of some of the very general problems that have arisen in more recent years.

In discussing my earlier experiences in physics, I hope you will pardon me if I become very personal. I justify such procedure on the ground that one can rightly speak with authority only in respect to one's own personal experiences. All else is of the nature of hearsay evidence, which may or may not be true. But one's own experiences are necessarily true, except in so far as they may be distorted by unsuspected lapses of memory.

The words "fifty years" in the title have a very literal meaning, for it was *exactly* fifty years ago today, lacking just nine days, that I arrived in Madison, to enter the University of Wisconsin as a freshman. I had no idea then what my major subject was to be, although I had always enjoyed science and mathematics in high school. But I roomed that first semester in the same house with Warren Mead, who later was to become one of this country's leading geologists. He was then working on his senior thesis which, in June of that year, was awarded the prize given annually for the best senior thesis.

Mead promptly persuaded me to become a geology major. But at Wisconsin one did not start geology until the junior year. The general course in geology must be preceded by the year courses in general physics and chemistry. So I took the elementary physics course, and that was the end of the geology major!

I would like to comment on the Wisconsin elementary physics course, given by Benjamin W. Snow—commonly known as Bennie Snow. In the January 1951 number of the *American Journal of Physics* there is a short article entitled "The Lectures of the Late Benjamin W. Snow,"[1] written by J. C. Blankenagel of the German department, Wesleyan University. In this article he praises Snow's lectures in most glowing terms.

Now I am really interested in good teaching, as I know very many of those here are also, and Snow's course furnishes an opportunity to consider for a moment what constitutes good teaching. Professor Snow had accumulated in 1906, when I took the course, probably the finest set of lecture demonstration apparatus in the country. So far as lecture demonstrations were concerned, his lectures were interesting and even exciting. In fact, this course was possibly the best known of any on the Madison campus.

But, Bennie Snow was interested *only* in elementary physics and this was the only course he taught. So far as I am aware he never engaged in any research and never published a research paper after completing his PhD thesis in 1892. To him physics was an incomparably beautiful, but *closed* subject. There was nothing in his lectures to suggest that there were things still to be discovered in physics, and hence no incentive to enter the field except to become a teacher and in turn show these same beautiful experiments to one's own students. Very possibly as a result of this situation there were only four senior majors in physics when I graduated, although three hundred students took Snow's course each year.

For students from outside departments, like Blankenagel, who would never again be exposed to physics, the course was ideal in giving a vivid picture of the experimental methods of exact science and of the beauty of physical phenomena. The course was also extremely easy, Professor Blankenagel to the contrary notwithstanding. There were no problem sets and no problems on the examinations—merely descriptive questions.

Now was Bennie Snow a good teacher, or wasn't he?

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Obviously it depends on what student you ask, and what you consider the proper objectives in the general physics course of the college of liberal arts.

When a small group of us started junior courses in physics, we had as instructor, among others, Charles E. Mendenhall, later president of our society (1923–1924), an extremely well-trained and well-informed physicist, as well as an excellent teacher. But his course, although as I know now of only standard difficulty, was such a change from our elementary work that for the first few weeks most of us had a pretty bad time. But we eventually got used to problems we actually had to solve ourselves, and, as I have said, four of us finally received the AB degree in physics.

My own senior thesis, on metallic reflection, was done under the direction of Leonard R. Ingersoll, who is still around and may be present today. It is peculiar the ideas one has of relative age. At that time I was a mere undergraduate student and Ingersoll was a professor. Hence he must have been at least twenty years older! But according to *American Men of Science* he is *now* just *seven* years older than I, and I suppose all you young people in physics consider both of us hopelessly old men and are surprised we are still able to navigate.

In September 1909 I started graduate work. As I recall, there were some fifteen graduate students in physics—maybe two or three more. Theoretical knowledge of that day was very fragmentary compared to the present situation. There was no real theory of the structure of either atoms or molecules. A few chemists, of whom possibly the leader was Louis Kahlenberg, then chairman of the chemistry department at Madison, even doubted the existence of atoms.

We had x-rays and rays from radioactive substances, but comparatively little was known about their nature. Bragg believed, in 1910, that γ- and x-rays were negative electrons to which a small amount of positive electricity had been added, sufficient to neutralize the charge, but with little effect on the mass.[2] Their true wave nature and wave lengths were first established by von Laue, Friedrich and Knipping, and Bragg,[3] in 1912 and 1913.

We had free electrons, whose e/m had been measured; also the Zeeman effect, from which a value of e/m had been obtained, agreeing with that for free electrons. That was the first proof that electrons formed a definite constituent of all atoms, and it was about all that was definitely known about the structure of atoms. That the atom contained a small, massive nucleus was first established by Rutherford[4] in 1911.

It was believed that band spectra were due to molecules, and line spectra to atoms. It was thought by some that an atom could not absorb or radiate light until it was ionized. Hence, for instance, the fact that sodium vapor first showed absorption at about 200°C was taken as a measure of the temperature at which ionization started in sodium vapor.

That a series of most important advances in our understanding of physical phenomena occurred during the four years, 1909 to 1913, of my graduate work, has just been indicated by two examples. Another important advance was that made by Millikan who, in the February 1910 issue of the *Philosophical Magazine*,[5] described his first measurements of the electronic charge by means of *individual* drops. In a paper[6] read before the American Physical Society in April 1910, he gave a more precise value—in fact a value to *five* significant figures, 4.9016×10^{-10} esu. Unfortunately, nearly all of these digits were *not* significant.

At the meeting of the AAAS in Minneapolis, December 1910, Millikan read an invited paper. At that time he was in the midst of a heated controversy with Ehrenhaft over the existence of the sub-electron. Incidentally, Ehrenhaft was one of those enigmas that appear occasionally in physics—a professor (at that time) in a distinguished university (Vienna), who was never right about anything.

I had the good fortune to sit next to Millikan at lunch one day during the meeting. He was bubbling over with enthusiasm. That, I submit, is a major qualification for first class research, *and* for first class teaching. Dr. Joel H. Hildebrand of the University of California, who has just retired as president of the American Chemical Society, in a recent symposium on "Productivity of Research",[7] lists six qualities of a good researcher, and the *first* one is "enthusiasm for significant knowledge".

In my early years in physics I had a low opinion of government research workers. (I hasten to add that that is no longer the case.) In my first visit to the Bureau of Standards I was in the office of Paul D. Foote when the clock struck four (I believe that that was the hour). Instantly everyone—or at least it seemed like everyone, except of course Dr. Foote—poured out of the building, to play tennis or to do something else far removed from his professional work.

Now that is *not* my idea of how good research is done. If something exciting is happening, one should be willing—in fact, eager—to stay overtime and even to work all night, if necessary, to finish it. In connection with this matter of union hours, I like to tell the following story. Dr. Harlow Shapley was at a meeting of the AAAS in Berkeley, many years ago, but I did not find an opportunity to talk with him until late in the meeting. I then apologized for the delay, saying that I had been working very hard, always six days a week and often seven. To which he gave the following unique reply: "Oh, *I* always work a *minimum* of *seven* days a week!" When someone says to me, as has happened more than once, "Why, don't you believe in the five-day week?" I like to quote this little episode.

But to return to our knowledge of physical phenomena, at the start of my graduate work. I have said that a few persons were even doubtful of the existence of atoms. Then the June 1911 number of the *Proceedings of the Royal Society*[8] arrived, with the first published pictures of cloud chamber tracks obtained by C. T. R. Wilson. You can well imagine the sensation caused by those pictures. After that it was hard not to believe in atoms.

THE late and great R. W. Wood was really responsible for my entrance into the field of spectroscopy and *also* into that of computing, which led eventually to my work on the general physical constants. The first three years of Dr. Wood's teaching career occurred at the University of Wisconsin (1897 to 1900) and his original work on the optical and spectroscopic properties of sodium vapor was done there. At the start of my own graduate research I was given Wood's original absorption tube and I proceeded to repeat some of his experiments.

Incidentally I would remark that at that time every graduate student started research work as soon as he became a graduate student, if he had not already started. In my own case I began research in my senior year and from that time on, for many years, I figure that from one-third to one-half of my total working time was devoted to research.

Under certain special experimental conditions Wood had obtained a band spectrum, from a tube containing sodium vapor, lying in the yellow and red regions. I also succeeded in obtaining it, and it looked to me very much like the First Deslandres Group of the positive band spectrum of nitrogen. So I decided to photograph the nitrogen spectrum, under higher dispersion, in order better to settle the matter. Then I discovered that this group of nitrogen bands, because of the relative lack of sensitivity in the yellow and red of the best dyed photographic plates, had never been obtained under high dispersion.

That accordingly became my PhD thesis, with exposures up to 100 hours in length, in the second order of the 21-foot concave grating. The temperature was not merely held constant during the exposures, it was continually varied to compensate for the varying barometric pressure. It was the first time that exposures of more than an hour or so in length had ever been taken, using a large grating, with no loss of definition.

This research was carried out under the nominal direction of Professor Mendenhall, who was officially in charge of *all* graduate research in physics; that was apparently part of his contract. He was not a spectroscopist and no course in spectroscopy was offered at Wisconsin. Hence what spectroscopic knowledge I acquired was obtained by private study. Thus I chose my own thesis subject and independently carried on my research. Later I shall contrast this with present conditions.

I have just remarked that Dr. Wood also started me on my computing career. I was asked, very early in my graduate work, to report at our weekly research colloquium on the recent work by Wood on the absorption spectrum of sodium vapor. He had, among other things, just extended the principal series to 47 members and had tested the Ritz formula on these new data. He found that it was quite unsatisfactory. To make the test Wood had used the frequencies of three lines to evaluate the three empirical constants of the equation, and had then tested it on three other lines. I repeated the calculations and found, to my amazement, that in the case of two of the three lines thus tested, he had made a numerical error. Actually the formula fitted very well and this work, extended to the other alkali metals, resulted in my first published paper in the field of spectroscopy.[9]

That was my initial discovery, but by no means my last, that some of the published results of even the most distinguished physicists may be incorrect, due solely to numerical errors. Later I found that they may be incorrect for lots of other reasons. But, as a matter of fact, I got into the field of determining most probable values of the general physical constants, in particular the atomic constants, only because it had been found that all the general atomic constants could be calculated by means of purely spectroscopic data, and I did feel able, as a result of my own experimental research, to judge the accuracy of such data.

Late in the summer of 1913 I finished my thesis and departed to teach at Syracuse University. Beginning on page one, volume twenty-six, of the *Philosophical Magazine* for July 1913, appears Bohr's first paper on the structure of the atom,[10] and with it began the modern theory of spectroscopy and of atomic structure. (The corresponding theory of molecular structure, on the basis of band spectra, originated with Schwarzschild and Heurlinger, 1916 to 1919.) [11]

In the fall of that same year (1913) I lectured on the Bohr atom before the Syracuse chapter of Sigma Xi. Instead of merely drawing the circular electron orbits around the nucleus that correspond to Bohr's equation, I used only a short segment of each circle and then represented the various spectral lines by straight lines connecting these segments. I do not know whether this was the first energy-level diagram ever drawn (although, of course, not to scale). It certainly was, so far as I am concerned, and I sent copies of it to Millikan and to McLennan, to straighten out a mix-up they had gotten into, just as a result of failure to reduce Bohr's theory to graphical form.

FOR the next ten years I tried to be a real missionary for the Bohr atom. I even managed to convert the chemists at California, where I had moved in 1918, and, with G. N. Lewis very much in the foreground with his cubical static atom, that was not easy. Things were really exciting in those days, even if we didn't have any cyclotrons to play with.

I want to add just two more examples to this highly personal recital. The first was kindly mentioned by Dr. Edward Condon, in his presidential address of 1947. He has been the most successful of all those who did their PhD research under my direction and I am very proud of him.

From an analysis of the heads of successive bands (or, more precisely, of the *origins*), in the case of a simple diatomic molecule, it is possible to calculate the frequency of vibration of the dipole, and the force constant of the vibration. From an analysis of the individual lines of the same bands, it is possible to calculate the nuclear separation and the increase of this separation, with increasing frequency of rotation of the dipole (or better the *dumbbell,* in the case of a nonpolar molecule). This centrifugal swelling of the molecule should depend on the *same* force constant, and I was able to show,[12] in the case of CN, that the two values of the force constant, calculated in these two completely independent ways, agreed to less than one percent. After that no one could tell me that diatomic molecules did not vibrate and rotate just like, to at least a very close approximation, an ordinary mechanical elastic system.

The other example is the story of 137, the approximate value of the fine structure constant α (or, more correctly, the *reciprocal* of α). Now it is my firm conviction that the values of dimensionless physical constants, such as the fine structure constant, can be determined only from experimental data combined, of course, with an adequate theory. But Eddington had a different idea. He apparently believed that from purely logical considerations he could deduce the value of such a constant, and in 1929 he thus derived [13] a value of 136.

With the then commonly accepted value of e (Millikan's oil-drop value) the experimental value of $1/\alpha$ was,[14] however, 137.3. But Eddington had just learned, to his obvious delight, of a possible new value of e, obtained from crystal data, some one-half percent higher than Millikan's value, which would yield very closely his derived value of 136.

On receiving Eddington's paper I immediately wrote to him, pointing out that every precision calculation of h required an assumed value of e. Hence if the value of e were actually to be raised by some half percent (which later proved to be the case) there *must* be a corresponding change in the adopted value of h. I also wrote that a calculation of such a new value of h led to a value of $1/\alpha$ very close to 137. Hence his value of 136 was quite impossible.[15]

Eddington replied by return mail (Feb. 3, 1929) in a handwritten letter—the most microscopic writing I had ever seen—saying that I was certainly correct, although he could not then understand how his calculations could be in error. But very shortly thereafter he did find that he had unfortunately "forgotten" one degree of freedom and that his calculated value [16] was indeed 137. From this episode I deduce that a little knowledge of the experimental facts is helpful to even the most philosophic of physicists.

I was actually awarded the PhD degree in June, 1914. During that year just 23 such degrees were awarded in physics in the entire country.[17] In 1954 *548* doctor's degrees in physics were awarded,[18] or 23.8 times as many, for that forty-year interval. In 1914 a total of 505 doctor's degrees in all were awarded, of which 48 percent were in the sciences. In 1954 this proportion had increased to 58 percent and the total number of degrees, in science and nonscience, to 9000, a 17.8-fold increase.

YOU may be surprised to learn that the proportional increase in physics, from 1914 to 1954, was by no means the largest among the sciences. In the case of engineering, the *average* number of doctor's degrees in 1913, '14, and '15 was just *two,* the individual totals being zero, four, and two. In 1954 *383* such degrees were awarded, a 191.5-fold increase. Then comes bacteriology with 82-fold, psychology with 61-fold, and agriculture with 25-fold. Finally, in *fifth* place, comes physics with the figure 23.8 just given. The average figure for all sciences is 21.4, with chemistry in *seventh* place, its increase being only 16.5-fold.

While the number of degree awards in physics was increasing by 23.8-fold, the membership of the American Physical Society increased only 17.3-fold. To be sure, the number of degrees given in a single year constitutes just one point on a curve, whereas the membership in our society is more properly correlated with the area under the curve. But it does look as though there were now many practicing physicists who should be members of our society, but are not.

In spite of the present importance of physics, we constitute an extremely small fraction of the total population. In fact *all* scientists and engineers constitute only one-half percent of the total population.[19] According to the 1953 report of the National Manpower Council [20] (a citizens' survey group appointed by Eisenhower, when he was president of Columbia University), there are half a million engineers and 200 000 physicians in the country, but only 18 000 physicists (an American Institute of Physics estimate). The 1950 census lists only 11 000 physicists of which only 3500 held the doctor's degree. An entire chapter of this excellent report is devoted to the question of physicists and their problems.

The great increase in the number of physicists during the past forty years has undoubtedly been induced, in large measure, by the demand on the part of both government and industry. I have been told that in World War I there was no classification of physicist. When the Armed Forces felt the need of a physicist (which was only occasionally), he was hired as a chemist! But we are more charitable, and you will note that in World War II, in which physicists were employed in droves, the chemists were *not* classified as physicists, although it is possible that they would not have objected.

I have no panacea for the present admitted critical shortage of physicists. Everyone seems to agree that to increase the supply we must begin with the high schools. A good description of the present high school situation has been given by P. F. Brandwein of the Forest Hills, Long Island, High School, in an article in the November 1955 number of the *American Journal of Physics.*[21] By coincidence, Dr. Brandwein is today addressing a meeting in Los Angeles on this same subject.

His article opens with the impressive figures: "The people of the United States own 31 percent of all radio and television sets, use 58 percent of the world's telephones, drive 76 percent of the world's automobiles, produce 40 percent of all electric power—and this with

6 percent of the world's land and 7 percent of the world's population. In one way or another, all of us use products that have their origin in some physicist's laboratory. But only 5 percent of the total high school population is in our high school physics classes." (I note at this point that in 1900 the corresponding figure was 20 percent.[22])

Brandwein writes that in 715 typical high schools, only 13 percent of eleventh and twelfth grade pupils are enrolled in physics, and that half of these schools offer neither physics nor chemistry. Furthermore, only in large schools with an enrollment of 1000 or more (comprising only 6 percent of the total) may one assume that the physics instructor teaches only physics. In the vast majority of schools he teaches most, if not all, of the sciences. Finally, I learn from *Chemical and Engineering News*,[23] the mean entering salary for high school teachers is only about $3300 for the school year. But for 1956 the chemical industry [24] is prepared to offer newly employed bachelors an average of some $4600, and those with the doctor's degree some $7000, each of course for the entire 12-month year. This is certainly *one* of the reasons for the present shortage of high school teachers of science.

Many persons have recently discussed, in published articles and speeches, the various measures that might be and should be taken to increase the supply of scientists. So far as potential college students as a whole are concerned, the College Entrance Examination Board in a recent report [25] estimates that *each year* 200 000 American high school graduates in the upper 30 percent bracket of ability fail to enter college, mainly because of insufficient financial backing or because of lack of motivation. The Board states that 100 000 new scholarships are needed immediately.

As a partial answer to this need, the Ford Foundation has made a grant of twenty million dollars [26] as an endowment for so-called "Merit Scholarships" for deserving high school students. The first winners of such scholarships will be announced on May 1, and the number was originally given as 350, but the latest information [27] is that 450 to 500 awards will be made.

A broad program for improving science teaching, particularly at the secondary school level, has been launched by the AAAS, with the aid of a $300 000 grant from the Carnegie Corporation of New York.[28] The DuPont Company is granting $900 000 for the coming year,[29] of which $200 000 are being given to fifty privately supported liberal arts colleges for the improvement of teaching. Other similar projects are underway.

The small relative number of persons who now earn a bachelor's degree in science is shown explicitly [30, 31] by the following figures: Eighty-four percent of primary school students enter high school. Of those entering high school 61 percent graduate. Of those who graduate, 47 percent enter college, and of these only 59 percent finally graduate, of whom only 26 percent obtain the degree in any science. Thus finally, of the original primary school population, only 4 percent eventually obtain a bachelor's degree in one science or another. In the case of the PhD degree, only 10 percent of the science degrees are in physics.[32] I do not have the corresponding data for the bachelor's degree, but if the same percentage holds, it means that only 0.4 percent of primary school students finally obtain even a bachelor's degree in physics.

The matter of *quality* is also important. The distinguished Commission on Human Resources and Advanced Training reports [33] that only *two-thirds* of the top 2 percent of high school graduates now graduate from college, with decreasing fractions down to *one-third* of the top half of high school graduates. The National Manpower Commission states [20] that for every high school graduate who ultimately earns a *doctoral* degree, there are twenty-five others who have the intellectual ability to do so but do not.

Regardless of precise figures, there is certainly a severe shortage of trained physicists at the present time and Dr. Marsh White,[34] among others, has estimated that this shortage will continue for at least another decade.

I wish now to consider briefly some of the more general problems that have arisen in recent years, not only for physicists, but for scientists in general. I freely admit that I am no expert in fields such as philosophy, economics, sociology, and political science. But then, neither is the man-in-the-street, who nevertheless seldom hesitates to express a vociferous opinion on such matters; and many of these problems are of vital concern to each one of us.

DR. P. W. Bridgman has given a number of addresses of a philosophical nature. Most of them are to be found in his book *Reflections of a Physicist*.[35] They include one, "Science and its Changing Social Environment", which represents half of his presidential address in 1943, noted earlier. The sort of problems he considers can be seen from some of the other titles namely: "Scientists and Social Responsibility", "Science and Freedom—Reflections of a Physicist", "Scientific Freedom and National Planning".

Dr. Bridgman feels quite pessimistic about present trends in physics, as you will learn. I am going to quote very briefly from these papers, in order to indicate the origin of his concern. I am then going to quote briefly from a Polish publication, to indicate what is now going on behind the Iron Curtain. I believe a comparison of these two sets of quotations may be instructive, and in some respects almost frightening.

I begin with Bridgman, with the understanding that these are isolated extracts from various addresses, the titles of which I have just read. First from his presidential address:

> (a) "Because of the heavy social impact of the products and techniques resulting from scientific investigation, there is a growing tendency in many quarters to maintain that science, and this of course includes physics, is the servant of society and that all

scientific activities should be under complete supervision and control by society or the state. This point of view is finding advocates among scientists themselves."

(b) "Closely connected with the thesis that scientific activity is a social function is the growing impulse to hold the scientist personally responsible for all the consequences of his discoveries."

(c) "If the scientist were required to make only those discoveries which could not wilfully be perverted to harmful uses, he would almost certainly feel himself so restricted that he would make no discoveries at all. Furthermore, it is impossible for a physicist or any one else limited by human fallibility to foresee all the consequences of a discovery, much less to balance all the good consequences against all the bad consequences."

I interject, at this point, the remark that this requirement would be like expecting Faraday, when he discovered electromagnetic induction, to foresee all the subsequent development of electrical machinery, not only up to the present, but on into the indefinite future.

Next from Bridgman's "Scientists and Social Responsibility":

"The social philosophy which seems to be spontaneously growing up among some of our scientists is, I believe, a short-range and inadequate philosophy. It is well known that the scientists who have shown the most articulate concern with the social implications of the atomic bomb are young. The philosophy that is coming into being betrays this. It is a youthful philosophy, enthusiastic, idealistic, and colored by eagerness for self-sacrifice. It glories in accepting the responsibility of science to society and refuses to countenance any concern of the scientist with his own interests, even if it can be demonstrated that these interests are also the interests of everyone."

Next from "Science and Freedom":

(a) "Mention of the stimulus of conversation with one's colleagues naturally prompts one to consider the increasing trend during the last few years to large-scale cooperative enterprises among physicists. The reasons for this are obvious in the enormously increasing size and expense of the apparatus necessary for modern physical research, such as the cyclotrons and the piles of nuclear physics. Although we may recognize that such instruments are necessary, we may nevertheless deplore some of the consequences."

(b) "The competition in this field is intense; rivalry between different groups at different universities can offer little opportunity for leisure or the scholarly digestion of results before publication." (I wish to underscore that last remark.)

(c) "During the war practically all the physicists in this country were diverted to war work of one sort or another, and a large part of them were engaged in large-scale enterprises which involved team work developed to its maximum efficiency. The older men, who had previously worked on their own problems in their own laboratories, put up with this as a patriotic necessity, to be tolerated only while they must, and to be escaped from as soon as decent. But the younger men, who had been drawn into the maelstrom before starting work for their Ph.D. degree, had never experienced independent work and did not know what it is like. Some of these younger men will continue in government work; others who return to academic circles will there join in the teams serving the mammoth instruments. The result is that a generation of physicists is growing up who have never exercised any particular degree of individual initiative, who have had no opportunity to experience its satisfactions or its possibilities, and who regard cooperative work in large teams as the normal thing. It is a natural corollary for them to feel that the objectives of these large teams must be something of large social significance."

At this point I interject that this trend toward "team-research" and the coordination of research activities is especially pronounced in large industrial laboratories and in this connection I feel that the almost classic comment of Dr. Charles Mees,[36] himself then director of research and a vice president of the Eastman Kodak Company, is decidedly worthy of repetition. Said Dr. Mees: "The best person to decide what research is to be done is the person who is doing research. The next best is the head of the department. After that you leave the field of best persons and meet increasingly worse groups. The first of these is the research director who is probably wrong more than half the time. Then comes a committee which is wrong most of the time. Finally there is the Committee of Company Vice Presidents, which is wrong all the time."

Returning now to Bridgman, I quote from his "Scientific Freedom and National Planning":

"From the long-range point of view the scientist has a deep mistrust of this continual drift of all the resources of the country into the hands of the government. It is an irreversible process, with everything eventually getting down to the dead level of government management—a sort of second law of social dynamics."

Bridgman also quotes a remark of Dr. Rabi, who was president of our society in 1950. Rabi stated in 1946, "If it was decided to control universities and university research, there could be no better way to do this than the way it is being done now."

So much for Bridgman and his views of conditions in this country in 1947, when all of the foregoing addresses were given, except his 1943 presidential address. Let us now turn to Poland, a country where there is 100 percent government support of science and, I may add, 100 percent government control.

It is my opinion that when one is discussing conditions behind the Iron Curtain, one should confine oneself to verbatim quotations from an authoritative local source, and preferably to remarks intended for home consumption. Accordingly I have here a 60-page booklet entitled "Science in New Poland", by Jan Dembowski,[37] published in London and sent to me in

May 1953 by the Polish Embassy in Washington. Dr. Dembowski, a biologist, is president of the new Polish Academy of Sciences, and the booklet is the translation of a lengthy address delivered by him to the First Congress of Polish Science, held in Warsaw, July 1951, when the Polish Academy was organized.

On the cover is a sketch of a truly magnificent 28-story building, the future home of the Academy, then being built with *Russian* labor *and* materials, as a *gift* from Russia to Poland. (I apologize for the use of the word "Russia". I am quite aware that, according to the postal authorities, there is no such country.)

Dembowski's address constitutes a remarkably revealing and detailed account of the present Soviet philosophy of science and of scientists. It is very much worth reading in full, but I must content myself with a few brief extracts, to which I shall give numbers, to show where the breaks occur. First as to Poland prior to World War II.

(1) "The development of our science in the past was to an overwhelming degree dependent on the capitalist system."

(2) "Capital, which controlled the economic life of the country, did not permit the development of fields of learning harmful to its interests."

(3) "It is easy to understand the meagre and one-sided interest taken by all the governments, serving capital, in matters relating to science."

(4) "The youth of Poland were trained on Western models. The immense influence of German science, which was regarded as the highest authority, was a fetter to our science. Later, it began to be replaced by American science. We were, however, almost completely cut off from the progressive science of the Soviet Union."

(5) "The lack of coordination in various fields of scientific research, the cultivation of science for science's sake, and so-called unfettered work without any kind of planned thinking to guide it, meant a complete misunderstanding of the social role of science."

(6) "The research worker working on some scientific subject cannot regard with indifference the kind of place his efforts will occupy in the system of human knowledge."

Now as to the Post-War period and the reasons for establishing the Polish Academy of Sciences.

(7) "Owing to the change in our social structure, it has become possible to cooperate more closely with the Soviet Union, to which all the People's Democracies owe their freedom and the possibility of peaceful building, and this cooperation is becoming increasingly close."

(8) "Our country is marching to Socialism, and under this system the role and importance of science is completely different from what it was before."

(9) "Experience has shown that academic deliberations on the question of the freedom of science, carried on by the *elite* in their ivory tower, are incapable of solving any kind of difficulty, but reality, abounding with life, ignores these difficulties."

(10) "We must break away from the idea that scientific work is the result of inspiration, which cannot be planned. The development of science is subject to its own laws, it is causally connected with the development of society and really important scientific problems do not come into being suddenly and without warning. They are always the product of a certain historical process."

(11) "There must arise in Poland a central scientific body with far-reaching authority and high technical competence, which would direct the whole organizational and ideological activity in the field of Polish science."

(12) "Scientific research in Poland can no longer be carried out in an out-of-date manner. In all centers of learning, whether academic or non-academic, research work must be organized and planned. The program of work should take into account not only problems arising from the development of science itself, but also problems connected with important and actual needs of the State. Therefore a central body must exist in the country possessing supreme authority and supreme competence in scientific questions, a body which will deal with the planning of research, the organization of science, and the coordination of work by individual centers. The Academy of Sciences will be precisely such a body."

(13) "I come now to the question of the organization of specialized scientific societies, of which there are nearly a hundred in the country, of diverse scientific levels. At the meetings of these societies the papers read are usually works of members of the society, very often small contributions to science. It is doubtful if the restriction of membership to professional workers is correct. We must give young people easy access to scientific societies. We ought to open such meetings to working people, rationalizers and shock-workers, whose perspicacity and good judgment will often surprise us. Every professor in charge of practical work in his own subject knows very well that young people without qualifications often see weaknesses in work which escape the attention of experienced people."

Now three short quotes on the "Popularization of Science."

(14) "The question of the popularization of science, which is of great social importance, is one of the major problems in our country."

(15) " 'There is no problem in the whole of human knowledge,' said Tolstoy, 'which cannot be explained to a ten-year-old child.' "

(16) "Science should come into close contact with the whole community. This is so in the Soviet Union, where every scientific achievement becomes common property, where the names of the Academicians are known to every schoolboy and where the Day of Science is celebrated by the whole nation."

To save time I omit all quotations from the section on "Scientific Publications" in which it is stated that academic textbooks are to be written by authors *designated* by the appropriate authorities, to cover fields of work also *specified* by the same authorities, etc. I close the quotations from this address by three extracts from the section "Training of Scientific Personnel".

(17) "The number of workers needed for scientific research will be established by the Academy of Sciences."

(18) "For a more detailed estimate of needs, it will be necessary, in addition, to fix norms for the amount of work to be done by scientific personnel. The number of scientific personnel will be directly dependent on those figures."

(19) "Contrary to a belief common amongst scientists who have an important scientific problem before them, we cannot wait for its solution to the happy moment when a genius will be born who will master and explain everything. Genius is a chance, rare phenomenon, which cannot be planned, and in the face of the colossal growth of modern knowledge, the genius has an increasingly difficult part to play. For that reason, we cannot depend on geniuses, and with increasing frequency we are trying to solve scientific questions by working on them in teams. . . . Today, a group of people of average ability, such as we can always mobilize, and who have a problem clearly marked out, who carry on organized research work and who make use of proper scientific methods, can accomplish more than a single genius working on his own. . . . Team research is an indispensable method for science."

When E. O. Lawrence was at the Geneva Conference last summer, he learned directly from one of the Soviet scientists there that next to the ruling politicians, the scientists are the highest paid group of persons in Soviet society. Aside from this point, which is admittedly not minor, their personal freedom—or rather lack of freedom—seems to me to show certain striking similarities to the situation of the slaves of ancient Egypt and Greece.

As all of you know, the USSR is now deliberately training an enormous number of future scientists and technicians. According to A. W. Davison, research chemist, speaking before the American Chemical Society last November,[38] Soviet Russia is believed to be spending one-fifth of its budget on education, with emphasis on mathematics and science. In 1950 it graduated only 28 000 engineers to our 50 000, but this past year it graduated 54 000 while our production had dropped to 19 000.

A recent report on Soviet Professional Manpower, by N. De Witt of the Harvard University Russian Research Center,[39] makes the following points, among others:

1. "The Soviet Union is graduating almost twice as many technical specialists in some fields as in the United States, but only a *small* number in the humanities."

2. "The Russians achieve their high ratio of professionals in the labor force by starting them off early in secondary school, giving them incentives and preferential treatment."

3. "Soviet young people, once committed to a career, cannot change their minds."

4. "In Russia, more than *60 percent* of the regular graduates are science majors." (This 60 percent figure contrasts with the *26 percent* previously quoted for America.)

There is no doubt that, purely as a matter of dollars and cents, support of science is a highly paying proposition. Thus an editorial [40] in *Science* of September 30, 1955, quotes an estimate by Raymond Ewell of the National Science Foundation that in the United States, over the past 25 years the *average* return of the investment in research and development (by all sources) is from *100* to *200* percent *per year!* Ewell notes further that from 1776 to 1954 (a span of 178 years) close to *forty billion* dollars have been spent here for research and development, and *half* of that in the *last six* years!

The US government alone is now spending about two billion dollars per year on scientific research and development,[41] and this sum represents some 35 percent of the entire financial support of such work.[42] Unfortunately, only 6 to 7 percent of the government's contribution is for *basic* research.

I have given the foregoing facts and figures about the USSR and this country as material worthy of your most serious consideration, and I am deliberately leaving it to you to draw from it your own conclusions.

IN the remaining few moments, I wish to speak very briefly on a highly controversial issue, namely *secrecy, security,* and *loyalty,* including so-called Loyalty Oaths. As I have already stated, I feel that one should try to restrict oneself to personal experiences. As one of those who have Q-clearance, I have had some personal contact with the FBI, and like everyone else in that category, I presume, the FBI has a nice fat folder on my past life. It does seem deplorable that in these United States such records should exist, filled with a multitude of comments, favorable and unfavorable, relevant and irrelevant, not to mention some that are completely false.

So much publicity has been given, during the past few years, to the problems of secrecy and security posed by the Cold War that it seems unnecessary to go into details, even if there were time. The entire sixty pages of the April 1955 number of the *Bulletin of the Atomic Scientists* are devoted just to this problem,[43] and I commend all of the seventeen papers of this issue to your serious consideration. They cover, collectively, practically every phase of this baffling problem. Among the authors of these articles are a former president of our organization, Dr. DuBridge, and one of our present officers, Dr. Goudsmit. In fact, speaking generally, I suppose this ten-year-old publication has had the most distinguished list of authors of any journal read by scientists (and, let us hope, also by many nonscientists).

A truly great American, Chief Justice Earl Warren, writes in the November 1955 issue of *Fortune* [44] regarding security procedures: "In the present struggle between our world and Communism, the temptation to imitate totalitarian security methods is a subtle temptation that must be resisted day by day, for it will be with us as long as totalitarianism itself."

I omit any discussion of the visa problem, which is certainly important, mainly because of lack of time, but also because the situation appears to be gradually improving. According to the Visa Committee of the Federation of American Scientists, reporting in the December 1955 number of the *Bulletin of the Atomic Scientists*,[45] this improvement is not the result of any change in the regulations, but comes from better co-operation between the visa office and the office of the scientific adviser of the State Department.

In conclusion, I come to the matter of loyalty oaths, with which unfortunately I have had extensive and almost tragic personal experience.[46] I begin by stating categorically that the so-called loyalty oath has no relation whatsoever to loyalty. Actual spies and disloyal citizens thrive on falsehood, and I have yet to hear of any such person voluntarily admitting his sins.

As I see it, a loyalty oath is merely a *form of ceremony*. Now many persons simply *adore* ceremony, while there are others who simply *abhor* it. Such divergent attitudes arise from basically different concepts of life, and are not easily changed. But certainly throughout the entire history of the human race ceremony has played, and continues to play, a conspicuous role.

It is naturally difficult to make any detailed comparison of prehistoric and modern practices, but it does appear that as civilization advances, ceremony becomes gradually of less importance. Thus, so far as can be determined, each person in long past ages conformed to the ceremonial customs of his tribe. If he did not, his life was likely to come to an abrupt end. But in modern times there is considerably more individual freedom in such matters. It is indeed quite possible that the advance of civilization can be measured in terms of the decrease of ceremony and mass action, and the corresponding increase in personal initiative and personal integrity.

My own point of view on oaths or pledges of any kind may be illustrated by an incident of my own boyhood, which I have been requested to include in this talk. When I was a small boy attending Sunday School, considerable pressure was exercised on us to sign a pledge to abstain from smoking and from drinking alcoholic liquors. So far as I can recall, I was the *only* member of my class who consistently refused to sign such a pledge, and I think it quite possible that in later years, I was the only member of the class who did *not* smoke or drink! If I had signed such a pledge, I would have considered it binding, and I felt then, as I do now, that no one has the right to ask a ten-year-old boy to decide what he will or will not do as an adult.

Returning to loyalty oaths, one observes that many of those who led the fight against them were persons who had fled from abroad just to escape from such impositions, which heralded the start of the dictatorial regimes of Hitler and Mussolini. But other persons, native born and completely loyal, repudiated them simply on principle, even when in some cases such action cost them their jobs.

As for myself, I am quite sure that true loyalty to a country cannot be instilled or increased by legislative action. On the contrary, it arises from the respect and admiration one has for the institutions and especially for the leaders of a country—for Abraham Lincoln, as an example, in our own case. It is, in fact, the ideas and ideals of *individuals* that, in the last analysis, have made our present civilization.

Our own most sacred obligation as scientist and as citizen is, I believe, to display complete personal integrity in every action, public or private, scientific or nonscientific, and the persons most worthy of either our respect or our loyalty are just those whose lives best exemplify such an ideal. Finally, it is only in a country where individual freedom exists that one has even the opportunity to live a life free from hypocrisy and intimidation and devoted to those ideals that are deemed most worthy.

I therefore enjoin each of you to decide, after mature consideration, what seems most worthy of your serious endeavor, and each of you, in a *properly* organized society, should then be free to act on that decision. For, to quote from a source nearly two thousand years old, "What is a man profited, if he gain the whole world, and lose his own soul?" I thank you!

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