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ATOMIC HYDROGEN AS AN AID TO INDUSTRIAL RESEARCH[1]

It is difficult for me to express adequately my appreciation of the honor you have conferred upon me through the award of this beautiful medal. I wish to thank you and also my other friends, Hendrick and Whitney, for the kind, although exaggerated words spoken about me.

I am sure that those of you who know Whitney well, or in fact all you who have just heard him speak, will understand that this medal should properly be regarded as a second award to him. The work for which this medal is now given was made possible by the remarkable inspiration and guidance which only Whitney can give.

Although I am a chemist by training, my work during recent years has been mostly in physics. The chemical work I have done has been largely purely scientific, started, at least, without thoughts of practical applications. Thus when I received notice of the action of the Perkin Medal Committee, while I was in Italy a few months ago, it puzzled me that I should have been chosen, especially when I considered that the medal is given for work in applied chemistry. I can assure you that I am not guilty of any deep laid plan to secure this medal. It came to me as a complete surprise—a surprise similar to those I have experienced when I have first realized that some purely scientific observations that I had made were capable of industrial applications.

On returning home from abroad, and finding that the committee had made the award mainly for my work on atomic hydrogen and its applications to welding, I was at first inclined to choose as a subject for this address an account of other work on atomic hydrogen completed more than ten years ago, but which I have neglected to publish except in such abbreviated form that it is nearly useless. Since one of the objects of the Perkin Medal is to stimulate research, it would seem particularly fitting that this award should thus lead directly to the publication of the results of investigations which would otherwise largely be lost.

[1] Address given on the occasion of the presentation of the Perkin medal, on January 13, at a joint meeting of the Society of Chemical Industry, Société de Chimie Industrielle, American Chemical Society and American Electrochemical Society.

The work that I refer to was experimental work extending over more than a year, on the kinetics of the interaction between hydrogen and oxygen at low pressures in contact with a hot tungsten filament. At temperatures below 1600°K the oxygen oxidizes the tungsten exactly as though no hydrogen were present, but an adsorbed monatomic film of oxygen poisons the filament so that the hydrogen can not be dissociated into atoms as it would be if there were no such film. The hydrogen is incapable of interacting directly with this adsorbed oxygen. However, after the oxygen has been used up by this reaction and gradual evaporation of the film has exposed a few tungsten atoms on the surface of the filament, the hydrogen suddenly and almost instantly removes the rest of the oxygen in the film and thus permits the dissociation of the remaining hydrogen. Without the hydrogen, the oxygen film would not evaporate appreciably. These experiments prove that the interaction between oxygen and hydrogen on tungsten takes place only between adjacent adsorbed atoms—a kind of flank attack. A direct action between the oxygen and hydrogen does not occur. These phenomena are clearly identical in character with the electro-chemical phenomena of passivity—a subject which has not been well understood.

This work, however, as you see, is pure physical chemistry, and so is not really a suitable subject for this address. I trust, however, that this award will stimulate research equally well by leading me to publish this material in its proper place at an early date.

Somewhat over a year ago I read a paper before the American Chemical Society on atomic hydrogen and its application to the welding of metals. Although the industrial applications of this process are developing rapidly, I have not been particularly closely associated with this recent progress, and therefore do not feel qualified to address you on this subject.

I believe the primary object of the Perkin Medal is to do honor to the memory of Sir William Perkin, that pioneer who devoted himself to pure scientific research after having led in the industrial applications of research for fifteen years. This object is best attained by encouraging the kind of research that he valued so highly. The medal should thus be regarded not as a reward for accomplishment nor as a prize to stimulate competition in research, but rather as a means of directing attention to the value of research and to the methods of research that are most productive. Having this in mind, I am going to tell you, although somewhat reluctantly, the history of some of my own work, in so far as it illustrates a method of industrial research that has proved valuable.

Two Types of Industrial Research

The leaders of industries are frequently conscious of the need of improvement in their processes and even of the need of new discoveries or inventions which will extend their activities.

It is thus logical and often extremely profitable to organize research laboratories to solve specific problems. Efficiency requires that the director shall assign to each worker a carefully planned program. Experiments which do not logically fit in with this program are to be discouraged.

This type of industrial research, which should often be called engineering rather than research, has frequently been very successful in solving specific problems, but usually along lines already foreseen.

This method, however, has serious limitations. Directors are rare who can foresee the solutions sufficiently well to plan out a good campaign of attack in advance. Then, too, the best type of research man does not like to be told too definitely what must be the objects of his experiments. To him scientific curiosity is usually a greater incentive than the hope of commercially useful results. Fortunately, however, with proper encouragement, this curiosity itself is a guide that may lead to fundamental discoveries, and thus may solve the specific problems in still better ways than could have been reached by a direct attack; or may lead to valuable by-products in the form of new lines of activity for the industrial organization.

Of course no industrial laboratory should neglect the possibilities of the first and older method of organized industrial research. I wish, however, to dwell this evening upon the merits of the second method in which pure science or scientific curiosity is the guide.

The History of the Gas-Filled Lamp

I first entered the research laboratory of the General Electric Company in the summer of 1909, expecting in the fall to return to Stevens Institute, where I had been teaching chemistry. Instead of assigning me to any definite work, Dr. Whitney suggested that I spend several days in the various rooms of the laboratory, becoming familiar with the work that was being done by the different men. He asked me to let him know what I found of most interest as a problem for the summer vacation.

A large part of the laboratory staff was busily engaged in the development of drawn tungsten wire made by the then new Coolidge process. A serious difficulty was being experienced in overcoming the "off-setting" of the filaments, a kind of brittleness which appeared only when the lamps were run on alternating current. Out of a large number of samples of wire, three had accidentally been produced which gave lamps that ran as well with alternating as with direct current, but it was not known just what had made these wires so good. It seemed to me that there was one factor that had not been considered, that is, that the off-setting might possibly be due to impurities in the wire in the form of gases. I therefore suggested to Dr. Whitney that I should like to heat various samples of wire in high vacuum and measure the quantities of gas obtained in each case.

In looking through the laboratory I had been particularly impressed with the remarkably good methods that were used for exhausting lamps. These methods were, I thought, far better than those known to scientific research workers. My desire to become more familiar with these methods was undoubtedly one of the factors that led me to select for my first research an investigation of the gas content of wires.

After starting the measurements that I had planned, I found that the filaments gave off surprisingly large quantities of gas. Within a couple of weeks I realized that something was entirely wrong with my apparatus, because from a small filament in a couple of days I obtained a quantity of gas which had, at atmospheric pressure, a volume 7,000 times that of the filament from which it appeared to have come; and even then there was no indication that this gas evolution was going to stop. It is true that in the literature, for example in J. J. Thomson's book on the "Conduction of Electricity Through Gases," one found many statements that metals in vacuum give off gases almost indefinitely, and that it is impossible to free metals from gas by heating. Still I thought that 7,000 times its own volume of gas was an entirely unreasonable amount to obtain from a filament. I spent most of the summer in trying to find where this gas came from, and never did investigate the different samples of wire to see how much gas they contained. How much more logical it would have been if I had dropped the work as soon as I found that I should not be able to get useful information on the "offsetting" problem by the method that I had adopted.

What I really learned during that summer was that glass surfaces which had not been heated a long time in vacuum slowly give off water vapor, and this reacts with a tungsten filament to produce hydrogen, and also that vaseline on a ground glass joint in the vacuum system gives off hydrocarbon vapor, which produces hydrogen and carbon monoxide.

To me, however, that summer's work was so interesting that I dreaded to return to the comparative monotony of teaching, and gladly accepted Dr. Whitney's offer to continue at work in the laboratory. No definite program of work was laid down. I was given first one assistant and then others to continue experiments on the sources of gas within vacuum apparatus, and a study of the effects produced by the introduction of various gases into tungsten filament lamps. The truth is that I was merely curious about the mysterious phenomena that occur in these lamps. Dr. Whitney had previously found that gases have a habit of disappearing in lamps, and no one knew where they went to, so I wanted to introduce each different kind of gas that I could lay my hands on into a lamp with a tungsten filament and find out definitely what happened to that gas.

It was the universal opinion among the lamp engineers with whom I came in contact that if only a much better vacuum could be produced in a lamp, a better lamp would result. Dr. Whitney particularly believed that every effort should be made to improve the vacuum, for all laboratory experience seemed to indicate that this was the hopeful line of attack on the problem of a better lamp. I felt, however, that I really didn't know how to produce a better vacuum, and instead, proposed to study the bad effects of gases by putting gases in the lamp. I hoped that in this way I would become so familiar with these effects of gas that I could extrapolate to zero gas pressure, and thus predict, without really trying it, how good the lamp would be if we could produce a perfect vacuum.

I should like to add here parenthetically, that this principle of research is one which I have found extremely useful on many occasions. When it is suspected that some useful result is to be obtained by avoiding certain undesired factors, but it is found that these factors are very difficult to avoid, then it is a good plan to increase deliberately each of these factors in turn so as to exaggerate their bad effects, and thus become so familiar with them that one can determine whether it is really worth while avoiding them. For example, if you have in lamps a vacuum as good as you know how to produce, but suspect that the lamps would be better if you had a vacuum say 100 times as good, it may be the best policy, instead of attempting to devise methods of improving the vacuum, to spoil the vacuum deliberately in known ways, and you may then find either that no improvement in vacuum is needed, or may find just how much better the vacuum needs to be.

During these first few years, while I was thus having such a good time satisfying my curiosity, and publishing various scientific papers on chemical reactions at low pressures, I frequently wondered whether it was quite fair that I should spend my whole time in an industrial organization on such purely scientific work, for I confess I didn't see what applications could be made of it, nor did I even have any applications in mind. Several times I talked the matter over with Dr. Whitney, saying that I could not tell where this work was going to lead us. He replied, however, that it was not necessary, as far as he was concerned, that it should lead anywhere. He would like to see me continue working along any fundamental lines that would give us more information in regard to the phenomena taking place in incandescent lamps, and that I should feel perfectly free to go ahead on any such lines that seemed of interest to me. For nearly three years I worked in this way with several assistants before any real application was made of any of the work that I had done.

In adopting this broad-minded attitude Dr. Whitney, I believe, showed himself to be a real pioneer in the new type of modern industrial research.

For my study of the effect of gases, I had to devise new types of vacuum apparatus. I needed particularly to be able to analyze the small quantities of gas that existed in the tungsten lamp. With some of this special apparatus I was able to make a practically complete quantitative analysis of an amount of gas which would occupy about 1 cubic millimeter at atmospheric pressure. In this sample of gas we could determine the percentages of oxygen, hydrogen, nitrogen, carbon dioxide, carbon monoxide and the inert gases.

In regard to the fate of the different gases which I introduced into the lamp bulb, I found that no two gases acted alike. Oxygen attacked the filament and formed tungstic oxide WO_3. That seemed simple enough, but I found that the kinetics of the reaction presented many features of considerable scientific interest.

In studying the effect of hydrogen, very peculiar phenomena were observed. A limited amount of hydrogen disappeared and became adsorbed on the bulb where it remained in a chemically active form, capable of reacting with oxygen at room temperature, even long after the tungsten filament had been allowed to cool. This suggested hydrogen atoms and seemed to confirm some conclusions that I had already drawn from observations on the heat losses from tungsten filaments in hydrogen at atmospheric pressure. In making squirted tungsten filaments and sometimes in cleaning the drawn wire, filaments were heated in this manner in hydrogen. Because of the fact that tungsten filaments melt at a temperature more than 1500° higher than platinum, it had seemed to me that tungsten furnishes a tool of particular value for the scientific study of phenomena in gases at high temperatures. From my work on lamps I knew approximately the relation between the resistance of tungsten wire and its temperature, and could thus use a tungsten wire as a kind of resistance thermometer. By connecting a voltmeter and an ammeter to the tungsten filament which was being heated in hydrogen, I could determine the temperature from the resistance and also find the heat loss from the filament in watts. I wanted to see if anything abnormal happened when the temperature was raised to the extremes which were only possible with tungsten.

The results greatly interested me, for they showed that the energy loss through the gas, which increased in proportion to the square of the temperature up to about 1800°K, increased at a much higher rate above that, until at the highest temperatures the energy varied in proportion to about the fifth power of the temperature. This result could be explained if the hydrogen at high temperatures were dissociated into atoms. The diffusion of the hydrogen atoms from the filament, and their recombination at a distance from it would cause an enormous increase in heat conduction. After publishing these preliminary results, I was naturally much interested in getting any other information I could in regard to the properties of these hydrogen atoms. A very large number of experiments, extending over several years, were thus made in this study of atomic hydrogen. Nearly all of these experiments would have seemed quite useless, or even foolish, to a man who was making a direct and logical attack on the problem of improving tungsten lamps.

When nitrogen at low pressure was introduced into a bulb containing a tungsten filament at extremely high temperatures, such as 2800°K, the nitrogen disappeared at a rate which was independent of its pressure—in other words, here was a case of a reaction of zeroth order. This suggested that the reaction velocity was limited by the rate at which the tungsten evaporated from the filament. To check this hypothesis the rate of loss of weight of filaments at various temperatures was measured in good vacuum. This rate varied with the temperature in accordance with known thermodynamic laws, and since the rate per unit area was independent of the size of the filament, it was concluded that the loss of weight was really due to evaporation and not to chemical action of residual gases or to electric currents that passed from the filament to the surrounding space.

A comparison of the rate of disappearance of nitrogen with the loss of weight in the filament showed that one molecule of nitrogen disappeared for every atom of tungsten that evaporated. A brown compound WN_2 was formed which deposited on the bulb and decomposed when water vapor was introduced, forming ammonia gas.

From time to time the question kept arising—how good would a lamp be if it had a perfect vacuum; and now, from studies of the character I have described, I began to have an answer. Hydrogen, oxygen, nitrogen, carbon monoxide, and in fact every gas that I introduced, with the exception of water vapor, did not produce blackening of the lamp bulb. The serious blackening that occurred with only small amounts of water vapor depended upon a cyclic reaction in which atomic hydrogen played an essential part. The water vapor molecules coming in contact with the hot filament produce a volatile oxide of tungsten, and the hydrogen is liberated in atomic form. The volatile oxide deposits on the bulb where it is reduced to the metallic state by the atomic hydrogen, while the water vapor produced returns to the filament, and causes the action to be repeated indefinitely. Thus a minute quantity of water vapor may cause a relatively enormous amount of tungsten to be carried to the bulb.

The question then arose whether the traces of water vapor, which might still exist in a well-exhausted lamp, were responsible for the blackening which limited the life or the efficiency of many of these lamps. We made some tests in which well-made lamps were kept completely immersed in liquid air during their life so that there could be no possibility of water vapor coming in contact with the filament. The rate of blackening, however, was exactly the same as if no liquid air had been used.

Having thus proved that the blackening of a well-made lamp was solely due to evaporation, I could conclude with certainty that the life of the lamp would not be appreciably improved even if we could produce a perfect vacuum.

Early in 1911 Mr. William Stanley, one of the pioneers in the electrical industry, felt that our company should do more fundamental work in connection with heating devices. Since I had become interested in the theory of heat losses from filaments in gases, I was glad to do work along these lines, so that I undertook to direct a small laboratory at Pittsfield, Mass., at which I spent about two days a week. Besides studying the heat losses from plane surfaces at various temperatures, I measured the heat losses from wires of various sizes in air at different temperatures, working at first with platinum wires, and was able to develop a theory of the heat losses which enabled me to calculate the loss from a wire of any size at any temperature in any gas, assuming however that the gas did not dissociate at high temperatures.

Having now a definite theoretical basis on which to calculate the normal loss by convection, I was able to prove that the abnormal rate of heat loss which I had previously observed with tungsten filaments at high temperatures in hydrogen was due to actual dissociation, and in fact I was thus able to calculate the heat of dissociation and the degree of dissociation at different temperatures.

However, to make sure of these conclusions, I wished to make measurements of the heat losses in gases which could not possibly dissociate, and therefore undertook experiments with heated tungsten wires in mercury vapor at atmospheric pressure. A little later I made experiments with nitrogen to see if this gas dissociated at high temperatures but found that it did not do so. In both of these gases the filaments could be maintained at temperatures close to the melting point for a far longer time than they could be if heated in vacuum at the same temperature. Thus the rate of evaporation was greatly decreased by the gas, many of the evaporating tungsten atoms being brought back to the filament after striking the gas molecules.

By this time I was familiar with all the harmful effects which gas can produce in contact with filaments and knew under what conditions these bad effects could be avoided. Particularly I realized the importance of avoiding even almost infinitesimal traces of water vapor. Thus, when I found a marked effect of mercury vapor and nitrogen in reducing the rate of evaporation, it occurred to me that it might be possible to operate a tungsten filament in gas at atmospheric pressure and obtain a long useful life. Of course, it would be necessary to raise the temperature far above that at which the filament could be operated in vacuum so as to compensate for the serious loss in efficiency due to convection, by the improved efficiency resulting from the rise in filament temperature. Whether or not the increased rate of evaporation, due to this increase in temperature, would more than offset the decrease in the rate due to the gas, was a matter that could only be tested by experiment.

In connection with the studies of the heat losses from filaments of various diameters at incandescent temperatures, I had found that the heat loss increased only very slowly with the diameter, so that the loss per unit area from a small filament was enormously greater than from a large filament. Calculations showed that it was hopeless to get practical lamps with filaments in nitrogen, if these filaments were of very small diameter. For example, a filament one mil in diameter, which corresponds to an ordinary 25 watt lamp, if run in nitrogen at atmospheric pressure would consume 4.8 watts per candle at a temperature of 2,400° K, which would give 1 watt per candle with a filament in vacuum. This great loss in efficiency is due to the cooling effect of gas. To bring back the efficiency of the gas-filled lamp to that of the vacuum lamp, it would be necessary to raise the temperature from 2,400 to 3,000° K, which would have caused a 2,000-fold increase in the rate of evaporation, and such an increase could certainly not be compensated for by the effect of the gas in retarding the evaporation.

With filaments of much larger diameter, however, the effect of the gas in decreasing the efficiency was not nearly so marked. We therefore constructed lamps having filaments of large diameter in the form of a single loop and filled these lamps with nitrogen at atmospheric pressure. We ran these lamps with a filament temperature so high that in spite of the gas the efficiency corresponded to about 0.8 watt per candle, instead of the usual 1 watt per candle at which we tested our vacuum lamps. We were disappointed to find that these lamps blackened much more rapidly than vacuum lamps of similar efficiency, so that the total useful life of the lamp was short.

This result, which is what most lamp engineers would have expected, seemed to indicate that the necessary rise in temperature to offset the heat losses by the gas increased the evaporation by more than the amount of the reduction in evaporation due to the gas. If I had not previously become so familiar with the behavior of various gases, this discouraging result might easily have stopped further experimenting in this direction. However, I noticed that the bulb had *blackened* during the short life of the lamp whereas from my knowledge of the interaction of tungsten and nitrogen I had expected a deposit of a clear brown color. I felt that the black deposit, therefore, could mean only one thing—water vapor, notwithstanding the fact that to avoid this water vapor we had taken precautions which were greater, I believe, than had ever been used before for the preparation of moisture-free gases and glass surfaces. We were thus led to take still greater precautions and use still larger bulbs so that the glass surfaces could not become overheated by the convection currents in the gas that rose from the filament. We were then soon able to make lamps having a life of over 1,000 hours with an efficiency about 30–40 per cent. better than could have been obtained with filaments in vacuum.

As I look back upon these experiments I feel that we were very fortunate at that time in not having had at our disposal a supply of argon gas. From theoretical reasons I had concluded that argon should be better than nitrogen, and if I had had argon I should therefore probably have tried it first. If these lamps had blackened, because of traces of water vapor, I would naturally have attributed this to the increase in evaporation caused by the high temperature, and would have had no reason for suspecting that water vapor was the cause of the trouble, for, of course, in argon a brown deposit would not be expected in any case.

The lamps that we were able to make in this way, with an improved efficiency, were limited to those which took a current of 5 amperes or more, so that the method was not applicable for 110-volt lamps with less than 500 watts. Some time later, however, it occurred to me that the benefits derived from the large diameter of the filament could be obtained with one of smaller diameter by coiling the filament in the form of a helix, bringing the turns of the helix very close together. In this way, and by the use of improved tungsten filaments that do not sag so readily at high temperatures, and by using argon instead of nitrogen, it has gradually been possible to construct gas-filled lamps which are better than vacuum lamps down to wattages of about 40 or 50 watts. These smaller lamps, although not much better in efficiency than the vacuum lamp, have the advantage of giving a much whiter light. In the case of the larger lamps, the use of the gas filling together with the special construction of the lamp more than doubles the efficiency.

The invention of the gas-filled lamp is thus nearly a direct result of experiments made for the purpose of studying atomic hydrogen. I had no other object in view when I first heated tungsten filaments in gases at atmospheric pressure.

Even at the time that I made these experiments at higher pressures, they would have seemed to me useless if my *prime object* had been to improve the tungsten lamp.

I hope I have made clear to you the important rôle that properly encouraged scientific curiosity can have in industrial research. This illustration that I have given is not at all exceptional. I could have given any one of several others equally well.

Many industrial laboratories have followed Dr. Whitney's lead in devoting a fairly large fraction of their activities to these rather purely scientific researches. Certain men at least are not expected to be responsible for practical applications, but are freely allowed to make fundamental scientific investigations. The type of man who does this work best can usually be attracted only to those industrial laboratories that have adopted this policy.

However, I do not believe that this second method of research is growing in popularity solely because it is found to be profitable. I feel rather that most of our leaders in industrial research are eager to adopt this method, in so far as economic factors may permit, because they realize the debt that modern industry owes to the pure science of the past and because the modern conceptions of service and the growing *esprit de corps* of American industry help make them glad of any opportunity to contribute to scientific knowledge. I know personally that such motives as these have guided Dr. Whitney in the leadership he has taken.

I believe in the near future there will be a much increased demand for men with scientific training who are capable of doing more independent thinking.

Better Education Needed

Our schools and universities devote so much effort to imparting information to students that they almost neglect the far more important function of teaching the student how to get for himself what knowledge of any subject he may need. Even in grammar school, children are crammed with more information on arbitrarily selected subjects than even the average well educated adult can retain

Of course students should be taught the fundamental principles of mathematics and of various sciences, as well as of other subjects, but much of the knowledge of data upon which these principles depend and other necessary information should be obtained by the efforts of the student through experimentation and individual reading.

As I look back on my own school and college days, it seems to me that the things of most value were learned spontaneously through interest aroused by a good teacher, while the required work was usually comparatively uninteresting. The university student should have leisure for some independent work and opportunities for continuing his interest in hobbies of various kinds which he should have had long before he entered college. I realize that it is difficult so to arouse the student's interest that he will spend the added leisure in these ways rather than in spending still more on the bleachers cheering the football team in their practice games. But a well planned effort is worthwhile.

The importance of arousing even a young boy's interest in independent work can hardly be overemphasized. My real interest in science was derived from my brother Arthur, who encouraged me to have a workshop at the age of 9, and later a laboratory when I was only 12.

I can illustrate my father's influence in stimulating independence by the following incident. When I was 12 I climbed one or two Swiss mountains of moderate height with my older brother Arthur. Soon after Arthur had to go to Heidelberg to arrange for his studies, thus leaving me with my mother and younger brother at a hotel in the Rhone Valley. I had become so enthusiastic over mountain climbing that I wished to climb everything in sight, but the dangers of Alpine climbing were such that my mother did not dare let me go alone. When my father arrived for a week-end visit from Paris, he consented to allow me to climb alone any mountain I liked if I would promise to do it in accord with the following three rules: (1) I must stay on a distinct trail; (2) I must use the same trail going and returning; (3) I must make certain of returning at 6 o'clock by allowing as much time for descending as for ascending. Before these rules went into effect, however, I had to prove that I could and would make such sketches, maps and notes of the trails used for the ascent that I could always return by the same route. I thus climbed several mountains about 7,000 feet high, often requiring several days of repeated effort before I could discover a route that led to the top. Perhaps it is this experience which makes me even to-day always wish to find my own way rather than be told the way.

Until I was fourteen I always hated school and did poorly at it. At a small boarding school in the suburbs of Paris, however, being an American and having a friend who was influential with the head of the school, I was freed from much of the absurdly rigorous discipline to which the French boys were subjected. Thus, I could spend time alone in the school laboratory and was encouraged by one of the teachers to learn to use logarithms and solve problems in trigonometry, subjects not required by the curriculum.

I have been fortunate in having many wonderful teachers. Three of them have been recipients of this Perkin Medal. Whittaker and Chandler were my teachers at Columbia and Whitney during the last eighteen years. Professor R. S. Woodward, at Columbia, in connection with his courses in mechanics, was extremely stimulating and encouraged me to choose and solve my own problems for class work instead of those required in the regular course.

I should like to see spontaneous work of this kind take a much more prominent part in our educational system—at least for students who have more than average ability.

The Value of Hobbies

Very great benefit may be derived from hobbies. Probably each person should have several of them. Just recently I met a small boy, only six years old, who had an overpowering, wide-eyed enthusiasm for collecting insects. He weighed each one of them within a milligram, and then, after desiccating them thoroughly over calcium chloride, weighed them again. Many elaborate notes and even correspondence resulted. I am afraid our universities, with their dormitories and other standardizations, tend to discourage such wholesome individual activities.

Of course, after talking of hobbies, I can not resist the temptation to tell you something of my own. Perhaps my most deeply rooted hobby is to understand the mechanism of simple and familiar natural phenomena. I will give only two illustrations, but these, I hope, will make you see how easy it is to find around us simple phenomena that are not well understood.

Every chemist knows that after he stirs a liquid in a beaker having a precipitate in the bottom, the precipitate collects near the center. Probably few of you know why this is so. It is not due to the slower velocity of rotation near the center, nor to the slower motion with respect to the glass. This is proved by the fact that if you put the beaker, with the precipitate in suspension in the liquid, upon a rotating table, the precipitate will collect in a ring as far from the center as possible, although the relative angular motion of the beaker and its contents are the same as before. A little study proves that the phenomena are due to unbalanced centrifugal forces. For example, when the liquid is stirred, so as to set it in rotation, centrifugal force produces a greater hydrostatic pressure near the walls of the beaker. But the liquid very close to the bottom surface of the beaker, because of friction, can not rotate so fast, and therefore the centrifugal force is not so great and does not counteract the radial hydrostatic pressure difference existing in the upper layers. The liquid in contact with the glass bottom is thus forced inwards and carries the precipitate with it.

The phenomena connected with the formation and the disappearance of ice in a large lake, such as Lake George, have interested me for years. One clear night at the end of December, when the water of a large bay was at a uniform temperature of not over 0.2° C. and the air temperature was −22° C., ice which formed slowly at some places on the shore, melted in a couple of minutes when pushed out a few meters from the shore. There was no wind in the bay, but a slight breeze over the central part of the lake caused a very slow circulation of water in the bay with a velocity of perhaps 1 or 2 cm. per second.

In contrast with this consider the phenomena observed one clear afternoon of the following April. The body of the lake was still covered with ice which was about 20 cm. thick, but close to the shore there were places where the ice had melted back for a distance of 5 meters or more. Although the air temperature was +3° C. and the water 10 cm. below the surface was at +2.5°, ice crystals about 50 cm. long formed in these pools in less than half an hour. After considerable analysis I believe I can explain this apparent paradox by the stability in the stratification of the water in April caused by the denser underlying warm water which had been heated by the sun. With this stability which prevented vertical convection the surface water could freeze because of the radiation into the clear sky. But in December the water temperature was so uniform that the differences of density were not sufficient to prevent vertical circulation, and thus the surface could not cool to the freezing point.

It appears then that a pool of water at +1° C., exposed to cold air with a slight wind can be made to freeze more rapidly if the water is heated from the bottom. Sometime I want to try this as an experiment.

All hobbies, however, stimulate individual action, and many develop wholesome curiosity. The child should acquire them early, and our educational system should foster them.

IRVING LANGMUIR

RESEARCH LABORATORIES,
GENERAL ELECTRIC COMPANY,
SCHENECTADY

JAMES CAMPBELL TODD

JAMES CAMPBELL TODD died at his home in Boulder, Colorado, the evening of January 6, 1928, following a long illness.

Born in Shreve, Ohio, March 17, 1874, he graduated from Wooster College in 1897, with a degree of bachelor of philosophy. He continued his studies in the University of Pennsylvania School of Medicine, from which he received the degree of M.D., in 1900.

While in Wooster College he held the position of assistant in biology during 1895–96. From 1900–01 he was resident physician in the Allegheny General Hospital, Pittsburgh. About this time his health failed, and he moved to Colorado, where he located in Denver.

He soon became identified in the field of medical education, first as assistant of pathology in the Denver and Gross College of Medicine during 1904–05, then as lecturer from 1905–08, later as associate professor from 1908–10, and assumed the professorship of the department in 1910.

On January 1, 1911, the University of Colorado School of Medicine absorbed the Denver and Gross College of Medicine, the two faculties were merged, and Dr. Todd became professor of pathology in the Boulder Division. He also acted as the secretary of this division until 1916. Since 1923 he has been premedical adviser in the university.

As the study of pathology broadened he felt that he was losing the contact in the fields of hematology and parasitology he desired. So in 1916 he became professor and the head of the department of clinical pathology which had just been created at his request. These positions he held at the time of his death.