times drank from that pump, have had distinguished careers—not indeed in medical science, but all notable for scholarly ability, broad humanity and public spirit.

Evidently those who drank the waters of that pump sixty years ago gained from it or from some other source such inspiration as Greek legend supposed that the poets drew from the spring of Arethusa on Parnassus, the fabled mountain of the Muses. And if men with such inspiration have achieved results verging on the marvelous within one generation their achievements should be an inspiration also for those who are to come after them in what is yet to be done. Truly if, as Oliver Wendell Holmes stated at the time when the cool water was flowing from that pump, the "state of medicine is the best index of the grade of a civilization," civilization in the sixty odd years since then has advanced as never before.

Nothing in this advance is more striking than the decrease in the deaths among children. In the time when Simon Flexner and his brothers passed the perils of birth, infancy and childhood, as in all previous centuries, ten or more children per wife or succession of wives worn out with child-bearing were not an unusual number born in a family; but often few, and sometimes none, survived to be grown. Now it is the knowledge of the diseases of childhood won by the men who drank from that pump and others like it that balances the diminished birthrate and that generally assures to us who now are old the survival and affection of our children and grandchildren.

INITIATIVE IN RESEARCH[1]

By Dr. COLIN GARFIELD FINK
COLUMBIA UNIVERSITY

FORTY years ago industrial research laboratories were practically unknown. To-day no industry can exist without a research laboratory. The phenomenal advances in aviation, electric communication, textiles, corrosion-resistant metals, plastics, vehicles of locomotion, paints and pigments, etc., etc. (to mention but a few of the hundreds of new products and processes introduced during these past forty years) would have been well-nigh impossible without the industrial research laboratories with their facilities and highly trained personnel.

And these research laboratories are not confined to this country, although by far the larger majority of them are now in America. But there are many laboratories in Europe, in Canada, in Russia and elsewhere that are of the very highest type and that have made important contributions to science and industry.

It has often been said that there is a very decided difference between American and European research and that American research was largely of the "development" type, whereas European was predominantly fundamental or radical research. During the early years of industrial research in this country the above statement was largely true. But it is no longer true to-day.

To avoid any misunderstanding, since we are going to enlarge upon "fundamental research," the usual interpretation of fundamental or radical research is that devoted to finding entirely new products or entirely new processes radically different from anything that has gone before. For example, we have John W. Hyatt's work on the production of billiard balls out of a plastic in place of elephants' tusks. This is a case of fundamental research, whereas development research would imply how to raise more elephants or how to alter their feed to develop longer and bigger tusks.

Another example is the research of D. MacFarlan Moore on electric lighting. Abandoning the incandescent lamp he devoted his energies to discovering the secret of the firefly's glow. To-day the fluorescent lamp is the outcome of Moore's researches, granting of course that in recent years a host of researchers have contributed to this fluorescent lamp. But here again this latter-day research was largely of the "development" type.

FUNDAMENTAL RESEARCH

Personally, during my forty years of research experience, I have always been interested primarily in *fundamental* research rather than development research. And my advice to the young student who has inclinations toward research is to choose the fundamental type rather than the development type. We grant that "the work is harder, the standards higher and the discipline more rigorous" than in any other phase of scientific or technical occupation. But on the other hand the interest and excitement, the stimulation and satisfaction are greater by far than in any other field of scientific or technical endeavor.

To give you some idea of what we mean by *initiative in research* we shall give you brief accounts of a few of the many researches that have been carried out under our direction.

THE PLATINUM SUBSTITUTE PROBLEM

When Edison made his first incandescent lamps at Menlo Park he used comparatively thick platinum

[1] Lecture delivered at the Stevens Institute of Technology on April 14, 1943.

wires in the seal to carry the current to the filament. Examining some of the old broken lamps we found in the dump at Menlo Park we estimated that at present prices for platinum the cost per lamp approximated \$5.00. But in Edison's day platinum was cheaper than gold. The problem of finding a leading-in wire decidedly cheaper than platinum was a very old problem—at least 30 years old—when brought to our attention. At the time I was carrying out researches at the Edison Lamp Works in Harrison, N. J.

It was generally agreed that the reason why platinum was being used as an air-tight seal in glass was on account of its coefficient of expansion—almost identical to that of glass, the lead potash glass universally used at the time. Accordingly, it was again generally agreed that any wire that was to be used in place of platinum would have to have the same coefficient of expansion as that of glass. But this idea had persisted for thirty years without bearing fruit: And it occurred to us that there must be something radically wrong with this idea—or possibly that some factor other than coefficient of expansion was at the basis of the solution of the problem. While our own efforts proceeded along fundamental lines, our works manager encouraged "development" research, that is, using shorter pieces of platinum and thinner pieces welded between ends of short copper wires, using platinum ribbon in place of platinum wire, etc., etc. Our platinum budget ran to 7½ million dollars a year and every bit of platinum per lamp saved was appreciable.

We tackled the problem from an entirely different angle. We argued that if a metal or alloy could be found that would adhere tenaciously to glass and would be very soft (like rubber, for example) then, no matter what the coefficient of expansion of the metal might be, this metal would expand and contract as dictated by the more rigid glass.

Accordingly, we selected five commercially available metals and annealed these "dead soft," as we say in metallurgy: nickel, copper, cobalt, iron and silver. To compare results we added platinum.

These six metals were in the shape of thin wires. These wires were sealed into glass and lamps were made up. The lamps were allowed to stand for three days and then were tested for leaks. The performance was surprisingly good, even though many leaky lamps had developed on standing. Examining the seals under the microscope we observed that the *copper and cobalt wires* showed that the wetting quality or the union between glass and metal was exceptionally good —even better in certain respects than between glass and platinum in spite of the fact that the coefficients of expansion for copper and cobalt are much higher than that for platinum—cobalt 138 per cent. of platinum and copper 188 per cent. of platinum; in other words, both cobalt and copper should contract away from the glass on cooling.

To further test our hypothesis we prepared another batch of samples of the six metals: But this time we prepared them in the shape of fine ribbons 3/16 inch wide and again thoroughly annealed.

We next took a glass rod and mounted it horizontally, then heated it at a half a dozen points and sealed the ribbons onto (not into) the glass. After cooling we attached weights to the other ends of the ribbons. Much to our satisfaction we found that the copper ribbon would support the heaviest weight, next the cobalt ribbon, then the nickel, the silver and the platinum ribbons and finally the iron ribbon which showed the poorest adhesion.

On the basis of these findings we developed a thin-walled copper tube seal[2] which was made to carry either a fraction of an ampere or several hundred amperes. The adhesion of copper to glass was so perfect that lamp tests showed fewer leaks than with platinum. From this first product a second one followed, the so-called dumet wire. In this we again applied the thin tube of copper but filled the copper tube with an alloy of nickel and iron which had a coefficient of expansion lower than glass.[3] This platinum substitute (dumet) is to-day used in all radio tubes, electric lamps and dozens of other types of evacuated glass containers.

THE INSOLUBLE ANODE PROBLEM

When this problem was brought to us the specifications were as follows: The anode must be practically insoluble in a copper sulfate solution containing nitric and hydrochloric acids besides sulfuric. A further specification was that the anode must not contain any element which even though entering the solution in minute quantities might co-deposit with the copper and thereby seriously affect the electrical conductivity.

The solution of this problem was approached from two angles: development research and fundamental research. In the development research we started out by testing anodically well-known acid-resistant alloys such as duriron and nichrome and modified the composition of these to increase their acid resistance. Many hundred alloys were made up, but not one of these was entirely satisfactory.

In the fundamental research approach we considered carefully the reactions that took place at the anode. There were principally two anodic reactions in which we were interested. One was the dissolution of metal—which we wanted to be practically zero— and the other was the evolution of oxygen—which we

[2] Fink and Koerner, U. S. Pat. 1,273,758 (July 23, 1918).

[3] Brit. Pat. 23,775 (October 17, 1912); U. S. Pat. 1,498,908 (June 24, 1924).

wanted to be 100 per cent. We concluded that our efforts be bent toward facilitating and promoting the evolution of anodic oxygen. Accordingly, we conceived the idea that if we used a copper plate and covered the surface with an oxygen catalyst the copper would not go into solution. We tried out several catalysts and lo and behold the scheme worked. But there was one hitch in the scheme: It worked only when current was applied: Shutting off the current with the copper plate still immersed would cause the plate to dissolve. Accordingly some provision had to be made for the protection of the copper plate during current interruptions. This part of the problem was solved by adding about 20 per cent. silicon to the copper so that the final anode[4] as introduced at Chuquicamata and in use for many years is a copper silicide alloy with a catalytic surface. The catalysts are manganese dioxide, tin oxide and lead dioxide.

Alplate

Alplate is the name applied to aluminum-coated steel, aluminum-coated nickel and other metals. The problem of substituting molten aluminum for molten zinc in hot galvanizing was a very old problem when we started our research. Many schemes had been tried, but not one of these developed into a commercial process. One big stumbling block in the past had been the refusal of the aluminum to wet the steel or other metal. You can stick an iron rod into a pot of liquid aluminum and upon withdrawing it the rod will be just as free from aluminum as it was to begin with.

In the development type of research investigators imitated the detailed practice used in hot galvanizing trying out various fluxes, temperatures, speeds, additions to the aluminum, etc. But all these experiments had failed.

In applying the principles of fundamental research it was necessary to carefully study the reason why liquid aluminum would not wet steel. It was not enough, however, to conclude that it was an oxide film on the aluminum that prevented the formation of a good bond between aluminum and steel. The problem now resolved itself into finding a method or means of avoiding the formation of the oxide film or adding some reagent that would counteract the formation of the oxide of aluminum.

One scheme that gave us much encouragement was the introduction of the steel rod or steel strip through a restricted orifice in the pot filled with molten aluminum. But this was not enough: Results were not uniformly good.

The next step solved the problem satisfactorily. It is well known that aluminum oxide is one of the most refractory oxides known and very difficult to reduce to metal. However, there is one reducing agent that will produce the metal and that is atomic hydrogen. It is also well known that iron will dissolve or take up hydrogen and that the amount taken up varies with the temperature. These were the building stones out of which we constructed our final process.

The steel to be coated with aluminum is heated in an atmosphere of hydrogen and allowed to absorb and adsorb as much hydrogen as possible. Then the steel passes into a bath of molten aluminum kept at a temperature *lower* than that of the hydrogen furnace so that the hydrogen is forced out of the steel during the very instant that the steel contacts the aluminum. As a net result a strongly adherent uniform coating of aluminum is produced on the steel.[5]

The Restoration of Badly Corroded Bronz Art Objects

This problem was brought to us by the Metropolitan Museum of Art. The various methods employed up to that time were very radical. In most cases knives, chisels and hammers were used, in other cases strong acids. But the results achieved were very unsatisfactory and the "restored" product obtained was seldom of any value from an artistic point of view.

Our approach to the problem was based on entirely different principles. We observed in a number of bronzes that the outer contour of the crust of corroded metal was an enlargement of a design which undoubtedly was the original design of the bronze. Further study and experiments on corroded bronze and copper objects led to our discovery that "the detail of design is retained by the crust." Of this we were very certain, and the next step was to find a method of reversing the process of corrosion and bringing back the original design. We decided that an electrochemical, cathodic process was the only practical solution. After some further experimentation the following procedure was standardized and this is now used all over the world: The corroded object is suspended as a cathode in a 2 per cent. solution of sodium hydroxide and a few milliamperes passed through the solution. Very gradually the carbonates and oxides are reduced back to metal. The electric current required is a matter of a few milliamperes per square foot of surface of the bronze or copper object. And the time required is usually a matter of days or weeks, depending upon how far the corrosion of the metal has proceeded. In many cases the objects submitted to us for restoration have no trace of metal left, just a mass of corrosion product.

This research on the restoration of badly corroded bronzes has led to other developments, notably the

[4] U. S. Patents Nos. 1,441,567 and 1,441,568 (January 9, 1923).

[5] U. S. Pat. No. 2,082,622 (June 1, 1937).

authentication and attribution of various objects of art and various articles of antiquity not necessarily artistic. The very observations made during restoration are among the most crucial observations in deciding whether a piece of metal, stone, wood or cloth is old or of recent origin.

Other Researches

In the few examples above we have tried to indicate the main points of difference between development research and fundamental or radical research. We might go on to tell you how these principles of radical research were applied to:

(1) Our researches on chromium plating and the discovery of the relatively narrow limits of catalytic agent quantities that must be added to the plating bath—a radically new observation, never before applied to any plating bath.

(2) Our various researches on tungsten, its crystal structure, crystal growth, its ductility, etc.

(3) Our research on bright nickel plating.

(4) On high chrome-irons.

(5) On electrolytic manganese.

(6) Our researches on corrosion of metals and various means of combating same, etc., etc.

New Problems

The world is full of new chemical and electrochemical problems that await the young investigator. Typical of these many "yet-to-be-solved" problems is the one on *rubber:* Although there are several synthetic rubbers that closely compete with the natural variety, a "complete" substitute is yet to be found. Might it not be worthwhile to investigate the possibilities of growing rubber trees in the temperate zone? We have had definite indications in our experiments that the tropical rubber tree, *hevea brasiliensis,* may be converted into a deciduous tree of the cooler climates by proper treatment and control of the soil constituents.

Among the numerous other problems that await the young lad with vision and courage are:

(1) The perfection of electric lighting ten times as efficient as any present type.

(2) An improved automobile gas engine operating at three or four times the efficiency of the present one.

(3) A paint for wooden structures that is rainproof and sunproof.

(4) An alloy of aluminum as resistant to fatigue as steel.

(5) A metal or other material to take the place of our rapidly dwindling resources of copper—or of lead.

(6) A material to take the place of leather for shoes with all the good, or even better, qualities of leather.

And many, many more problems.

Throughout the ages there have always been young men endowed with the research spirit or research instinct. Very often this is a latent talent that needs to be aroused. And the best procedure in arousing this most valuable talent is to become interested in one or the other phase of science or engineering and then to select one or the other individual topic and apply oneself diligently to this. But reading alone is not sufficient in preparing for research. The laboratory or workshop—no matter how primitive and incomplete—is a most essential adjunct. Reading without experimentation hardly ever leads to the desired results. On the other hand, experimentation with little or no library work frequently leads to radical discoveries.

And in conclusion let me say to the young man: The opportunities in research are greater to-day than ever before. And the chances of finding new products and new processes have never been equalled in the past.

OBITUARY

WILLIAM FRANCIS MAGIE
1858–1943

William Francis Magie was born in Elizabeth, N. J., on December 14, 1858, and died in Princeton on June 6, 1943. He was the son of William Jay Magie, a former chancellor of the State of New Jersey. He graduated from Princeton as valedictorian of the class of 1879, a class that had many other distinguished members, including Woodrow Wilson.

After graduation he remained in Princeton as assistant to Dr. Brackett, then Henry professor of physics. Having decided to make physics his life work it was natural at that time for him to go to Germany to pursue advanced work. He matriculated at the University of Berlin and took his doctor's degree under the direction of Helmholtz in 1885. His dissertation was an experimental study of the theory of capillarity.

Returning to Princeton he was appointed to an instructorship in physics during the presidency of James McCosh, advancing to a professorship in 1890. In 1889 Dr. Brackett founded the graduate school of electrical engineering at Princeton and devoted most of his attention to it. Although Dr. Brackett remained the chairman of the department of physics, Magie became more and more responsible for the actual administration of the department, and its expansion from very small beginnings was very largely