perchlorate is lost in the sodium chloride residues. The other 4 per cent. is in the perchloric acid as sodium perchlorate. The perchloric acid is free from chlorides. The process does not work with potassium perchlorate and is unsatisfactory with barium perchlorate.

UNIVERSITY OF INDIANA, BLOOMINGTON.

ORGANIZATION OF INDUSTRIAL RESEARCH.[1]

BY WILLIS R. WHITNEY.

Received November 6, 1909.

The intimate connection between the purely scientific research of a people and its advance in the art of good living cannot be too frequently discussed. The organization of industrial research involves arranging and maintaining a body of involute parts as an operative whole of highest efficiency. It is never perfectly accomplished, and the fact that improvement can always be made is an incentive for its discussion.

A recent copy of *Life* has this to say, which, without straining, bears directly upon industrial research:

"This is the most interesting country in the world. The game here is the biggest that is being anywhere played. The problems of humanity that are being worked out here are the greatest problems under consideration, and the prospect of solving them is better than it is anywhere else."

Lord Bacon said: "The real and legitimate goal of the sciences is the endowment of human life with new invention and riches." He, in turn, cited King Solomon, who said, "it is the glory of God to conceal a thing, but the glory of a king to search it out."

Bacon distinguishes three degrees of ambition:

First, that of men anxious to enlarge *their own* power *in* their own country. This is "vulgar and degenerate."

Second, that of men who strive to enlarge the power and empire of their *country* over mankind. This is "more dignified, but not less covetous."

Third, that of those who strive to enlarge the power and empire of *mankind* in general over the *universe*. Evidently this is the best, and is the real ambition, whether recognized or not by himself, of any good experimenter.

For purposes of systematic analysis, the subject, "Organization of Industrial Research," may be divided into two parts:

Part one, the personal or mental organization, with its requirements, etc.

Part two, the objective or material organization.

For brevity, these may be called the mind and the matter organizations.

The former, or personal, I will subdivide into such parts as:

Its training and characteristics.

Division of its labors.

Its records, etc.

The objective or matter organization, I divide into:

---

[1] An address delivered at the Twentieth Anniversary of Clark University, Sept. 17, 1909.

The fields for material research.

The laboratory equipment and systems of its material co-operation.

Naturally, the personal comes first, relatively and chronologically, and the mental precedes the material. The personal factor is *everything* in industrial research. Strangely enough, it is everywhere and always dominant, while every other factor is sometimes recessive. In an organization "A" cannot work well with "B" because one is too slow, too fast, to egotistical, too jealous, too narrow, etc. Nowhere else do the personal traits protrude so much as in concerted research. And so I hold that above all, as an industrial experimenter, I should like as broad a *human* training as possible, before any other specific one. This probably means little more than acquirement of a *demonstrated* desire to play fair, and it may be no more applicable to this field than to others.

To one always in close touch with research, it seems as though there is an immutable law of nature which may be stated as follows: (It is an application of the principle of reversible reactions so as to include the reactions of the mind.)

The equilibrium between mental and material conception is so sensitive that anything which, to the fair mind, seems possible, is to the trained persistence permissible. If this should be proven not strictly true, it would still be a good working hypothesis for a research organization.

This theory requires, then, a certain characteristic in the generally successful research operator. This is recognized in *optimistic activity* and, to my mind, should be placed first among the requisites. It is placed above *knowledge*, because, without it, little that is new will ever be done except by accident. With active optimism, even in absence of more than average knowledge, useful discoveries are almost sure to be made.

Speaking from personal analysis and from the observation of others, I would say that general-chemical and physical knowledge may sometimes be as much a detriment as a help to one imbued *only* with a *need* of solving new problems. A possible explanation is this: We always reason deductively. We apply general laws in attempting to answer specific questions. To any specific problem of research there are usually general laws which may seem to forbid the solution. These laws are known and revered. Naturally, the unknown, specific ways by which it may be solved are more or less hidden. An illustration may not be out of place here:

Cotton may be dissolved in a solution of zinc chloride. The solution may be squirted through a die into alcohol in such a way that a smooth, coagulated cellulose thread is thereby obtained. This may be heated so as to give a solid, compact and pure carbon filament. Many are thus made. But as a new problem, it would certainly appear quite impracticable to one who might have a fairly extensive knowledge of the chemistry of the materials. Generally speaking, zinc chloride solution does *not* dissolve cellulose. Only a strong solution, kept at a high temperature for a long time, will give the desired solution. In general, too, it could *not* be squirted and coagulated into a smooth thread. Very specific conditions are necessary. Finally, the treatment with gradually rising temperature, which alone succeeds in giving the compact carbon filament, is a matter of specific detail. The places in this process where general reasoning points to failure are numberless. Years of multiplied effort are necessary to perfect such a process. Once established, it is easily analyzed along the lines of understood reason and theories of reactions may be based upon the facts. But such processes are not laid out greatly in advance of their accomplishment. The successful steps are found among the many which are actually attempted, and something more than general knowledge is necessary. This something is hopeful pertinacity, optimistic activity. To a chemist imbued with fair knowledge, it was recently apparently useless to attempt such an experiment as the continual removal of traces of hydrogen from oxygen by passing the gas through a red-hot iron pipe. He had seen iron wire burned rapidly in oxygen, he tried wrought iron and the iron was oxidized, and his knowledge was vindicated, but he also tried cast iron and found that it did not burn and that it would operate perfectly. A scramble for an explanation evolved the theory that the silicon burning to silica protected the iron. *Ex. postfacto* theories are permissible.

As the mental world is constituted, optimists are greatly in the minority, when one counts those only who are also imbued with knowledge. Therefore, in practice, the optimist must be used to crystallize the efforts of others less optimistic. Thus, any large industrial research laboratory is soon *perforce*, systematized into organized clusters of people, working along distinct and different lines. This permits, in our case, of the combined use, to maximum efficiency, of the delicate hands of young women, the strength and skill of trained mechanics, the mind of the useful dreamer, the precision and knowledge of the skilful chemist, and the data of the accurate electrical engineer.

Simple mathematical axioms make clear the fact that a group of operators working together on a subject, are related to the same group operating separately, as a power is related to a simple sum. This principle holds as well among a group of groups and to related subjects. It is evident, for example, that knowledge gained along the line of insulation would be of use in a study of conduction, and that the man who had studied the reduction of tungstic oxide by carbon *in vacuo* could help the one who is working with a pressure furnace, upon the equilibrium between carbon monoxide and carbon dioxide. Therefore, the strength of a research department, properly operated, should rise exponentially with its numbers.

To this audience, the importance of highest advance in specific chemical and physical training will probably be apparent, but an expression of it may be of use. The supply of highly trained men is below the demand. There is a healthy supply of moderately trained men. This applies to all general, scientific training. Let me give more concrete ideas. There are a hundred chemists who can fill satisfactorily an analyst's position, to one who knows what J. J. Thomson has done or who reads Drude's *Annalen.* Reading the *Annalen* is not a "sine qua non," but it is an indicator of no little merit. If a chemist or a physicist is not sufficiently interested to keep informed, he is probably not going to work at high efficiency as an investigator. This does not preclude the possibility of splendid research work being done by some one who is confined to a very limited field of vision, but such cases are the exception and cannot be used as bases for common application. In general, the man with the best tools and with the best knowledge and experience in their use, will advance most rapidly in industrial research. In my own experience, we frequently have a line of work which demands the addition to the force of well trained men. The difficulty which stands out most markedly when considering this problem is usually the scarcity of men who are highly enough trained along the line of pure research. While in many fields of *industrial* research new and brilliant discoveries will continue to be made suddenly and, as it were, out of new cloth, still many more are being made by the most careful application of highly refined methods and knowledge, to processes which already seem at first pretty well worked out. This *intensive farming* is most promising and demands the highest skill. It is to-day most difficult to find American trained men who can do this work. It is a German attribute which we would do well to make our own.

If the chemist is only a chemist or the physicist confined to pure physics, he is liable to overestimation of the laws he learns. He should be something of a "mental mixer," one who has enough history, enough psychology, and enough faith to read possibility of acquirement for the future out of knowledge of attainments in the past.

As we have said, one of the most practical detriments to successful industrial research is that automatic action of the mind which recognizes the possible grounds for a failure quicker than it sees the probable ways to success. Research needs more aviators. Those of us who feel the work-horse brand on our work have a call to cultivate a *flying* spirit, and are to be condemned only if we stand still.

In this connection, I am in favor of anything which helps train the American student in the path of sanguine research. It can be done by research men themselves, but probably not by others. It is not the *knowledge* which the student preparing for research needs, so much as the spirit of the investigator. His thoughts should not be fettered by laws, but helped by them to fly. This can be done best by those who are optimistic almost to the extinction of reason.

A search in the research laboratories of the world to-day would disclose large numbers of J. J. Thomson men, Ostwald men, Nernst men, van't Hoff men. The teacher probably made the school. The investigator probably endowed the students, not with facts alone, but with spirits. We are not of that hopeless class who assume that the sparks of genius are only Heaven-sent, but we are inclined to adopt as an axiom that man is flexible, auto-corrigible and mentally elastic beyond limit. Therefore the rare genius in research, as elsewhere, is the one most given to hopeful effort.

To dwell for a moment upon points in a system for co-operation of a research force, I will describe our own scheme.

The present corps comprises about eighty people, about thirty of whom are college men, mostly chemists. Every man or woman on the research staff is expected to give undivided effort to the work. Whatever invention results from his work becomes the property of the company. I believe that no other way is practicable. An attempt to reward systematically such labors by a scheme of royalty payment is more impracticable than the operation of a manufacturing plant upon a graded scheme of profit sharing. In this case an immediate and fairly equitable division of profits is sometimes possible. In research, the problem itself is an asset of the organization. Both the equipment and the risks belong to the organization. The accumulated experience of the force as a whole is its property. Finally, the privilege of directing the work of operators along lines where no direct financial benefit (or an immeasurable one) to the company could ever be determined, must belong to it. Every operator is expected to keep good notes and his books become a part of the laboratory files. In most cases weekly typewritten reports are made by each worker, and copies of these also become part of accessible library files. For purposes of establishment of dates, etc., witnesses who read and understand the notes also endorse them. Photographs of apparatus, curves, etc., are frequently added wherever useful, and each room of the laboratory is photographed regularly and the dated photographs are bound in books, to record standing conditions. Wherever practicable, single sheets, of standard report size, are printed to cover oft-repeating data, so that the experimenter regularly fills in certain blanks, as, for example, in experiments on carbon motor brushes: the composition of the particular lot, temperature and time of drying and firing, hardness, resistivity, tensile strength, and all other tests of the product. The use of plotted curves on standard millimeter paper, for use where one property of material is studied as a function of some other variable, is very common in our reports. This occurs, for example, in practically all cases where electric furnace work is described, and where the changes undergone by incandescent lamps during their life are recorded.

These conditions are the result of eight years of development. The system has been subjected to many changes and may still be greatly improved. It is possible to have such a complex system of record that efficiency is sacrificed. We have reached the present stage because of frequent indications of previous weakness in the simpler methods. Very few good investigators can keep good notes. The more interested the investigator becomes, the more difficult it seems for him to carefully record his passing work. His eyes and mind are always upon the exciting and more interesting advance. It seems not so tempting to actually make history by the writing as to metaphorically make it by the conception or experiment.

We now come to the material side of the subject.

In the early days, the same hands which mined the iron ore and operated the bellows, also forged the sword and plowshare and touched the goods which were the equivalent in exchange. The records of the development through which the distribution of the steps of such processes has gone is what we call the history of man. It is not always easy to recognize the extent to which this development is progressing in our own time. Statistics ought to show us, but these often fail to impress us. It may be that if used to a limited extent to armor an argument, a few data will be of interest in connection with industrial research.

The known chemical compounds of the earth are myriads. The still unknown, but knowable, are certainly many myriads more, but any consideration of either great mass is too huge a task. We may, however, consider for a moment a part of the alphabet from which that language is made. We will consider research as applied to the *metallic elements* alone.

There are about 75 elements. About two-thirds are metals. Of these, only a very few can be said to have been the subject of much industrial research. It is impossible to accurately measure the extent to which an element has been studied with a view to its possible use by the race, but we have no difficulty in recognizing that iron and copper have been much studied, while calcium and silicon have not. In these illustrations we have *not* selected *rare* elements. The calcium and silicon, which have been least used by man thus far, are more common than copper or iron. A natural explanation of the lack of development of such elements is a lack of need, but this is possibly incorrect. Copper, iron, etc., were certainly first obtained by accident as distinct from design. The uses to which they could be put were later developed by trial. The finding of some uses established the further supply, which insured the subsequent discovery of new uses. This mirrors the history now being made by new elements such as silicon. Only in the past year the commercial production of this element has been begun, and about 500 tons were sold for a deoxidizer in steel-making. Thus a substance absolutely out of reach of almost every chemist a few years ago, can now be obtained as cheaply as zinc.

Similarly, future needs, which only calcium, for example, can meet, are certain to be developed. More calcium will then be made. The cost of production will be reduced and the field of its usefulness will again and ever afterward continue to broaden. Never in the history of the world has the rate of iron production been so great as at present (nearly two million tons a month by the U. S. Steel Company alone). Copper is being mined more rapidly than ever before. We have ourselves seen the industrial birth and growth of a new metal which points to the great possibilities in case of the other unused elements. I refer to aluminium. Only two to three tons were made as late as 1884 while furnaces now exist which are capable of yielding three to four times this quantity every hour of the day and night. Its *present* uses could only have been, and were, very imperfectly predicted, before actual industrial research made tentative use of it. So it must be with other elements. One is not too bold who assumes that all the elements which are found in abundance will be industrially utilized when they have been economically isolated and thoroughly investigated.

I am considering the metallic elements only in order to point out in a concrete manner the need of high-quality research, physical, chemical, electrical, etc., in the *simplest* field. Evidently this field, among compounds of the elements, is again bounded only by the infinite. I am impressed with the idea that the commonest elements in nature have not been *studied* with anything like the care which has been given to those for which the demands are already developed.

In our age, a single investigator will probably not isolate, in large quantities, the metal tellurium, for example, and also put it to use to fill one of his individual needs, as did the warrior who first fashioned an iron blade or axe. The men who develop the myriad uses to which the common element titanium will be put, will have to rely upon the previous work of many investigators. It is in this respect that the conditions are continually changing, and always in one direction. I call it the direction of specific complexity. Our wants are very complex. We are learning to demand very specific properties. It is this fact which makes necessary the research work of the specialist, the specific or narrow investigation of the pure scientist, the pioneer work of the trail-blazer, the crude and hurried trials by the inventor, the long and exacting developments of the practical application in the factory, etc. Demands for new materials do not really precede the discovery of the product, any more than the demand for high-speed tool steel preceded the discovery of the properties of the chrome-tungsten-iron alloys. With the material discovered, its properties known, the world apparently could then hardly get along without it. This means that necessity is not the mother of invention. Knowledge and experiment are its parents. It sometimes happens that a successful search is made for unknown material to fill well recognized and predetermined requirements. It *more* often happens that the acquirement of knowledge of the previously unknown properties of a material suggests its trial for some new use. These facts strongly indicate the value of knowledge of properties of materials and indicate a way for research.

Among the recently developed uses for modern metals which were certainly not surmised until the metal itself had been made easily available, are the use of aluminium and silicon as deoxidizers in steel-making, where all the silicon and a large part of the aluminium are now used. This discovery of utility by experiment, rather than the discovery of material by *force of necessity*, is again illustrated by the metals titanium and vanadium. The former is used in arc lamps because it was found, by experiment, to give a good light. (Your Worcester streets are lighted by it.) The latter has been surprisingly useful in steel-making, where a fraction of 1 per cent. has been found to impart additional strength to the steel. In this way, about a thousand tons of vanadium are now used annually in America.

When the first step is taken from the study of the supply, production and utilities of our metallic elements, the next step is apparently along the lines of alloys and we readily see how quickly the field widens. The recent great advances in scientific foundation for much study are to be attributed to the physical chemists, to such men as Tamman and his school. In their work we begin to see the magnitude of the alloy field. There are probably over a *thousand pairs* of metals whose properties as alloys are still absolutely unstudied, and for alloys of three or more metals the number is legion.

It seems as though our advance could be quickened by a greater intimacy with the newly cheapened elements. When sodium, chlorine, bromine, silicon, magnesium, chromium, cobalt, manganese, tungsten, etc., etc., are many times as available or cheap as they were only ten years ago, it is probable that the possible uses are not up-to-date.

The field of material research really divides into two parts: the search for more economical production and the search for wider application. These two go hand in hand. If the one advances, the other is led along. In this way, in our laboratory, the knowledge of such elements as carbon, as in its forms of graphite in lamp filaments, in motor brushes, in electrodes, etc., has been widely and continually advanced. The result is not a conclusion that we know all about carbon, but rather that it still presents a wonderful field for useful research.

From the materials worked *upon*, to the tools is a step. Our experience here is concrete and clear, and we want to record our impressions. Good tools, new tools, rare tools, are most valuable. No good tool lives long for a single use alone. Many times we have questioned the advisability of installing some new apparatus—a vacuum furnace, a pair of metal rolls, some special galvanometer, some microscope, an hydraulic press, a power hammer, a steam digester, etc., etc. Never, after it became a part of the equipment, has it seemed possible to proceed without it. In the single case of the electric vacuum furnace, for example, our laboratory has made almost continual use of from three to eight for the past five years. The laboratory, piped several years ago with high vacuum and with electrolytic hydrogen, besides steam, air, water and gas, will probably never operate without them.

Similarly, this applies to a library. In general, the most useful and fertile of our investigators use the library the most. This is as it should be. The recorded research work in a library of a few thousand volumes frequently represents the work of millions of work-hours, and there is little excuse for not availing oneself of the published experience of others. A library containing ten of the leading research journals of the world may be said to have in each volume about 100,000 available brain-power-hours. So a library corresponds to a charged storage battery of great capacity.

RESEARCH LABORATORY, GENERAL ELECTRIC CO.,
SCHENECTADY, N. Y.

NOTE.

The Speedy Detection of Potassium in Small Amounts.—The use of sodium cobaltinitrite as a reagent for the detection of potassium has been known for a long time, but very little attention appears to have been given to its delicacy. Crookes[1] quotes De Koninck as saying that the precipitate is still formed at a dilution of 1/1000 KCl, but not at 1/2000. The writer also obtained by an indirect method the proportion of 1/1600 K_2O as the point at which precipitation would not occur.

A careful investigation of this reaction is given by W. C. Bray,[2] who finds the sensitiveness to be far greater than the above figures. During the course of a study of this reaction for quantitative purposes, the writer was fortunate enough to find a means of greatly shortening the time required for the test. Bray dissolves a given amount of potassium as potassium chloride in 5 cc. of water, adds a little acetic acid and 5 cc. of the sodium cobaltinitrite reagent (containing 0.5 g. Co in 100 cc.), then allows it to stand until there is a turbidity formed, afterward bringing the precipitate onto a white filter paper where it can be easily seen. The writer has found that if to the solution prepared as described there be added an equal volume of strong alcohol the precipitate will be formed in a very short time, so short in fact that if enough potassium is present to give a test at all it will by this means be thrown down in a few minutes, where several hours would be required otherwise. The following table shows the comparative times required by the procedure used by Bray and that where alcohol is added.

[1] "Select Methods in Chemical Analysis," 1.

[2] THIS JOURNAL, **31**, 621, 633 (1909).