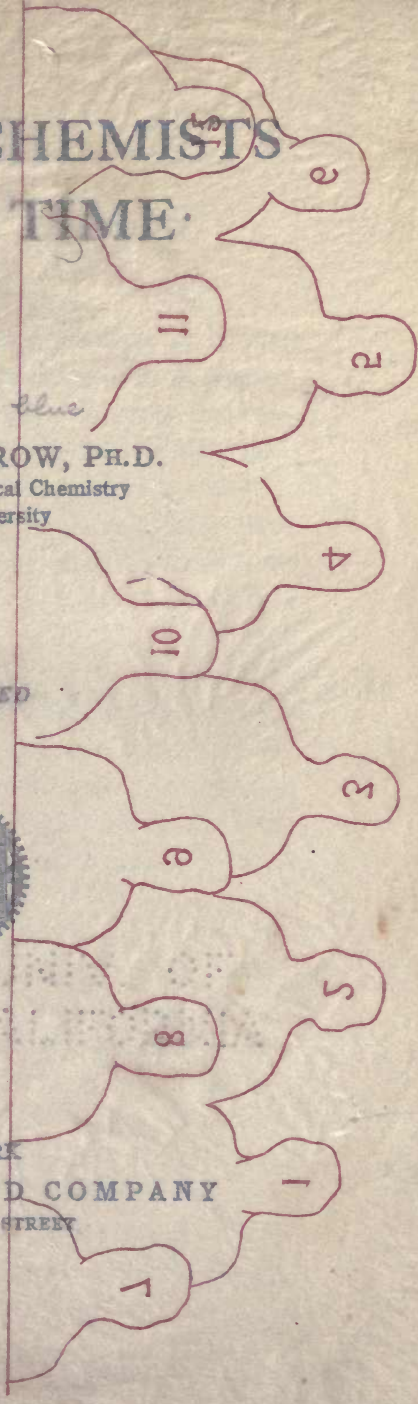
EMINENT CHEMISTS OF OUR TIME

EMINENT CHEMISTS OF OUR TIME

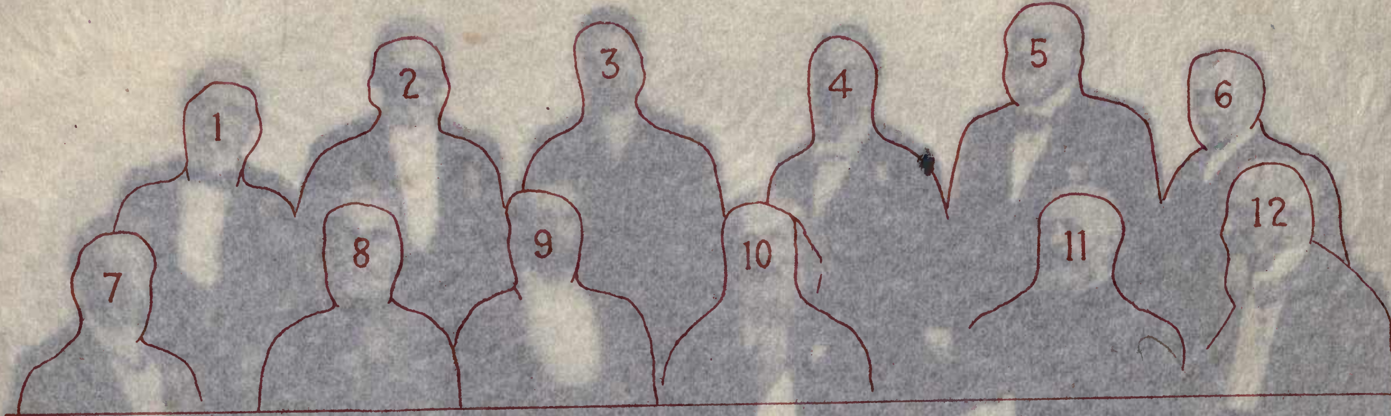
BY

BENJAMIN HARROW, PH.D.
Associate in Physiological Chemistry
Columbia University

ILLUSTRATED



NEW YORK
D. VAN NOSTRAND COMPANY
EIGHT WARREN STREET
1920



1. Ladenburg 2. Jörgensen 3. Hjelt 4. Landolt 5. Winkler 6. Thorpe
7. van't Hoff 8. Beilstein 9. Ramsay 10. Mendeleeff 11. von Baeyer 12. Cossa

(Reproduced by the kindness of Prof. Ernst Cohen, of the Univ. of Utrecht, Holland)

This photograph showing several eminent chemists was taken at one of the international scientific gatherings.

EMINENT CHEMISTS OF OUR TIME

BY

BENJAMIN HARROW, PH.D.
Associate in Physiological Chemistry
Columbia University

ILLUSTRATED

NEW YORK
D. VAN NOSTRAND COMPANY
EIGHT WARREN STREET
1920

Printed in the U. S. A.

PREFACE

We have several books dealing with the history of chemistry; there are a number of biographies of pioneer chemists; but, so far as I am aware—and this includes books in French and German as well as in English—the chemists of *our time* have been ignored completely. The Dickenses and Thackerays of chemistry have received attention—not any too much, to be sure; but the moderns, the Anatole Frances and Wells, have received none.

To fill such a want is the object of this work. How much these men and woman who are here treated are of our time may be gauged from the following: of the eleven whose lives and work are discussed, one died in 1897 (through suicide, be it added); three, in 1907; one, in 1911; one, in 1916; one, in 1919; and four are still alive.

The question may very naturally be asked, why were just these eleven selected? To this I would answer, that, with the historical perspective in mind, I wished to review the achievements of those men whose work is indissolubly bound up with the progress of chemistry during the last generation or so. I wished, then, to write a history of chemistry of our times by centering it around some of its leading figures.

This book aims to fill the wants of three classes of men:
1. The chemist who wishes an account of the labors of

some of the most illustrious men in his profession.

2. The scientist, other than the chemist, who desires information in a closely related field. What physicist can ignore the work of Mme. Curie? What biologist or medical man is not indebted to van't Hoff, Arrhenius and Fischer? And how has industry profited by the labors of Moissan and Perkin! These instances could be multiplied.

3. The layman who wants a non-technical account of some of the more remarkable achievements in a science which is entering more and more into our daily lives.

This work emphasizes the personal side; it is a "human document"; but there are ample references to, and discussions of noteworthy achievements. The book is so written that any layman, without any previous knowledge of chemistry, can get an intelligent idea of the man and his work.

Without generous help from many quarters a work of this kind would be quite impossible. I wish here to express my special indebtedness to the following: Dr. H. Arctowski, N. Y. Public Library; Prof. Svante Arrhenius, Nobel Institute, Stockholm, Sweden; Prof. W. D. Bancroft, Cornell Univ.; Prof. Ernst Cohen, Univ. of Utrecht, Holland; Madame M. Curie, Curie Laboratory, Paris, France; Prof. Jacques Loeb, Rockefeller Institute, N. Y.; Prof. W. H. Perkin, Oxford Univ., England; Prof. Ira Remsen, Johns Hopkins Univ.; and Prof. T. W. Richards, Harvard Univ. I am particularly indebted to my teachers and friends, Prof. W. J. Gies, Columbia Univ.,

and the late Prof. R. Meldola, Finsbury College, London, England; to my colleagues, Dr. E. G. Miller, Jr., Columbia Univ., and Mr. J. E. Whitsit, De Witt Clinton High School, N. Y.; and to my wife.

I wish also to thank the editors of *Science*, the *Journal of the Franklin Institute* and *Scientific Monthly* for permission to reprint some of the articles.[1]

BENJAMIN HARROW

New York, 1920.

[1] The work as originally written consisted of two parts: the "lives" (which constitutes the present volume) and the "work." The latter was an exhaustive review of the scientific work of the chemists under discussion. Complete bibliographies were appended to each article. However, as my intention was to write a popular volume, and as the second portion dealing with the "work" would have unduly enlarged the book, I decided to postpone publishing this part for the present.

CONTENTS

LIST OF ILLUSTRATIONS

INTRODUCTION

MODERN chemistry, little more than a century old, shows several outstanding landmarks in its evolutionary course. These may be classified into (1) The Foundation Period; (2) The Classification Period; (3) The Physico-Chemical Period; and (4) The Period of Radio-Activity.

1. *The Foundation Period.* Many regard Lavoisier (1743–94) as the father of modern chemistry. He was unquestionably one of its chief founders, if only because of the importance he attached to the use of the balance. With its help he gave us our modern idea of combustion, and established the law of the conservation of mass, which tells us that in all chemical reactions the total weight of the products formed is always equal to the weight of the reacting substances. Matter, then, may undergo change, but it cannot be created, and it cannot be destroyed.

2. *The Classification Period.* Boyle (1627–91) was the first to distinguish clearly between elements and compounds—substances which cannot, and substances which can be decomposed. The atomic theory of Dalton (1766–1844), with its conception of the atom as the unit in all chemical changes, must rank in importance with Lavoisier's pioneer work in quantitative chemistry. The atom and the molecule were further studied by Avogadro (1776–1856) and Cannizzaro (1826–1910), with results which led to the system of chemical nomenclature in common use today. Studies in the structure of compounds, and the classification of the elements

according to Mendeléeff's periodic system, were the logical consequences of the earlier work on the atom.

In more recent times Ramsay, Richards and Moseley have added much to our knowledge of the periodic system, which, in many ways, must be regarded as the starting point of some of the more recent discoveries and hypotheses in chemistry.

Side by side with these fundamental conceptions, chemists, fired by the work of Liebig (1803–73) and Wöhler (1800–82), were giving much attention to the chemistry of the carbon compounds which, in number, seemed infinite. Brilliant exponents of *organic* chemistry—which is the common name given to the chemistry of the carbon compounds—were Perkin and Victor Meyer.

3. *The Physico-Chemical Period.* Organic chemistry grew to greater and greater proportions. Even as late as the eighties of the past century the "organicists" were not merely in the ascendency, but had all but well-nigh supplanted the "inorganicists," *i.e.*, the chemists who specialized in all compounds *except* those of carbon. Then came a remarkable change. This was partly due to Moissan's brilliant work in inorganic chemistry, which made clear to the scientific public that this phase of chemistry still had rich fields that awaited cultivation; but, to a greater degree, to van't Hoff, Arrhenius and Ostwald, who founded a new and tremendously important branch of the science—physical chemistry.

Perhaps it would be more correct to say that these three did not so much create a new branch of the science, as that they interpreted chemistry in a more rational, more mathematical, and therefore more rigorous fashion; the catalogue of facts gave place to a discussion of far-reaching principles.

Some, fired by Moissan's genius, re-entered the field of inorganic chemistry; many of the younger generation turned to the physico-chemists; some, however, fascinated by such brilliant work as Fischer's application of synthetic chemistry to biology and medicine, extended their researches into the domain of physiological chemistry.

4. *The Period of Radio-Activity.* The study by physicists of the discharge of electricity through gases ultimately led to the discovery of radium by Madame Curie. To-day radio-activity is a distinct science; yet Mme. Curie began her researches as late as 1898!

Radioactivity has already shed a flood of light on the structure of the atom. It has shown conclusively that the atom is far from being the smallest possible particle, though it has, if anything, confirmed Dalton's original view that chemical reactions take place between atoms.

Of transcendent importance is the conclusion these studies lead to: that whereas chemistry deals with reactions *between* atoms, radioactivity deals with reactions *within* the atom. The two types of activity are quite distinct from one another; to such an extent, in fact, that whereas chemical reactions can be controlled, radioactivity has thus far proved entirely beyond the control of man, for no human device seems to increase or decrease such activity.

Addendum

Chemistry in America. The history of chemistry in America is discussed in the article on Remsen. Here it needs but to be pointed out that Remsen bears the same relation to the vast army of brilliant American chemists of to-day that Johns Hopkins University bears to higher education in the United States.

The various items discussed in this introduction may now be tabulated in chronological order:

1661. Boyle: elements.
1777. Lavoisier: combustion and conservation of mass.
1808. Dalton: atomic theory.
1811. Avogadro: molecules.
1828. Wöhler: synthesis of urea—the first case of the artificial production of a typical animal product.
1856. Perkin: discovery of mauve, the first dye obtained from coal-tar.
1858. Cannizzaro: atom and molecule.
1865. Kekulé suggests ring formula for benzene.
1869. Mendeléeff: periodic system of the elements.
1874. van't Hoff and Le Bel: structural chemistry (theory of the asymmetric carbon atom).
1876. Remsen is appointed professor of chemistry at Johns Hopkins University.
1884. Victor Meyer discovers thiophene, opening up an immense chapter in organic chemistry.
1885. Emil Fischer begins work on the synthesis of sugars.
1886. Moissan: isolation of fluorine.
1887. van't Hoff: theory of solution.
1887. Arrhenius: theory of electrolytic dissociation.
1894. Ramsay and Raleigh discover argon.
1898. Mme. Curie: radium.
1913. Moseley: atomic numbers (see the article on Richards).
1914. Richards: radioactive lead.

WILLIAM HENRY PERKIN

AS every school child knows to-day, the illuminating gas we use in our homes is largely obtained from the dry distillation of coal; but many men and women even to-day are not aware that, in addition to illuminating gas, other products of far-reaching commercial importance are also obtained from this same coal.

Among these, coal-tar stands out pre-eminently. Not so many years ago it was a waste and a nuisance; to-day it rivals the coal-gas in utility.

From this dirty black tar, by a series of distillations, we get benzene and toluene and naphthalene and anthracene—to mention but four important substances—which are the starting point for countless products of the dye and synthetic drug variety.

Out of benzene, for example, we can get aniline, and from the latter, Perkin, in 1856, obtained the first artificial dyestuff ever produced.

Born in England, the dye industry was reared and developed in Germany; and Germany owes much of its greatness, and very much of its downfall to it. For the dye industry proved but a nucleus for many other related industries. Thus dyes gave rise to the manufacture of sulphuric and nitric acids and caustic soda; these in turn to artificial fertilizers, explosives and chlorine; and the latter to poison gas with all its concomitants. The medicine in small doses and the poison in large; chlorine as an antiseptic and chlorine as a destroyer—give them but the wrong twist, and man's ingenuity becomes positively harmful.

Perkin was born in London in 1838. He was the youngest son of George Fowler Perkin, a builder and contractor, who had apparently decided his son's future before the latter had discarded his swaddling clothes. Perkin, Jr., was to be an architect.

But Perkin, Jr., had not yet decided for himself. Perhaps it was a street car conductor one day, a prime minister the next, and an engine driver the third. And then again, watching his father's carpenters at work, he wished to become a mechanic of some kind; and plans for buildings fired him with the ambition of becoming a painter.

In any case, in his thirteenth year he had an opportunity of watching some experiments on crystallization. It goes without saying that he forwith decided to be a chemist.

Were it not that about this time Perkin entered the City of London School, and there came in contact with one of the science masters, Mr. Thomas Hall, this latest decision might have been as fleeting as his previous ones.

The City of London School, like all important educational institutions of the day, considered science as an imposter in the curriculum, so that whilst Latin received a considerable slice of the day's attention, poor little chemistry could be squeezed in only in the interval set aside for lunch.

A few boys, and among them Perkin, were sufficiently interested to forego many of their lunches and watch "Tommy Hall" perform experiments.

Hall's infectious personality made young Perkin all-enthusiastic. He was going to be a chemist, and he was going to the Royal College of Science, of which, and of its renowned chemical professor, Hall had told him much.

Hall's earnest pleading finally overcame the father's opposition, and in his fifteenth year Perkin entered the College. "Mr. W. Crookes,"[1] the assistant, was the one immediately in charge.

The head professor was Hofmann, an imported product. So suggestive and illustrative were the great chemist's lectures that, in the second semester, Perkin begged and obtained permission to hear them once again.

In the laboratory Perkin was put through the routine in qualitative and quantitative chemistry, Bunsen's gas analysis methods serving as an appendix. This was followed by a research problem on anthracene, carried out under Hofmann's direction, which yielded negative results, but which paved the way for successful work later. His second problem on naphthylamine proved somewhat more successful, and was subsequently published in the *Chemical Journal*—the first of more than eighty papers to appear from his pen.

When but seventeen Perkin already had shown his mettle to such an extent that Hofmann appointed him to an assistantship. This otherwise flattering appointment had, however, the handicap that it left Perkin no time for research. To overcome this the enthusiastic boy fixed up a laboratory in his own home, and there, in the evenings, and in vacation time, the lad tried explorations into unknown regions.

The celebrated experiment which was to give the 17-year-old lad immortality for all time was carried out in the little home laboratory in the Easter vacation of 1856. It arose from some comments by Hofmann on the desirability and the possibility of preparing the alkaloid, quinine, artificially.

[1] The late Sir W. Crookes.

Starting first with toluidine, and then, when toluidine gave unsatisfactory results, with aniline—both being products of coal tar—Perkin treated a salt of the latter with bichromate of potash and obtained a dirty black precipitate.

Dirty, slimy precipitates had been obtained before and had, as a rule, been discarded as objectionable by-products. Perkin's first instinct to throw the "rubbish" away was overcome by a second, which urged him to make a more careful examination. And this soon resulted in the isolation of the first dye ever produced from coal tar—the now well-known aniline purple or mauve!

A sample of the dye was sent to Messrs. Pullar, of Perth, with the request that it be tried on silk. "If your discovery does not make the goods too expensive, it is decidedly one of the most valuable that has come out for a long time . . ." was the answer. Trials on cotton were not so successful, mainly because suitable mordants were not known. This second result somewhat dampened the enthusiasm of our young friend.

Nevertheless, Perkin decided to patent the process, and, if possible, to improve the product, as well as to find improved means of application.

Full of hope and courage, the young lad had decided to stake his future on the success or failure of this enterprise. He was going to leave the Royal College of Science, and with the financial backing of his father—who seems to have had a sublime faith in his son's ability—he was going to build a factory where the dye could be produced in quantity.

Hofmann was shown the dye and was told of the resolution. The well-meaning professor, who seemed to have had more than a passing fondness for the lad, tried all he could to persuade Perkin against any such undertaking. And let it be added that in that day, to any man with any practical common sense, Perkin's venture seemed doomed from the start.

A site for the factory was obtained at Greenford Green, near Harrow, and the building commenced in June, 1857.

"At this time," wrote Perkin years later, "neither I nor my friends had seen the inside of a chemical works, and whatever knowledge I had was obtained from books. This, however, was not so serious a drawback as at first it might appear to be; as the kind of apparatus required and the character of the operations to be performed were so entirely different from any in use that there was but little to copy from."

The practical difficulties Perkin had to overcome were such that, in comparison, the actual discovery of the dye seems a small affair. Since most of the apparatus that was required could not be obtained, it had first to be devised, then tested, and finally applied.

Nor was this all. Raw materials necessary for the manufacture of the dye were as scarce as some rare elements are to-day. Aniline itself was little more than a curiosity, and one of the first problems was to devise methods of manufacturing it from benzene.

The country was searched high and low for benzene. Finally Messrs. Miller and Co., of Glasgow, were found to be able to supply Perkin with some quantity, but the price was $1.25 a gallon, and the quality so poor that it had to be redistilled.

Now the first step in the conversion of benzene to aniline was to form nitrobenzene, and this required nitric and sulphuric acids in addition to benzene. Here again the market did not offer a nitric acid strong enough for the purpose. This had first to be manufactured from Chili saltpeter and oil of vitriol (sulphuric acid), and special apparatus had to be devised.

Béchamp's discovery three years earlier, that nitrobenzene could be converted into aniline by the action of finely divided iron and acetic acid was now developed for industrial use, and here again special apparatus had to be devised.

To-day the most fundamental operations in every dye factory are nitration—the conversion, say, of benzene to nitrobenzene—and reduction—the conversion of nitrobenzene to aniline. The mode of procedure, the technique, the apparatus—all are based on the work of this eighteen-year-old lad. Only those who have attempted to repeat on an industrial scale what has been successfully carried out in the laboratory on a small scale, will appreciate the difficulties to be overcome, and the extraordinary ability that Perkin must have possessed to have overcome them. Think of a Baeyer who synthesized indigo in his university laboratory, and then think of the twenty years of continuous labor that was required before the *Badische Anilin Fabrik*, with its hundreds of expert chemists and mechanics, was in a position to produce indigo in quantity. And it would have taken them and others much longer but for the pioneer work of young Perkin.

Some have described Perkin's discovery as accidental. Perhaps it was. But consider the way it was perfected and made available; consider with what extraordinary ability every related topic was handled; consider how every move was a new move, with no previous experience to guide him; and who but one endowed with the quality of genius could have overcome all this? Hertz discovered the key to wireless telegraphy, but Marconi brought it within reach of all of us; Baeyer first synthesized indigo, but the combined labors of chemists in the largest chemical factory in the world were necessary before artificial indigo began to compete with the natural product; Perkin both isolated the first artificial dyestuff and made it useful to man.

In less than six months aniline purple—"Tyrian purple" it was at first called—was being used for silk dyeing in a Mr. Keith's dye-house. The demand for it became so great that many other concerns in England, and particularly in France, began its manufacture. In France it was renamed "mauve," and "mauve" it has remained to this day.

Perkin's improvements continued uninterruptedly, and his financial success grew beyond all expectations. He found that the uneven color often obtained in dyeing on silk could be entirely remedied by dyeing in a soap bath. The use of tannin as one of the mordants made it applicable to cotton, and shades of various kinds and depths of any degree could be attained without any difficulty. A process for its use in calico printing was also worked out successfully.

When, three years later, Verguin discovered the important magenta—or, as it is sometimes called, fuchsine—and later still Hofmann, his rosaniline, various details in the manufacture of mauve and its application to silk, cotton and calico printing, were appropriated bodily.

Young Perkin had given tremendous impetus to research in pure and applied chemistry. In the preparation of dyes, substances which had, until then, been curiosities, had now become necessities, and methods for their preparation had to be devised. This led to incalculable research in organic chemistry. In fact, it is hardly too much to say that the basis for most of the development in organic chemistry since 1856 lies in Perkin's discovery of mauve.

Industry has not been the only benefactor. It will be remembered that using the dye, methylene blue, as a staining agent, Koch discovered the bacilli of tuberculosis and cholera. And coal-tar dyes are to-day used in every histological and bacteriological laboratory.

So rapid had been the progress of the industry that in 1861, Perkin who, though only 23, was already recognized as the leading English authority, was asked by the Chemical Society to lecture on coloring matters derived from coal-tar, and on this occasion the great Michael Faraday, who was present, warmly congratulated Perkin upon his fine lecture.

Such dimensions has the coal-tar industry assumed since then that in 1913, at one single factory, the Baeyer works, in Elberfeld, Germany, there were employed 8,000 workman and 330 university trained chemists.

Says *Punch:*

> There's hardly a thing that a man can name
> Of use or beauty in life's small game
> But you can extract in alembic or jar
> From the "physical basis" of black coal-tar—
> Oil and ointment, and wax and wine,
> And the lovely colors called aniline;
> You can make anything from a salve to a star,
> If you only know how, from black coal-tar.

In his little laboratory at the factory the various attempts made in improving the methods of manufacture were not the only time-consuming factors. The chemical constitution of mauve and related dyes, as well as purely organic questions not in any way related to dyes, also engaged Perkin's attention, and he began to contribute what was to prove an uninterrupted stream of papers to the Transactions of the Chemical Society In 1866 he was elected to a Fellowship in the Royal Society.

The year 1868 is memorable in the annals of chemistry as dating the first artificial production of alizarin, the important coloring matter which until then had been obtained exclusively from the madder root. This great triumph was due to the labors of Graebe and Liebermann. But the triumph for the time being was purely a scientific one. The process as worked out by these two chemists was far too costly to compete with the method used in extracting the dye from the madder root.

The starting point to the artificial production of alizarin was anthracene, another important coal-tar product. It so happened that the first piece of research Perkin had ever been connected with was related to anthracene, a topic taken up on the recommendation of his teacher, Hofmann. Naturally, Graebe and Liebermann's synthesis aroused his interest. He wished to find some method of producing it at less cost.

In less than a year Perkin had solved the problem. A modification of the method dispensed with the use of bromine, which was very costly. A patent was taken out in June, 1869, at about the same time that Perkin's process had been discovered quite independently by Graebe, Liebermann and Caro.

Just as in the case of mauve, the supply of raw materials and the mastery of technical details, involved much labor and ingenuity.

To begin with, a constant and generous supply of anthracene was necessary. But where was this to be had? The tar distillers had had no use for it, and had not troubled to separate it in the distillation of tar. Many, indeed, there were among them who did not even know of its existence.

With the help of his brother, the various distillers in the country were visited and the method of isolating the anthracene from the tar distillate was shown them. The promise that all anthracene thus obtained would be bought and generously paid for, assured the Perkins of a plentiful supply.

The purification of the anthracene so obtained, the details of the entire process of manufacturing alizarin, and the types of apparatus to be employed, were all exhaustively investigated. By the end of 1869 one ton of the coloring matter in the form of a paste had been made. This was increased to 40 tons in 1870, and to 220 tons in 1871. Until 1873, when the Germans also began manufacturing it, the Greenwood Green works were the sole suppliers.

In 1874 Perkin sold his factory, and from henceforth devoted himself exclusively to pure research.

Perkin exemplifies the type, more common than is often supposed, though one entirely beyond the comprehension of the average business man, who loves the quiet pursuit of research beyond aught else. Perkin exploited his discovery solely with the view of providing himself with an income, modest in the extreme, but sufficient for his extremely simple wants. To explore unknown fields at leisure and to be freed from all money matters whilst doing so, were his aims.

When Perkin left the Royal College of Science at 17 he had this in mind. Financial insecurity may spur you on, but to give the very best that is in you requires freedom from such burdens.

What led him to give up the factory and to devote himself exclusively to pure science was sheer love of the subject. It is the type of love which, when associated with genius, has led to the world's greatest literary and artistic productions.

After 1874 Perkin moved to a new house in Sudbury, and continued to use the old one as the laboratory.

His research work from now on touched but lightly upon the dye situation. Until 1881 it centered much around the action of acetic anhydride on a group of organic compounds known as aldehydes. The first important result that was here achieved was the synthesis of coumarin, an odorous substance found in the tonka bean. This was the first case of the production of a vegetable perfume from a coal-tar product.

These researches culminated in the now classical *Perkin's Synthesis* of unsaturated fatty acids— a group reaction which is studied by every student in chemistry to-day.

In 1879 Perkin was the recipient of the Royal Medal of the Royal Society, the other awards of the year going to Clausius, for his investigation of the Mechanical Theory of Heat, and Lecoq de Boisboudron, for the discovery of the element gallium. The president addressed Perkin as follows:

" Mr. William Perkin has been, for more than twenty years, one of the most industrious and successful investigators of Organic Chemistry.

" Mr. Perkin is the originator of one of the most important branches of chemical industry, that of the manufacture of dyes from coal-tar derivatives.

" Forty-three years ago the production of a violet-blue color by the addition of chloride of lime to oil obtained from coal-tar was first noticed, and this having afterwards been ascertained to be due to the existence of the organic base known as aniline, the production of the coloration was for many years used as a very delicate test for that substance.

" The violet color in question, which was soon afterwards also produced by other oxidizing agents, appeared, however, to be quite fugitive, and the possibility of fixing and obtaining in a state of purity the aniline product which gave rise to it, appears not to have occurred to chemists until Mr. Perkin successfully grappled with the subject in 1856, and produced the beautiful coloring matter known as aniline violet, or mauve, the production of which, on a large scale, by Mr. Perkin, laid the foundation of the coal-tar color industry.

"His more recent researches on anthracene derivatives, especially on artificial alizarine, the coloring matter identical with that obtained from madder, rank among the most important work, and some of them have greatly contributed to the successful manufacture of alizarine in this country.

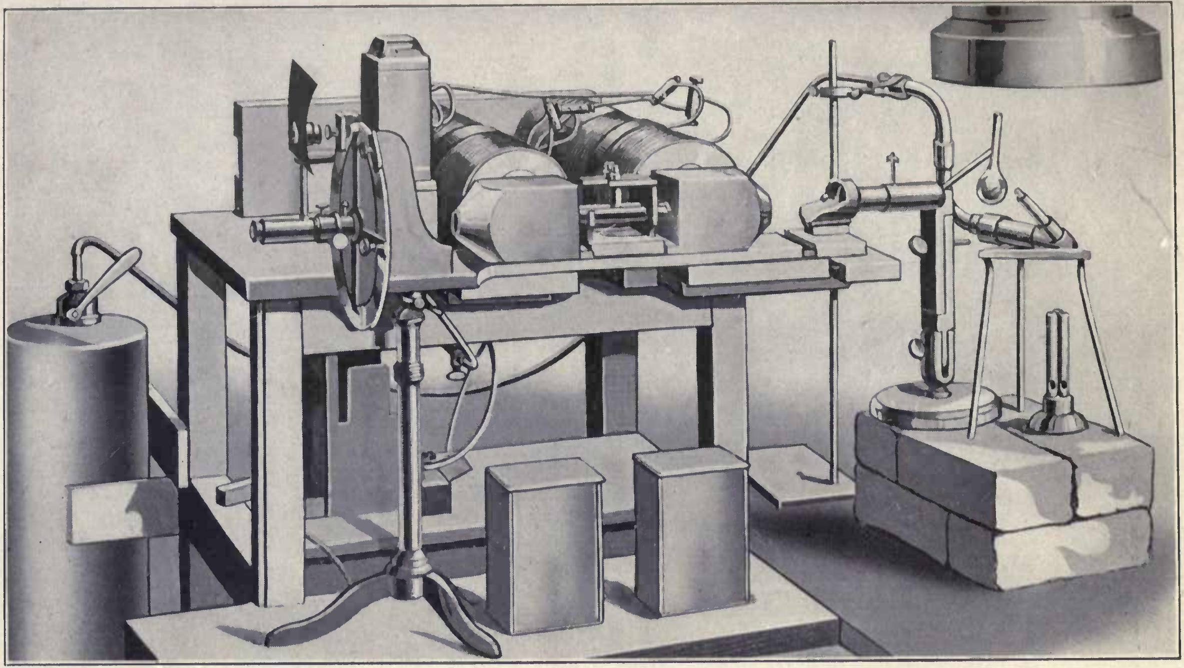
"Among the very numerous researches of purely scientific interest which Mr. Perkin has published, a series on the hydrides of salicyl and their derivatives, may be specially referred to; but among the most prominent of his admirable investigations are those resulting in the synthesis of coumarin, the odiferous principle of the tonquin bean and the sweet-scented woodstuff, and its homologues.

"The artificial production of glycocoll and of tartaric acid by Mr. Perkin conjointly with Mr. Duppa afford other admirable examples of synthetical research. . . .

"It is seldom that an investigator of organic chemistry has extended his researches over so wide a range as is the case with Mr. Perkin, and his work has always commanded the admiration of chemists for its accuracy and completeness, and for the originality of its conception."

In 1881 Perkin turned his attention in an entirely new direction, that of the relationship between the physical properties and the chemical constitution of substances. Gladstone, Brühl, and others were already busy connecting such physical manifestations as refraction and dispersion with chemical constitution. Perkin now introduced a third physical property, first discovered by Faraday: the power substances possess of rotating the plane of polarisation when placed in a magnetic field.

With this general topic Perkin was engaged to the year of his death. His work has thrown a flood of light



Apparatus used by Perkin in his researches on the optical activity and chemical constitution of substances.
[Reproduced from the *Journal of the Chemical Society* (London).]

upon the constitution of almost every type of organic compound, some, such as acetoacetic ester and benzene, being of extraordinary fascination to every chemist.

There are chemists—and H. E. Armstrong is among them—who regard this phase of Perkin's life work as his crowning achievement. If it has not received such general recognition as his earlier work, that is to be largely ascribed to a lack of knowledge of physics which prevailed among chemists until quite recently. However, even as far back as 1889 Perkin was presented with the Davy Medal of the Royal Society as a reward for his magnetic studies.

The year 1906 marked the fiftieth anniversary of the founding of the coal-tar industry, and the entire scientific world stirred itself to do honor to the founder. A meeting was held on July 26 of that year at the Royal Institution in London, over which Prof. R. Meldola, the president of the Chemical Society, presided, and those in attendance included some of the most distinguished representatives of science in the world.

The first part of the meeting consisted in the presentation of his portrait (painted by A. S. Cope, A.R.A.) to the guest of the evening. A bust of Perkin (executed by Mr. Pomeroy, A.R.A.) for the library of the Chemical Society, was next shown. In addition the chairman stated that a fund of several thousand pounds had been collected for the endowment of chemical research in the name of "Sir William Henry Perkin" (he had been knighted in the meantime).

Prof. Emil Fischer, president of the German Chemical Society, presented to Perkin the Hofmann Medal, which was accompanied with this address: *Die Deutsche Chemische Gesellschaft hat Herrn Dr. W. H. Perkin in London für ausgezeichnete Leistungen auf dem Gebiete der Organischen Chemie, im besonderen für* *die Begründung der Teerfarben-Industrie, den Hofmann-Preis verliehen. Berlin, im Juli, 1906. Der Präsident: E. Fischer. Die Schriftführer: C. Schotten, W. Will.*

Prof. A. Haller, representing France, presented Perkin with the Lavoisier Medal, with this address: *La Société Chimique de Paris, a l'occasion du Jubilée destinée à célébrer la cinquantiéme anniversaire de la découverte de la première matière colorante dérivée de la houille, et comme témoignage de haute estime pour ses travaux, est heureuse d'offrir au Dr. William Henri Perkin, Inventeur de la Mauvéine (1865), sa Médaille de Lavoisier a l'effigie de celui qui fut l'un des premiers et des plus illustres applicateurs des Sciences Chimiques à l'industrie et à la prospérité publiques. Le Secrétaire-Général: A. Béhal. Le Président de la Société Chimique de Paris: Armand Gautier. Juillet, 1906.*

Addresses were also delivered by Dr. Baekeland, representing the chemists of America; Prof. Paul Friedlander, on behalf of the scientific and technical chemists of Austria; Prof. P. van Romburgh, Holland; Prof. H. Rupe, Switzerland; Lord Kelvin, representing the Royal Society; and Prof. Meldola, on behalf of the English Chemical Society.

A passage from the Chemical Society's report is worth quoting: ". . . However highly your technical achievements be rated, those who have been intimately associated with you must feel that the example which you have set by your rectitude as well as by your modesty and sincerity of purpose is of chiefest value. That you should have been able, as a very young man, to overcome the extraordinary difficulties incident to the establishment of an entirely novel industry 50 years ago is a clear proof that you were possessed in an unusual degree of courage, independence of character, judgment, and resourcefulness; but even more striking is your return into the fold of scientific workers and the ardor with which you have devoted yourself to the prosecution of abstract physico-chemical inquiries of exceptional difficulty. In the account of your renowned master, Hofmann, you have stated that one of your great fears on entering into technical work was that it might prevent your continuing research work; that you should have felt such regret at such a period is sufficiently remarkable, and it must be a source of enduring satisfaction to you to know that your later scientific work deserves, in the opinion of many, to rank certainly no less than your earlier."

How much Perkin was appreciated in Germany, where the coal-tar industry had developed into such gigantic proportions, is shown by the delegation that came from that country. There were Prof. Bernthsen, Dr. H. Caro and Dr. Ehrhardt, of the *Badische Anilin und Soda-Fabrik;* Dr. Aug. Clemm, Herr R. Bablich, and Dr. E. Ullrich, *Farbwerke, Meister, Lucius, und Brüning;* Dr. Klingeman, *Casella and Co.*, Prof. Carl Duisberg and Dr. Nieme, *Farbenfabriken, Elberfeld*, and Prof. Liebermann—in short, the cream of Germany's industrial chemical fraternity.

And there were messages from Prof. Beilstein (Petrograd), Prof. Ciamician (Bologna), Prof. Canizzaro (Rome), Prof. Jorgensen (Copenhagen), Prof. Takayama (Tokyo), Prof. Adolf Baeyer (Munich), Prof. J. W. Brühl (Heidelberg), Prof. G. Lunge (Zurich), and Prof. Hugo Schiff (Florence)—an international band of illustrious scholars.

In the autumn following the jubilee celebrations in London, Sir William Perkin accepted an invitation from the American Committee to visit its shores. Various gatherings were held in his honor in New York, Boston, Washington, etc.

In New York a dinner was tendered him at Delmonico's, with the veteran Prof. Chandler, of Columbia, in the chair. Dr. W. H. Nichols presented him with the first impress of the Perkin Medal, since awarded annually to the American chemist who has most distinguished himself by his services to applied chemistry; and Dr. W. F. Hillebrand, president of the American Chemical Society, presented the diploma of honorary membership of the society to the guest of the evening. Other speakers included President Ira Remsen of Johns Hopkins, Prof. Nernst of Berlin, and Dr. W. H. Wiley, chief chemist of the Dept. of Agriculture, Washington.

Perkin died on July 14, 1907.

Aside from his scientific achievements, Perkin's life was extremely uneventful. To him his science was his life, and he seems to have had no avocation. We find no romantic dash, no such many-sidedness, as characterised his great countryman, Ramsay, for example. With modesty carried to the extreme, only the privileged few knew anything of the man, and even Prof. Meldola, an intimate friend of many years' standing, could give but few personal touches of the man in his otherwise excellent obituary address, delivered to the members of the Chemical Society. ". . . I thank God, to whom I owe everything, for all His goodness to me, and ascribe to Him all the praise and honor." This was Perkin's review of his life in 1906. A blameless Christian, a perfect gentleman, a fine type of the old conservative, he lived unobtrusively, worked quietly and intensively, worshipped God, and respected his neighbor. To us, living in days of turmoil and upheaval, such a personage already belongs to an age long past.

Perkin was twice married. His first wife was a daughter of the late Mr. John Lisset. Some years after her death he married a daughter of Mr. Herman Molwo. Mrs. Perkin, three sons, and four daughters, survive him.

His sons are all noted chemists. One of them, Arthur George, is a technical expert, and another, William Henry, is professor of chemistry at Oxford. This Oxford professor is without doubt the foremost organic chemist in England to-day. His work on polymethylenes, alkaloids, camphor, terpenes, etc., is of the highest order.

Like that other grand Englishman, Darwin, Perkin, the genius, begot Perkins of genius. Not always are the Gods so kind to the children of geniuses.

> To great ends and projects had thy life been given;
> Right well and nobly has the goal been won;
> For this, O Great Discoverer, thou hast striven;
> Take, then, our thanks, for all that thou hast done.
>
> (Nora Hastings,—dedicated to Perkin.)

References

Much of the biographical material has been supplied by Perkin himself in his Hofmann Memorial Lecture (1). Prof. Meldola's appreciative article (2) is largely based on this, though valuable additional material, particularly that relating to the technical development of Perkin's dye, is to be found here. The Jubilee volume (3) contains interesting items. Perkin's scientific papers were published in the *Journal of the Chemical Society* (London).

1. W. H. Perkin: The Origin of the Coal-Tar Industry, and the Contributions of Hofmann and his Pupils. *Journal of the Chemical Society* (London), *69*, 556 (1886).

2. Raphael Meldola: Perkin Obituary Notice. *Journal of the Chemical Society* (London), *93*, 2214 (1908).
3. R. Meldola, A. G. Green, and J. C. Cain: Jubilee of the Discovery of Mauve and of the Foundation of the Coal-Tar Color Industry by Sir W. H. Perkin. (Printed by G. E. Wright, at the *Times* Office, London, and published by the Perkin Memorial Committee, 1906.)

DMITRI IVANOWITCH MENDELÉEFF

RUSSIA, the land of mystery to her western neighbors, occasionally startles us by the intellectual giants she produces. The world has long sung praises of Tolstoy and Tschaikowsky, and scientists have shown no less admiration for thephysiologist, Pavloff, and the chemist, Mendeléeff.

Mendeléeff's *Periodic Law* has shown how the elements, the chemist's building-stones, can be grouped to exhibit striking family resemblances. The chaos of the sixties gave place to a law of nature in the seventies, and the law paved the way for the more remarkable discoveries of the present era.

Dmitri Ivanowitch Mendeléeff was born in Tobolsk, Siberia, on February 7, 1834. He was the youngest of eleven, fourteen or seventeen children—authorities seem to differ. On the paternal side Mendeléeff came from priestly stock, his grandfather, Pawal Maksimowitch Sokoloff, occupying a modest position in the Greek Church ruled by the Holy Synod. Since celibacy is not obligatory for the lower clergy of this church, Pawal took advantage of such permission and married. Of his four sons, Wassili, Iwan, Timofei and Alexander, the second, Iwan, came to be called Mendeléeff because early in life he dealt (exchanged) in horses ("mjenu djelatj" = to make an exchange).

Iwan in time became a student of the chief Pedagogical Institute in Petrograd, and sometime after his graduation the government appointed him director of the gymnasium at Tobolsk. Here he met and married Maria Korniloff.

The Korniloffs belonged to an old Russian family that had settled in Tobolsk early in 1700. They were the first to introduce the manufacture of paper and glass in Siberia. In 1787 Maria's father established a printing press at Tobolsk, and two years later he began the publication of the *Irtysch*, the first newspaper ever published in Siberia.

A family tradition had it that in a previous generation one of the Korniloffs had married a Khirgis Tartar beauty, thereby admixing their pure Russian with Mongolian blood. Some of the descendants showed unquestionable oriental features, but not Dmitri, the chemist.

Mendeléeff's name has been spelled in any number of ways. Sometimes it has appeared as Mendeleyef, sometimes Mendelejef, at other times Mendelejeff, and still again Mendéléeff, Mendelejew, and Mendeléeff. We have selected the last as perhaps the least confusing to English ears.

Dmitri, Iwan and Maria's youngest child, was his mother's pet, who referred to him in the endearing diminutive, *Mitjenka.*

Soon after Dmitri's birth his father became blind from a cataract in both eyes, and this terrible calamity forced him to resign from his position at the gymnasium. The government's grant of a pension of one thousand rubles ($500) was hardly enough to keep body and soul together.

At this stage Dmitri's mother, despite the invalid on her hands, and the eight remaining children that needed attention, took charge of glass works belonging to her family, and directed the factory for a number of years with surprising efficiency.

Dmitri showed an exceptional memory from the first. When seven years old he was sent to the gymnasium at

Tobolsk, and here he excelled in mathematics, physics and history, but for languages, and particularly Latin, he showed no inclination. To his last day his repugnance for the classics never left him.

To Tobolsk many of Russia's political prisoners were sent. In those days some of them belonging to the *Dekabrists* were there. The *Dekabrists* were a group of literary men who headed a revolution in 1825 with the object of establishing a constitutional government in Russia. The scheme ended in failure. Five of the leaders were executed, and many of the others were exiled to Siberia. Among these exiles in Tobolsk was one, Bessagrin, who eventually married one of Dmitri's elder sisters, Olga, and it was from this Bessagrin that Dmitri received his "coaching" in science, and his enthusiasm for it.

In 1849, in his sixteenth year, Mendeléeff graduated from the gymnasium. But for his deficiency in the classics he might have obtained a government stipend to continue his studies at a University. As it was, the government refused all help.

Two years before this, in 1847, Mendeléeff's father died of consumption, and to add to the mother's plentiful store of troubles, the glass works which she had managed so ably, were completely destroyed by fire.

Nothing daunted, and despite her age—she was 57 then—Mrs. Mendeléeff, with the two remaining children she still had to care for, Dmitri and his sister Elizabeth, left her native city for Moscow.

She had hoped that in Moscow Dmitri could be entered as a student of the university. But there were stumbling blocks. Dmitri's record did not show that he had been at the head of his class. Neither did Dmitri's mother know any people of political importance, and without such acquaintances the only other way of removing the barrier would have been an ample supply of funds.

Foiled in this attempt, the three proceeded to Petrograd. Pletnoff, the director of the Central Pedagogic Institute in Petrograd, had known Dmitri's father very well, and through his assistance young Mendeléeff was admitted as a student of the physico-mathematical department of the Institute, and further helped financially by the government.

In this same year his noble mother, full to the brim with years of suffering, died. In the preface to his book on *Solutions*, published years later, Mendeléeff feelingly refers to the woman who sacrificed so much for him:

"This investigation is dedicated to the memory of a mother by her youngest offspring. Conducting a factory she could educate him only by her own work. She instructed by example, corrected with love, and in order to devote him to science she left Siberia with him, spending thus her last resources and strength.

"When dying she said, 'Refrain from illusions, insist on work and not on words. Patiently search divine and scientific truth.' She understood how often dialectical methods deceive, how much there is still to be learned, and how, with the aid of science without violence, with love but firmness, all superstition, untruth and error are removed, bringing in their stead the safety of undiscovered truth, freedom for further development, general welfare, and inward happiness. Dmitri Mendeléef regards as sacred a mother's dying words."

The Pedagogical Institute, which was altogether abolished in 1858, was a special training school for secondary or high-school teachers. Though its students met in the same buildings as did the university students, they were a separate body. Their professors, however, were usually also those who occupied chairs at the university.

The more noteworthy of Mendeléeff's teachers were Woskrensky (chemistry), Emil Lenz (physics), Ostrogradsky (mathematics), Ruprecht (botany), F. Brandt (zoology), Kutorga (mineralogy), and Sawitsch (astronomy). With all of these men, particularly with Woskrensky, his standing was very high, and on his graduation he received a gold medal for all-round excellence.

Whether because of much physical hardship, or because of a delicate constitution, is not clear, but towards the end of the course at the Institute his health altogether failed him. Pirogoff, the famous surgeon, claimed that only a sojourn in the south could prolong his life, and then only for some six or seven months!

Famous surgeons, like other famous specialists, are known to make mistakes, and Mendeléeff lived for many more years. But his trip to the Crimea unquestionably saved him.

This trip south he was enabled to undertake by the government appointment which he received as chief science master of the gymnasium im Simferopol, in the Crimea. On the outbreak of the Crimean War Mendeléeff was transferred to the gymnasium in Odessa.

In 1856 Mendeléeff returned to Petrograd. His research on *specific volumes* earned him his master's degree in chemistry, and also an appointment as *privatdocent* at the university.

The decided promise which Mendeléeff had shown led the Minister of Public Instruction to grant him permission to visit and work in foreign laboratories. In this way Mendeléeff, between 1859 to 1861, first worked in Regnault's laboratory in Paris, and then in Bunsen's, in Heidelberg. In neither place did he work directly under the master, but quite independently on his own subject of the physical properties of liquids. In Heidelberg the young Russian went so far as to set up a laboratory of his own.

Perhaps the most significant event in his European travels was his attendance at the Karlsruhe Congress of Chemists in 1860. Here occurred the battle royal on atomic weights, led by the Italian, Cannizarro, which ultimately paved the way for our present well-defined system of chemical structures. Who can doubt that Cannizarro's exposition of the fundamental necessity of atomic weights for elements gave Mendeléeff ideas concerning possible relationships among the elements?

On his return to Petrograd in 1861, Mendeléeff was granted the Doctor of Science degree for a thesis on the combination of alcohol with water. Soon afterwards he was appointed Professor of Chemistry at the Technological Institute.

The general dearth of good chemistry text-books in the Russian language led Mendeléeff to write one on organic chemistry. His amazing industry is shown by the fact that he completed this book of 500 pages in two months! In spite of the rapidity with which it was written, the book established itself as the best of its kind in the language, and the Domidoff Prize of the Petrograd Academy was awarded the author.

In 1869, at the age of thirty two, Mendeléeff was appointed Professor of General Chemistry at the University. His colleague in the organic chemistry department, Butlerow, was Fischer's principal forerunner in synthetic work on the sugars.

Despite lectures, supervision of the laboratory and various executive duties, Mendeléeff translated Wagner's *Chemische Technologie*, a work of several volumes, into Russian, and was very active in research work.

In March, 1869, Mendeléeff presented to the Russian Chemical Society his immortal paper on *The Relation of the Properties to the Atomic Weights of the Elements.*

Mendeléeff was not the first to believe that the elements were not merely disconnected elementary bodies. Thus Döbereiner in 1829 pointed out that a number of the elements could be grouped in "triads" in such a way that the arithmetic mean of the atomic weights of the first and third would give that of the second.

At this point some idea of atomic weight must be given the general reader. Atomic weight sounds like the weight of an atom. That, in reality, is quite an exaggeration. Atoms are much too small to be seen, let alone weighed. The number representing the atomic weight of an element is not the *absolute* but the *relative* weight of the atom. Thus, when we say that the atomic weight of nitrogen is 14 we mean that its atom is 14 times as heavy as the atom of hydrogen (which, because it is the lightest element known, is taken as unity), or that its weight is 14 if the weight of the atom of oxygen is 16. We can get such numbers by weighing many millions of atoms of each element (constituting small particles which can be seen) and then comparing their weights with the weight of a standard element such as hydrogen or oxygen. The actual details are too technical to be discussed here.

Dumas, some thirty years after Döbereiner, advanced a similar hypothesis, extending it to groups in organic chemistry. But to Newlands, an Englishman, belongs the honor of having been the first to see fairly clearly how the eighty-odd elements could be grouped to show their relationships. In a paper read before the English Chemical Society in 1866, Newlands showed that the elements could be arranged in groups of eight along horizontal lines in such a way that elements in the vertical columns would be those with similar properties. The law of *octaves* was given to this grouping of eights.

The reception of the theory by Newland's fellow-chemists was anything but encouraging. One ostentatious busybody wished to know whether Newlands had tried to arrange the elements according to their initial letters! Another suggested new possibilities in the field of music with the law of *octaves!* The upshot of the affair was that poor Newlands was sent home thoroughly ridiculed, and his paper was refused publication in the society's journal. That, however, did not prevent the Royal Society from making some amends twenty-one years later by awarding him its Davy Medal for the very paper which its sister organisation had refused to print!

It must be added, however, in excuse for the scepticism of the scientists of the day, but in no excuse for their arrogance, that Newlands had not put his theory to as thorough a test as he might have done. In its incompleted form its suggestions were too vague for men steeped in experimental work.

But Mendeléeff's paper three years later removed most of the objections, and forced the attention of the chemists to his scheme. Mendeléeff left nothing for granted; his statements were accompanied by rigorous experimental proofs.

It will be seen from the table on p. 27 that when the elements are grouped in the *ascending* order of their atomic weights they exhibit an evident periodicity of properties; thus the ninth, neon, resembles the first, helium,[1] the tenth, sodium, resembles the second,

[1] Hydrogen, the lightest element, does not find an appropriate place in the table.

Series	Zero group	Group I	Group II	Group III	Group IV	Group V	Group VI	Group VII	Group VIII		
0	x	—	—	—	—	—	—	—	—	—	—
1	y	Hydrogen, H = 1.008	—	—	—	—	—	—	—	—	—
2	Helium, He = 4.0	Lithium, Li = 7.03	Beryllium, Be = 9.1	Boron, B = 11.0	Carbon, C = 12.0	Nitrogen, N = 14.04	Oxygen, O = 16.0	Fluorine, F = 19.0	—	—	—
3	Neon, Ne = 19.9	Sodium, Na = 23.05	Magnesium, Mg = 24.1	Aluminium, Al = 27.0	Silicon, Si = 28.4	Phosphorus, P = 31.0	Sulphur, S = 32.06	Chlorine, Cl = 35.45	—	—	—
4	Argon, Ar = 38	Potassium, K = 39.1	Calcium, Ca = 40.1	Scandium, Sc = 44.1	Titanium, Ti = 48.1	Vanadium, V = 51.4	Chromium, Cr = 52.1	Manganese, Mn = 55.0	Iron, Fe = 55.9	Cobalt, Co = 59	Nickel, Ni = 59 (Cu)
5	—	Copper, Cu = 63.6	Zinc, Zn = 65.4	Gallium, Ga = 70.0	Germanium, Ge = 72.3	Arsenic, As = 75.0	Selenium, Se = 79.0	Bromine, Br = 79.95	—	—	—
6	Krypton, Kr = 81.8	Rubidium, Rb = 85.4	Strontium, Sr = 87.6	Yttrium, Y = 89.0	Zirconium, Zr = 90.6	Niobium, Nb = 94.0	Molybdenum, Mo = 96.0	—	Ruthenium, Ru = 101.7	Rhodium, Rh = 103.0	Palladium, Pd = 106.5 (Ag)
7	—	Silver, Ag = 107.9	Cadmium, Cd = 112.4	Indium, In = 114.0	Tin, Sn = 119.0	Antimony, Sb = 120.0	Tellurium, Te = 127	Iodine, I = 127	—	—	—
8	Xenon, Xe = 128	Cæsium, Cs = 132.9	Barium, Ba = 137.4	Lanthanum, La = 139	Cerium, Ce = 140	—	—	—	—	—	—
9	—	—	—	—	—	—	—	—	—	—	—
10	—	—	—	Ytterbium, Yb = 173	—	Tantalum Ta = 183.0	Tungsten, W = 184	—	Osmium, Os = 191	Iridium, Ir = 193	Platinum, Pt = 194.9 (Au)
11	—	Gold, Au = 197.2	Mercury, Hg = 200.0	Thallium, Tl = 204.1	Lead, Pb = 206.9	Bismuth, Bi = 208	—	—	—	—	—
12	Nt 222.4	—	Radium, Rd = 224	—	Thorium, Th = 232	—	Uranium, U = 239	—	—	—	—

lithium, and so on. In other words, the elements in the *vertical* columns show striking similarities in properties. Such is the gist of this law, though its details are much more complicated.

What were its immediate results? To begin with, a number of the elements did not fit in with Mendeléeff's scheme. Forthwith Mendeléeff announced that the fault lay with incorrect atomic weights which had been assigned these elements.

Mendeléeff proved right in all such cases. Thus, to take one example, the then accepted atomic weight for gold was 196.2; accordingly it should have been placed *before* such elements as platinum, iridium and osmium, with atomic weights of 196.7, 196.7 and 198.6 respectively. But Mendeléeff insisted upon putting gold after these elements, claiming that their atomic weights, and not his table needed revision. Subsequently, a revision of their atomic weights gave these results:

Osmium 190.9, Iridium 193.1, Platinum 195.2 and Gold 197.2, which was precisely the order in which Mendeléeff had originally placed them.

But Mendeléeff did something far more daring. The grouping according to Mendeléeff's scheme resulted in certain gaps being left unfilled. This, said Mendeléeff, was due to elements which awaited discovery. By a careful consideration of the properties of adjacent elements the great Russian *predicted* the properties of these undiscovered elements.

A case will be cited. To one of these unknown elements Mendeléeff gave the name *ekasilicon*, and certain properties were predicted for it. In 1886 Winkler discovered *germanium*, which showed identical properties with this *ekasilicon*, as the following comparison will show:

	Mendeléeff's Ekasilicon	Winkler's Germanium
Atomic weight	Es, 72	Ge, 72.5
Density	Es, 5.5	Ge, 5.469
Density of oxide	EsO_2, 4.7	GeO_2, 4.703
Density of chloride	$EsCl_4$, 1.9	$GeCl_4$, 1.887
Boiling point of chloride	Less than 100 degrees centigrade	86 degrees dentigrade
Density of ethide	$Es(C_2H_5)_4$, 0.96	$Ge(C_2H_5)_4$, less than 1
Boiling point of ethide	160°	160°

These wonderful predictions did more to convince scientists of the validity of the law than anything else could have done. The soundness of a theory is best exemplified by the use to which it can be put. Does it explain anomalies? Does it guide along future paths of investigation? The Periodic Law has more than fulfilled these requirements. As a beacon it stands out as prominently in the history of chemistry as does Dalton's Atomic Theory, which is at the very foundation of our science to-day. Some of the most startling discoveries of our time, such as the rare gases of the atmosphere (see Ramsay) and the radioelements (see Curie and Richards) are directly attributable to the Periodic Law.[2]

The same year that saw the publication of Mendeléeff's immortal paper, that is, in 1869, also witnessed the publication of his *Principles of Chemistry*, which in some

[2] It should be mentioned that Chancourtois in France, and Lothar Meyer in Germany, also suggested periodic classification of the elements. Lothar Meyer, in particular, with his atomic volumes—the volumes occupied by atomic weights of the elements—was able to uncover some striking analogies. Lothar Meyer and Mendeléeff's papers were published in the same year—1869. The time unquestionably was ripe for some such formulation. In a similar way, Darwin and Wallace, ten years earlier, unfolded the origin of species quite independently of one another.

ways stands alone among chemical books. One of its unique features is the very elaborate footnotes in smaller print, which occupy more space than the actual text, and which are mainly taken up with the personal views of the author. These footnotes give the key to any number of new problems, and are the source of perennial inspiration to readers.

The two volumes of the *Principles* have gone through many editions in many languages (including English), and its text seems little antiquated even to-day, which is an exceptionally high compliment to be paid a chemical work that has been before the public for fifty years. In the first chapter of volume II the reader will find an illuminating account of the author's Periodic Law.[3]

Till his death, in 1907, Mendeléeff worked and wrote incessantly. He, together with his co-workers, published more than two hundred and fifty articles, touching every phase of chemistry. Indeed there is not a branch of our science but was enriched by his contributions. Abstruse subjects such as the properties of liquids, theories of solution and the development of the gas laws, seem but distantly connected with the pressing problems of the day, though they are not so far removed as the layman is apt to think. The constitution of the upper atmosphere, the aether, seems a metaphysical problem perhaps. But in addition to such profound investigations in chemical philosophy, Mendeléeff proved of much practical value to the government and the people of Russia by his exhaustive investigations of the Baku oil fields.

Mendeléeff's first report on the naphtha springs in the Caucusus was issued as early as 1866. In 1876, in order to get further first-hand information, he visited

[3] Though commonly known as the Periodic *Law*, the Periodic *System* is a much better name for it.

the Pennsylvania oil fields. The possible exhaustion of the Baku petroleum led the Russian Government to requisition his services in 1886. His suggestions led to fruitful results.

In 1887, during a solar eclipse, Mendeléeff ascended alone in a balloon to make various scientific observations. This ascent was not without its perils, and gave some anxious moments to his assistants, but it had its reward in the local fame which it earned him; "for the peasant women thereafter used to tell that Dmitri Ivanovitsch flew on a bubble and pierced the sky, and for this the authorities made him a chemist!"

In 1882 Mendeléeff and Lothar Meyer were awarded the Davy Medal of the Royal Society, the Copley Medal going to Arthur Cayley, the mathematician, and a Royal Medal, to the late Lord Rayleigh. "Like every great step in our knowledge of the order of nature," said the president, William Spottiswoode, "this periodic series not only enables us to see clearly much what we could not see before; it also raises new difficulties, and points to many problems which need investigation. It is certainly a most important extension of the science of chemistry."

Mendeléeff was chosen for the Copley Medallist, the Royal Society's highest award, in 1905. By this time he had reached the very zenith of his fame. "Men deléeff," said Sir William Huggins, "stands high among the great philosophical chemists of the last century."

At various other times he was honored with degrees from Princeton, Oxford, Cambridge and Göttingen, and in 1889 he won the Faraday Medal of the English Chemical Society.

These marks of recognition, gratifying as they were, could hardly compensate for the annoyances which

Mendeléeff experienced as professor at the university. Whether envious because of his reputation, or finding him unacceptable because he was not a well-defined autocrat, the Academy at Petrograd black-balled him. The Ministry of Education considered him far too much of a liberal, whereas many of the students were of the opinion that he never went far enough. He does not seem to have been particularly welcome in either opposing camp.

Occasionally, because of his neutrality, Mendeléeff attempted to act as mediator. On one of these occasions, in 1890, after serious disturbances at the university by the students, resulting, as usual, from the ruthless suppression by the police of any semblance of freedom of thought, Mendeléeff partly pacified the undergraduates by promising to present their petition to the Minister of Education. This was enough to bring down the wrath of the official ministry upon him. In a very sharp note he was told to steer clear of aught but what concerned him as teacher of chemistry. Mendeléeff felt this sting so deeply that he resigned from his chair at the university. Some amends were made three years later when Sergius Witte, the Minister of Finance, appointed him Director of the Bureau of Weights and Measures—a post he retained until his death.

Those who have read his *Principles* can form some opinion of what a stimulating lecturer Mendeléeff must have been. We would have expected the author of the *Periodic Law* to have emphasised the co-ordinated links in the chain, and to have presented a unified picture of the whole subject of chemistry. Such, indeed, is the testimony of his students. Mr. I. Goldenberg writes: "I was a student in the Technological Institute from 1867–9. Mendeléeff was our professor, and in 1868 taught organic chemistry. The previous course by the professor of inorganic chemistry consisted of a collection of recipes, very hard to remember, but, thanks to Mendeléeff, I began to perceive that chemistry was really a science.

"The most remarkable thing at his lectures was that the mind of his audience worked with his, forseeing the conclusions he might arrive at, and feeling happy when he did reach these conclusions. More than once he said, 'I do not wish to cram you with facts, but I want you to be able to read chemical treatises and other literature, to be able to analyse them, and, in fact, to understand chemistry. And you should remember that hypotheses are not theories.'

"He was considered among the students a liberal man, and they thought of him as a comrade. More than once during a disturbance between the students and the administration Mendeléeff supported the students, and under his influence many matters were put right."

Prince Peter Kropotkin, the well-known Russian socialist, was also one of Mendeléeff's students. "I had the good fortune," writes the Prince, "to follow, in 1867–9, his lectures on both organic and inorganic chemistry. The former was an abridged course, which he had the admirable idea to deliver for us students of the mathematical branch of the physico-mathematical faculty.

". . . Imagine each of these notes [referring to the footnotes in the *Principles*] developed into a beautiful improvisation, with all the freshness of thought of a man who, while he speaks, evolves all the arguments. for and against, there on the spot.

"The hall was always crowded with something like two hundred students, many of whom, I am afraid, could not follow Mendeléeff, but for the few of us who could it was a stimulant to the intellect and a lesson in scientific thinking which must have left deep traces in their development, as it did in mine."

In 1863, two years after his appointment at the Technological Institute, Mendeléeff married his first wife (*née* Lesthoff). With her he had a son, Vladimir, who died in 1899 at the age of thirty four, and a daughter, Olga. This marriage proved an extremely unhappy one. For some time they lived apart, and finally they were divorced. In 1877 he fell in love with a young lady artist, Anna Ivanovna Popova, of Cossack origin, and the two were married in 1881.

From his second wife Mendeléeff received his very decided views on art. These found characteristic expression in a letter he wrote to the Russian daily, *Goloss* (the voice) on the subject of a picture by Kouindji, *Night in the Ukraine:* "Landscape was depicted in antiquity, but was not in favor in those days. Even the great masters of the sixteenth century made use of it merely as a frame to their pictures. It was the human form which inspired artists of that epoch; even the gods and the Almighty himself appeared to their minds in human shape. In this alone they found the infinite, the inspiring, the divine. And this was because they worshipped human mind and human spirit.

"This found expression in science in an exceptional development of mathematical logic, metaphysics and politics. Later, however, men lost faith in the absolute and original power of human reason, and they discovered that the study of external nature assists even in the correct appreciation of the nature of the human inner self. Thus nature became an object of study; a natural science arose unknown either to antiquity or to the period of the Renaissance.

"Observation and experience, inductive reasoning, submission to the inevitable, soon gave rise to a new and more powerful, more productive method of seeking truth. It thus became evident that human nature, including its consciousness and reason, is merely a part of the whole, which is easier to comprehend as such from the study of external nature than of the inner man. External nature thus ceased to be subservient to man and became his equal, his friend. . . . Inductive and experimental science became a crown of knowledge, royal physics and mathematics had now to be content with modest questioning of nature.

"Landscape painting was born simultaneously with the change, or perhaps a little earlier. Thus it will probably come to pass that our age will hereafter be known as the epoch of natural science in philosophy and of landscape in art. Both derive their materials from sources external to man. . . . Man has, however, not been lost sight of as an object of study and of artistic creation, but he now appears, not as a potentate or as a microcosm, but merely as part of a complex whole."

Mendeléeff's wife adorned his study with pen sketches of such scientific celebrities as Lavoisier, Descartes, Newton, Galileo, Copernicus, Graham, Mitscherlich, Rose, Chevreul, Faraday, Berthelot, Dumas, etc.

The family first lived at the university, then in a house specially built for the Director of the Bureau of Weights and Measures. In this house his children by his second wife were born: Lioubov (Aimee), Ivan (Jean), and the twins Maria and Vassili (Basile).

In appearance Mendeléeff was a genuine Slav. Medium in height, rather powerfully set, with an abundance of hair reminding one of a Paderewski, expressive blue eyes, high cheek bones, an immense forehead, he commanded attention wherever he went. At home he went about in loose garments of his own design, somewhat after the fashion of his illustrious compatriot, Tolstoy.

For all the pomp of court life, in fact, for any ostentatious display, he had nothing but contempt. His presentation to Tsar Alexander III was made possible only by the permission which was given him to wear anything he pleased. This embraced non-interference with his proud locks.

His democracy showed itself in peculiar ways. For example, he always insisted on travelling third class in his short journeys from Petrograd to his estate, but at the station his driver, Zassorin, was always at hand with the *troika* and a pair of magnificent greys, and the somewhat shabby third class traveller became suddenly transformed into the wealthy landowner.

Mendeléeff was a Russian of the temperamental variety—a quite common variety of Russian; he was rather hard to live with, at times smooth and silky in speech, at other times quite uncontrollable in temper, and for no apparent reason.

Though unconcerned as to his personal appearance, Mendeléeff was extremely sensitive as to the way people received him. He knew himself to be a genius, and he expected people to pay homage. In this connection Sir William Ramsay tells of an amusing incident which occurred at a dinner in London, given to W. H. Perkin in 1884: "Iwas very early at the dinner and was putting off time, looking at the names of people to be present, when a peculiar foreigner, every hair of whose head acted in independence of every other, came up bowing. I said, 'We are to have a good attendance I think.' He said, 'I do not spik English.' I said, 'Vielleicht sprechen Sie Deutch?' He replied, 'Ja ein wenig. Ich bin Mendeléeff.' I did not say, 'Ich bin Ramsay,' but 'Ich heisse Ramsay,' which was perhaps more modest. His method reminded me of 'the only Jones.' Well, we had twenty minutes or so before anyone else turned up and we talked our mutual subject fairly out. He is a nice sort of a fellow, but his German is not perfect. He said he was raised in East Siberia and knew no Russian even till he was seventeen years old. I suppose he is a Kalmuck, or one of these outlandish creatures."

In 1900 the Prussian Academy celebrated its two-hundredth anniversary, and the University of Petrograd sent Mendeléeff as its delegate. At the banquet van't Hoff presided over one of the side tables, with Ladenburg (the Breslau representative) to the right, and Mendeléeff to the left of him. Mendeléeff was an inveterate smoker, and simply chafed because he could not eat and smoke alternately. Ladenburg tells us that immediately after the soup Mendeléeff began to pump those around him as to whether he could be allowed to smoke. They answered him that that was out of the question. But he repeated his question after the first, and after the second courses. Then dear old van't Hoff, who hated to see anyone suffer so, stepped in with the risky suggestion that he also would join in a smoke. And the two went to it, to the great relief of Mendeléeff, who from then on proved an enjoyable companion. But the sad side of the incident was that van't Hoff, who had begun to show incipient signs of tuberculosis, had been expressly forbidden smoking.

The present outcry against the classics, and the belief by many in America and England that a portion of the classical scholarship of statesmen could well be displaced by scientific information, was echoed by Mendeléeff long before the World War emphasised the imperative necessity of a utilitarian education. In 1901 he published a pamphlet on *Remarks on Public Instruction in Russia*, in which there occurs the following:

"The fundamental direction of Russian education should be living and real, not based on dead languages, grammatical rules, and dialectical discussions, which without experimental control, bring self-deceit, illusion, presumption, and selfishness."

Universal peace and the brotherhood of nations, says Mendeléeff, with, we are afraid, a super-abundance of confidence in his view, can only be brought about by a vital realism in schools. "For such reforms are required many strong realists; classicists are only fit to be landowners, capitalists, civil servants, men of letters, critics, describing and discussing, but helping only indirectly the cause of popular needs. We could live at the present day without a Plato, but a double number of Newtons is required to discover the secrets of nature, and to bring life into harmony with the laws of nature."

From such remarks the reader may conclude that Mendeléeff was perilously near being a radical. As a matter of fact this is no nearer the truth than the inference that because he used the third class railway compartment he was to be considered one of the people. Mendeléeff, in fact, was regarded by many as a rigid monarchist. The Russo-Japanese War, for example, found him in the camp of the jingos. The revolutionary outbreaks during the war, and Russia's defeat, unquestionably hastened his end. Scientific Russia, which had bestirred itself to great undertakings in 1904 in honor of the Master's seventieth celebration, found itself little encouraged in its proceedings by the broken spirit in Petrograd.

When he was in his library and wrote articles, Mendeléeff described himself as an "evolutionist of peacable type."

His attitude towards women was equally characteristic. To show his broad-mindedness, he employed some of them at the Bureau of Weights and Measures, and even lectured to them. But he did not hesitate to make clear that they were decidedly inferior to men in intellect. Feminists, he declared, perhaps with some truth, aimed not so much at equality of political position as at opportunities for work, to escape inactivity.

His day's work done, Mendeléeff would retire to his estate at Tuer, Boblova, and dine at six. Then he was very fond of company, and could be seen at his best. Mendeléeff at his best had hardly a peer, particularly when the subject turned to the philosophy of science. After dinner, if alone with his family, he would puff at his cigarette and usually read books of adventure—Fenimore Cooper, Jules Verne and the like. Sometimes, being really fond of literature, he would read deeper things. Among Russians, Maicoff and Tuttcheff were his favorites; outside of his own country he loved Byron best. Byron, as we shall see, was also van't Hoff's literary hero.

The theatre saw Mendeléeff seldom, but music was a favored form of recreation. In this field of art he had decided preference for Beethoven.

"But of all things I love nothing more in life than to have my children around me;" which brings us to the most lovable side of Mendeléeff's personality, and here we shall leave him.

Mendeléeff died in 1907 from an attack of pneumonia. Just prior to falling into an unconscious state, he had requested that Jules Verne's *Journey to the North Pole* be read to him.

Tolstoy commands no more dominating position in literature than does Mendeléeff in chemistry. Both belong to the world at large, and the world is thankful to them and to Russia for having enriched the intellect of so many of us.

References

Some of the facts come from private sources. I have, however, drawn freely on Prof. Tilden's article (1). Prof. Walden's essay (2) also proved very useful. Sir Edward Thorpe's sketch (3) carries us up to 1889. Mendeléeff's book (5) is well worth examination. Other references are 4, 6 and 7.

1. W. A. Tilden: Mendeléeff Memorial Lecture. *Journal of the Chemical Society* (London), *95*, 2007 (1908).
2. P. Walden: Dmitri Iwanowitsch Mendelejeff. *Berichte der deutchen chemischen Gesellschaft* (Berlin), *41*, 4719 (1908).
3. Sir Edward Thorpe: Essays in Historical Chemistry (Macmillan and Co. 1911).
4. D. I. Mendeléeff: An Attempt Towards a Chemical Conception of the Ether (Longmans, Green and Co. 1904).
5. D. I. Mendeléeff: The Principles of Chemistry. 2 vols. (Longmans, Green and Co. 1905.)
6. F. P. Venable: The Development of the Periodic Law (Chemical Publishing Company. 1896).
7. A. E. Garett: The Periodic Law (D. Appleton and Co. 1909).

WILLIAM RAMSAY

IN that elegant tribute to Ramsay, written in the days when comradeship between the scientists of England and Germany was close, Ostwald summarizes him as one belonging to the romantic type in science. Romantic he was, for his imagination was unlimited. The secret of Ramsay's great triumphs lay in the fact that with this imagination there was a well-balanced knowledge of the science, with a seer's insight into the significance of its laws. Bold in the conception of a problem, he was brilliant beyond comparison in its execution. With no fetish to hold him, with the mantle of the prophet about him, and with amazing manipulative skill, he layed bare, in rapid succession, a regular little battalion of new gases in the atmosphere, followed by transmutation experiments which made the scientific world gasp and hold its breath in expectancy of the next dare-devil leap.

This genius, born in Glasgow in 1852, did not spring from any geniuses, but like many another man of talent, his stock was of a fairly ordinary type. To be sure, there was an uncle with a reputation as a geologist, and his own father had some scientific tastes, but nothing at all to warrant such outpourings in the offspring. When eleven years old he joined the Third Latin Class of the Glasgow Academy, and during the three succeeding years at the institution he did little Latin, gained no prizes, and did much dreaming. Ramsay describes himself in a short autobiography as "to a certain extent precocious, though idle and dreamy youngster." This fits in with Ostwald's theory of the genius: "The precociousness is a practically universal phenomenon of incipient genius, and the dreamy quality indicates that original production of thought which lies at the basis of all creative activity." Even thus early he evinced a passion for languages, for it is recorded that during sermon time at church he read the French and German texts of the Bible and translated them into English. In after years, as president of an international scientific gathering, he would astound the assembly by addressing them successively in French, German and Italian.

His introduction to chemistry came in quite an unexpected way. A football skirmish resulted in his breaking a leg, and to lessen the monotony of convalescence, Ramsay read Graham's Chemistry, with the object, as he frankly confesses, of learning how to make fireworks. During the next four years his bedroom was full of bottles, and test tubes, and often full of strange odors and of startling noises. But systematic chemistry was not taken up till 1869, three years after he had entered the University of Glasgow. Then, it seems, the passion came on, and with it, a passion for the cognate science, physics. This resulted in an introduction to William Thompson (later Lord Kelvin), the professor, who set the youngster upon the elevating task of getting the "kinks" out of a bundle of copper wire, an operation which lasted a week. It is to be presumed that Thompson was favorably impressed with the manner in which this piece of research was carried out, for Ramsay was immediately introduced to a quadrant electrometer and asked to study its construction and use.

A year's introductory study of chemistry decided Ramsay upon his career, and with his parents' blessing he set out for Heidelberg in 1870, to be exchanged for Tübingen some months later. In Tübingen ruled Fittig, whose lectures were "distinct and clear," whose scholarship was sound, and whose research was methodical. The two years spent at Tübingen were full of work and little play. "I was up this morning," he writes to his father, "at 5.30 and studied and took my breakfast from 6 to 7,—a class from 7 to 8, one from 8 to 9, from 9 to 3 laboratory (I lunch now to have more time for work, and don't dine till 6), and from 3 to 5 I studied, then from 5 to 6 lecture, and then I dined. And now at 8 I must start again." And so this was kept up—all the time, curiously enough, with emphasis on organic chemistry, a branch of the science which Ramsay almost wholly abandoned in his later and most productive years—till the time for the Ph.D. examination. "On Monday at 7 it began and lasted till half-past 12; then in the afternoon from 3 to 8, so we had a good spell of it." The questions in chemistry were: (*a*) the resemblances and differences between the compounds of carbon and silicon, and (*b*) the relation between glycerine and its newer derivatives and the other compounds containing three atoms of carbon; in physics: (*a*) the different methods for determining the specific gravity of gases and vapors, and (*b*) the phenomena which may be observed in crystals in polarised light. "I managed to answer the first perfectly, the second however, not so well, and the two questions in physics pretty well. Then to-night we had the oral exam. The five professors who compose the faculty were there. Fittig gave some very difficult questions. Reusch (Physics), on the other hand, very easy ones. . . . We had to dress up and put on white kids, and I had to get a 'tile' especially for the occasion. Then we were sent out after the exam. for about 5 minutes and were then called in and formally told we had passed."

A dissertation on "toluic and nitrotoluic acids," which gave no glimpse of the future before him, completed Ramsay's Ph.D. requirements, and he returned to Glasgow, where he became assistant in the Young Laboratory of Technical Chemistry. And now Ramsay had to turn his attention from organic to inorganic chemistry, for most of the courses at the technical school were devoted to the latter. Though the physico-chemistry background was entirely lacking, and therefore the knowledge obtained could hardly have been more than miscellaneous, innumerable facts were picked up and stored for future reference.

An opening as tutorial assistant at Glasgow University offered the possibilities of a more congenial academic atmosphere, and also the hope of continuing his interrupted research in organic chemistry. "The cellars of the University Laboratory contained a large collection of fractions of 'Dippel-Oil' prepared by Professor Thomas Anderson. These were regarded by Ferguson (his successor), whose interest in chemistry was almost entirely that of an antiquary, more or less in the light of museum specimens, and he was horrified when Ramsay suggested that he should be allowed to 'investigate' them, but he eventually gave way to Ramsay's importunity. The result was a very substantial addition to our knowledge of the pyridine bases and their derivatives." [1]

The chemistry of dyes and explosives was not to be his life work. How he turned from this to the more mathematical branch of the subject is ascribed by Ramsay himself to problems he encountered in attempts to determine the molecular weights of some of his organic compounds by the Victor Meyer vapor density method. But we must also add that Ramsay, with that

[1] Sir James Dobbie.

instinct for detecting the truly important among a mass of new theories and facts, which was one of his greatest assets, early foresaw the part the new science of physical chemistry would play in the development of chemistry. Thus he was one of the earliest in England to appreciate the true significance of Guldberg and Waage's *Law of Mass action*, just as, at a later date, he was among the first to seize upon and translate van't Hoff's celebrated paper on the analogy between the state of substances in solution and the same when in a state of gas. The Victor Meyer method suggested to him experiments on the volume of liquids at their boiling point, and this in turn gave rise to a whole series of new possibilities, the experimental side of which kept him and his collaborators, particularly Young and Shields, busy even after he had settled in University College years later.[2]

For six years Ramsay remained assistant at Glasgow University, and though during that time he had been a candidate for several chairs and lectureships, nothing came of any of them. So discouraged did he become that there was much discussion in the family as to the advisability of starting business as a chemical manufacturer. But before this scheme could be put into execution a vacancy at University College, Bristol, presented itself.

The story goes that his knowledge of Dutch saved the day. According to this account one of the members of the University Council, a minister, was much perplexed with a Dutch text in his possession, and Ramsay volun-

[2] "It was while blowing the bulbs used in this research (the volumes of liquids at their boiling point), I believe, that he first became aware of the value of the asset he possessed for physical work in his skill as a glass-blower. He had learnt the art at Tübingen, although it was only in his later researches that his marvellous manipulative power was fully developed."—Sir James Dobbie.

teered a translation. The result was Ramsay's appointment by a majority of one!

The stipend was fixed at a minimum of £400 ($2,000) per year. "The professor," read the contract, "will be required to give three lectures per week for the first two terms, say 60 lectures, together with class instruction in connection therewith . . . and a short course of lectures in the third term. He will also be required to superintend the laboratory during the whole session, and to give evening lectures once a week during the first two terms, together with class instruction in connection therewith. . . . The scheme of the College contemplates the possibility of occasional lectures being delivered in neighboring towns by the Professor or his assistant. . . . In connection with the Cloth working Industry, special instruction in dyeing, etc. may be required under an arrangement not yet concluded with the worshipful the Cloth-workers' Company of London."

The professor, not yet turned thirty, was to be kept busy on the job, with very little opportunity for research—an altogether minor consideration to the worthy councillors. But they had not reckoned on Ramsay's energy and capacity. Determinations of the density of gases, of the specific volumes of liquids at their boiling point, of the vapor pressures and critical constants of liquids were soon in full blast. And then came those classical determinations on the thermal properties of solids and liquids, and on evaporation and dissociation, most of which was done with his assistant, Young, which continued at full blast for the next five years until Ramsay's transfer to London. This appointment came in 1887. By that time Ramsay's reputation was such that the following year he was elected an F.R.S. (Fellow of the Royal Society).

In London his physico-chemical researches were further extended. Among these, particular mention should be made of perhaps the most brilliant of them all—the measurement of surface tension up to the critical temperature, which led to the well-known law supplying us with a method for determining the molecular weight of liquids. Here Ramsay had an able assistant in Shields.

In 1890 the British Association met at Leeds, and two of the great Continental founders of modern physical Chemistry, van't Hoff and Ostwald, were present. Ramsay, who represented the school in England, naturally took a keen interest in this meeting. "Ramsay and Ostwald met for the first time as fellow-guests in my house, which became accordingly a sort of cyclonic center of the polemical storm that raged during the whole week. . . . The discussion was incessant. . . . I remember conducting a party to Fountains Abbey on the Saturday and hearing nothing but talk of the ionic theory amid the beauties of Studley Royal. The climax, however, was reached the next day, Sunday. The discussion began at luncheon when Fitzgerald raised the question of the molecular integrity of the salt in the soup and walked round the table with a diagram to confound van't Hoff and Ostwald. . . . Ramsay was no silent spectator. Being a convinced ionist, he was eager in helping out the expositions of Ostwald, whose English at that time was imperfect and explosive, and his wit and humor played over the whole proceedings. . . . It was the beginning of relations of great mutual sympathy and regard between Ramsay and Ostwald, which lasted till they were divided by their respective national sympathies at the unhappy outbreak of war." [3]

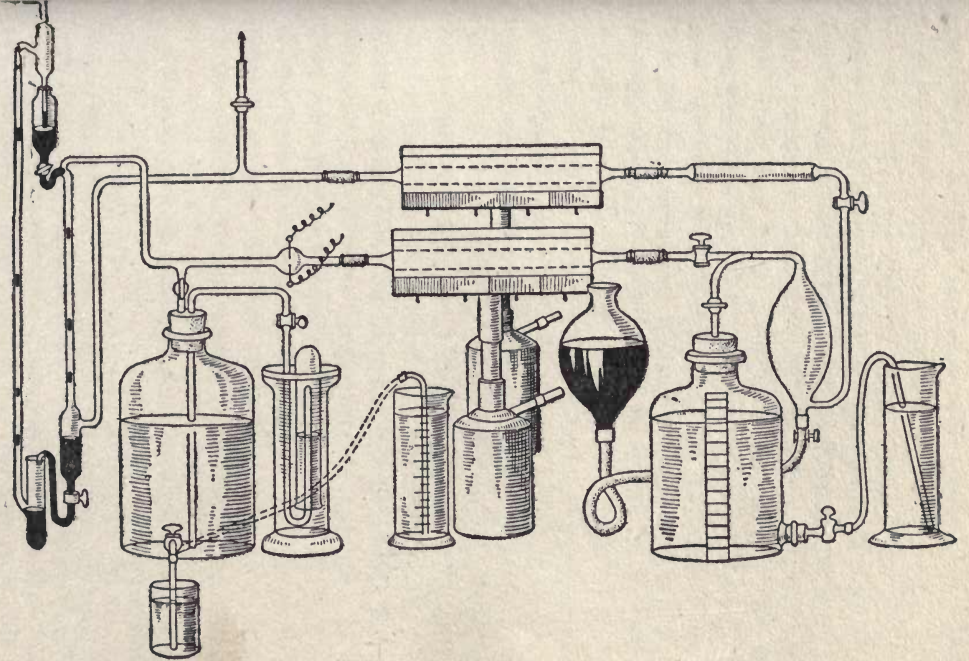
[3] Professor Smithells.

And now we come to a momentous event in the career of our hero. Lord Raleigh had for some time been engaged in determinations of the exact densities of a number of gases. Among these was nitrogen. In his experiments Raleigh found that the density of nitrogen obtained from the air was slightly but consistently higher than that obtained from artificial sources. Writing to *Nature* (1892) he says: "I am much puzzled by some results as to the density of nitrogen and shall be obliged if any of your chemical readers can offer suggestions as to the cause. According to two methods of preparation I obtain quite distinct values. The relative difference, amounting to about 1/1000 part, is small in itself; but it lies entirely outside the errors of experiment." The difference in the weights of one liter of the gas obtained in the one case from atmospheric air and in the other from ammonia varied by about 6 in 1,200, or about 0.5 percent, but the accuracy of the method did not involve an error of more than 0.02 percent.

With that keen scent for any promising material Ramsay immediately took up the problem. Some years previous he had found that nitrogen is absorbed fairly readily by magnesium. This suggested to him that by first getting rid of the oxygen in the air, and passing the remaining nitrogen repeatedly over heated magnesium, any other gas that might possibly be present in the atmosphere would remain unabsorbed. This unabsorbed gas was isolated and found to give a characteristic spectrum. The name *argon* (Gk., inert) was given to the newly discovered ingredient of the atmosphere. It proved to be more refractory than the comparatively inert nitrogen: it just simply would not make friends and combine with any other element!

Shortly after this, Ramsay's attention was called to some experiments of Hillebrandt, of the U. S. Geological

5



Ramsay's apparatus for the isolation of argon, a gas of the atmosphere. [Reproduced from the *Transactions of the Royal Society* (London).]

Survey, in which he obtained a gas believed to be nitrogen from certain minerals, particularly one called clèveite, but which was now suspected to contain argon as well. Ramsay lost no time. From it he obtained argon, to be sure, but also another gas, with a spectrum all its own, which showed it to be identical with an element present in the chromosphere of the sun, and which until then had been considered peculiar to the sun. Lockyer years ago gave the name "helium" to it, and now Ramsay had rediscovered it on mother earth. But let the discoverer himself tell the exciting news. On the 24th of March, 1895, he writes to his wife:[4] "Let's take the biggest piece of news first. I bottled the new gas in a vacuum tube, and arranged so that I could see its spectrum and that of argon in the same spectroscope at the same time. There is argon in the gas; but there was a magnificent yellow line, brilliantly bright, not coincident with but very close to the sodium yellow line. I was puzzled but began to smell a rat. I told Crookes,[5] and on Saturday morning when Harley, Shields,[6] and I were looking at the spectrum in the dark room a telegram came from Crookes. He had sent a copy here[7] and I enclose that copy. You may wonder what it means. Helium is the name given to a line in the solar spectrum, known to belong to an element,

[4] Ramsay married Margaret, daughter of George Stevenson Buchanan, in August, 1881, soon after he had been appointed Principal of Bristol College—a position he attained one year after his arrival in Bristol. This union proved a particularly happy one. "To have such a helpmate as my wife has brought me happiness which I must acknowledge with the greatest thankfulness." And at a later date he wrote to a friend: "You have got a good son and daughter and that is much to rejoice at. So have I."

[5] Sir William Crooks, the famous physicist and chemist.

[6] His two assistants.

[7] 12 Arundel Gardens, their home.

but that element has hitherto been unknown on earth. . . . It is quite overwhelming and beats argon. I telegraphed to Berthelot[8] at once yesterday—'Gaz obtenu par moi clèvite melange argon helium. Crookes identifie spectre. Faites communication Academie lundi.—Ramsay.' . . . I have written Lord Raleigh and I'll send a note to the R.S. [Royal Society] to-morrow. . . ."

The first public account of helium was given to a semi-bewildered audience at the annual meeting of the chemical society in 1895, on the occasion of the presentation of the Faraday medal to Lord Raleigh. Further investigations proved that helium occurred in quite a number of minerals and mineral waters. To Kayser, however, was left the proof of its presence in the air. Like argon it simply refused to combine with any other substance.

To the ancients air was a source of investigation, and it had remained so. Till 1894 no one, least of all a scientist,[9] would have suspected the existence in the atmosphere of undiscovered elements. Ramsay and Raleigh's discovery shook the scientific world. Recognition came from all parts. Lord Kelvin, as president of the Royal Society, presented Ramsay with the Davy Medal, with the following comment: ". . . The researches on which the award of the Davy Medal to Professor Ramsay is chiefly founded are, firstly, those which he has carried on, in conjunction with Lord Raleigh, in the investigation of the properties of argon, and in the discovery of improved and rapid methods of getting it from the atmosphere; and secondly, the discovery in certain rare minerals, of a new elementary gas which appears to be identical with the hitherto hypothetical solar element, to which Mr. Lockyer many years

[8] A famous French chemist.

[9] Cavendish, in 1785, did suspect some such possibility.

ago gave the name of 'helium.'[1] . . . The conferring of the Davy Medal on Professor Ramsay is a crowning act of recognition of his work on argon and helium which has already been recognised as worthy of honor by scientific societies in other countries. For his discoveries of these gases he has already been awarded the Foreign Membership of the Societe Philosophique de Geneve and of the Leyden Philosophical Society. He has had the Barnard Medal of Columbia College awarded to him by the American Academy of Sciences, and within the last few weeks he has been elected a Foreign Correspondent of the French Academie des Sciences."

Such was the excitement aroused by these discoveries that even young students were filled with the epidemic. We are told that "answers to examination questions showed that oxygen as a constituent of our air was almost forgotten in the anxiety on the part of the candidate to show that he or she knew all about argon."

But Ramsay had not yet sufficiently dumbfounded his scientific confrères. From a careful study of Mendeléeff's periodic grouping of the elements, he came to the conclusion that another inert gas ought to exist between helium and argon, employing a process of reasoning quite analogous to one used by the celebrated Russian many years before when, with the help of his periodic table, he predicted the discovery of new elements. Ramsay ransacked every possible source for this new element: minerals from all parts of the globe, mineral waters from Britain, France and Iceland; meteorites from interstellar space—all without result. A clue was at length obtained when he found that by diffusion argon could be separated into a lighter and heavier portion. This suggested the presence of the unknown gas as an impurity

[1]And helium, the inert gas, a chemical curiosity in 1895, is now displacing hydrogen in baloons!

in argon. It was evident that the unknown gas, if present, could be there in minute quantities only to have escaped detection. That meant that the larger the quantity of argon employed the better the possibilities of getting appreciable quantities of the unknown constituent.

A simple method of separating the constituents in a mixture of liquids is to boil the mixture, and collect fractions of the condensed vapor. Each constituent will usually go off at a fairly definite temperature. This, in principle, was the method employed by Ramsay, and his assistant, Travers. They prepared to begin with, no less than 15 liters of *liquid* argon! "On distilling liquid argon, the first portions of the gas to boil off were found to be lighter than argon; and on allowing the liquid air to boil off slowly, heavier gases came off at last. It was easy to recognise these gases by help of the spectroscope, for the light gas, to which we gave the name *neon* or 'the new one,' when electrically excited emits a brilliant flame colored light; and one of the heavy gases, which we called *krypton* or 'the hidden one' is characterised by two brilliant lines, one in the yellow and one in the green part of the spectrum. The third gas, named *xenon* or 'the stranger' gives out a greenish-blue light, and is remarkable for a very complex spectrum in which blue lines are conspicuous." [10]

A trio, neon, xenon, krypton, added to helium and argon—making five new gases—and all in the atmosphere!

Further recognition came from the Chemical Society of London. They awarded Ramsay the Longstaff medal, given triennially to the Fellow of the Chemical Society who, in the opinion of the Council, has done the most to promote Chemical science by research. "If I may

[10] Ramsay, quoted by Letts.

say a word of disparagement," added Mr. Vernon Harcourt, the president, in presenting the medal, "it is"—and here we can see the twinkle in his eye—"that these elements (argon, helium, etc.) are hardly worthy of the position in which they are placed. If other elements were of the same unsociable character Chemistry would not exist."

Ramsay's studies on helium led him to ponder over this question: why is helium found in only minerals which contain uranium and thorium—substances which give rise to radio-active phenomena? Attempts to answer this led him into the field of radio-activity, with results which even surpassed his investigations on the inert gases of the atmosphere. In 1903, in conjunction with Soddy, he succeeded in proving that helium, an element, could be produced from radium, another element. The transmutation of the elements come to life again! Those poor, foolish old alchemists, we were always led to believe, wasted their lives in vain attempts to transmute the base metals into gold. And here comes the dashing Ramsay, bold, as usual, to audacity, and calmly announces that *his* experiments prove the alchemists not to have been such fools after all!

Succeeding experiments on the action of radium salts on copper and lead solutions led Ramsay to believe that copper and lead can undergo disintegration into sodium and lithium respectively—two entirely different elements! These latter claims still wait to be verified, but there is reasonable hope for assuming that various experimenters throughout the world will soon undertake the task of carefully repeating the entire work, now that peace is once again with us.[11]

A fitting award for these achievements was the bestowal of the Nobel Prize to Ramsay in 1904. The dis-

[11] See the article on Madame Curie.

tribution of the prizes took place in Stockholm on December 10th of that year, in the presence of King Oscar and the royal family, foreign ministers and members of the cabinet, and many leading representatives of science, art and literature. After speeches had been delivered by the vice-president and other representatives of the Nobel Committee, and of the Academies of Science, medicine and literature, King Oscar personally presented Lord Rayleigh (prize winner in physics), Sir William Ramsay[12] (chemistry) and Professor Pavloff (physiology) with their prizes, together with diplomas and gold medals.[13] The distribution of the prizes was followed by a banquet, at which the Crown Prince presided. Count Morner proposed the health of Professor Pavloff, Professor Petterson that of Sir William Ramsay, and Professor Hasselberg that of Lord Rayleigh. The following day Ramsay delivered a lecture on argon and helium at the Academy of Sciences, which was followed by a dinner given in his honor by King Oscar. Writing from Switzerland to a friend some weeks later Ramsay says: "We had a most gorgeous time for nearly a week, dining with all the celetrities, including old King Oscar. The old gentleman was very kindly and took Lord R. and me into his private room and showed us all his curiosities, the portraits of his sons when they were children and his reliques of Gustavus Adolphus and of Charles XII. The Crown Prince told Mag (his wife) that it was a difficult job to be a king, thereby confirming the Swan of Avon. He said that whatever one supposed a Norwegian would do he invariably did the opposite. Indeed there was nearly a bloodless revolution while

[12] Ramsay had been created a Knight Commander of the Bath (K.C.B.) in 1902, which carried with it the title of "Sir."

[13] The sum of money attached to each prize amounts to about $40,000.

we were there; the Prime Minister of Norway was there and I believe the dilemma was only postponed."

Ramsay remained at University College until 1912, when he retired. Two years prior to this, in conjunction with Dr. Gray, he determined the density of the emanation obtained from radium (which Ramsay named *niton*) involving the mastery of experimental detail which established him once for all as the great wizard of the laboratory. The total volume of the gas under examination was not much beyond 1/10 cubic millimeter—a bubble which can scarcely be seen. To weigh this amount at all accurately required a balance turning with a load not greater than 1/100,000 milligram.

When war broke out Ramsay placed his services at the disposal of the government. Much he could not do. In July, 1915, he writes to a friend that he had had several huge polypi extracted from his left nostril. "I have stood them for years, one gets into the habit of bearing discomforts, but it is a great relief." The relief was to be only temporary. Another operation became necessary in November. "I was in the surgeon's hands on November 10th and again on the 13th, and he did an operation on my left antrum for a tumor, I believe very successfully. Since then, last Monday, I was irradiated for 24 hrs. with X-rays as a precaution against recurrence. Luckily it is of the kind which can be stopped by Radium. I have had a very bad time." He died on July 23, 1916.

Ramsay had lived not a long life, but a very fruitful and happy one. Writing to president Ira Remsen, of Johns Hopkins, a few months before his death, Ramsay concludes his letter with "Well, I am tired, and must stop. I look back on my long friendship with you[14] as

[14] Dating back to the Tübingen days.

a very happy episode in a very happy life; for my life has been a very happy one."

Ramsay was many-sided. He was an excellent example of the very opposite of Punch's dry-as-dust philosopher. Among musicians[15] and among artists[16] he held his own, for he was an accomplished amateur in both groups. As a linguist he probably has had few equals among scientists. And those of us who, as late as 1912, heard him move a vote of thanks to Professor Gabriel Bertrand, of the Sorbonne, after the latter's lecture to the members of the International Congress of Chemists, will have formed a pretty good picture of his charm and ability as a speaker.

Of the many letters that have been preserved, perhaps none sums up so well the characteristics of Ramsay as the following, written to his friend, Dr. Dobbie:

"LE HAVRE,

"Monday, the Something or other August, 1877.

"*My dear Dobbie*,

"Some fool of a Frenchman has stolen all the paper belonging to the French Association, and has left only this half sheet with Le Havre at the top. From the preceding sentence you will have already guessed that the French Ass. is capering around Havre at present, that I form one of the distinguished foreign members, and

[15] "I spent many evenings at their home, where William (Ramsay) enlivened the company with songs, which in later years were greeted with enthusiastic applause by his students at social evenings of the University College Students' Club. . . . He had a very good voice, played his own accompaniments, and was an expert whistler."—Otto Hehner, a friend.

[16] "Another amusement of Ramsay's was sketching in water colors, an art in which he possessed no inconsiderable share of the talent which belongs to his cousins, Sir Andrew Ramsay's family."—Sir James Dobbie.

that all is going as merrily as a marriage bell. Voici 5 jours that I find myself here. I went to Paris with three spirits more wicked than myself, lawyers—a fearful compound 3 lawyers and a chemist—just like NCl_3 for all the world, liable to explode at any moment. . . . I have made the acquaintance with a whole lot of chemists, Dutch and French, and have found an old Dutchman named Gunning ravished to find someone who shares his ideas about *matter*, chemical combination, etc. We excurted yesterday the whole day and talked French and German alternately all the time. When we wanted to be particularly distinct French was all the go. For energy and strong denunciation German came of use. You can't say 'Potz-teufel!' in French or 'Donnerwetter potztausend sacramento!' An old cove, also a Dutchman, DeVrig, with bowly legs and a visage like this (sketch profile) is also a very nice old boy. The nose is the chief feature of resemblance in the annexed representation. Wurtz and Schukenberger are both Alsatians and of course are much more gemüthlich than the echter Franzose, but on the whole the fellows I have got to know are very pleasant. Some of the younger lot and I kneipe every evening. Then we bathe every day too in fine stormy water.[17] Eh bien, what is there to say of more? I am going straight back to Glasgow on Wednesday by the special steamer to

[17] "He (Ramsay) was a very strong and graceful swimmer and could dive further than any amateur I have seen. When we were in Paris in 1876 the four of us used to go to one of the baths in the Seine every forenoon, and after the first time, when Ramsay was ready to dive, the bathman would pass round the word that the Englishman was going to dive, and everyone in the establishment, including the washerwoman outside, would crowd in and take up positions to watch him. He dived the whole length of the bath and sometimes turned there under water and came back a part of the length."—H. B. Fyfe, a life-long friend.

Glasgow. My money is about done, so I must bolt. . . . By the way I forgot to tell you that I had the cheek to read a communication on picoline, in French, which was received with loud applause. There was some remarks made afterwards very favorable, tho' I say it as shouldn't say it. Adoo. Write to Glasgow and tell me Wie's Geht. "Yours very Sincerely,

"W. RAMSAY."

References

For much of the material I am indebted to Tilden's life of Ramsay (1). A fine appreciation of Ramsay at his prime is given by Ostwald (2). Soddy's (3) is a lovely tribute by a gifted writer. T. C. Chaudhuri (4) is responsible for an appreciative little memoir, full of oriental coloring. Ramsay's two books (5, 6) deal with the gases of the atmosphere and radium.

1. Sir W. A. Tilden: Sir William Ramsay (Macmillan and Co. 1918).
2. Wilhelm Ostwald: Sir William Ramsay. *Nature* (London), *88*, 339 (1912).
3. Frederick Soddy: Sir William Ramsay. *Nature* (London), *97*, 482 (1916).
4. T. C. Chaudhuri: Sir William Ramsay (Butterworth and Co., India. 1918).
5. William Ramsay: The Gases of the Atmosphere (Macmillan and Co. 1902).
6. William Ramsay: Essays Biographical and Chemical (Constable and Co., London. 1908). (See the chapter on radium and its products.)

THEODORE WILLIAM RICHARDS

DURING the latter half of the nineteenth century William T. Richards rose to a position of prominence among American artists. His paintings of landscape, particularly his interpretations of the varying aspects of the ocean beating upon beach and rock, won high praise and eventually earned for him the gold medal of the Pennsylvania Academy of Fine Arts. His wife, Anna Matlock, whom he married in 1856 when some twenty-odd years old, was like her husband, a woman of artistic talent, though in her case it showed itself in the publication of verse. Of their six children, one of whom, Herbert Maule, is to-day a professor of botany at Barnard College, and two others, Mrs. Eleanor French Price and Mrs. Wm. Tenney Brewster are painters, we are particularly interested in the fourth, Theodore William, who was born in the house of his grandfather, Dr. Charles F. Matlock, in Germantown, Philadelphia, on Jan. 31, 1868.

The family were in very comfortable circumstances. In addition to their home in Germantown they had a summer one in Newport, and occasionally they would forsake both for extensive travels in Europe.

The poor schools in Pennsylvania at that time, as well as the uncertainty of the family's stay at any one place for any length of time, made it necessary for the children to receive privately their most elementary education. For this task Mrs. Richards was eminently well fitted. Young Theodore gradually passed from "Alice" to history and languages, and with little effort quickly overtook his playmates who attended school.

Naturally the boy's first desire was to become an artist. Was not his father the greatest of men, and could a son of his do less than follow in his footsteps? Filial reverence lost none of its force with time, but a desire to paint, slowly and quite unconsciously, gave place to a desire to become a scientist. This showed itself even before he was thirteen.

The query naturally suggests itself, what started him on this track? His mother and father, aside from art, were very much interested in Tennyson and Browning, and literature in general. An intimate friend of the family's was Frank R. Stockton, the author. From none of these three could Theodore have obtained much scientific inspiration.

There remained then his grandfather, the doctor, and still another close friend of the family's—Josiah Parsons Cooke, Professor of Chemistry at Harvard. That the boy got much of his inspiration from this Harvard professor seems pretty certain. Even before he entered Harvard Young Richards had already mastered Cooke's *The New Chemistry*, and was quite a match for many of the students with several years' chemistry to their credit.

Genius young Richards could well have inherited, in part at least, from his parents; the bent of this genius towards science must to a certain extent be credited to Cooke; but the further quality of taking infinite pains with details, so essential to every scientist, and one which Richards possesses in a supreme degree, seems to have been directly transmitted from father to son. Note this description of the artist: "He stood for hours in the early days of Atlantic City or Cape May with folded arms, studying the motions of the sea—until people thought him insane. After days of gazing, he made pencil notes of the action of the water. He even stood for hours in a bathing suit among the waves, trying to analyse the motion."

Yet still another inheritance. What soon strikes a reader in glancing over Richards' contributions to chemistry is the fine unity of purpose which pervades all his work: a desire to penetrate ever deeper into the mysteries of creation. This philosophical bent may be traced to his mother, whose verses abound with fine feeling and deep thought.

Richards, barely fifteen, entered Haverford College, Pennsylvania, with this advice from his mother in his pocket:

> Fear not to go where fearless Science leads,
> Who holds the keys of God.

At Haverford, aided by a retentive memory and a desire for knowledge, Richards made rapid strides, particularly in chemistry and astronomy. But he was not a bookworm; though somewhat delicate in physique, with eyes that needed careful nursing, he took an active part in the less strenuous exercises such as lawn tennis, skating and swimming.

But Cooke was not at Haverford, and Richards wanted Cooke. He wanted him badly now because he, Richards, also wanted to be a chemist, and because he, like Cooke, was particularly interested in the philosophy of chemistry. Then there were other men at Harvard whose acquaintance Richards was anxious to make. Wolcot Gibbs, C. L. Jackson, and H. B. Hill were men who counted in chemical councils of the day.

Richards, then, wanted to complete his bachelor's degree at Harvard. The reasons he gave for desiring to change were quite sufficient for his parents. They understood and encouraged, as they continued to do to the end of their days. Their motto from the first was: give him the best that's in you, but let nature play its part; guide much, but force nothing. So Richards set out for Cambridge, there to join the senior class.

In the following year (1886) Richards splendidly justified the cherished hopes of his parents by graduating with *summa cum laude* and highest honors in chemistry. There could be no further question as to his future. He had made a brilliant start in chemistry, and chemistry it was to be.

When one considers the extent to which research in America is carried to-day it comes as a surprise to learn that even as late as 1880 very few research investigators were to be found at any one of the colleges. At Harvard, for example, although the Erving Professorship of Chemistry had been founded as early as 1792, Josiah Parsons Cooke (1827–94) was the first occupant of the chair to take any real interest in investigations. These led to problems dealing with the combining proportions of elements to form compounds.

Combining proportions of elements is glibly enough discussed by every high school boy, but Cooke could penetrate much below the surface of things, and Cooke led his students on his own philosophic path. Needless to add, Richards was one of the enthusiastic followers.

Under Cooke's guidance Richards began an investigation of the atomic weight of oxygen. [See the article on Mendeléeff for the meaning of atomic weights.]

Richards soon showed that the accepted atomic weight for oxygen was too high. But more than that: the method of procedure had elements of novelty, and the extraordinary care taken to avoid errors in manipulation centred attention upon the work.

The use of copper oxide in the determination of the atomic weight of oxygen made it most desirable to be certain of the purity of this substance. Its somewhat anomalous behavior led the young investigator to question the accuracy of the accepted atomic weight of copper, and by a careful investigation of the matter, in the course of which he showed that the copper oxide which previous investigators had used contained nitrogen as an impurity, Richards came to the conclusion that the atomic weight of copper as given by other investigators was too high. The differences to be sure were fractions of one percent, but they were entirely beyond all possibilities of experimental error.

These two researches were conducted before Richards reached his twentieth year. Two results immediately followed therefrom: the boy Richards had become a force to be reckoned with, and he had discovered just that particular department of the science for which he was best fitted.

In 1888, at the age of twenty, Richards received his Ph.D. "Before this, the greatest wish of my life had begun to develop—namely, an intense desire to know something more definite about the material and energetic structure of the universe in which our lot is cast. Advancement in academic position, although prized because necessary in order that a normal life should be possible, was subordinate to this great interest. At first perhaps my desire began as a feeling little above mere curiosity, but by degrees I realized that gain in knowledge would mean for humanity gain in power, which I thought of primarily as gain in power for good. By instinct and education, although not by formal connection, I was of the Society of Friends (or Quakers), in whose minds peace and goodwill to men were foremost; and I dwelt little upon the sinister uses to which the increased power found by science could be put. . . . It is not the fault of science if mankind is so little civilized as to misuse its great potential benefits. . . ."

"The atomic weights seem to be among the primal mysteries of the universe. They are values which no man by taking thought can change; they seem to be independent of place and time. They are silent witnesses of the very beginnings of things, and their half-hidden, half-disclosed numerical relations, in connection with the undoubted similarities in chemical properties of certain groups of elements, only increase one's curiosity concerning them. . . ."

We see here clearly enough that even thus early in life atomic weight determinations to Richards were a means and not an end. To get finally at fundamentals required in the meantime years of patient labor, ingenuity and skill.

Richards, of course, was not the pioneer in atomic weight determinations. From the time of Dalton more than one hundred years ago, many workers had pointed out their significance. Prominent among these were Avogadro and Cannizzaro, two Italian scientists; Berzelius, a Swede; and Stas a Belgian. The classification of the elements based on their atomic weights resulted in Mendeléeff's *Periodic Law*, which in turn gave rise to much further experimental work to explain apparent inconsistencies in the then accepted atomic weights. Mendeléeff's *Law* also offered food for much reflection. Why could the weights of the elements be so arranged as to exhibit at a glance the close chemical and physical relationship of many of them? Was this relation due to their origin from some parent substance?

Reflections such as these led Richards to the view that an answer to such a question could be obtained only by a much more careful examination of properties of the elements, and among these, atomic weight stood first on the list.[1]

[1] Recently (1913–1914) Mosely, an English physicist, by studying the high-frequency spectra emitted by different elements when used

The great promise he had shown, and the hearty support which he received from Cooke, enabled Richards to secure one of those valuable Harvard Travelling Fellowships, and during 1888–89 he spent much of the time at Göttingen, where he became acquainted with Victor Meyer and his vapor density method, Walter Hempel and his gas manipulations, and worked directly with Paul Jannasch on the estimation of oil of vitriol in the presence of iron. On his way home he stayed in England long enough to form friendships which were to prove life-long.

What Richards got from his travels abroad is much what the young graduate gets by attending large scientific gatherings; he saw in flesh and blood men whose fame had reached him, he was introduced to some of them, and caught their enthusiasm and lofty vision.

On his return to Harvard Richards was appointed to an assistantship, and two years later he became an instructor. Needless to add, the interrupted work on

as targets in an X-ray bulb, has shown "that there is in the atom a fundamental quantity, which increases by regular steps as we pass from one element to the next. This quantity can only be the charge on the central positive nucleus."—Mosely, quoted by Lowry, *Historical Introduction to Chemistry*, p. 493, 1915. (See also the article on Madame Curie.) Mosely's "quantities," the "atomic numbers," are the source of much scientific activity at present. Of Mosely, the author of these "atomic numbers," who was killed in the Great War, Prof. R. A. Millikan, the distinguished physicist of the University of Chicago, has this to say: "In a research which is destined to rank as one of the dozen most brilliant in conception, skilful in execution, and illuminating in results in the history of science, a young man but twenty-six years old threw open the windows through which we can now glimpse the subatomic world with a definiteness and certainty never even dreamt of before. Had the European war had no other result than the snuffing out of this young life, that alone would make it one of the most hideous and most irreparable crimes in history."

atomic weights was resumed with vigor. Some finishing touches which he gave to his copper work, in the course of which barium in the shape of one of its salts had to be used, pointed to the next line of attack. His results led him to the view that the atomic weight of barium was even less well known than that of copper had been.

We see that the elements were never selected at random, but like most careful and thoughtful work, one experiment led to another, and each succeeding experiment showed elaborate improvements over its predecessor. Thus in this barium determination Richards first carefully chose a compound of the element which could be easily prepared in the pure state, which could be dried without decomposition, and which could be readily analysed. The compound once selected, it was now prepared in no less than seven different ways, and each one was found to have the same composition. Such was the accuracy of the procedure that two of the results for the atomic weight of barium differed by no more than one six-thousandth of an ounce, and these were shown to vary markedly with the value then in vogue.

The errors which other experimenters had fallen into with their barium determinations made it more than probable that those errors had been repeated with strontium, an element chemically very closely allied to barium. Such, indeed, proved the case; and here, as before, new figures were given and the old errors explained.

In this strontium experiment Richards set a record for exact methods of procedure which have never been surpassed, and which formed the basis for most of his subsequent work on atomic weights. Here, also, by the introduction of his bottling device, which gave assurance that purified materials could be kept uncontaminated with any moisture, and the use of the *nephelometer*, which detected minute traces of suspended material, "two errors were obviated . . . which have perhaps ruined more previous investigations than any other two causes. . . ."

The standards which Richards has set for his work are summed up in this remark of his: "Every substance must be assumed to be impure, every reaction must be assumed to be incomplete, every measurement must be assumed to contain error, until proof to the contrary can be obtained."

Such merit could not go unrewarded; in 1894 Richards was promoted to an assistant professorship. In the following year the fame of Ostwald's school at Leipzig, and the desire to become more proficient in physical chemistry, a science which he clearly foresaw he would use extensively, led him once again to Germany, and here he remained for a semester. Not long after his return Richards married Miss Miriam Stuart Thayer, the daughter of Professor J. H. Thayer, the New Testament scholar. They have a daughter and two sons.

Fame Richards had already attained, but there was a danger in another direction. Aside from his salary, Richards had nothing, and the salary was too small for a man with family. Passionately interested as he was in research, Richards realized only too clearly that it was not a "money-getting-employment." "Money-getting" meant weary hours of labor, and such occupation could hardly be engaged in, side by side with research, without impairing either the one, or the other, or, what is worse, one's health. At this critical hour the father stepped in:

"My father . . . advised me to devote myself . . . to research . . . he supported this advice in a very practical way and offered . . . to help me, out of his none too plentiful means, in case of a pinch, rather than permit me to engage in the distracting task of making money by occupations outside of my main interest. Later, after my marriage in 1896, when new cares presented themselves, and when he saw that there was danger of my overworking, he placed into my hands a sum of money large enough to enable me to feel that I could take a year's rest from academic work, if that should prove necessary. The relief from worry, afforded by this sum in a savings bank, made the vacation unnecessary."

"There is no question that this generous and thoughtful confidence was a very important factor in the success of a not very optimistic and somewhat delicate young man, then entirely without any capital except his brains; and it would be impossible to exaggerate my feeling of gratitude. My wife also heartily sympathised with my desire to conduct investigation, and did all in her power to encourage the work."

Encouraged in this way, Richards threw himself into his work with a wholeheartedness and enthusiasm which knew no bounds. Step by step, with one research giving rise to another, he redetermined the atomic weights of such elements as zinc, magnesium, nickel, cobalt, iron, silver, carbon, nitrogen, etc., and in each case the figures he obtained showed differences with those obtained by other workers, many of whom were masters in the field. These differences were shown to be the necessary result of various inaccuracies which other men had fallen into,—inaccuracies, in many cases, due to a lack of knowledge of certain very necessary physico-chemical principles. As showing the uniform excellency of Richards' work it may be pointed out that in every instance the consensus of scientific opinion has been overwhelmingly in favor of his results. "One's confidence in the work," writes Richards, "cannot but be increased by the fact that in spite of the many years which have passed since some of the work was done [this was written in 1910], not one of these values has been shown to be seriously in error, and in every case the Harvard value has been accepted by the International Committee on Atomic Weights and by the world at large as more accurate than previous work of others."

Much of his earlier work appeared in the *Proceedings of the American Academy of Arts and Sciences*, but with the growth of the American Chemical Society, and the consequent growth of its *Journal*, many of the more recent papers have found their way into this *Journal*. Some have been reprinted by the *Carnegie Institution* of Washington, an organisation which, by its financial assistance, has made much of the work possible. A volume embracing all of Richards' papers up to 1909 was published in German under the title, *Untersuchungen über Atomgewichte*.

The extent of these researches has necessitated the assistance of many students. These flocked to Harvard in large numbers. As early as 1895, when Richards was but 27, students began to work under his direction, and their number has steadily grown until to-day there is quite a little army of them. Some of them, such as G. N. Lewis, L. J. Henderson, Grinnel Jones, Baxter and Cushman, are already among the very best chemists of America.

In 1901 Richards was appointed to a full professorship at Harvard. This came after his declination of an offer from the authorities at the University of Göttingen, Germany, which showed how far his fame even then had travelled. Two years later he was made chairman of the department, and in 1907, in fulfilment of arrangements which had been entered into between Harvard and the

German Government, Richard was selected as Exchange Professor at Berlin University for that current year, and during his brief stay there he introduced some of his classical experimental methods into German laboritories.

Before his departure from Berlin, Richards delivered an address to the members of the German Chemical Society. From a description in the *Chemiker-Zeitung* we gather that the big amphitheatre in the *Hofmannhaus* (the headquarters of the Society) was filled to overflowing, " scholars from every part of the country being attracted." Among the audience were such well-known chemists and physicists as Graebe (the president), Emil Fischer, Landolt, Nernst, Lampe, Brauner, Liebermann, Buchner, Planck, Pinner, Ladenburg, Gabriel, Witt, Bernthsen, Warburg and Biltz. Richards' address, dealing with his later researches on atomic weights, was received with much enthusiasm (" Der Vortrag wurde mit ausserordentlichem Beifall aufgennomen "), and the president in his comments, declared that the two foremost authorities on atomic weights in the last hundred years, Berzelius and Stas, now gave way to Richards. " The light, which before radiated from Europe to America, is now brilliantly reflected back again."

It has been emphasised that Richards' atomic weight determinations were merely a means, and that the end in view was a deeper knowledge of fundamentals. This led him to investigate other properties of the elements besides weight, such as compressibility, melting point, etc. The development of a theory which assumed that atoms, and not merely the spaces between them, are compressible has borne wonderful fruit, and has splendidly correlated many properties of matter. " In developing this theory, I endeavoured always to avoid confounding hypothetical inferences with reality, trying

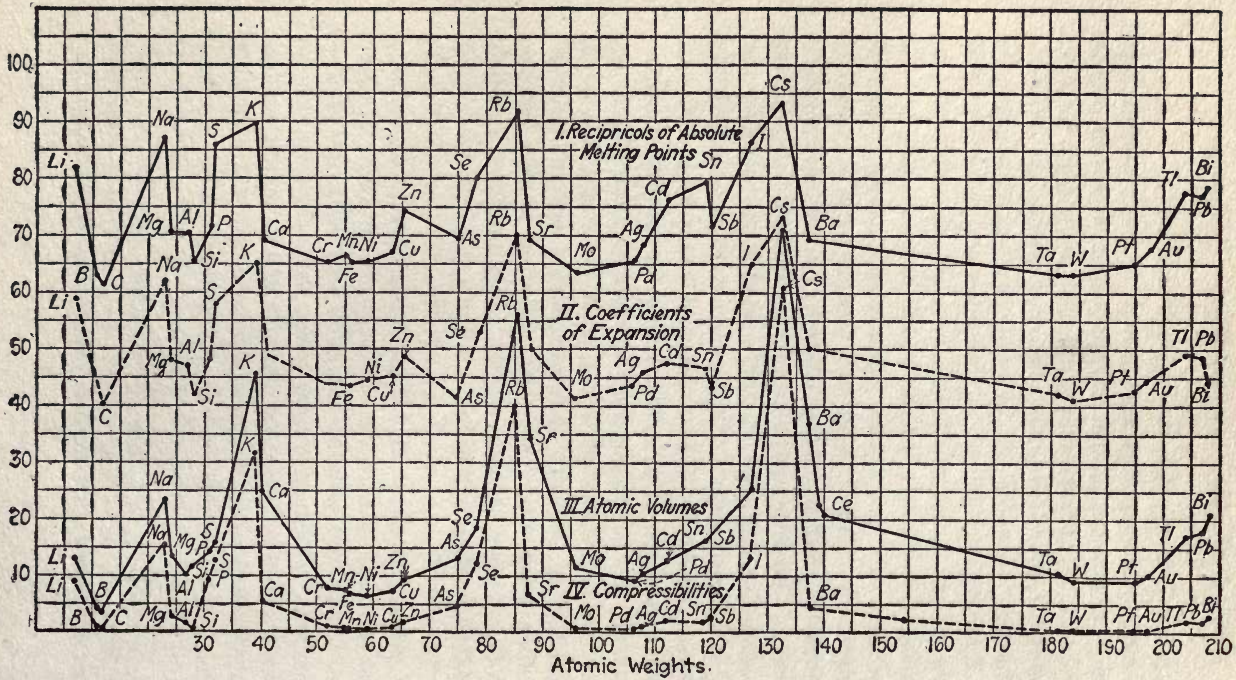


Chart prepared by Richards to show the relation of the atomic weights of the elements to some of their other properties. Notice the similarity in the shape of the curves. [Reproduced from the *Journal of the American Chemical Society.*]

to follow in the footsteps of Michael Faraday, who always distinguished between the dreams and the facts." How well various properties of elements are correlated is graphically represented on the opposite page, the curves being a reproduction from one of Richards' most recent papers.

Even a casual glance at these curves will answer the few critics, quite ill-informed as to the nature of the work, who, though readily admitting Richards' extraordinary skill in technique, claim that it shows no striking originality. We have heard similar remarks made of Richards' illustrious co-worker at the Harvard Medical School, Otto Folin. Folin has devoted much of his time to the improvement of the quantitative methods employed in urine, and later, in blood analysis. Aside from having shown how unsatisfactory many of the quantitative methods previously used are, and, as a consequence, how worthless are all the conclusions of a chemical nature drawn from them, Folin has been led, among other things, to his beautiful theory of protein metabolism, which is the very cornerstone of clinical teaching to-day. Folin's improvement of quantitative methods had all these possibilities in mind.

Precisely the same is true of Richards' improvements of atomic weight determinations. Quantity, through Lavoisier, laid the basis of our modern science of chemistry, and the greater the refinements in quantitative methods the greater the progress. In Richards we have not only a master of quantitative manipulation, but a master interpreter of these procedures, and it is the combination which makes him a great master in our field.[2]

[2] As showing how quite unexpected practical applications may result from work of scientific interest only, the following may be cited: copper ore is purchased upon a metal value, established by chemical analysis, a value based upon the weight of copper atoms

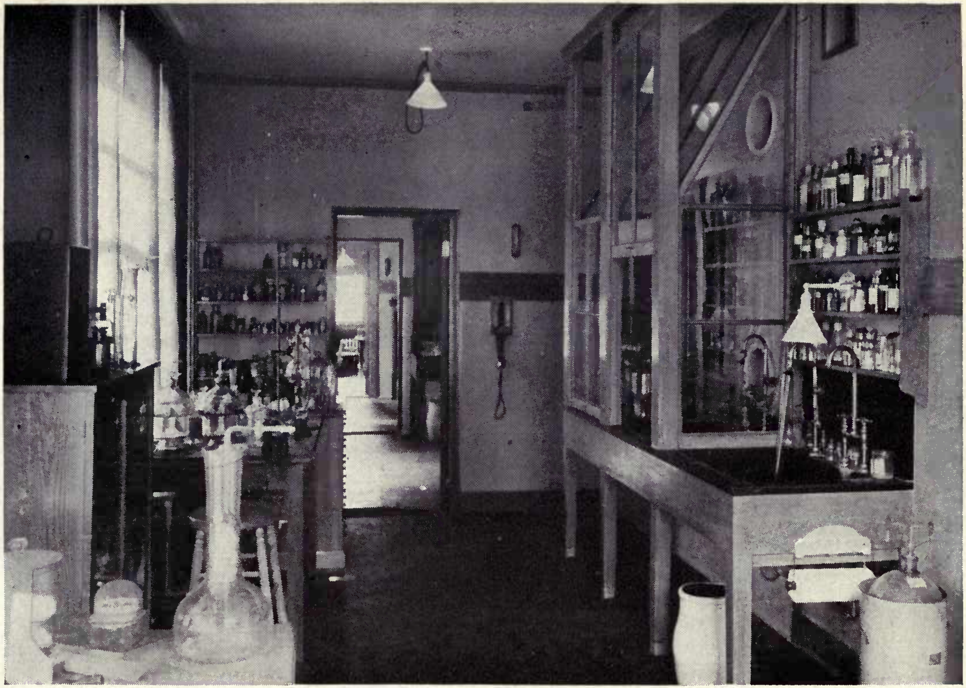
In 1911 Richards was presented with the Faraday Medal of the English Chemical Society, and on this occasion delivered an address *The Fundamental Properties of the Elements*, which is one of the most stimulating the present writer has ever read. Of the impression it made on its hearers, Prof. Dixon's opinion may be quoted:[3] "We have listened to-night to a story that is more entrancing than any fairy tale, because as we followed the flight of the lecturer's imagination, we knew that that flight was surely guided and controlled by a man who has measured and weighed the elements with an accuracy hitherto unknown. Concerning the weights of the element, our old European ideas of finality have been overthrown by Professor Richards and his school, and we are at this moment seeing the fulfilment of the prophecy of Canning when he said, 'I look to the new world to redress the balance of the old."'

The following year Richards was appointed to the Erving Professorship of Chemistry and made Director of the Wolcott Gibbs Memorial Laboratory, a post which he still holds.

This Wolcott Gibbs Laboratory, which was completed in 1913, and which is devoted exclusively to research in physical and inorganic chemistry, was named after one of Harvard's professors of chemistry. Its erection was made possible through the generosity of the late Professor Morris Loeb, himself a pupil of Wolcott Gibbs.

in the ore. Until the Harvard experimental results were announced this atomic weight was represented as 63.2; whereas the experiments showed the figure to be 63.6. Evidently this difference of two-fifths of one percent means an increase in value to the seller of about $4,000 on one million dollars' worth of ore.

[3] Dixon is professor of chemistry at the University of Manchester, and one of the past presidents of the English Chemical Society.



One of the rooms in the Wolcott Gibbs Laboratory, Harvard. [Reproduced from the *Harvard Alumni Bulletin.*]

For the type of work in which Richards is engaged the Gibbs Laboratory is probably the best equipped in the world.

The building has six floors available for work: three regular stories, a very light and convenient basement, a sub-basement for especially constant temperature work entirely underground, and a practicable roof. It contains no lecture rooms but is divided into many rooms of small sizes, the majority of them intended for one or two investigators. Balance rooms,[4] dark rooms, rooms designed for chemical and physical laboratories (because much of the work lies on the border-line of physics and chemistry), and other prerequisites for accurate experimentation, abound. Pipes are laid for hot and cold water, distilled water, steam, compressed air, oxygen, and vacuum, as well as for gas; and electricity of many voltages is available at suitable plugs throughout. An automatic electric lift is used for transferring the apparatus, and telephones connect all the important rooms.

Hollow bricks and doubly glazed windows with tight weather-strips protect the building from heat and cold, and the temperature of almost every room is automatically regulated. The ventillating plant provides filtered air, hence the building is extraordinarily free from dust throughout.

But we have yet to tell of Richards' greatest triumph, a direct result of his atomic weight determinations. In the spring of 1914 Richards startled the scientific world

[4] The balances weigh accurately one forty-millionth part of an ounce. With their aid it is possible to weigh a short light mark made by a lead pencil. The material is weighed in a platinum receptacle which is carefully regulated to the temperature of the rest of the balance, otherwise an ascending current of air would be generated if the crucible were even slightly warmer, making it lighter on the balance. The balance is confined in a glass case containing dried air.

by his announcement that lead obtained from radioactive minerals has a lower atomic weight than the lead obtained from any other source.

A little reflection is needed to appreciate the full significance of this statement. Until then no case of variation in the atomic weight of an element had ever been shown. Copper, silver, iron, etc., had been obtained from various ores in different parts of the world, and many thousands of analyses had been run by many hundreds of investigators everywhere, yet the atomic weight of each element remained a fixed number. Wherever variations arose, these were invariably traced to inaccuracies in experimentation; and indeed a fixed tenet in the faith of every chemist became that the atomic weights of the elements are unalterable.

But radioactivity came to shake this faith, as it has shaken the faith of so many other scientific beliefs. Who was to settle such a question if not the master of atomic weight determinations? Ramsay and Soddy in England, and Fajans and Bredig in Germany, urged Richards to undertake this work. Fajans sent his assistant, Max Lembert, with several valuable samples of radio-active ores containing lead, to assist in the research.

Radioactive ores from Ceylon, from Colorado, from England, from Bohemia, from Norway, were carefully purified, and the atomic weight of the lead present determined with all the extraordinary refinements that his brother workers expected of Richards. The mean of many results gave the value of 206.6 for the atomic weight of radioactive lead, as compared to 207.2 for common lead—a difference small enough, but altogether beyond any experimental error. The most amazing feature of the whole situation was that, outside of this difference in atomic weight, and, therefore, density, the two varieties of lead were exactly the same in all respects, physically and chemically.

Now Rutherford and Soddy had worked out a theory of radioactive disintegration by which, starting with uranium, that element broke down in stages into a number of other elements, the last of which was lead. From this hypothesis the theoretical atomic weight for lead could be deduced. This was found to be 206.07. Richards' experimental figure was 206.08,—a difference then of one one-hundredth, and a percentage difference of about one two-thousandth. Never in the history of science was there a more complete agreement between theory and fact.

This had its award in the Nobel Prize which came to him in that year (1914). In 1916 Richards was awarded the Franklin Medal of the Franklin Institute, Philadelphia, founded for the recognition of those workers in physical science or technology, without regard to country, whose efforts, in the opinion of the Institute, have done most to advance a knowledge of physical science or its applications.

In addition to these awards, Richards has been the recipient of many other honors. At various times different universities—Yale, Harvard, Cambridge, Oxford, Manchester, Prag, Christiania, Haverford, Pittsburgh, Clark and Berlin—have granted him honorary degrees. In 1910, the London Royal Society bestowed its Davy Medal upon him, and in 1912 he received the Willard Gibbs Medal of the American Chemical Society. He has been twice elected to the presidency of the American Chemical Society. In 1917 he was elected President of the American Association for the Advancement of Science for that year. Recently (May, 1919) he was nominated for the presidency of the American Academy of Arts and Sciences. He is a member of most of the scientific organisations of Europe and America.

Here is a reporter's description of the man and his surroundings: "You find the offices of the director on the second floor. Presently the door of the inner room opens and you hear the conclusion of a little conference. . . . There are some remarks about 'the determination of *Q* and the elimination of that error,' and then you are invited into the private apartment of Professor Theodore William Richards. . . ."

"The room is large and cheerful and the visitor is slightly surprised to note that it contains few tokens of the laboratory work to which the building is dedicated. . . . The eye catches at once an artistic portrait upon the wall of a chemist at work with his retorts and tubes, and inquiry secures the information that this is a photograph of a Burne-Jones painting of [the late] Lord Raleigh, the Chancellor of Cambridge University [and the renowned physicist]. Above the mantle stands a portrait of Michael Faraday.

"The visitor expresses some surprise as he notes also that several water-color drawings adorn the room. 'Is there any reason why such a room should be devoid of beauty?' asks the Director, and later you learn that Prof. Richards himself likes to sketch. . . . Two of the water colors are the work of his father, one a scene at Monhegan, the other a view of rocks, shale and waves at Newport.

"Meantime you have been studying the man himself. He is of medium height, sturdily made, with grey hair, eyes that look keenly through his glasses, and a genial manner. His face is oval, the smile comes readily—he confesses to a feeling of humor, as might be surmised from the twinkle that frequently is caught lurking in his eyes—and the movements are quick and definite. The general impression is that of a business man with many affairs pressing upon his attention rather than that fancy which most persons have of a chemist working with minute and patient care upon some scientific problem."

And now let Prof. Richards act as autobiographer: "Although I have been able to accomplish only a very small part of that which has been planned, the work has interested the chemical world beyond all expectation; indeed the possibility of much outside interest had not been anticipated. . . . The splendid Nobel Prize (which has grown to be world-renowned above all other forms of recognition, not only because of its magnificence [some $40,000 go with it] but also because of the list of great men whose names grace its earlier records), gave pleasure which it is impossible to exaggerate.

"The award will be a lively inspiration to try to do better work in the future, and, moreover, its provisions will help to smooth the way toward more accomplishment, both by providing help for the present, and by relieving worry for the years to come.

"All those marks of kindness and generosity on the part of one's friends and colleagues bring great satisfaction and happiness; but they cause also a sense of humility and responsibility. One cannot help wishing that one's incomplete attainments, so richly rewarded, came nearer to the ideal; and one cannot help feeling that he must strive doubly hard in the future to be worthy of having received such great tokens of confidence and honor."

Richards, together with his students, has thus far published some 200 papers—the results of research. Many of them have become classics in our science. Yet Richards is very little over fifty to-day. What may we not expect in the years to come!

References

Part of the information comes from private sources. Morris's life of Richards' father (1) gives us a picture of the family. Richards himself is responsible for a delightful autobiographical sketch of his early days, prepared at the request of the editor of the Swedish *Vecko-Journalen* (2). An unusually well-informed newspaper account of Prof. Richards and his work appeared in the Boston *Sunday Herald* in 1915 (3). A description of the Wolcott Gibbs Memorial Laboratory appeared in the *Harvard Alumni Bulletin* for 1913 (4). Excellent summaries of Prof. Richards' work may be found in *Science* for 1915 (5), 1916 (6) and 1919 (7), and in the *English Chemical Journal* (8).

1. H. S. Morris: William T. Richards. A Brief Outline of his Life and Art (J. B. Lippincott Co., Philadelphia, 1912).
2. T. W. Richards: Retrospect. *Vecko Journalen* (Stockholm), Feb. 20, 1916.
3. Aonn.: Professor Richards Wins Nobel Prize. *The Sunday Herald* (Boston), Nov. 21, 1915.
4. T. W. Richards: The Wolcott Gibbs Memorial Laboratory. *Harvard Alumni Bulletin*, March 26, 1913.
5. T. W. Richards: Recent Researches in the Wolcott Gibbs Memorial Laboratory of Harvard University. *Science*, Dec. 17, 1915.
6. T. W. Richards: Ideals of Chemical Investigation. *Science*, July 14, 1916.
7. T. W. Richards: The Problem of Radioactive Lead. *Science*, Jan. 3, 1919.
8. T. W. Richards: The Fundamental Properties of the Elements (Faraday Lecture). *Journal of the Chemical Society* (London), *97*, 1201 (1911).

JACOBUS HENRICUS VAN'T HOFF

YOU have two substances: they both have the same atoms, the same number of atoms, in the same proportion by weight. So far as you can make out, they both have the same structural formula. Yet they show decided differences in properties. They have different crystalline structures and different optical properties, for example. What are we to make of this?

Such was Pasteur's problem with his famous tartaric acids. Such was Wislicenus's difficulty with his lactic acids. Structural formulas, as written on paper—in *two* dimensions therefore—failed utterly to show any differences in these compounds.

Now, of course, it did not require any very keen insight on the part of Pasteur, Wislicenus, and others, to realise that real molecules occupy not *two* but *three* dimensions, and that at best, paper formulas were a useful, but not a real mode of representation. Were the differences in these compounds to be ascribed to differences in the internal structure of the molecule, and if so, was there any possible method of showing this?

The twenty-two-year-old van't Hoff, already dissatisfied with these paper pictures, and pondering over the more profound question as to the possible way in which the atoms themselves are held together in the molecule, introduced the conception of molecular structure based on the tetrahedron, and with it gave an impetus to the development of organic chemistry which is felt with added force from day to day. One need but mention the carbohydrates and proteins to realise how much we owe the knowledge of the chemistry of these substances to van't Hoff's new branch of the science—*stereochemistry*.[1]

But stereochemistry was simply a branch development, as it were, of the main inquiry which van't Hoff set about to solve: the kinetics of chemical action. In any chemical reaction we see the beginning, and we see the end of the reaction—we seem to know little or nothing of the steps in between. What may they be, and if so, what laws govern them? What of the velocity of chemical reactions and of the various phases of chemical equilibrium?

These reflections gave rise to one of the most remarkable books in the whole realm of chemistry—van't Hoff's *Chemical Dynamics*, in which the application of pure mathematics to chemistry finds one of its first and clearest expressions.

And this study culminated in one of the great generalisations in the science—the analogy between substances in solution and those in a gaseous form.

Van't Hof, Vant hof, Vant hoff, vant hof, van't Hof, van't Hoff—so run the pleasant little variations in name from 1600 on. In the middle of the nineteenth century a worthy scion of this well-known Dutch family, accompanied by his young wife, transferred his medical practise from the little town of Sommelsdijk to the flourishing city of Rotterdam, and in August, 1852, Alida Jacoba van't Hoff gave birth to Jacobus Henricus, Jr., destined to become the master chemical thinker of our generation.

Henry's early days alternated between attendance at Kindergarden and pleasant vacations spent with his grandparents at Middleharnis, made famous by Hob-

[1] The Frenchman Le Bel, quite independently, and only a month or two after van't Hoff's article appeared in print, advanced practically the same stereometric conception.

bema's picture of the place. The kindergarten was followed by the elementary school, and this in turn by the "Hoogere Burgerschool," where Henry achieved a reputation for scholarship, for speculation and for day-dreaming.

At the secondary school van't Hoff first received instruction in chemistry, and as with many another beginner, the excitement of cutting and bending glass, preparing, collecting and examining gases, and possible explosions of all kinds, led the youngster to repeat and extend many of the "stunts" at home. The parents and friends were not exactly invited to these exhibitions, for the practical young Dutchman declared that rich feasts should be paid for! And paid for they were. With the money collected, more apparatus was bought, and more bombing expeditions were undertaken.

In 1869, at the age of 17, he matriculated at Leyden University, with the following result: mathematics and mechanics, excellent; physical sciences, very good; history, civics and economics, good; languages and literature, fair; drawing, fair—altogether not a bad comparative estimate of his knowledge in later years.

But what was he to do now? His own tastes led him to entomology and to literature, neither of which seemed practical enough, however, to the young Dutchman. After much family discussion it was decided that Henry proceed immediately to the Delft Polytechnic school, there to equip himself as an engineer. Once a successful engineer and a local celebrity it would be easy to return to his first loves.

To Delft went young Henry, then, and with a determination to do or die, he at once plunged into the work before him. For the next two years he knew little of companionship and outside pleasures. The work for the greater part was distinctly a "grind." He graduated in 1871.

In the meantime two things had happened which made him question the desirability of pursuing a technical career. He had spent one of his vacations working in a sugar factory, and found much of this work distinctly monotonous. Was this to be his life work? The thought made him shudder a little.

And there was still another factor. Oudeman's chemistry lectures had made a very deep impression on him. Oudeman was an excellent speculator in his subject, and as we can now readily understand, such a man was precisely the kind of inspiration van't Hoff needed.

After finishing his course at Delft, Henry pursuaded his parents to allow him to continue his studies at Leyden, with the particular object of rounding out his mathematical knowledge. He had now quite decided to become a chemist. What, then, had mathematics to do with it—mathematics, to prepare for a chemical career in the seventies? At this point one does not know whom to credit more with the instinct of prophecy: his teacher Oudeman, or Henry himself. Of this we are certain: that even at this early age van't Hoff was quite dissatisfied with the purely descriptive state of chemical knowledge. To be encyclopedic only might be bookwormish, but surely not scientific.

At the end of a year Leyden grew monotonous. He had gained some mathematics, but little chemistry. To Bonn, then, where reigned the illustrious Kekulé, the founder of the theory of the benzene ring, and the speculator of his day.

"In Leyden everything was prose—the surroundings, the city, the people. In Bonn all was poetry." So wrote van't Hoff many years later. Was this due to Kekulé's influence? To some extent, no doubt. But there were other factors. Perhaps a closer examination of the man will enlighten us.

Van't Hoff, to be sure, had always been extremely industrious, and had had little leisure—or inclination, for that matter—to romp with acquaintances; but the time that he did have was largely passed in a world within. He speculated, he dreamt, he romaniticised. Comptes and Whewall and Taine gave him basis for speculation, and Burns, Heine and, above all, Byron, for his romanticism. To the end of his day Byron remained his god, and much of van't Hoff's early life and thought were modelled after that of the poet. Had not Byron declared that Burton's *Anatomy of Melancholy* was one of the most instructive books that had ever been written? Forthwith does van't Hoff plunge into Burton, with results that are obvious during his student days at least. Does not Byron tell us that Napoleon is the first man in Europe? So says van't Hoff.

"This much is certain," writes he; "if Byron had not had a dog, I would not have had one, and if Alcibiades had not had one, neither of us would have been possessors of one. But what if Byron had possessed a donkey? . . ."

Such was Byron's influence that at moments when the differential and integral calculus were not absorbing him and the inner self became dominant, the scientist often aspired to become a poet. But if a poet, it must be, in spirit and expression, as a humble follower of the great master. So we find that at Bonn, when one day, coming into the laboratory, he heard the awful news of the suicide of a fair fellow-worker, he rushed to his study and penned the following:

Elegy on the Death of a Lady Student at Bonn

Thy day is done, young champion of the free!
Thy glory and thy suffering are past,
As a weak beauteous flower's, where no tree
Can shelter it from cruel Autumn's blast;
Which dies in silence lovely to the last;
Gone as a day in spring, gone as the dream
Of one that wakes no more; and must it be
That thoughtful loneliness passes unseen,
Oh! shall thy hapless lot be lost in Lethe's stream!

This is not Byron, and yet not so bad for a young chemist, writing in a language not his own.

Fortunately for our science, van't Hoff did not receive much encouragement from a fellow poet, and once again he turned his eyes to chemistry and Bonn and Kekulé.

Here for the first time van't Hoff came into a new world. A celebrated university, situated where there were

A blending of all beauties; streams and dells,
Fruit, foliage, crag, wood, cornfield, mountain, vine,
And chiefless castles breathing stern farewells
From gay but leafy walls, where Ruin greenly dwells,

with students from every corner of the globe, and with a life so utterly at variance with his experiences hitherto, what wonder that his sensitive nature was filled with love and poetry for the place? "The laboratory is a temple!" writes he to his father; ". . . and in the lecture room there are to be seen daily about a hundred of our most promising young men, gathered from ten different states, to hear and to see Kekulé, whose fame has spread itself over half the world."

In the laboratory van't Hoff worked with twelve others at research in organic chemistry, and came into immediate contact with the assistant, Wallach, whose work on the terpenes and camphor was to become epoch-making.

Having finished a rather routine piece of work on the synthesis of propionic acid, and having, by the end of about two years, largely outlived his enthusiasm for Bonn, van't Hoff turned his wandering gaze toward Paris.

Outside of his *wanderlust*, just what his object was in going to Paris to study under Wurtz, is not clear. He seems to have done little laboratory work there, but his mind was full of speculations of all sorts, particularly of one which was to find expression shortly. "Il était si tranquille qu'on ne faisait pas grande attention à lui." Such was the opinion of his fellow-students, including Le Bel, through whose head were running ideas very similar to those of van't Hoff's; yet not a word was interchanged between the two regarding their speculations!

In the summer of 1874, after a six months' stay in Paris, he returned to Utrecht to complete his doctor's requirements. This degree he attained in December of the same year for another routine research on cyanacetic and malonic acids, and yet four months before he had published an eleven-page pamphlet on *The Structure of the Atoms in Space*, which was to give him an international reputation!

Van't Hoff's practical common sense—a nationalistic trait, one might add—is nowhere seen to better advantage. He might have offered his eleven-page pamphlet for a dissertation, but the probabilities of its acceptance would have been extremely small. Revolutionary ideas are not, as a rule, welcomed in dissertations, and if incorporated, may be thrown out, with such comments as "vague," "fanciful," "unscientific."

To explain cases of isomerism which structural formulas failed to solve, van't Hoff introduced the idea that in such molecules the carbon atom is at the center of a tetrahedran, with its four lines, representing its tetravalency, radiating towards the four points of the tetrahedran, all four equidistinct from the central carbon point. If at these ends we have four *different* atoms or groups, we can have at least two such compounds, one the image of the other, and *not* superposable.

At first this pamphlet made no impression. It was written in Dutch, which meant at best but a local audience, and it dealt with such novel ideas that most of the scientists of his own land would have dismissed it as a piece of wild imagination, particularly since its author was entirely unknown.

To give it a wider circulation van't Hoff translated his work into French under the title of *La Chimie dans L'espace*. This was all the more necessary since Le Bel, in November, 1874—that is, some two months after van't Hoff's publication—read a paper before the French chemical society, containing much the same views. It cannot be emphasised too strongly at this point that the two had come to practically the same conclusion quite independently of one another. As has happened before, and since that period, the time was ripe for some such discovery.

Over a year passed and nothing happened. Then came from Johannes Wislicenus, already a mighty force in organic chemistry, a letter which is as complimentary to the writer's extraordinary perpicacity as it is of the talent to the man addressed. "Let me tell you," he writes, "that your theoretical development [of the subject] has given me much satisfaction. I see in it not only an exceptionally talented attempt at explaining hitherto insoluble problems, but something which will give a wholly new impetus to our subject, and will thereby become epoch-making. . . . In a short time you will see, I hope, the interest I take in your work by my own researches in the field."

The letter concluded with a request to allow Dr. Herrmann, one of Wislecenus's assistants, to translate the work into German, which would then be introduced to the [German] public by a preface from the pen of Wislecenus himself.

The translation made its appearance in 1876 under the title of *Die Lagerung der Atome in Raume.* Like Byron after the publication of *Childe Harrold*, van't Hoff awoke to find himself famous.

But like Byron, again, his fame brought some bitter attacks. Of extreme virulence was one from Hermann Kolbe, the well-known Leipsig professor. "A Dr. van't Hoff"—so runs the diatribe—"of the Veterinary College, Utrecht [he had in the meantime been appointed to an assistantship at this place] appears to have no taste for exact chemical research. He finds it a less arduous task to amount his Pegasus (evidently borrowed from the veterinary College) and to soar to his chemical Parnassus, there to reveal in his *La Chimie dans l'espace* how he finds the atoms situated in the world's space.

"His hallucinations met with but little encouragement from the prosaic chemical public. Dr. F. Hermann, assistant at the Agricultural Institute of Heidelberg, therefore undertook to give them further publicity by means of a German edition. . . . It is not possible, even cursorily, to criticise this paper, since its fanciful nonsense carefully avoids any basis of fact, and is quite unintelligible to the calm investigator. . . ."

Kolbe goes on to deplore the times. To think that an unknown chemist should be given a ready ear when he talks of the most difficult of problems, and particularly when he treats them with such perfect assurance!

As for Wislicenus, who praised it in an introduction—"Herewith Wislicenus makes it clear that he has gone over from the camp of the true investigators to that of the speculative philosophers of ominous memory, who are separated by only a thin medium from spiritualism "[!]

If I quote Kolbe's criticism at some length it is only to show—for the *n*th time, no doubt—how very often some of the most powerful intellects of the day completely misunderstand the germ of a new idea. And Kolbe was a most representative scientist of his time. Yet to-day there is not an elementary book in organic or physical chemistry but devotes no inconsiderable portion of its text to stereochemistry!

During the two critical years of 1874 to 76, that is, from the time of the publication of his pamphlet to the time when the great letter came from the great Wislecenus, van't Hoff spent many an anxious and despondent hour. As with Huxley and crowds of other despairing young climbers, the Dutchman thought much of emigrating to a distant land—Australia, perhaps. This desire was much strengthened by the cold reception he received from know-it-all school directors to pompous college professors, whenever he applied for a position. "He looks rather slovenly. I'm afraid that he'll have lots of trouble with the students." So runs a representative commentary by an important school official.

For the fact that migration did not carry off van't Hoff to a distant land and to an unknown end we have his parents to thank. They constantly counselled patience and persistence. Fortunately, also, these parents of his were comfortably off, and this avoided distractions from his goal, which might otherwise have easily ruined a brilliant career—as it has done in innumerable cases.

Patience! Its first illustration was seen in the following advertisement which appeared in a Utrecht daily newspaper:

"Dr. J. H. van't Hoff ('Technology') will give private lessons in chemistry, physics, etc. Address Mrs. Kortebos, Spoorstrat, C."

The pupils came ever so slowly and time hung ever so heavily. This was not an unmixed misfortune, for during his leisure hours further ideas in organic chemistry began to crystallise in his head, with results which led to another fruitful volume not so very long afterwards—*Views regarding Organic Chemistry.*

Things changed at length—probably as a direct result of Wislicenus's letter. In 1876 he was appointed assistant at the Veterinary School in Utrecht, and in the following year he became lecturer at the University of Amsterdam.

In the meantime, in spite of Kolbe's criticism, van't Hoff's views on the atoms in space were finding welcome acceptance throughout Europe. His name was on the lips of scientific men everywhere, for his theories had given untold possibilities in the field of experimental chemistry.

His introductory lecture, *Imagination in Science*, was a masterly vindication of his own attitude towards the subject, and incidentally a splendid answer to Kolbe's criticism. The gist of it is contained in the conclusion, quoted from one of his favorite historians, Buckle: "There is a spiritual, a poetic, and for aught we know a spontaneous and uncaused element in the human mind, which ever and anon, suddenly and without warning, gives us a glimpse and a forecast of the future, and urges us to seize truth as it were by anticipation."

No wonder, then, that in 1878, when but 26 years old, he became the faculty's unanimous choice for the chair of chemistry (to which, sad to relate, mineralogy and geology were at first added).

This was very quickly and very appropriately followed by van't Hoff's marriage to Johanna Francina Mees, the daughter of a Rotterdam merchant. Jenny had been courted from the "Burgerschool" days up.

For the next eighteen years van't Hoff remained at Amsterdam. They were his most fruitful years. When in 1896 he was called to Berlin, van't Hoff had become the most renowned physical chemist of his day.

The early days of his professorship gave him little leisure. Five lectures per week in organic chemistry, and one each in mineralogy, crystallography, geology and palaentology, together with supervision of the laboratory, which provided for the instruction of graduate students, beginners in chemistry, and medical students—all this with but two assistants. Little wonder, indeed, that during these years of exacting teaching and executive duties the name of van't Hoff was quite absent from the pages of the chemical journals. But that, of course, does not mean that his imagination was not as active as ever. It was during these years of much routine, chiefly in the spare moments between supper and bedtime, that the ideas which found their expression in the *Etude de Dynamique Chimique*—the *Revolution Chimique*, as it has been called—were evolved.

This great work appeared in 1884. Speaking to the German chemical society ten years later, van't Hoff told that audience that the origin of these studies was to be traced to his difficulty in explaining certain oxidation processes. For example, oxidation takes place much more slowly with methane than with methyl alcohol. To explain this and other such changes a study of the velocity of reactions became imperative. But the work had an even grander aim, as the preface outlines: "Progress in general in any science passes through two distinct phases. At the beginning all scientific research is of a descriptive or systematic kind. Later it becomes rational or philosophical. It has not been otherwise with chemistry. . . . In the second phase of the development, the researches are not limited to collecting and co-ordinating the materials, but these pass to the study of causal relations. The initial interest which they had in a new substance has now disappeared; while the knowledge of its chemical composition and of its properties have a much greater value, becoming the starting-point in the discovery of causal relations. The history of every science consists in the evolution of the descriptive period into the rational period."

At first the reception accorded this work suggested that given to his *Atoms in Space*, that is, it was very quietly ignored. In this case, however, the question of language, or the standing of the author, had nothing to do with it. In 1884 van't Hoff was already a mighty figure, and the French language circulated throughout Europe. The truth was that the chemists were ill-prepared for any mathematical applications to their subject. This time criticism gave place to silence.

However, from far-off Sweden came a reverberating echo. In one of the current journals, the *Nordisk Revy*, for March 1885, appeared an exhaustive review of van't Hoff's book, in which, among other things, the reviewer had this to say: "Though the author has already achieved prominence by his success in unlocking the secrets of nature, his former accomplishments are put into the shade with the appearance of this work."

The reviewer was none other than Svante Arrhenius, then quite unknown, but later a figure to compare with van't Hoff himself--and no higher compliment can be paid.

As with his earlier work, the *Etude* is to-day regarded as one of our classics.

Towards the end of the *Etude* we already find a clear expression of the relation of osmotic pressure in liquids to the pressure exerted by gases—an analogy which soon led to a remarkable elucidation of our knowledge of solution.

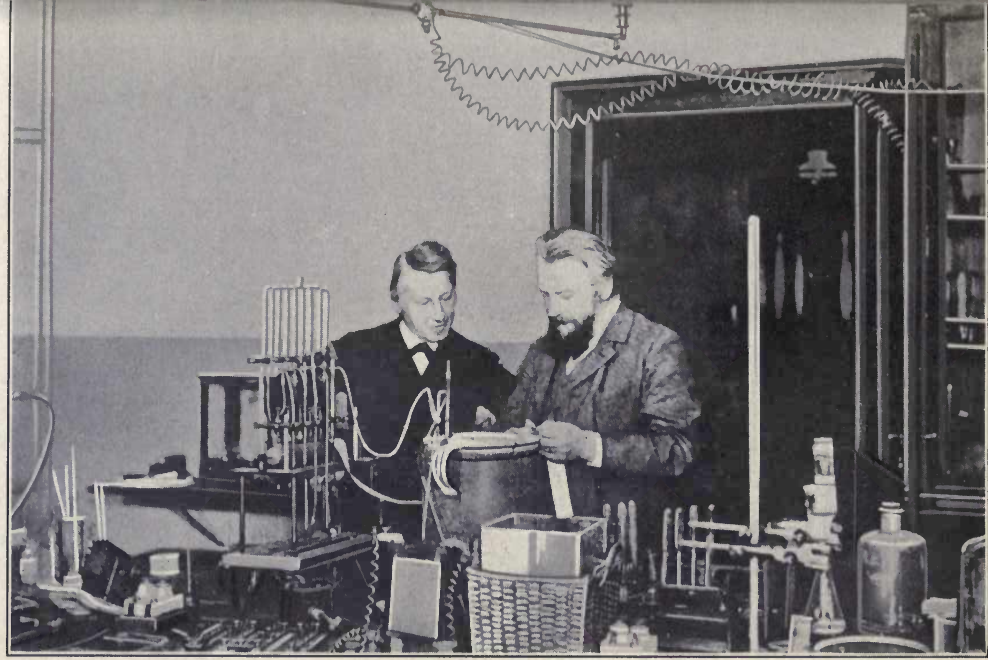
Sugar and salt are dissolved in water; what happens to the sugar and the salt? In what state are they while in solution?

Connecting the preliminary and apparently disconnected results of Raoult on freezing point depression, and Traube and Pfeffer on osmotic pressure and its measurement, van't Hoff enunciated his most celebrated law: A substance in solution behaves as if it were a gas, occupying a volume equal to the solvent.

The year 1887 may be regarded as the most important in the history of physical chemistry. To begin with, the second volume of Ostwald's *Lehrbuch der allgemeinen Chemie*,—the basis for all modern text-books on the subject,—made its appearance. Further, the first number of the *Zeitschrift für physikalische Chemie*, edited under the joint auspices of Ostwald and van't Hoff, was issued. And last, but not least, van't Hoff's article (revised) on the role of osmotic pressure in the analogy between solutions and gases, and Arrhenius's essay on the dissociation of substances dissolved in water, was published in volume I of the *Zeitschrift*.

As the era of modern chemistry starts with Lavoisier, so the science of physical chemistry starts with the three musketeers, van't Hoff, Arrhenius and Ostwald.

In this same year the chair of physical chemistry at Leipzig was offered van't Hoff. Upon this offer coming to the ears of the Amsterdam authorities, attractive counter proposals were immediately advanced. The most alluring of these was that a physics-chemical institute was to be built expressly for him. This was put into effect immediately.



van't Hoff and Ostwald in Ostwald's laboratory. [Reproduced from the *Zeitschrift für physikalische Chemie*.

During his remaining years in Amsterdam the experimental possibilities to which the *Etude* pointed were rigorously examined by van't Hoff and many students drawn by his fame from all quarters of the globe. Among the latter may be mentioned van Deventer, Spring, Reicher, Arrhenius, Cohen, Bredig, Goldschmidt, Eykman, Meyerhoffer, Ewan, and Bancroft (of Cornell)—names known wherever physical chemistry flourishes.

In 1893 van't Hoff, together with Le Bel, were presented with the Davy Medal of the Royal Society (of London), "in recognition of the introduction of the theory of asymmetric carbon and its use in explaining the constitution of optically active carbon compounds."[1] Such was the progress which the theory had made in the meantime, despite Kolbe.

The Germans had made one attempt to capture the great Dutchman, and they were not yet ready to admit defeat. Upon the death of August Kundt, in 1894, the

[1] The history of this Davy Medal is of uncommon interest. As a result of innumerable explosions in the English coal mines, with consequent loss of life, a society for preventing such accidents was founded in 1813. One of its first measures was to engage the services of Humphrey Davy, the celebrated chemist. Within a few weeks after his appointment Davy announced the discovery of his wonderful little safety lamp in the following words: "My results have been successful far beyond my expectations. I trust the safe lamp will answer all the objects of the collier. . . . I have never received so much pleasure from the result of any of my chemical labors, for I trust the cause of humanity will gain something by it." The colliers were not ungrateful. They presented Davy with a silver plate valued at 1,500 pounds. This plate Davy disposed of in his will as follows: ". . . I wish her [his wife] to enjoy the use of my plate during her life, and she will leave it to my brother in case he survives her, and if to any child of his who may be capable of using it; but if he is not in a situation to use or to enjoy it then I wish it to be melted and given to the Royal Society to provide a medal to be given annually for the most important discovery in Chemistry made in Europe or Anglo-America. . . ."

Berlin faculty unanimously suggested van't Hoff's name for the chair of experimental physics. Max Planck, the faculty's representative, was sent on a special mission to Amsterdam. Althoff, the representative in the Prussian Ministry of Education, sent van't Hoff an additional message urging him to come to Berlin and talk matters over. Finally, when some hesitation still prevailed, Emil Fischer was commissioned to use his good offices.

Van't Hoff, Jr., and van't Hoff, Sr., weighed the pros and cons carefully. The offer was an unusual one, and the honor extraordinary, but the duties of an active professor at Berlin were not light, and here in Amsterdam the authorities, ever afraid to lose their gifted countryman, were ready at the first sign to lighten his burdens, or increase his equipment. So van't Hoff, with papa's advice, once again said nay.

But Berlin wanted van't Hoff. Was it a question of too many hours of university teaching? Very well, then; this will be cut down to an absurd minimum. Since he is to hold a professorship, some lectures at the University must be delivered, but unless otherwise desired, these lectures need not exceed one per week. The rest of the time shall be van't Hoff's absolutely. Further, a private laboratory, equipped for any type of research van't Hoff shall elect, will be provided.

Need we wonder that he fell victim? "When for the past twenty-years, year in and year out, one teaches that potassium permanganate is an oxidising agent, one gets a little tired," was van't Hoff's comment. ". . . Of course, I have a very good position here in Amsterdam—that cannot be denied. But there is a difference between good and good. And when invitations are always rejected, there comes a time when no more invitations are received."

The German universities get the best brains their land can offer, and when better brains still are found beyond their border, the most alluring offers are sent forth. Thus it happened that at a later date attractions were held out to Arrhenius, and even our own Richards had difficulty in freeing himself from the Göttingen clutches. If only the Anglo-Saxons would follow suit here! If only in leaving the whey of German university training they would be careful to retain any cream! What a joy it would be to see Manchester scrambling for a Noyes, or California for a Soddy!

It goes without saying that van't Hoff's migration met with criticism in Holland. He was pictured as unpatriotic, and as being ready to grab all he could get, never being satisfied with what he had. Even the Dutch *Punch* did not spare him. Picturing van't Hoff in conversation with a fish, the following caricatures were presented:

(1) Dr. v't H: Fish—fish in the sea, bring me a cap and gown.
Fish: Here it is.

(2) Dr. v't H: Fish—fish in the sea, bring me a laboratory.
Fish: Here it is.

(3) Dr. v't H: Fish—fish in the sea, bring me an Order of the Crown.
Fish: Here it is.

(4) Dr. v't H: Fish—fish in the sea—
Fish: Still not enough? Adieu!

Writing to his friend Cohen from Charlottenburg (on the outskirts of Berlin) on April 23, 1896, van't Hoff says: "This is quite a new life, and I look forward with hope to the future. . . . Our apartment here [Uhlandstrasse 39] is excellent, and the situation all that can be desired—half within, and half without the town. A pleasant walk takes us to *Grünenwald* [a forest nearby], from where we can return by train if desired, and the station is quite near the house.

"I now find much more time to be with my family, and this has particular attractions amidst strange surroundings. The children all go to school. Everyone of them, with the exception of Goof [the youngest] has cried at one time or another because things were not quite what they were before. But children acclimatize themselves quickly enough provided they are healthy, and the air here seems excellent.

"I have attended two meetings of the Academy, which seem quite attractive under the stimulus of a respectable cup of coffee. On Wednesday I shall give my first lecture (one per week) as part of my duties as *ordentlicher honorarprofessor.*

"For the time being my laboratory consists of an apartment, which I have rented near our home, and this I shall equip with Meyerhoffer's help [Meyerhoffer was van't Hoff's favorite assistant in Amsterdam whom he had induced to come to Berlin].

"We intend to begin research work on the Stassfurt salt deposits. . . . The foundation for everything has been laid, and so far as I can see everything looks bright and cheerful. . . .

"My ever well-disposed wife and I pay quite a number of visits to the celebrities, whom I do not always know how to entertain, and whom I am forever mistaking for other folks. Three dinners are in prospect. . . ."

The task which van't Hoff now set himself was to make an exhaustive investigation of the potash deposits in Stassfurt, Germany. When we remember that until the outbreak of the world war the entire world was practically dependent for its potash—to be used as a constituent in fertilisers—upon these Stassfurt deposits, the value of any research connected with them can well be understood.

Of the substances present, the mineral, carnallite, is by far the most important. The question which van't Hoff first asked himself, and one which became the keynote to all his subsequent work, was: "Carnallite being a compound of magnesium and potassium chlorides and water, what arises when these three substances are brought together in different proportions, at different temperatures, and the escape of the water is prevented?"

Between 1896 and 1906 more than fifty papers were published on this and related subjects by van't Hoff and collaborators, of whom Meyerhoffer stands out preeminently. The work is of the most complicated kind, and no one has yet been found who has been bold enough to attempt a critical appraisal. This much seems certain: that while the work is a splendid application to industry of the phase rule by Willard Gibbs, the Yale professor, it is overshadowed in originality by van't Hoff's earlier contributions.

In 1906 van't Hoff turned his attention to one of the most fascinating problems in biochemistry: the nature of enzymes—those substances, present in all cells, which bring about the chemical changes in the organism so essential to life. The one or two papers on this subject, which appeared immediately prior to his last illness, were full of pregnant possibilities, and showed the master at his best.

In 1900 van't Hoff was elected president of the German chemical society; and in the following year he became the first recipient of the Nobel Prize in chemistry, Röntgen receiving the physics prize, and Behring, the one in medicine. In 1909 the Prussian Academy of

Science presented him with the Helmholtz medal, the highest honor which they could bestow.

Van't Hoff, never robust, had been a sufferer of tuberculosis for a number of years. The dread disease took hold of him with particular virulence towards the end of 1910, and it was soon apparent that he could not hope to hold out much longer. On March 1, 1911, at the age of 59, the greatest Dutchman of our times breathed his last. With his beloved Byron can we say that here was one "too soon return'd to earth."

When fame had come a-plenty, van't Hoff was much in demand at scientific gatherings. Such travelling as attendance at these meetings made necessary was undertaken with little hardship after his singularly fortunate Berlin appointment, and he loved to mingle with his scientific *confrères*.

In 1890 he attended the British Association meeting at Leeds, and took an active part in the discussion on solution (see the article on Ramsay). In 1893 he delivered an address on *La Force osmotique* before the *Societe chimique de Paris*, which probably explains why in the following year he was nominated for the Legion of Honor on the ground of his "remarquables travaux sur la chimie dans l'espace"! In 1894 he addressed the *Deutschen chemischen Gesellschaft* on "Wie die Theorie der Lösungen entstand."[1]

[1] The late Prof. H. C. Jones, who was pursuing graduate studies in Germany at the time, and who was present at this lecture, thus describes the event: "There sat in the front row Helmholtz, Ostwald, Emil Fischer, and a number of other men of science were present, whose names have become household words. These included Landolt, Kossel, Jahn, Tiemann, Will, Witt, and many others.

"The entrance of Helmholtz into the lecture room made an impression that will not be forgotten. Helmholz had attended the

In 1898 van't Hoff, as the triple delegate of the Univ. of Berlin, of the Academy, and of the Chemical Society, undertook a trip to Stockholm to attend a Berzelius celebration. To honor the memory of the immortal Swedish chemist was doubtless his desire, but a still greater incentive for this journey was the opportunity it afforded to be with his friend Arrhenius.

Three years later we find him on his way to the United States to attend the tenth anniversary of the founding of the University of Chicago (see addendum); and before the year is out he is to be found in London, in the Royal Institution, holding forth "in perfect English syntax, with here and there a modification of the vowels which indicated that the language was not his native

World's Fair in Chicago, and on his return home, when disembarking at Bremen, had slipped and fallen down the stairway of the ship. He, as is well known, ruptured a blood vessel on the head, which at the time nearly caused his death from loss of blood. . . . When Helmholtz appeared at the top of the lecture room, Emil Fischer ran and assisted him down the steps to a seat in the front row of the hall; the greatest physicist of the day aided by the most active organic chemist of that period.

"The object in inviting Van't Hoff to lecture in Berlin at that time, was to see and hear him with the possibility of calling him to that great university. His fame had already spread, and the real greatness of the man was even then beginning to be pretty fully realized. . . .

"This was the first time I had ever seen Van't Hoff. There arose to speak a slight figure of scarcely average height, with long, rather coarse hair, and with an extremely modest demeanor. This, as is well known to those who knew Van't Hoff at all closely, was one of his most striking characteristics. The speaker at first seemed a little nervous, due no doubt in part to the character of the audience he was facing, and in part to the fact that he probably suspected the motive in asking him to lecture in Berlin just at that time. . . . Van't Hoff had not proceeded far with the lecture, when any initial nervousness entirely disappeared, and his manner of presentation made a deep and lasting impression upon his audience."

tongue." The theme was the life and labors of Raoult, the eminent French physicist, who had but recently died, and whose work was so indissolubly bound with that of van't Hoff. The concluding words of this lecture apply as much to van't Hoff as to Raoult: "Yet his (Raoult's) character may be read in his papers: activity, patience, tenacity to an extreme degree in pursuing an aim, having an eye as much for detail as for vaster and vaster horizens, absolute independence of mind, power of criticising or of admitting without passion the views of others as well as his own, and of testing both with the same calm conviction that the last word must rest with experiment; this is what we read in every page and what the whole chemical world may know."

Two years later (in 1903) he is in England once more—this time in Manchester, in the city where once reigned a John Dalton and a James Prescott Joule, of whom Manchester ought to be far prouder than she is (which is saying no more of Manchester than what might be said of many another English or American city). One hundred years had passed since Dalton had brought forward his Atomic Theory, and the university of his native city now celebrated the event in becoming fashion. What the university authorities thought of van't Hoff may still be gauged to-day by anyone who enters the chemical laboratory of the university. At its entrance is a tablet with this enscription: "This stone was laid by Professor J. H. van't Hoff, 20th May, 1903, in commemoration of the centenary of Dalton's Atomic Theory."

In the following year we find him in Munich, sent there to represent the chemical society at the celebration of Baeyer's seventieth birthday. Van't Hoff had a very soft spot for the great Baeyer, the master of the chemistry of indigo and countless other organic substances, who, as far back as 1875, had declared of the *Atoms in Space*, "Da ist wirklich mal wieder ein neuer guter Gedanke in unsere Wissenschaft gekommen, der reiche Fruchte tragen wird."

A journey to Vienna in 1906, to attend a conference of Austrian engineers and architects, who—strange to relate!—were eager to hear van't Hoff on the subject of thermochemistry, gave him unusual pleasure. "Vienna—that was delightful," he writes; "I shall never forget those days. Profs. Klaudy and von Juptner had arranged for everything, and every hour was accounted for. It was only with the greatest difficulty that I could escape sometimes. I was really amazed at the things I could still do at my age. Don't ask me what I have seen. I have seen everyone except the Kaiser, and have done everything except rest. But it was all so lovely."

That same year he and his wife were in Italy to witness an eruption of Vesuvius. Whatever enjoyment the two got out of this trip was more than offset by van't Hoff's disease, which at this stage gripped him with added force.

We have seen how in early life van't Hoff was the poet and romanticist. In later years poetry was all but forgotten. His thoughts were with his chemistry everywhere, and at all times. Even music served but to concentrate his mind upon a problem, for he has told us that "good music makes it very pleasant to think of other things" — "other things" being, perhaps, the velocity of some reaction. Towards the end of his life, when doctors' orders forbade mental effort, he branched out into novel reading, and passed time with Turgenieff and Zola—the latter, in particular, a strange antithesis to the Byron of his youth.

Van't Hoff had never been robust, and ceaseless mental activity added to the uncertain elements in his state of health.[1] Hayfever was a regular yearly visitor, and in later years tuberculosis added to his afflictions.[2]

Van't Hoff's wife and four children, Johanna Francina (b. 1880) (who married privat-docent Ulrich Behn in 1905), Aleida Jacoba (b. 1882) (who married Dr. Charles W. Snyder, of Baltimore), Jacobus Hendricus (b. 1883), and Govery Jacob (b. 1889) survive him.

Addendum

van't Hoff in America

On the occasion of its tenth anniversary, the University of Chicago invited some distinguished foreign scholars to attend its celebration. Among these was van't Hoff. Whilst on his journey van't Hoff kept a brief diary which has since found its way into Ernest Cohen's life of the great Dutch chemist.

No sooner were the necessary arrangements completed with Nef, representing the University of Chicago, than further invitations began to pour in from the American Chemical Society, from Yale, from Richards at Harvard, from Bancroft at Cornell, from Loeb at Wood's Hole, etc.

With his wife by his side, and with a dose of sodium cyanide in his pocket, to be used in case of accident—a typical European custom—van't Hoff set sail from Rotterdam on May 21, 1901. Being a Dutch celebrity,

[1] "Van't Hoff . . . not only worked under high tension, but he seemed to live under high tension. When one saw him on the street he moved as if on rubber, and this kind of living would, in time, of necessity react upon the nervous system."—Prof. Harry C. Jones.

[2] "van't Hoff, as is well known [to whom?] contracted tuberculosis, probably while studying an eruption of Vesuvius. He thought that the dust lacerated his throat and lungs, and that the *tubercle bacillus* then began its work."—Prof. H. C. Jones.

the directors of the Holland-American Line set aside a stateroom for his use, and at table he sat with the captain on one side of him and the Dutch Consul to St. Paul on the other.

The voyage, aside from a day of rough weather, was, on the whole, a pleasant one. Professor Webster Wells, of Boston, and Dr. Pettijohn, of Chicago, whom he met on board, proved agreeable companions. During the spare moments when talk and play did not occupy him, van't Hoff busied himself with Loeb's work.

After landing in New York, where his pockets were searched by a custom-house official as though he were a pickpocket (!), van't Hoff registered at the Savoy Hotel. Here troubles soon began. The taxi-man proved exorbitant. The wash basin in his room had unexpected possibilities. The shades simply could not be moved, as though defiant of European authority. And the trunk, without which outdoor life was not to be thought of, simply would not show up.

In good time things righted themselves somewhat. With the arrival of the trunk a brief stroll was undertaken. Everything was greeted with open-mouthed astonishment. Much was found that was beautiful; much that was ugly; but everywhere something very distinctively American was encountered. Upon his return, cards from Professor Chandler, from his son-in-law, Pellew, and from a reporter of the *New York Tribune*, together with an invitation to the Century Club, awaited him. This was evidently the beginning of American hospitality.

At luncheon there was a welcome introduction to ice-water—an unknown luxury in Europe. After the mid-day meal, Miss Maltby, of Barnard, whom van't Hoff had met in Göttingen, called on him and his wife, and the trio started out on a stroll through Central Park and the Zoo, thence by bus to the "glorious" Hudson and Grant's Tomb, and finally to Barnard and the girls for supper.

The following day visits to Hale, to Chandler and to Pellew were planned. Brooklyn proved too complicated a center, and Hale could not be located. However, a sight of Brooklyn Bridge partially repaid his disappointment, for this structure aroused much admiration from the artistic scientist. The homes of Chandler and Pellew, "with their well-dressed ladies," were easier to find.

Not being expected in Chicago for some days, van't Hoff decided to visit some places of interest in this country. The first to be selected was Baltimore, with its Ira Remsen and Johns Hopkins. The country, as viewed from a Pullman, did not excite him much. One feature was the large posters along the road, announcing such items as "Baker's 5c Cigars, Generously Good," or "Omega Oil For Sore Feet, Stops Pain, For Headaches, For Everything." That, at least, was America with a vengeance! Passing into Philadelphia over the Delaware recalled the story of the famous crossing and the chain of dramatic events that followed it.

Baltimore was much more after his own heart. There was none of that breathless living so characteristic of the Empire City. Here people lived more on the style of the Rotterdammers and Amsterdammers.

At the University he met his old pupil, Harry C. Jones, whose open-hearted laughter, with his "all right" and "first-rate" and "that's it" won van't Hoff completely. Here he was shown the first of the series of classical researches on osmotic pressure, so intimately associated with the name of Morse.

The greeting by President Remsen and the Faculty in the Senate House was most cordial. "Really great" was a phrase used, and van't Hoff felt satisfied. The lunch at Remsen's which followed it, however, was too exclusively American; particularly the grape-fruit, which van't Hoff had not, as yet, cultivated a taste for.

On to Washington! More south! More negroes!! Fans!!!

Here the trusty Baedeker did yeoman service—whether at the Capitol, or at Howard University (a university for negroes!), or at the Geological Survey, or at the Smithsonian Institution, or at Mount Vernon. There was much to admire. And Day and Clarke and Hillebrandt, of all of whom he had heard much, he was glad to meet.

Over the Lehigh Valley to Mauch Chunk, the "American Switzerland," with its immense coal-fields, and thence to Ithaca. Here some delightful hours were spent with Bancroft and his wife. An introduction to President Schurman gave occasion for a discussion of the influence of the money-kings on the development of American universities. This was apropos of the dismissal of a professor who professed leanings towards socialism. Their next stop was in Buffalo, where the Pan-American Exposition and the grand Niagara Falls were visited.

From Buffalo van't Hoff proceeded direct to Chicago. The Pullman arrangements were an unpleasant surprise to him. He recalled how traveling from Paris to Strassburg each passenger had his own little room with his own wash-stand. But these common sleeping quarters, stiflingly hot and uncomfortable, with one wash-stand for all!

At Chicago Nef had undertaken to look after his comfort, and the result was everything that could be desired. His suite at the Hotel Windemere was ducal in pretentiousness.

The first part of the celebration consisted of a reception tendered by Mr. Rockefeller. Here he made the acquaintance of Stieglitz and Alexander Smith. In the afternoon van't Hoff delivered the first of his promised addresses, and this duly made its appearance in *Science*. Later on, Nef took him to a baseball game which was to be played between Chicago and Michigan, and here, for the first time, van't Hoff really understood just what baseball is. It would seem that while in Washington he had one day watched a steamer crowded with lively young girls depart for a baseball game. At that time our learned professor was of the opinion that baseball was some sort of a dance!

In the evening the president tendered a dinner to his guests. Van't Hoff was seated between M. Cambon, the French Ambassador, and Professor Goodwin, of Harvard. Goodwin considered van't Hoff's speech on the occasion—"American Ideals"—the best, because it was the shortest! Rockefeller's presence made wine or beer out of the question.

Following this came the general reception, which was most noteworthy for the immense crowd that had gathered there. Van't Hoff retired to a quiet corner with Alexander Smith, "an extraordinary tall colleague."

The following day—June 18—began with the laying of the foundation stone. The heat was terrific, and poor van't Hoff fell quite asleep during the long-drawn-out speeches.

Then came the awarding of degrees. All the honorary recipients were there, with the exception of the Russian, who had got his dates confused because of sticking too close to his Russian Calendar!

Fully one half of the students who received degrees were girls. This was an excellent augury for the future, thought van't Hoff, and the thought he conveyed to an acquaintance sitting near-by. This man explained the University's point of view by saying that the authorities did not greatly encourage the girl graduates to seek positions, but did like to see these same girls marry rich men. Why? Because it would then be the duty of these girls to interest their rich husbands in the needs of the University. Was the man serious?

van't Hoff was among a few to receive the honorary degree of Doctor of Laws.

At 1 P.M. came the alumni dinner, and van't Hoff was honored by being seated next to Rockefeller. Very little conversation was carried on with the oil magnate, because this gentleman seemed much too preoccupied with his coming speech. When Rockefeller's turn did come, he commenced with a story about a negro who was asked what he thought of Jesus, to which the negro replied, "I have nothing against Him." With this, Rockefeller turned to the public and said, "I have nothing against you." Van't Hoff does not tell us how the millionaire further developed his speech.

Again not a drop of alcohol on the table! Again Rockefeller's influence!

The next four or five days were mainly occupied with the preparation and deliverance of the lectures—since published and translated into English by Alexander Smith.

On the 24th of June van't Hoff departed for Cambridge. At Boston he was met by Richards, who had provided for his comfort as liberally as had Nef at Chicago.

On the 26th, which was the day of Harvard's Commencement, van't Hoff was presented for his honorary degree as "the greatest living physical chemist," a statement which was received with much applause. The lunch at Memorial Hall which followed was chiefly memorable because of Roosevelt's presence. The well-advertised teeth showed prominently. The evening was spent at the homes of Richards and Münsterberg. The following day, with Jackson and Richards as guides, Boston's sights were carefully inspected. In the evening he was the chief guest at a dinner which included President Eliot, Richards, Jackson, Pickering, Trowbridge, Hill, Michael and Bancroft. Gibbs and Crafts sent regrets. Van't Hoff was seated next to Eliot, who discussed with him the possibility of losing Richards, at that time considered as a probable candidate for the chair of chemistry at Göttingen—an unusual distinction for an American.

Van't Hoff took his departure from this country highly impressed with all that he had seen. He prophesied that within fifty years American universities would seriously rival those in Europe. It is but nineteen years since he has been here, but his prophecy has already come true.

References

Cohen's life of his great master (1) contains most of the available biographical material. For references to atoms in space, see 2, 3 and 4; for organic chemistry, 5; chemical dynamics, 6 and 7; theory of solution, 8; Stassfurt deposits, 9.

1. Ernst Cohen: Jacobus Henricus Van't Hoff: Sein Leben und Wirken (Akademische Verlagsgessellschaft, Leipzig. 1912).
2. J. H. van't Hoff: The Arrangement of Atoms in Space (Longmans, Green and Co. 1898).
3. J. H. van't Hoff: Chemistry in Space (Clarendon Press, Oxford. 1891).
4. J. H. van't Hoff: Stereochemistry. *Encycl. Britannica, 25,* 890 (1911).
5. J. H. van't Hoff: Ansichten über die organische Chemie (Vieweg und Sohn, Braunschweig. 1881).

6. J. H. van't Hoff: Studies in Chemical Dynamics (Chemical Publishing Co., Easton, Pa. 1896).
7. J. H. van't Hoff: Lectures on Theoretical and Physical Chemistry. Part 1. Chemical Dynamics. Part 2. Chemical Statics. Part 3. Relation Between Properties and Composition (Edwin Arnold, London. 1899).
8. H. C. Jones: The Modern Theory of Solution (Memoirs by Pfeffer, van't Hoff, Arrhenius and Raoult) (Harper Brothers. 1899).
9. J. H. van't Hoff: Physical Chemistry in the Service of the Sciences (English version by Alexander Smith) (University of Chicago Press, Chicago. 1903).

SVANTE ARRHENIUS

ARRHENIUS'S fame rests secure on his Theory of Electrolytic Dissociation, which postulates that those substances which, when dissolved in water or any other solvent, are good conductors of electricity, are also those substances which, in solution, largely decompose, or *dissociate*, into atoms, or groups of atoms, carrying powerful electric charges (the so-called "ions"). The theory was a direct outcome of van't Hoff's osmotic pressure studies, and its effect on the development of every phase of chemistry has been incalculable. That it is as sound in principle as Dalton's Atomic Theory or Mendeléeff's Periodic Law can hardly be doubted, for it, like the others, has helped to clear up many mysteries and to pave the way for many new discoveries. Its services have extended beyond chemistry and invaded the realms of the physiologist, the botanist, the zoologist and the medical man. One may mention the insight it gives us into the mechanism by which the blood maintains its remarkable neutrality, and the light it has shed upon various phases of cellular activity.

Arrhenius' later contributions to bacteriology and astronomy stamp him as one of the most versatile, as well as one of the most extraordinary men of our age.

Svante Arrhenius was born in Wyk, near Upsala, Sweden, on February 19, 1859. His father and mother (née Thumburg) traced their descent back to many a generation.

Soon after Arrhenius's birth his parents moved to Upsala, Sweden, and there young Svante received his public and high-school education, matriculating at 17 with an exceptionally fine record in mathematics, physics and biology—three subjects, in which his genius was to find splendid scope.

For the next five years he pursued his studies at the University of Upsala, specializing in mathematics, physics, and to some extent in chemistry. In this last subject he had Cleve for professor, and Cleve's lectures on organic chemistry gave Arrhenius food for thought. The simplest formula for cane sugar, said Cleve, was $C_{12}H_{22}O_{11}$; the strong probabilities were that the actual formula was a multiple of this, but there was no known way of finding out. Why not? thought Arrhenius, to whom things "unknowable" presented an irresistible fascination. And he forthwith set out to solve the problem of determining the molecular weight of the sugar by some electrical means,—electricity being the key to all difficulties.

All Arrhenius's attempts ended in failure. In the meantime, Raoult, the professor at Grenoble, France, had solved the mystery by his freezing-point determinations, but many days were to pass before the voice from Grenoble would reach Upsala.

Arrhenius's attempts led him to investigate the conductivity of solutions (with respect to the electric current), and by one of those happy strokes which often decide a man's fate or career, he chose dilute rather than concentrated solutions.

These experiments were carried out in Stockholm during 1881–84, for Upsala offered few favorable facilities. Edlung, the professor of physics, and the great authority on electricity, dissuaded Arrhenius from all chemical pursuits, possibly because he himself knew little chemistry. Arrhenius thanked him for his advice and went his own way; but Edlung undoubtedly gave him that foundation in the science of electricity without which his great discovery would have been impossible.

Our young experimenter had not groped his way many miles before he formed the opinion that in dilute solutions there was a complete dissociation, or cleavage of the molecules.

These were startlingly heterodox views. Did this young physicist assert that when common salt (the chemical name for which is sodium chloride) is dissolved in water, the salt dissociates into its components sodium and chlorine? Absurd! Sodium is a poisonous white metal, which violently attacks water as soon as it comes in contact with it; chlorine is a yellow-colored, suffocating gas, only too well known to the present generation. But neither sodium, nor chlorine, nor anything like these two elements makes its appearance when salt is dissolved in water.

Answered Arrhenius, meekly, but nevertheless with conviction, the chlorine and the sodium that are freed are not freed as chlorine and sodium atoms, but as chlorine and sodium "ions" (borrowing a word coined by Faraday), which are atoms (and sometimes groups of atoms) carrying powerful electric charges; these electric charges powerfully modify the properties of the elements.

What, then, does an electric current do when it passes through the solution? How, under these circumstances, do you explain the formation of hydrogen and chlorine at the two poles?

That's simple, said the twenty-odd year old Swede. The current does not dissociate the salt—the water does that; the electric current merely directs the path of the ions, sending the sodium ions to the cathode, and the chlorine ions to the anode. There the opposite electrical charges neutralise one another and sodium and chlorine *atoms* remain. The sodium atom is no sooner liberated than it attacks the water, decomposes it, forms caustic soda, and liberates hydrogen; so that the net result of the operation is to form caustic soda and to liberate the two gases hydrogen and chlorine.

The explanation was simple enough and fitted the facts remarkably well, but Arrhenius had disadvantages to contend against. He was a mere boy and quite unknown, and his professors were men of renown, who, like most men beyond a certain age, unlearn with difficulty, and adopt new ideas only when painful necessity makes any other course impossible. But at this time there was no such necessity. Arrhenius was a candidate for the doctor's degree, and without counting the consequences, he incorporated many of these heteredox views in his thesis with the elaborate title: *Recherches sur la conductibilité galvanique des electrolytes*—(1) *conductibilité galvanique des solutions aqueous extrêmement diluêes;* (2) *thêorie chimique des êlectrolytes.*

No wonder the professors were up in arms. What right had a candidate for a doctor's degree to express views so diametrically opposed to those held by the authorities?

At this time Arrhenius had not yet made the acquaintance of van't Hoff, otherwise that immortal Dutchman, no less immortal because of his good, hard commonsense, might have advised his colleague in Sweden to present a stereotyped research for the Ph.D. and reserve his more valuable work for another occasion—just as van't Hoff himself had done several years before in Utrecht.

Fortunately for Arrhenius he began to scent difficulties just in the nick of time. Instead, therefore, of saying that in a dilute solution there was total dissociation, he declared himself in favor of the view that in solution salts consist of two different kinds of molecules, the inactive—"this expression did not look dangerous"—and the active, the latter only conducting electricity. In a moment of happy inspiration, Arrhenius added that the active molecules are in a state described by Clausius.

Now Clausius was the physicist of the physicists of his time whom the Stockholm School simply venerated, and truly enough Clausius had expressed views closely resembling Arrhenius's, though not carried to so logical a conclusion. Said Arrhenius to an American scientific gathering not many years ago: "He [Clausius] was a great authority, therefore it could not be regarded as unwise to share his ideas."

A careful review of Berthellot's thermo-chemical studies led Arrhenius to the view that the strongest acids were also the best conductors of electricity.

"The next step was also quite clear: the active molecules, which are active in regard to electricity, are also active in regard to chemical properties, and that was the great step. . . . I got that idea on the night of the 17th of May in the year 1883, and I could not sleep that night until I had worked through the whole problem."

Everything followed from this: the constant amount of heat formed when strong acids and strong bases react (due to the formation of undissociated water in every reaction of this kind); the reaction of electrolytes (substances which conduct electricity) as being due to the reaction of the ions first formed; etc.

"I had deduced a rather great number of different properties which had not been explained before; but I must say that this circumstance made no very great impression upon my professor at Upsala."

"I came to my professor, Cleve, whom I admire very much, and I said, 'I have a new theory of electrical conductivity as a cause of chemical reactions.' He said, 'This is very interesting,' and then said, 'Goodbye!' He explained to me later [when Arrhenius was presented with the Nobel prize] that he knew very well that there are so many different theories formed, and that they are all almost certain to be wrong, for after a short time they disappear; and therefore by using the statistical manner of forming his ideas he concluded that my theory also would not exist long" [!]

Newlands' Law of Octaves anticipated the Periodic Law, but the ridicule that was heaped upon it by members of the English chemical society completely discouraged him. Not so Arrhenius. Having failed in his own country, he turned to foreign lands and wrote to Clausius, Thomson, and—again by a happy inspiration—Ostwald. The first two replied in a friendly tone: "They were glad to make my acquaintance, but not much more."

Ostwald, however, was deeply impressed. He had worked much on the chemical activity of acids, and now, with the help of Arrhenius's dissertation, he investigated their electrical activity, and found that the two ran proportionally.

In later years, when Arrhenius's theory had well nigh assumed the majesty of a law, Ostwald was fond of relating how he got, on the same day, the Swede's dissertation, a toothache and a nice daughter. "That was too much for one day," was Arrhenius's comment; "the worst was the dissertation, for the others developed quite normally."

"The worst was the dissertation." Quite true. The struggle was but in its infancy.

He had made, however, one all-powerful adherent. In Ostwald he found a man who is the expounder *par excellence*. What Huxley was to Darwin, Ostwald became to Arrhenius; and Ostwald is a first-class scientist, a gifted writer and a fighter to be feared—further unmistakable resemblances to the great Huxley of the Victorian period. The battle of the "ions" in the eighties and nineties waxed just as hot as the battles over the descent of man in the sixties and the seventies.

The analogy may be carried a step further. In Darwin's days the battle was no less severe, though such choice spirits as Malthus and Lyell had anticipated, and to a certain extent paved the way for Darwin's work. So prior to Arrhenius's day the rumblings of a storm were announced by Valson and Raoult and Gay-Lussac and Williamson and Clausius. Even Lord Rayleigh, as president of the British Association for the Advancement of Science in 1884 said: ". . . from the further study of electrolysis we may expect to gain improved views as to the nature of chemical reactions, and of the forces concerned in bringing them about. . . . I cannot help thinking that the next great advance, of which we have already seen some foreshadowing, will come on this side."

What could be plainer? But Rayleigh, renowned physicist that he was, spoke as a voice in the wilderness. The multitude could not and would not see.

Ostwald came to see Arrhenius in Stockholm to talk matters over, and, incidentally, to give a certain amount of prestige to the young doctor. In Upsala Ostwald saw Cleve who, taking up a water solution, said to the Riga professor, "And you also are a believer in these little sodium atoms swimming around?"—to which Ostwald replied that he thought there was some truth in that idea. "Cleve threw a look at me which clearly showed that he didn't think much of my chemical knowledge."

The university authorities granted Arrhenius the doctor's degree, but their commendation—"*non sine laude approbateur*"—showed that the dissertation had aroused no great enthusiasm in their breasts.

Arrhenius now decided to do what many an American prodigy has been forced to do: he decided to leave his country and fight for recognition in foreign lands. He knew well enough that should he come back crowned by the approval of the great masters of Europe, the former scoffers would become his loudest admirers. So he made arrangements to accept Ostwald's hospitality in Riga and pursue further investigations at the polytechnic school there.

Both met later at the *Naturforscher-versammlung* (similar to our Association for the Advancement of Science) in Magdeburg, with the object of proceeding to Riga together after the conclusion of that gathering. But the illness of Arrhenius's father temporarily upset all plans, and Arrhenius returned home.

His father died in the spring of 1885, and about a year later Arrhenius set out for Riga, materially eased by a stipend which he had received from the Swedish Academy at the earnest solicitation of his teacher, Edlung.

Ostwald had set aside part of his own private laboratory for Arrhenius's use, and though the two did not work together, they had ample opportunity for intimate discussion, and this led to a friendship which grows stronger day by day.

After spending the winter, spring and summer with Ostwald, Arrhenius, true to his undertaking, left for Würzburg to study under Kohlrausch. Here he came upon van't Hoff's celebrated memoir on osmotic pressure, in which Raoult's work was extensively discussed. It now became quite clear to Arrhenius that all electrolytes consist of the equivalent of at least two molecules and not one—that a molecule of common salt (sodium chloride) when dissolved in water, produces the effect of two molecules, due to the formation of the two ions, sodium and chlorine, each of which behaves as if it were a molecule. These conclusions now rested upon chemical, electrical and thermodynamic evidence.[1]

The above explanation made clear certain anomalous results which van't Hoff obtained in his experiments on osmotic pressure. In some instances the osmotic pressure was twice as great as what might have been expected from theoretical considerations. This "double bombardment" of the molecules, for which van't Hoff made allowances in a mathematical equation to express the reaction, was now seen to be due to the bombardment of ions. For every molecule two ions were formed, and each ion behaved as a molecule.

This led to a correspondence which culminated in a rare friendship between the two foremost physical chemists of the age.

Writing to van't Hoff from Würzburg in 1887, Arrhenius makes inquiries as to the possibilities of working in his laboratory in Amsterdam. The prompt reply has more than a cordial ring. Van't Hoff advises the Swedish scientist to come somewhat before the vacation is completely over "so that I may give my entire attention to your visit."

[1] Prof. Jacques Loeb informs me that van't Hoff's first paper on osmotic pressure was submitted to the Swedish Academy, and the secretary of that body passed it on to Arrhenius for an expression of opinion. In van't Hoff's paper Arrhenius found the data which supplied the missing links to his theory of electrolytic dissociation.

After a brief interval spent with Boltzmann in Grätz, Arrhenius proceeded to Amsterdam, and became the first foreign student of the physico-chemical laboratory there. Here, as in Riga, Arrhenius's irresistible personality won all hearts. Before many days he was "Dear Svante" to the head of the place, and on terms of intimacy with Mrs. van't Hoff, Eykman, Reicher and Van Deventer—the three last being, at that time, the most active workers at the laboratory.

If Ostwald did much for his Swedish protégé it is but fair to say that van't Hoff did little less. The Stockholm authorities were never for a moment left in doubt as to the opinions these illustrious men had formed of Arrhenius. They were directly responsible for Arrhenius's ultimate appointment in Stockholm, despite the most strenuous objections from the local body.

Van't Hoff and Arrhenius were much together in later years. These two, together with their champion, Ostwald, formed a friendship which is rare even in scientific circles. The two great creators, supported by their great interpreter, made up a trio which led the way in the onward march towards a more rational chemistry.

In 1910, some months before van't Hoff's death, Arrhenius paid him a visit in his Berlin home. Writing to Prof. Ernst Cohen, Arrhenius has this to say of what was to prove the last occasion on which he was to see his friend: "At first van't Hoff looked quite a pathetic figure. His voice, always so musical, was now quite hoarse. He was forced to lie on the sofa for pretty nearly the whole day. One morning Schmidt, of the ministry of education, paid him a visit. Van't Hoff was somewhat uncomfortable because this man found him lying down. Later van't Hoff said to me, 'These fellows think that one must be quite a lazy man to be lying down. But as a matter of fact I read constantly, and make as good progress as if I were sitting up.' I comforted him with the remark that I had done more reading in bed than out of it. I noticed, however, that when he read he soon got tired and put his book aside. There is no question but that he must take the utmost care of himself not to allow matters to take a turn for the worse.

"He accompanied me to the Stettin station. We drank three glasses of beer. This was followed by a return to his good old self. The eyes began to twinkle, and the little stories to flow.

"He was sorry that we could not remain together longer. 'We are getting old quickly—particularly I,' said he, sorrowfully."

From Amsterdam Arrhenius proceeded to Leipzig, to the university of which Ostwald had recently been appointed, and here he gave the finishing touches to his now classical paper on electrolytic dissociation—a more finished product than his doctor's dissertation. An extract was first sent to Sir Oliver Lodge, and the paper appeared in its entirety, together with van't Hoff's equally celebrated one on the analogy between the gaseous and the dissolved state, in volume I of the newly-created *Zeitschrift für physikalische Chemie*. Rarely, if ever, in the history of chemistry have two such epoch-making papers been published side by side in the same number of a scientific journal.

Their publication in 1887 did not lead to immediate recognition, but it did lead to fierce opposition on the part of many and thereby gave its authors much notoriety, so that to every chemist and physicist the name of Arrhenius became familiar if only as one associated with wild ideas of a post-impressionistic school. The 1890 British Association meeting at Leeds gave rise to verbal cannon which in intensity has been equalled only by a former meeting of this organisation in which Huxley and a bishop played a leading rōle (see Ramsay). In Berlin the wise *privat-docenten* spoke learnedly of immature thoughts based on a quicksand foundation. One or two did hint that an idea or two was not wanting, but that only a Helmholtz could have developed these. Even in far-off America Kahlenberg, of Wisconsin, the leading anti-ionist, concluded from his studies as late as 1900[1] that the dissociation theory was incorrect and doomed to early extinction. But just as in England the agent for the firm of "Ions" had a pretty skilful representative in the person of Ramsay, so here H. C. Jones, and later T. W. Richards, A. A. Noyes, W. D. Bancroft, J. L. R. Morgan, and others who had imbibed their knowledge from the Leipzig school, proved able defenders.

In the meantime the "wild army of Ionians," as Horstmann had dubbed the celebrated trio, were making no end of noise throughout Europe. Leipzig became the headquarters of the concern, and Ostwald the director. Ostwald's great *Lehrbuch der Allgemeinen Chemie*, his *Zeitschrift* and his splendidly equipped physico-chemical laboratory which the university authorities had specially built for him, attrcated enthusiastic students from all over the world who, with their Ph.D.'s in their pocket, with their minds filled with their "ionic" dissertations and Ostwald's "ionic" lectures, and, what is far more to the point, with an understanding, after several years of earnest study, of the true merits of the case, spread the new gospel far and wide.

[1] It should be added, in justice to Kahlenberg, that some of his criticisms cannot be lightly passed over. That there are imperfections in the theory Arrhenius himself has been the first to admit, but it is hard to see how, when it has helped to explain so much in our science, it does not contain the germ of some great truth.

In France alone, strangely enough, the new fashion was very slow of adoption. This is all the more strange since two of Arrhenius's illustrious forerunners, Gay-Lussac and Raoult, hailed from there. Perhaps the second startling development in modern chemistry, radium, which had its origin towards the close of the last century not far from the historic buildings of the Sorbonne, absorbed the French too much.

In 1891, only four years after the publication of his paper, Arrhenius was offered a professorship at Giessen, the university made famous by Liebig who, in the minds of a public overfed on "cures" of all kinds, is associated with "Liebig's Beef Extract." But the Swede politely declined and accepted in its stead a modest lectureship at the Stockholm High School.[1] Four years later he was appointed professor, though not without a struggle; which clearly showed how strongly opposed the men there were to his views.

Arrhenius upon closer acquaintance quickly converted enemies into friends, so that we find that five years after his appointment as lecturer he is nominated Rector,[2] and renominated three times in succession. The third time Arrhenius simply had to refuse, since executive duties were eating too much into his research time.

The Germans had tried once to get hold of van't Hoff, and tried again when the first attempt was unsuccessful, the second time with better results. Their strategy was now repeated. Having failed to get Arrhenius for Giessen they, in 1905, offered him a post similar to the one which van't Hoff had accepted several years before—as "Academiker" in Berlin; which meant a

[1] It should be made clear at this point that the continental idea of a high school is more the equivalent of a university.

[2] A position not strictly comparable to any we have in this country. Its nearest approach is that of president of a university.

full professorship, a private laboratory, a compulsory lecture of once a week and perfect freedom the rest of the time, and an income quite sufficient for modest wants. This he also refused. His countrymen, now quite convinced that the world outside of Sweden was ready to acclaim him as one of Sweden's greatest sons, invited him to become Director of the Nobel Institute for Physical Chemistry in Stockholm, a post he still holds. Recently (1919) he was elected vice-president of the Nobel Board of Trustees.

Arrhenius's training, as we have seen, had as much—and more—of physics and mathematics, as chemistry. His great teacher, Edlung, whose electrical problems led him to cosmogenic ones also, probably fired Arrhenius with a desire to invade the domain of astronomy. At the Stockholm High School he gave a course of lectures on cosmic physics, embracing the heavens, earth and atmosphere, which were published in 1901 in a volume of over one thousand pages. This led him to problems which were insoluble if the views then held were applied. The key to much of his difficulty he found in introducing the conception of "radiation pressure"—a pressure exerted by rays of light, of heat or of any other kind of radiation when falling upon a surface. With this conception in mind, Kelvin's and Helmholtz's theory of panspermia—that life-giving seeds drift about in space—gains in probability; for, by the introduction of "radiation pressure," the difficulty of explaining how germs transported from one planet to another in a time through which their life can be preserved, is largely removed.

Solar systems, according to Arrhenius, are evolved from nebulae by collision of suns. Around newly-formed suns there circulate smaller celestial bodies which cool more rapidly than the central sun. "When these satellites have provided themselves with a central crust, which will partly be covered by water, they may, under favorable conditions, harbor organic life, as the earth and probably also Venus and Mars do."

Arrhenius agrees with Helmholtz in denying the transformation of inorganic matter to organic matter endowed with "life." Helmholtz in 1871 said: "It seems to me a perfectly just procedure, if we, after the failure of all our attempts to produce organisms from lifeless matter, put the question, whether life has had a beginning at all, or whether seeds have not been carried from one planet to another and have developed everywhere where they have fallen on fertile soil."

This theory of panspermia, as further developed by Arrhenius, postulates that the seeds of life, floating in space, occasionally encounter planets, and, provided the condition on these planets is favorable, these seeds, so deposited, may blossom further.

If one remembers that the spores of many bacteria are about one millionth of an inch in diameter, it is conceivable that the radiation pressure of a sun would be sufficient to start them off into space.

A body moving at the average speed of a train, say thirty-seven miles an hour, would take one hundred and fifty years to go from the earth to Mars, and seventy thousand million years from the solar system to the nearest fixed star, *Alpha Centauri.* This seems a trifle long for a germ to remain alive! However, the conception of radiation pressure as a force reduces the time to twenty days and nine thousand years respectively.

Twenty days seems reasonable, but nine thousand years! Here again other factors must be taken into consideration—the intense cold, light, dryness, etc., in interstellar space. Both biology and chemistry give Arrhenius's fertile mind a helping hand.

To begin with, spores of bacteria have been kept for more than six months at two hundred degrees (centigrade) below zero without appreciable injury. Further, germs of splenic fever, for example, have been shown by Roux, of the famous Pasteur Institute in France, to remain intact by means of light in a vacuum—a condition somewhat comparable to that existing in interstellar space. Over sulphuric acid, one of the most powerful substances for absorbing moisture, spores have been kept for twenty weeks without losing their vitality.

And now for the climax, with the physico-chemist to the forefront!

It is well known that all chemical reactions are considerably reduced at low temperatures. A fall of ten degrees (centigrade) reduces the speed of a reaction in the ratio of five to two. "The loss of vitality in interstellar space at two hundred and twenty degrees below zero would be more than one hundred million times less rapid than the loss at ten degrees—which means that a journey of three million years through space would be no more injurious than a single day of exposure to terrestrial spring temperature." So what's a mere nine thousand years!

In Arrhenius's books, *Worlds in the Making*, and *The Destiny of the Stars*, these fascinating problems which fire the imagination are treated at length.

It needs to be emphasised here that the meteoric theories of Kelvin, Helmholtz and Arrhenius, while giving us an idea as to the mode of transportation of germs, are irrelevant in so far as origin goes, for in their attempt to explain the first sign of life on this planet they presuppose the existence of a germ elsewhere. Merely to say that life has had no beginning is begging the question. If we must have a hypothesis—and this for thinking men is too irresistible—we might as well be as bold as Schäfer, the Edinburgh physiologist, who holds that life originated as a result of the gradual evolution of inanimate material. In process of time the simple substance became more and more complex and ultimately emerged as the living germ—the nitrogenous colloid.

But Schäfer goes a step further. Why are we to suppose that this happened but once, as all theories with regard to origin have thus far assumed? Why are we to suppose that at one time in the dim past a series of fortunate accidents made life possible? Is it not more logical to assume that these evolutionary processes are going on to-day and will continue to do so?

Though even Huxley was of the opinion that at one time there was "an evolution of living protoplasm from not living matter," the idea that we should not relegate the process to some remote period in the past is a comparatively new one, and has not by any means received the approval of many otherwise loyal chemico-physiologists. These argue, with no small show of reason, that continuous life production would imply similar terrestrial conditions throughout the ages; and this we know not to be the case.

The ultra-scientific view, of which Schäfer is a shining example,[1] is based primarily upon analogy—a very valuable method provided its limitations are not abused, and provided, also, sufficient experimental data are at hand. The movement of oil drops and the interchange of substance in osmosis are certainly quicksand foundations upon which to build inter-relationship theories of the animate and the inanimate. This superficial connection between these physical changes and life processes fails to stand the test of adaptation and coordination—to name but two characteristic features of the vital sub-

[1] See also Prof. Jacque Loeb's *Mechanistic Conception of Life.*

stance. Indeed, our knowledge is so remarkably extensive that we cannot as yet state the simplest *vital* manifestation in terms of science.

If, then, Arrhenius and all others, have failed to solve the riddle as to the origin of life, he has practically solved the mystery of the transfer of life from one planet to another—which in itself is a great triumph.[1]

If Arrhenius has thought on the subject of life in interstellar space, he has also given attention to the possible better understanding of the living organism by the application of his refined physico-chemical methods to it. In his two books, *Quantitative Laws in Biological Chemistry* and *Immuno-Chemistry*, his views are elaborated in a highly suggestive way.

In the preface to the first of these he says: "The development of chemical science in the last thirty years shows a steadily increasing tendency to elucidate the nature and reactions of substances produced by living organisms."

The problem has been attacked in two ways—(*a*) by the organic chemist, such as Fischer or Kossel, who has elucidated the structure of the molecule, and (*b*) the physico-chemist, who investigates the nature of chemical processes. Biochemists, says Arrhenius, have thus far shown themselves to be averse to the second method.[2]

"Biological chemistry cannot develop into a real science without the aid of the exact methods offered by physical chemistry [quite true]. The aversion shown by

[1] It should be added that the several romantic touches in Arrhenius's cosmic studies have made many scientists hesitate to accept his views without reserve. On the whole, it does seem as if Arrhenius's reputation will rest more on his theory of electrolytic dissociation than on his astronomical work.

[2] This, by the way, is not true any more. In America, particularly, the physico-chemist as physiologist is not rare; witness Jacques Loeb, L. J. Henderson, D. D. van Slyke, K. G. Falk, etc.

bio-chemists [in the past] who have in most cases a medical education [this is certainly not true either of America or England] to exact methods is easily understood. . . . The physical chemists have found that the biochemical theories, which are still accepted in medical circles, are founded on an absolutely unreliable basis, and must be replaced by other notions agreeing with the fundamental laws of general chemistry."

Arrhenius's work in this field has been largely in immuno-chemistry—that which deals with the protective agents developed by a body when a toxin, or poison, is injected into the system. The most celebrated attempt to explain the mechanism of this reaction—which since von Berhing's immortal studies have largely absorbed the labors of many bacteriologists—is that known as the Ehrlich "side-chain" theory, which, in its simplest terms, tells us that each toxic substance has two groups attached to it—a "toxophore" group, with which it exerts its poisonous effects, and a "haptophore" group, by means of which it attaches itself to the "receptor" group which is found in every cell, the "heptaphore" and the "receptor" just fitting one another. This combination of cells in the body and the toxins leads to an extra production of "receptor" groups, some of which are thrown off and appear in the blood stream. It is these which constitute the antibodies—the protective bodies of the organism.

Ehrlich was of the opinion that the toxin and antitoxin neutralise one another in much the same way that a strong base neutralises a strong acid. Arrhenius, however, combats this view, claiming that the union is of a much looser type, belonging to a class known as "reversible reactions." He compares it rather to the union of a *weak* acid and a weak base, and has applied a well-known mathematical equation in chemical dynamics which goes under the name of Guldberg and Waage's *Law of Mass Action.*

It should, however, be added that experimenters are not wanting—and they are physico-chemico-bacteriologists and *not* necessarily medical men—who regard the toxin-antitoxin combination in the light of an "adsorption" phenomena,—in some such way, say, that animal charcoal removes colored impurities from vinegar or a raw sugar solution.

By 1909, the 25th anniversary of the publication of the theory of electrolytic dissociation, all serious opposition to the more important points in the theory had disappeared, and when Ostwald decided to honor the founder by dedicating a whole volume of the *Zeitschrift* to him, many of the foremost leaders of chemical thought contributed articles for the occasion. One may mention Abegg, Bancroft (Cornell), Le Blanc (Leipzig), Bodenstein, LeChatelier (Paris), Ciamician (Bologna), Dawson, van Deventer (Amsterdam), H. Euler, H. C. Jones (Johns Hopkins), W. Ostwald, G. Tammann, A. E. Taylor (Pennsylvania), R. Wegscheider (Vienna) and H. J. Hamburger.

The reaction of the theory of electrolytic dissociation on the chemists who witnessed its birth and watched its growth was well expressed by Sir William Tilden in 1914, when Arrhenius was the recipient of the Faraday Medal of the English Chemical Society: "With regard to the theory of electrolytic dissociation, which has been the subject of the discourse this evening, my experience, perhaps, is very much that of a good many others, and probably the majority in this room. When it first began to be discussed seriously, close upon twenty years ago, I confess I was among those who were strongly hostile. But I felt, as time went on, that I had to lay before my students . . . at any rate an exposition of what other people believed in regard to this department of the theory of chemistry; and it was my experience that by merely presenting these views, so new and so unacceptable as they were to me at that time, I gradually got to feel that they were inevitable, and that they were absolutely necessary. . . ."

Even his own countrymen, with the weight of foreign authority entirely against them, could no longer ignore Arrhenius, and to attack him was no longer safe for one's reputation; so they compromised and presented him with the Nobel prize!

We in America are justly proud of the fact that we were among the earliest to recognise this genius from the north of Europe. He has received and has accepted a number of invitations to lecture here and to enjoy our hospitality. In 1904, at the St. Louis Exposition Arrhenius was one of a group of distinguished foreign visitors which also included Ramsay, van't Hoff, Moissan Ostwald and Hugo De Vries. As late as 1911 he gave a series of lectures at our principal university centers. Fairly tall and bulky and robust, he suggests more the prosperous business man than the dried-up philosopher. Like his German and his French, his English, aside from an accent, is clear and correct, and his thoughts are expressed with little effort in this foreign tongue of his. His lectures are like his books—his sentences give rise to pages of reflection.

The Dutch and the Swedes counting, politically, among the smaller European powers, have given the world two of the greatest, if not the two greatest chemists of our time. Happy will be that nation that will be in a position to replace every Krupp factory with a great university and every super-dreadnaught with a van't Hoff or an Arrhenius!

References

Some of the sources of information are private. A delightful account of the origin and development of the theory of electrolytic dissociation has been given by Arrhenius himself in a lecture delivered to the Chicago members of the American Chemical Society in 1911, on the occasion of the presentation of the Willard Gibbs Medal to him (1). Wilhelm Ostwald contributed a characteristically striking portrait of the man and his work when the 25th anniversary of the publication of Arrhenius's classical paper was celebrated (2). The late Prof. H. C. Jones, a pupil of Ostwald, van't Hoff and Arrhenius, has some good touches of all three in his book, *The New Era in Chemistry* (3). Cohen, in his van't Hoff (4) devotes much space to the rare friendship which existed between the great Dutch and Swedish masters.

Arrhenius's classical paper on the theory of electrolytic dissociation (5) has been translated into English by Jones (6). Arrhenius himself is responsible for a volume on the theory of solutions (7). The influence Arrhenius's theory has had in laying the foundations for our modern chemistry is well exemplified in the volumes by Smith (8) and Stieglitz (9).

Cosmic problems are discussed in 10 and 11, and bio- and immuno-chemistry, in 12 and 13.

1. Svante Arrhenius: Electrolytic Dissociation. *Journal of the American Chemical Society*, *34*, 353 (1912).
2. Wilhelm Ostwald: Svante August Arrhenius. *Zeitschrift für physikalische Chemie* (Leipzig), *65*, V (1909).
3. H. C. Jones: New Era in Chemistry (D. Van Nostrand Co. 1913).
4. Ernst Cohen: Jacobus Henricus van't Hoff (Akademische Verlagsgesellschaft, Leipzig. 1912).
5. Svante Arrhenius: Ueber die Dissociation der in Wasser gelösten Stoffe. *Zeitschrift für physikalische Chemie*, *1*, 631 (1887).

6. H. C. Jones: The Modern Theory of Solution (Harper and Brothers. 1899).
7. Svante Arrhenius: Theories of Solution (Yale University Press. 1912).
8. Alexander Smith: Introduction to Inorganic Chemistry (The Century Co. 1917).
9. Julius Stieglitz: The Elements of Qualitative Analysis (The Century Co. 1913).
10. Svante Arrhenius: Worlds in the Making (Harper and Brothers. 1908).
11. Svante Arrhenius: The Destinies of the Stars (G. P. Putnam's Sons. 1918).
12. Svante Arrhenius: Quantitative Laws in Biological Chemistry (G. Bell and Sons, London. 1915).
13. Svante Arrhenius: Immuno-Chemistry (Macmillan Co. 1907).

HENRY MOISSAN

THE year 1907 was a particularly sad one for the world of science. Within a few months of Moissan's death science lost such intellectual giants as Perkin, Mendeléeff, Berthelot, the French chemist, Boltzmann, the Austrian mathematical physicist, Sir Michael Foster, the English physiologist, and Prof. Marshall Ward, the English botanist.

In the history of chemistry France occupies a proud position. One of her sons, Lavoisier of immortal memory, is the founder of the science of modern chemistry. Another, Berthollet, had much to do with developing a chemical nomenclature. Berthollet's assistant and successor, Gay-Lussac, has given us the celebrated law of gases known by his name. Dumas was a master of atomic weight determinations. Berthelot was a minister of state, as well as a great authority on thermochemistry. In St-Claire Deville we have one of the founders of physical chemistry. Pierre Curie had much to do with the discovery of radium.

Moissan rightfully takes his place among such illustrious scholars. He began his labors at a time when chemists had all but deserted the field of inorganic chemistry for the chemistry of the carbon compounds. The cry had been raised that inorganic chemistry had exhausted itself. Moissan's work soon convinced people that the cry was a false one. Inorganic chemistry had, and still has, rich fields for investigators. What was needed was a man of genius; and such a man was found in the person of Moissan.

Starting with his isolation of fluorine, the most active of the elements, and one closely allied to chlorine of gas cloud fame, Moissan, from a study of the compounds of fluorine, was led to his celebrated experiment on the artificial production of the diamond, and this latter in turn led to the electric furnace. With the electric furnace, scores of hitherto scarcely known elements and compounds were prepared; among them, calcium carbide, the source of acetylene.

Moissan's work, unlike many of the other great workers in the field, had an immediate practical bearing which the layman could appreciate. Thus the electric furnace readily found a place in metallurgy, and the need for acetylene gave rise to an immense calcium carbide industry. Yet Moissan remained a comparatively poor man to the day of his death. His discoveries, instead of being patented, were published in the French chemical journals, to be used by readers in any way they saw fit. He was a professor, and as such he was employed by, and worked for the people. The discovery itself, and not what the discovery could bring to him, counted with Moissan.

In this connection it is important to emphasise something else. One must not measure the greatness of a man of science by the standard whether his work can find immediate application in everyday life. Were such a test to be applied, very few great scientists would remain. The application of the laws and discoveries of science come with time,—sometimes sooner, sometimes later, but come they do. It is therefore particularly difficult to point out the practical significance of the more recent contributions to chemistry. Yet even here results often show themselves sooner than expected. Thus, to take two cases at random, van't Hoff's profound studies of chemical dynamics have had no small share in contributing to the solution of the synthesis of ammonia from its elements; and Arrhenius's theory of electrolytic dissociation has opened up new vistas in biological research.

Ferdinand Frederick Henri Moissan, to give him his full name, was born in Paris on September 28, 1852. We can afford to be even a little more specific; we can add that the name of the street was Rue Montholon, and the number of the house, 5.

His father, a native of Toulouse, held a position with the *Compagnie des Chemins de Fer de l'Est.* His mother (*née* Mitelle) belonged to an Orleans family.

In 1864 the family moved to the small city of Meaux, and here Henri was sent to the municipal school.

Among the teachers at the school was one, James, who taught mathematics and the natural sciences. The good directors were evidently of the opinion that while it may take several men to master one subject such as Greek, it probably does not take more than one to master several subjects such as chemistry, physics, astronomy, biology, etc.—with mathematics thrown in to give more symmetry to the list. However, James was a very good teacher, and he early recognised in Moissan a boy out of the ordinary. James offered to give Moissan private lessons in addition to the instruction at school; this the boy gratefully accepted.

In addition to James's exposition of the sciences, Moissan had another helper in his father. His father's particular science was chemistry, and Moissan began to receive elementary instructions in chemistry when he was fourteen years old. " J'avais commencé à manipuler de l'âge de 14 à 15 ans," writes Moissan; " et mes premières leçons de chimie, données par mon père, sont encore gravées dans ma mémorie."

Probably because of financial difficulties, Moissan left the school in 1870 without passing his university entrance examination, to the keen disappointment of his teacher, James.

Moissan set out for Paris. His preference for chemistry led him to seek a position as an apprentice in a drug store, or apothecary's shop. Such a position he found at a pharmacist's located at the corner of *Rue Pernelle* and *Rue Saint-Denis;* and here soon afterwards he achieved his first victory over nature by saving a man's life who had attempted suicide with a dose of arsenic.

Duties at the store gave no time for study, and without passing several important examinations there was no hope of ever becoming a pharmacist.

At this point it is perhaps necessary to inform some readers that the pharmacist in France or Germany is one who has gone through a much more thorough course of training in preparation for the practise of his profession than the druggist (self-styled "chemist") in England or America. As a matter of fact, the pharmaceutical student is very much of a university student, and his training is correspondingly thorough.

Moissan had a school chum, Jules Plicque, who attended Dehérain's lectures at the *Musée d'Histoire Naturelle*, and Plicque told Moissan wonderful things of Dehérain and the Museum. Moissan paid more and more attention to these accounts. He was ambitious; he wanted to become a *real* scientist, and for this, further schooling was necessary.

Moissan quit his "job" in 1872 and went to Frémy at the *Musée.* He supported himself as best he could by giving private lessons, and lived in the hope that some day he would be an industrial chemist making as much as 3,600 francs per year! Three thousand six hundred francs was the very maximum to which this lad of twenty aspired. How poor financially he was then can well be imagined.

Two years later Moissan exchanged Frémy for Dehérain, the teacher of his friend Plicque. Dehérain soon took notice of Moissan. The young man's leaning towards industrial chemistry was not discouraged by his teacher, but hopes were also held out that good work, coupled with the fulfilment of several university requirements, might lead to an academic position.

An academic position was what Moissan wanted far more than any industrial one, but until then the poor lad had thought any such goal entirely beyond his reach.

He now prepared actively for his university degrees. For the time being much of the chemistry work had to give place to the classics and physics—subjects which he had neglected since his school days. In 1874, after several attempts, he obtained his bachelor's degree,[1] and in 1877, his *Licencié es Sciences*.

Even during these days of hardship life had its bright spots. At the Museum he formed a close friendship with Vesque, the botanist, and Etard, the chemist; and during his army service at Lille in 1876 he got to know Beclere, Siredey and Walter, all three medical men. These six formed a very close circle. Not only was science fostered among them, but literature and the arts were also cultivated.

This intellectual group proved of immense value to Moissan, whose irregular education needed polish to round it out. He acquired a taste for painting, sculpture, historical studies and belles-lettres, and incidentally

[1] To get a bachelor's degree at the University of Paris, or at an English university—particularly London, exhaustive final examinations, theoretical and practical, have to be passed. It is not unusual even for good students to fail in their first attempt.

mastered his own language in a way which was of invaluable help to him later as lecturer and writer.

This love of literature led the young man to attempt the writing of a play—so often an emotional outlet for the youths below and above twenty. The play must have had merits, for it came near being produced at the *Odéon.* Perhaps it was as well that the play was not produced, for it might have made him neither a good dramatist nor a good chemist. "Je crois que j'ai mieux fait de faire de la chimie," was Moissan's own comment.

The days of youth and health and hope are always delicious memories. Moissan loved to recall the times when he and his friends, poor in pocket but rich in mind, lived and laughed and were happy. Vesque, who, with his violin, gave meaning to Beethoven, did much to spiritualise the souls of the little company.

Dehérain being interested in plant physiological chemistry, Moissan's first research naturally fell in this field. It dealt with the interchange of oxygen and carbon dioxide in the leaves of plants, and was used as a part thesis for the apothecary's license.

But even during the progress of this research Moissan had decided not to specialise in organic chemistry. Dehérain's advice against such a step did not change Moissan's decision; the young man wished to turn his attention to inorganic chemistry. But did Moissan know that inorganic chemistry offered but a barren field? No matter, said Moissan, it can still be cultivated.

We are not sure just what led Moissan to such a happy choice. Perhaps Dumas' complaint in 1876 had something to do with it. "Notre pays," said Dumas, "tient largement sa place en chimie organique, il néglige trop la chimie de corps inorganiques." And what was true of France was true of the rest of Europe. Yet even France had a man, St.-Claire Deville, whose fame did not rest upon his organic chemistry researches. Neither, however, did they deal with the purely inorganic, for the vast subject of dissociation belongs to a third branch of the science—physical chemistry.

Whatever the reason, nothing could have been more fortunate. What the renaissance was to the revival of learning in Europe, Moissan became to the revival of inorganic chemical scholarship in the universities and factories.

Of his three hundred papers or so, almost every one deals with experimental inorganic chemistry. Very few touch even upon theory. They were published either in the proceedings of the French Academy, in the *Annales de Chimie et de Phisique*, or in the *Bulletin de la Société chimique de Paris*.

In 1879 Moissan obtained his diploma of *Pharmacien de première Classe*, and in the following year he was granted the degree of *Docteur es Sciences physiques* with the presentation of a thesis on the oxides of chromium—one of his earliest papers in his newly-chosen field.

The first academic appointment came to him when he was twenty-seven years old. It was as *Répétiteur* [instructor] *de Physique* at the Agronomic Institute. In the following year he was made *Maître de Conférences* [lecture assistant] and *Chef des Travaux Pratiques* [senior demonstrator, or associate] at the *Ecole Supériore de Pharmacie.*

Before he left the town of Mieux several years previously, Moissan became acquainted with one Lugan, a pharmacist, and incidentally with his daughter. Lugan had a perfect passion for chemistry, and hence followed Moissan's career with much interest. Moissan on his side liked Lugan, Lugan's chemistry and Lugan's daughter. In 1882 Moissan's courtship and prospects had both made sufficient strides for marriage to appear within the bounds of reason. The *docteur* was not only accepted by *Leonie*, but Leonie's papa provided comfortably for the pair.

With a stroke Moissan became the happiest of men. The marriage proved as perfect as a marriage between two human beings can possibly be, and the income provided by the father-in-law removed the chief source of worry for the future. In 1885 a third member of the family, Louis, joined them. "If I am not in my laboratory I want to be in my home." What better commentary on the home atmosphere is needed than this remark of Moissan's?

The work which, beginning in 1884, led Moissan to his first great achievement, the isolation of fluorine, has a history.

Fluorine in the form of its compounds had long been known. Without ever having been isolated, the element was included in the group of elements known as the *halogens*, or salt producers, because its salts showed striking similarities to salts of the rest of the group. The commonest member of this family is chlorine, and its sodium salt, sodium chloride, is the table salt so indispensable as a food. The other elements belonging to the halogens are bromine and iodine.

Chlorine was discovered as far back as 1774 by Scheele, the famous Swedish chemist. In 1811 Courtois discovered iodine in the ashes of sea-weed, and fifteen years later Balard discovered bromine. It was not, however, till 1886 that the fourth, and last member of the family, fluorine, was isolated by Moissan. The activity of this element—it is the *most active* (*i.e.*, chemically active) element known—had prevented its isolation prior to this date.

Scheele himself, who was familiar with the acid derived from fluorine, hydrofluoric acid, began experiments on the latter substance towards the close of the eighteenth century, but nothing came of them. Davy, the English chemist, made an attempt in 1813 to isolate fluorine by passing an electric current through hydrofluoric acid. The method, with modifications, was successfully used by Moissan later on; but in Davy's case the fluorine was no sooner liberated than it attacked the water and anything else that happened to be present, at the same time being itself transformed into one of its compounds. Gay-Lussac and Thenard were not more fortunate.

Knox, a Scotsman, spent three years on this problem, and then had to go to Italy to recruit his health which was shattered by the unavoidable inhalation of the vapors of toxic gases. Louyet, another worker, died of their effects. In 1850 Frémy, one of Moissan's teachers, came near to success by his preparation of anhydrous (that is, water-free) hydrofluoric acid.

Moissan attacked the problem in 1884 "in the uncertain hope of at last being able to isolate the element." By the distillation of a mixture of arsenious oxide, oil of vitriol and fluorspar, he obtained a fluoride of arsenic which, when electrolysed, gave him arsenic and a gas which immediately attacked the platinum electrode.

Moissan now returned to Davy's and Frémy's experiments. Davy's hydrofluoric acid alone would not do because it contained water, and Frémy's anhydrous variety had the drawback in that it was a non-conductor of electricity. Moissan's success depended upon the fact that the addition of potassium acid fluoride to the anhydrous hydrofluoric acid converted the latter into a conductor.

To withstand the action of fluorine, the apparatus was made of an alloy of platinum and iridium, an extremely expensive combination. Later, however, Moissan found that copper could be substituted, for though the fluorine attacks the copper, the resulting copper fluoride acts as a protective coating, and prevents further disintegration of the vessel and loss of the fluorine.

On June 28, 1886, Debray, acting on behalf of Moissan (who was not yet a member) announced to the French Academy Moissan's isolation of fluorine. Such an announcement was much too important to be passed over without further notice. The president appointed Berthelot, Debray and Frémy to investigate and report on Moissan's work.

Lo and behold! in the presence of these august men Moissan could not get any fluorine! He tried and tried, but no fluorine! The following day the substitution of new materials for old ones solved the difficulty, and soon after that the Academy's representatives were convinced of the legitimacy of Moissan's claim that he had really succeeded in isolating this most elusive of all the elements.

Moissan showed that no element was safe from the attacks of fluorine; it readily combined with most of them to form fluorides. But with Ramsay's "inert" gases of the atmosphere, such as argon or helium, it showed no action whatsoever.

Much later, in conjunction with Dewar, the famous English experimenter on the liquefaction of gases, Moissan succeeded in liquefying fluorine at a temperature of 185 degrees (centigrade) below zero; and even at this temperature, though the liquid no longer has any action on glass, it still attacks hydrogen and hydrocarbons. This is remarkable, for we know that just as an increase of temperature accelerates chemical reaction, so a decrease of temperature retards it. At 185° below zero few, if any substances, have much chemical action.

But another very remarkable fact must now be cited. The researches of Victor Meyer in Germany, and particularly those of Dixon and Baker in England, have shown that substances tend to combine less and less the drier they are. If in addition to being *absolutely* dry, the substances are also absolutely *pure*, it is questionable if any chemical reaction is at all possible. In any case, in this connection it is interesting to note that perfectly dry fluorine has no action on clean, dry glass!

Moissan's researches on fluorine were published in book form in 1891 and republished in 1914 as one of a series belonging to *Les Classiques de la Science.*

Ostwald several years ago in his *Klassiker* commenced the republication in pamphlet form of some of the more classical researches in the history of chemistry. A French committee consisting of H. Abraham, H. Gautier, H. Le Chatelier and J. Lemoine, arranged for the French public a *Classiques* comparable to the German *Klassiker.* Beyond one or two sporadic attempts, nothing like these have appeared in English. Why? Are we forever to lag behind?

Before dismissing the subject of fluorine, it should be added that recently W. L. Argo, an American electrochemist, has suceeded, by a modification of the Moissan method, in getting fluorine easily and in quantity.

Moissan's success in isolating fluorine did not go unrewarded. The Academy awarded him the *Prix la Caze* prize of 10,000 francs, and soon afterwards (in 1886) he was appointed professor of toxicology at the *Ecole de Pharmacie*, in succession to Bouis, the discoverer of caprylic alcohol. Now for the first time

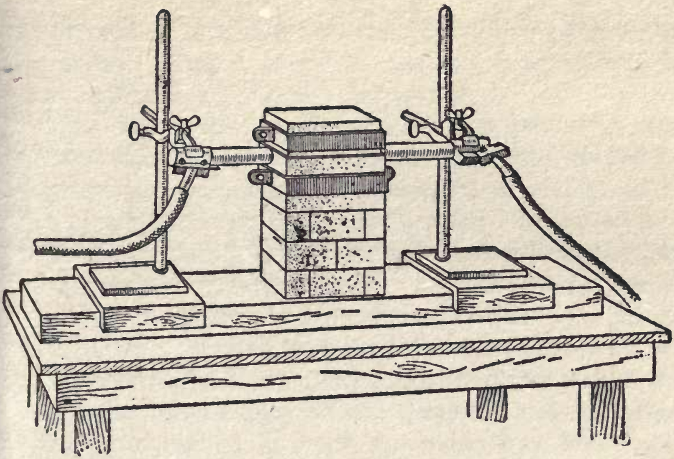
Moissan had his own laboratory—a small one, but yet his own.

The isolation of fluorine was quickly followed up by an exhaustive study of the combinations of fluorine with other substances. Among these were the compounds of fluorine with carbon. Moissan had dim hopes that by utilising the activity of fluorine the carbon could be separated in the crystalline form of diamond. Moissan found that he could get two combinations of carbon and fluorine, but these, when decomposed, left only common carbon. This led him to a systematic study of the varieties of carbon, and the methods of changing one variety into another.

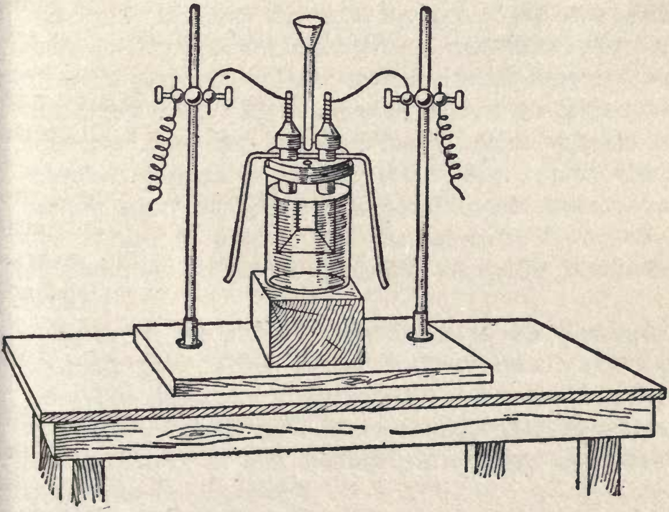
Diamond, graphite, lampblack, boneblack and large percentages of coal and coke, are really nothing more than different forms of one element, carbon. The chemist gives the name "allotropic" to such different forms of one element. Allotropic elements show the same composition, though the internal structure of the atoms are probably different. Diamond, graphite, lampblack, etc., when completely burned, all give carbon dioxide and nothing else, proving the identity of these allotropic forms.

It is easy enough to convert diamond into one of the other forms of carbon by strongly heating it, but until Moissan's time no one had succeeded in the reverse process. Before, however, this could be accomplished, Moissan had to devise some scheme for getting much higher temperatures than were then available. This led to his famous electric furnace.

In its simplest form (see diagram on the opposite page) it consisted of two blocks of lime with central cavities for the crucible containing the material to be used, and horizontal cavities for the carbon electrodes. The furnace measured some 6″ x 6″ x 7″, and required



Moissan's electric furnace.



Moissan's apparatus for preparing fluorine. [Reproduced from Moissan's books.]

a current of four horse-power (about 60 amperes and 50 volts). With it Moissan obtained temperatures in the neighborhood of 4000° Centigrade.

Now out in Arizona Dr. Foote, a mineralogist, had shown that the Canyon Diablo meteorite contained microscopic diamonds, and Moissan's careful study of the possible formation of these precious stones led him to the belief that they were formed from ordinary carbon as a result of great pressure. Accordingly, in one of his experiments Moissan heated some pure iron mixed with carbon (obtained from the calcination of cane sugar) in his electric furnace. The iron melted like wax at the enormous temperature of the furnace, and dissolved portions of carbon in much the same way that water dissolves common salt.

After a few minutes at 4,000° centigrade, the crucible containing the molten mixture was plunged into cold water. In this way the outer surface of the iron cooled more quickly than the inner portion, and thereby brought a terrific pressure to bear upon the inner contents, still in a liquid state. By this means, part of the carbon was converted into the diamond form. After suitable removal of various impurities, the residue, partly transparent, partly black, and microscopic in size and amount, was shown to possess the characteristic hardness of diamond, as well as its crystalline structure (octahedral facets).

However, the artificial production of the diamond, a scientific fact to-day, is not a commercial success as yet. The small size of the stones, and the cost of their production, make it quite improbable that, for the present, the laboratory of the chemist will attempt to compete with nature's laboratory.

As with Madame Curie's discovery of radium several years later, the artificial production of the diamond was splendid material for newspaper gossip, and poor Moissan, the most modest of men, found himself lionised by all Paris. Diamonds, said the newspapers, could be made so easily by Henri Moissan, that they would soon be had for the mere asking. What would the De Beers Company in South Africa do?

Many of Moissan's subsequent experiments were made with the help of the electric furnace. The preliminary operations were first carried out at the works of the Edison Company in Avenue Trudaine; later the basement of the college was equipped for this purpose.

By means of the electric furnace and the high heat thereby afforded, Moissan liquefied and volatilised such metals as copper, silver, platinum, gold, tin, iron, etc. Extensive researches on the combinations of the elements with carbon, boron and silicon to form carbides, borides and silicides respectively, were carried out. Perhaps the most notable of these was the preparation of calcium carbide, which in the presence of water yields the important illuminating gas, acetylene. Moissan also prepared silicon carbide, or carborundum, but he does not seem to have attached any importance to this discovery. The method of preparation was also a poor one. The discovery of carborundum is therefore very rightfully assigned to Acheson, the American industrial chemist, who, working quite independently, and using a much more practical method (sand and coke) for its preparation, arrived at the same result, and immediately took out a patent for the process.

The study of carbides also led Moissan to a theory of the origin of petroleum. In brief, Moissan's view was that water, acting on carbides, gave rise to various hydrocarbons which, when mixed, constitute petroleum.

With the electric furnace as with fluorine, Moissan embodied the results of his researches in book form under the title *Le Four Electrique.* In the preface to this work we find an admirable spirit admirably expressed: "But what I cannot convey in the following pages is the keen pleasure which I have experienced in the pursuit of these discoveries. To plough a new furrow; to have full scope to follow my own inclination; to see on all sides new subjects of study bursting upon me; that awakens a true joy which only those can experience who have themselves tasted the delights of research."

The work consists of four chapters. In the first, various types of the electric furnace are discussed. In the second, the results of studies on the three varieties of carbon—the diamond, the graphite and amorphous carbon—are recorded. Chapter three deals with the preparation of several simple substances by means of the electric furnace, and also describes researches on the preparation of chromium, manganese, molybdenum, tungsten, uranium, vanadium, zirconium, titanium, silicon and aluminium.[1] Chapter four describes the preparation of various carbides, silicides and borides, calcium carbide receiving particular attention.

In 1904 Moissan, as chief editor, published the *Traité de Chimie Minéraile*, a comprehensive work (in five volumes) on inorganic chemistry. His collaborators numbered some of the most distinguished French chemists, such as Gautier, Le Chatelier, Sabatier, etc.

It has been pointed out that in 1886 Moissan became professor of toxicology at the School of Pharmacy. It was not until thirteen years later that he succeeded to the chair of "mineral" or inorganic chemistry. Strangely enough, during all these years, though his research work

[1] The feasability of preparing aluminium (or, as it is sometimes called, aluminum) on a large scale was first successfully demonstrated by Hall, an American, in 1886.

was pre-eminently inorganic, his lectures dealt with an entirely different subject.

In 1900, on the retirement of Troost, Moissan was unanimously chosen Professor of Inorganic Chemistry in the *Faculté des Sciences* in the University of Paris; he, however, retained his title of professor at the *Ecole de Pharmacie.*

In 1888, as a result of his isolation of fluorine, Moissan was elected a member of the Academy of Medicine. Three years later Cahours' death left a vacant seat in the *Academie des Sciences.* To fill this place the names of Moissan, Grimaux, Ditte, Jungfleisch and Le Bel were submitted. After a discussion of two hours the committee decided to nominate Moissan and Grimaux. The latter was subsequently defeated by eleven votes, and Moissan thereby became the *confrère* of Berthelot, Friedel, Schützenberger and Troost. Election to the Academy is the highest honor a French man of science can attain in his own country.

In 1896 the English Royal Society awarded its Davy Medal to Moissan, "in recognition," said the president, Lord Lister, "of his great merits and achievements as an investigator. The electric furnace of M. Moissan has become the most powerful synthetical and analytical engine in the laboratory of the chemist." Moissan, proceeded the president, had obtained substances whose very xeistence had been undreamt of. It was impossible to foresee the bounds to this new field of research.

In this same year the Royal Society awarded its Copley medal to Carl Gegenbauer, the Heidelberg anatomist, the Royal Medal to Archibald Geikie, "the most distinguished British geologist," and the Rumford medal was divided between Phillip Lenard and W. C. Röntgen, whose work paved the way for the discovery of radium several years later.

In 1903 Moissan was selected as Hofmann Medallist of the German chemical society; and in 1906 he was awarded the Nobel Prize for chemistry. The other Nobel winners for 1906 were J. J. Thomson, the distinguished English physicist, Camillo Golgi, of Pavia and Ramon y Cajal, of Madrid—both anatomists, Carducci, the Italian poet, and Theodore Roosevelt.

"Moissan," says Ramsay, who knew him well, "was a practised speaker and a perfect expositor. His lectures at the Sorbonne were crowded with enthusiastic students, all eager to catch every word, and he kept their attention for one and three quarter hours at a time by a clear, lucid exposition, copiously illustrated by well-devised experiments.

"His command of language was admirable; it was French at its best. The charm of his personality and his evident joy in exposition gave keen pleasure to his auditors. He will live long in the memories of all who were privileged to know him, as a man full of human kindness, of tact, and of true love of the subject which he adorned by his life and work."

At five in the afternoon the doors of the big lecture room were opened, and the students made a rush for front seats. For the next fifteen minutes, until the appearance of the professor, the young men passed the time by shouting and singing songs. Punctually at five-fifteen Moissan would walk in, and immediately a prolonged sh— sh— resounded through the hall. Woe to the student who made his appearance after five-fifteen! The booing and stamping left the late intruder in no false notion as to the opinions of his fellow-students.

Moissan was little of a speculator. His papers are remarkably free of theories; they record merely the work done in the laboratory, and the conclusions to be drawn from such work. But it does not follow that

Moissan had no definite goal in mind, or that he failed to grasp the significance of facts and theories. On the contrary, few men have followed up clues so systematically, or drawn such sound conclusions from their work. But Moissan was essentially a "practical" man, who loved to handle things in the laboratory, rather than speculate about them in his office. He is the author of no hypothesis, of no theory;—certainly of no law; but as an experimenter few have rivalled him.

"Je me suis appliqué," wrote Moissan, "à cultiver cette chimie minérale que l'on croyait épuisée, et je pense que mes travaux, ainsi que le belle reserches des savants anglais, ont pu démontrer que cette science réserve encore bien des dècouvertes a ceux qui voudront l'aimer et l'étudier avec tenacité."

Moissan's fame attracted foreign students, particularly after his invention of the electric furnace, which opened up such vast possibilities in research at universities and industrial plants. In 1899, in addition to a number of French workers, Moissan had in his research laboratory two Germans, one Austrian, one Englishman, one American and two Norwegians.

Despite research which was often not quantitative in character, and usually planned on an industrial scale, Moissan insisted upon scrupulous cleanliness in the laboratory. A few drops of water on the laboratory floor would make Moissan exclaim, "Qui a fait cela?" He certainly gave the lie to Riess's remark that chemistry is the dirtiest part of physics!

With his wife and his son, Louis—his only child—Moissan spent his vacations travelling through picturesque parts of Europe. But as a representative of the French Academy, his trips were often extended to include centers of learning. Thus in 1904 we find him at the St. Louis Exposition in company with such distinguished foreign delegates as Hugo de Vries, Ramsay, Arrhenius, Ostwald, etc.

Moissan died in 1907 from an acute attack of appendicitis. There can be little question that the inhalation of toxic gases such as fluorine and carbon monoxide—the latter a by-product of the electric furnace—shortened his life by a number of years.

"My life," said Moissan towards the close of his career, "has been of the simplest—happy in my laboratory and in my home."

G. B. Shaw, in his preface to *Overruled*, tells us that "industry is the most effective check on gallantry." That certainly helps to explain why research workers in science are, almost without an exception, very happily married.

On August 10, 1915, Louis, Moissan's only son, died on the field of battle. The young man who, prior to the outbreak of the war, was an assistant at the college made famous by his father, the *Ecole de Pharmacie*, left to this institution the capital sum of 200,000 francs for the foundation of two prizes—one for chemistry (*prix Moissan*), and one for pharmacy (*prix Lugan*), in memory, respectively, of his father and mother (*née* Lugan).

References

Paul Lebeau, one of Moissan's assistants, wrote a very comprehensive review of the life and labors of his master (1). Alfred Stock, another of Moissan's students, is the author of an equally good obituary notice (2). Sir William Ramsay's *Moissan Memorial Lecture* (3) is a rather poor specimen of the gifted Englishman's productions.

Moissan's researches on fluorine have been published in book form (4). His work on the electric furnace (5) devotes a chapter to his experiments on the diamond. Sir William Crooke's article on artificial gems in the *Encycl. Britannica* (6) is well worth consulting.

1. Paul Lebeau: Henri Moissan. *Bulletin de la société chimique de France* (Paris), *3*, 1 (1908).
2. Alfred Stock: Henri Moissan. *Berichte der deutchen chemischen Gesellschaft* (Berlin), *40*, 5099 (1907).
3. Sir William Ramsay: Moissan Memorial Lecture. *Journal of the Chemical Society*, *101*, 477 (1912).
4. Henri Moissan: Le Fluor (Libraire Armand Colin, Paris. 1914).
5. Henri Moissan: Le Four Electrique (G. Steinheil, Paris. 1897).
6. Encycl. Britannica, 11th ed.

MARIE SKLODOWSKA CURIE

"FRANCE," says Anatole France, "has two geniuses—Rodin and Madame Curie." The foremost scientist of France, and the greatest woman scientist in the history of mankind, she counts politically less than many a man fit for the lunatic asylum. And as if to encourage that conception of woman to which so many men cling tenaciously, the French Academy, numbering among its members the *élite* of French intellect, decide that woman, be she ever so much a genius, cannot be admitted into their sanctum. If further proof were needed that intellect often runs counter to freedom, and that scientists who work so strenuously for an enlargement of their scientific horizon often belong to the most reactionary group in politics, the case of Madame Curie affords an excellent example.

Within the space of ten short years this woman has created a new science, radioactivity, and this has opened up more fertile chemical soil than any other discovery in the history of science. It has given us the first clear insight into the chemist's promised land, the nature and possible structure of the atom, and holds possibilities which could hardly have been hoped for from the accumulated labors of scientists during the last hundred years. In speed of progress radioactivity is to the science which has gone before what the aeroplane is to the tortoise.

This momentous discovery belongs to Madame Curie. To be sure, the way was paved for her by many; to be sure, her husband was a good helpmate; but in spite of analogous work in various parts of the world by the world's most gifted scientists, this woman triumphed where all others failed, and to her belongs the reward. Since her great discovery towards the close of the eighteenth century, her researches on radioactivity have but added to her glorious reputation, so that to-day she stands crowned as the greatest woman and among the very greatest scientists of all times.

The inherent qualities which go to the making of genius certainly never have been the exclusive possession of half mankind, but whereas the male geniuses have, at times, been allowed to blossom, the females belonging to this species, have until recently, been suppressed with a Cossack's ferocity and a Cossack's justice. The past four years of critical history from which mankind has just emerged will, perhaps, help to remove the mental fog which has incapacitated many a man from using his brains to the advantage of himself and of the world.

Madame Marie Sklodowska Curie was born in Varsovie or Warsaw, Poland, on November 7, 1867. Her father, Dr. Sklodowski ("squadoffski"—to give it the Polish pronunciation) was a professor in the gymnasium of the town, and locally known as a good teacher and sound scholar. The death of her mother left little Marie much adrift, though a brother and sister were there to share the misery; and were it not that from her earliest years a magnetic force attracted her to the father's laboratory, Marie would have been left much to herself, for her father's life was his work. As it was, the girl's love for science made the father her worshipper, and until she was old enough to attend school, Dr. Sklodowski was her sole teacher.

The part of Poland in which Marie lived had become part of Russia, the two remaining portions having gone to Russia's appetizing neighbors, Germany and Austria. It was bad enough for a Russian to have lived in Russia under the Czar's regime, but for the Pole conditions were about as intolerable as for the Jew, and the sensitive girl, fired by her father's patriotism, came to hate the Russian persecutors with the zeal of a religious fanatic. Revolution was in the air; everybody who was anybody—the Pole and the Finn because of the Russian, and the Russian because of the autocracy—was a revolutionist, ready at any time to taste misery in Siberia for the holy cause. Marie joined the ranks. Meetings were held, plans drawn, and prayers offered for the success of the independent movement. Unfortunately, the police got wind of the affair. A number of Dr. Sklodowski's students were among the ringleaders, and Marie herself was more than a mere onlooker.

This led to her decision to leave Poland. Her first intention was to proceed to Cracow, the seat of an historic university. Cracow, the ancient capital of Poland, was now part of the Poland belonging to Austria, whose rule, however, was quite benevolent as compared to the rule of her Russian neighbor. Here, unlike Warsaw, the Polish language was allowed, and Polish history and literature cultivated.

But Marie had visions. She wanted a bigger university still, and a bigger town, yet a town that would remind her of her beloved Warsaw. Paris was such a place. Even as far back as 1810 Napoleon had recognised the relationship, for he said, "Varsovie et une petite Paris." To Paris then went Mlle. Sklodofska, just as many of her countrymen had done before.

Times change. In those days Mlle. Sklodofska would hardly have dared to hope that within fifty years her beloved fatherland would come into its own again, and, as a buffer power between Russia and Germany, help to preserve the peace of Europe. Chopin and Sienkiewicz no longer live to witness this glorious day, but Conrad from London and Mme. Curie from Paris can watch Poland's revival and its effort to rehabilitate itself among the nations.

Miss Sklodofska did not arrive in Paris as a conquering hero. Far from it. Her pockets were empty and her acquaintances few. She established herself in the "east side" section of the town, in a small back room, four flights high, to which she carried her own coal. Her diet consisted of bread and milk for so long that, as she herself has said, she had to acquire anew the taste for wine and meat. Ten cents were her daily expenses, and this she made largely by private tutoring, and later, by preparing the furnace and washing bottles at the Sorbonne.

To other geniuses, Ramsay and van't Hoff, for example, such struggles were unknown. They were given what they wanted and were encouraged to do their best. The struggle for existence was not a problem to them. To Mme. Curie, once outside her father's home, this struggle became paramount. Yet to conclude from this, as many wiseacres are fond of telling us, that the struggle made the woman, is as near the truth as to conclude that its absence made Ramsay or van't Hoff. Material comforts make the path easier, and their absence make it infinitely more difficult. That Madame Curie did not succumb, as many another budding genius has under like circumstances, is an accident as a result of which the world has been made much the wiser.

In those days the head of the physical science department at the Sorbonne was Gabriel Lippmann, whose pioneer work in color photography is known wherever physics flourishes. He was attracted by the superior knowledge which Miss Sklodofska showed in the execution of her work, which developed from washing bottles to setting up apparatus. Henri Poincaré, the great mathematical philosopher, and a brother of the late president of France, was another one upon whom this young girl had made an impression. They acquainted themselves with her history. Lippmann got into touch with her father in Warsaw. The result was that Marie was put into the hands of Pierre Curie, one of Lippmann's most promising pupils.

Given a scholar, an impressionable young man, one who had met few people and who had become absorbed in his work, and a bright girl, with a personality, and a keen interest in the same type of work; given further that the man and the woman see one another daily for the greater part of the day, and the possible outcome might have been forseen. "What a grand thing it would be to unite our lives and work together for the good of science and humanity," runs one letter from Pierre. "For the good of science and humanity" smacks of too much altruism in a marriage proposal, but innocent Pierre Curie meant well, and Miss Sklodofska understood and sympathised and accepted.

So in 1895 the two were married, both poor in life's necessities, but rich in sympathy toward, and understanding of one another. Curie continued his researches on the construction and use of electrometers and condensers, and Mme. Curie assisted in this, and also prepared herself for her degree. Within three years she gained her *licenciée ès Sciences mathêmatique et ès Sciences physiques*, and unlike Pasteur or Ehrlich, who made a poor impression on the examiners, Mme. Curie passed her examination in brilliant style. Here again no moral should be drawn; not all poor students become Pasteurs, nor do all senior wranglers become Curies.

We now come to Madame Curie's immortal piece of work. To get the proper perspective a short introduction is necessary.

From about 1860 on, many interesting but disconnected observations had been made on the passage of electricity through a tube from which nearly all the air had been pumped out. In 1879 Sir William Crookes discovered that peculiar rays were emitted from the negative pole, to which he gave the name "cathode rays." Much later J. J. Thomson and others showed that these rays were negative particles of electricity, or "electrons," each electron weighing about one two-thousandth that of the lightest atom known, namely hydrogen.

Then, in 1895, came Röntgen's discovery of the X-rays by impinging the cathode rays on the walls of a glass vessel. The application to medicine of these X-rays was immediately recognised when it was noticed that they could penetrate flesh. Röntgen made the further observation that the X-rays act on photographic plates in their neighborhood.

One year later Becquerel, studying the general behavior of phosphorescent bodies, had occasion to examine the element uranium and its compounds, and these substances gave off rays which resembled the X-rays in their affect on a photographic plate. He further made the extremely important observation that the rays "ionised" the air about them; or, what is the same thing, converted the air about them from an insulator to a conductor of electricity. A gold-leaf electroscope, which had been previously charged with electricity so that its two leaves diverged, was discharged with the consequent collapse of the leaves so soon as uranium, or one of its compounds, was brought near it.

This brings us to Madame Curie's work. Adopting Becquerel's method of detecting the presence of these rays by their action on a gold-leaf electroscope, she made a systematic investigation of various elements and their compounds with the view to finding whether any of them possessed this ray-emitting power. Only one other apart from uranium, namely thorium, was found to possess such a property.

But the next observation was a momentous one. Madame Curie noticed that a sample of pitchblende, a mineral from which most of the uranium is extracted, showed an activity which was four to five times as great as the activity produced by the total amount of pure uranium that could be extracted from this sample.

There was but one thing to conclude from this, and that was that some other element, more active than uranium, was present in the pitchblende.

The work until this point had been done by *Madame Curie exclusively*. From now on her husband joined her.

It required but little calculation to show that the unknown element, if present in the ore, would be there in extremely minute quantity; the importance, therefore, of starting with large quantities of pitchblende in order to extract the element from it was obvious.

Through the kindness of the Austrian government, which owned the extensive uranium mines in Joachimsthal, Bohemia, the Curies were presented with one ton of pitchblende from which the uranium had been removed.

Most of the common, and quite a number of the uncommon elements are present in pitchblende, so that the analytical procedure of separating one element from another, and examining each fraction so obtained, is a tedious and difficult one.

The plan adopted by the Curies was to submit each fraction to the electroscopic examination. Naturally the greater the conductivity, the more active the fraction. In this way a constant and invaluable check on the experiments was always at hand.

The large quantity of raw material made it necessary to conduct the initial experiments in a factory. The quantities were gradually narrowed down until the test tubes of the laboratory could hold them comfortably. The fraction containing the common element bismuth showed the presence of a powerful radioactive substance, which, after many trials, was partially separated and named *polonium*, in honor of Madame Curie's native country.

Further examination showed that the fraction containing the element barium had even more powerful radioactive properties, and by some of the most exhaustive and painstaking experiments in the history of our science, recalling those of Welsbach on the rare earths, Madame Curie succeeded in separating a salt of barium from the salt of the new element, to which she gave the name of *radium*. Radium as an element had baffled all attempts at isolation in the pure state until 1910, when our heroine solved this problem, but even the salt of radium showed itself to be two and a half million times as active as uranium!

The radiations from radium were shown to ionise air, to act on photographic plates, to change the color of minerals and gems, to impart a deep violet color to the glass tube which contained the radium salt, to convert ordinary oxygen to its more active form, ozone, to produce traces of peroxide of hydrogen in the presence of water, to destroy minute organisms, and to kill cells of skins and produce sores.

That radium is really a new element, and not some compound or mixture, is proved beyond doubt by the very distinctive spectrum it gives. The wave-lengths of the lines of this spectrum are mathematically connected with the spectra given by the elements barium, calcium and strontium, and this relationship, together with its similarity in chemical property to barium, places radium in the class of what are known as *alkaline earth* metals.

The subsequent development of radioactivity has been due to the labors of many workers in many countries. Besides Madame Curie and her husband, one may mention their assistant Debienne, Rutherford, Soddy and Ramsay in England, and Boltwood in America.

The value of this work may be gauged by the recognition these men have received. Rutherford has lately succeeded J. J. Thomson to the Cavendish Professorship of Physics at Cambridge, and Soddy has made rapid jumps from a lectureship at Glasgow University to a professorship at Edinburgh, and within the last few months, to a newly-created chair of chemistry at Oxford. Boltwood has been made director of the chemical department at Yale University. The reputation of all three rests primarily upon their researches in radioactivity.

A brief general account may now be given.

Radium gives off three types of rays, and these are distinguished by the Greek letters α, β, and γ. The α-rays have been shown to be atoms of helium which are thrown off with a velocity of thirty thousand kilometers per second, or about one tenth that of light. That helium is one of the products obtained from radium has been shown by the work of Ramsay and Soddy (which see).

Unlike the α-particles, which are charged with positive electricity, the β-particles are negatively charged ("electrons"), and are shot out with a velocity equivalent to light. They are identical with Crookes' "cathode rays."

A powerful magnetic field will bend the α-rays in one direction and the β-rays in the opposite direction. The magnet has no effect upon the γ-rays. These last are identical with the X-rays. The X-rays are further distinguished by their penetrating power. Whereas the α-particles are stopped by a sheet of paper or aluminium foil one two-hundred-and-fiftieth of an inch in thickness, and the β-rays pass through gold-leaf and through aluminium foil up to two-fifths of an inch in thickness, the γ-rays penetrate thick layers of metals.

The stoppage of these various particles by the air molecules with which they come in contact generates much heat. One of the most remarkable things about this remarkable element is that the temperature around radium is about three degrees higher than the temperature beyond its immediate neighborhood. To put this in another way, radium emits every hour enough heat to raise the temperature of its own weight of water from the temperature of ice to that of the boiling point of water. And what is more amazing still, its heat-generating power seems to be inexhaustible.

In 1902 Rutherford and Soddy advanced their "disintegration" theory, which leads us to believe that the α-particles obtained from radioactive elements such as radium and uranium are due to the disintegration of the *atoms* of these elements. All subsequent studies have brilliantly confirmed their hypothesis. Whereas chemical changes are changes brought about *between* atoms, radioactivity results from the changes within the atom, and unlike chemical reactions, we have no known methods of controlling radiactive changes. We cannot start them and we cannot stop them. The temperature of the electric arc is as ineffective as a temperature of two hundred degrees below zero. No appliance known to man, no operation known to the scientist, shows any results which our senses can recognise.

This opens up a new area which in size to that already explored may be compared to the size of America with reference to the rest of the earth. Indeed, Madame Curie is the Columbus who has discovered another continent in science.

For what are the possibilities? In the first place, radium has had a profound influence in modifying our views regarding the structure of matter. Dalton many years ago had postulated in his Atomic Theory that matter is made up of ultimate and indivisible particles which he called atoms. These atoms are active in chemical changes, but even in these changes the atoms do not become subdivided. We still agree with Dalton that chemical changes are brought about by atoms, and that these atoms do not subdivide in the course of such changes, but we can no longer say that the atom is the smallest particle. Far from it. The later researches of J. J. Thomson and others lead us to the belief that each atom is a solar system unto itself, with a positively charged nucleus for its sun, and negatively charged electrons, representing the planets, etc., surrounding it.

The radioactivity of the elements thorium, uranium and radium is due *to the breaking up of their atoms*, with the consequent enormous liberation of energy. Aside from these three, no other element shows any such properties. May it not be possible, then, that in the future some means will be found to cause the atoms of other elements to disintegrate, and thereby to liberate the enormous energy which must be stored in them? Will the energy of the future depend upon this discovery? The burning of coal is a chemical change, and therefore extra-atomic; will the energy of the future be intra-atomic?

One other factor must be touched upon. If a radium salt is heated strongly, or dissolved in water and the water evaporated, the residue seems to show little radioactive power. If this residue be kept for a month it can be shown to have recovered all its lost power. This experiment can be repeated indefinitely.

If now the experiment is conducted a little more carefully, it can be shown that the initial loss of radioactivity is due to the escape of a gas which evolves the rays, in quality and quantity, that the residue has lost. This gas or "emanation" was carefully examined by Ramsay and shown to be a new element belonging to the inert gases of the atmosphere, to which the name of *niton* was given (see Ramsay).

The further interesting fact was brought out that on standing, the "emanation" gradually loses its radioactive power, and its rate of loss is strictly proportional to the rate of gain of radioactive power in the solid radium residue!

The transmutation of one element into another—the dream of the alchemists when they wanted to transmute the base metals into gold—is an established fact to-day. Radium, we know, breaks up into two other elements, niton and helium; the niton breaks up still further into a simpler element, and also gives off an atom of helium.[1]

[1] Recently (June, 1919) Rutherford has performed some experiments which lead him to the conclusion that when the element nitrogen is bombarded with α-particles "the atoms arising from the collision . . . are not nitrogen atoms, but probably charged atoms of hydrogen [another element] . . ." The importance to be attached to this observation is that for the first time since the discovery of radioactivity, a method has been devised by which an element may be *deliberately* converted into another element. Hitherto the observed cases of transmutation—such as disintegration of radium cited above—have been those over which man has so far had no control.

The process has been traced experimentally through quite a number of stages, but the peculiar feature of this disintegration process is that at each step an atom of helium is set free. Why just helium? This is one of several puzzles that awaits solution.

Coming to more immediate and practical considerations, the application of radium in the treatment of a number of diseases, particularly those due to growths, such as cancer, has come to the foreground. Definite cures have not yet been established, but many well-endowed establishments, such as the Crocker Research Institute of New York, and the Radium Institute in Paris, are devoting much time and skill to experimental conditions.

Such then is this fascinating study which has led us on our journey from the minutest particles which the eye can see (minute suspensions) to particles which the eye can see only with the help of the most powerful ultra-miscroscope (colloids), and then on to molecules which are formed when a substance like sugar is dissolved in water, and which never have been seen by mortal eye, and still further to the atoms formed when molecules break up, and yet still further to the electrons which result from the breaking up of atoms, and which in size are one two-thousandth that of the lightest atom known. If astronomy sees the infinitely big in such distances as those from the earth to the nearest fixed star, chemistry and physics approach the infinitely small in comparing the size of man with that of the electron.

Madame Curie's pioneer work on radium lasted from 1898 to 1902—some four years. In 1903 the results of her work were presented to the Paris faculty in the form of a thesis for the doctor of science degree. The title page reads, *Thèse Présentée a la Faculté des Sciences* *de Paris pour obtenir le grade de Docteur ès Sciences physiques.*

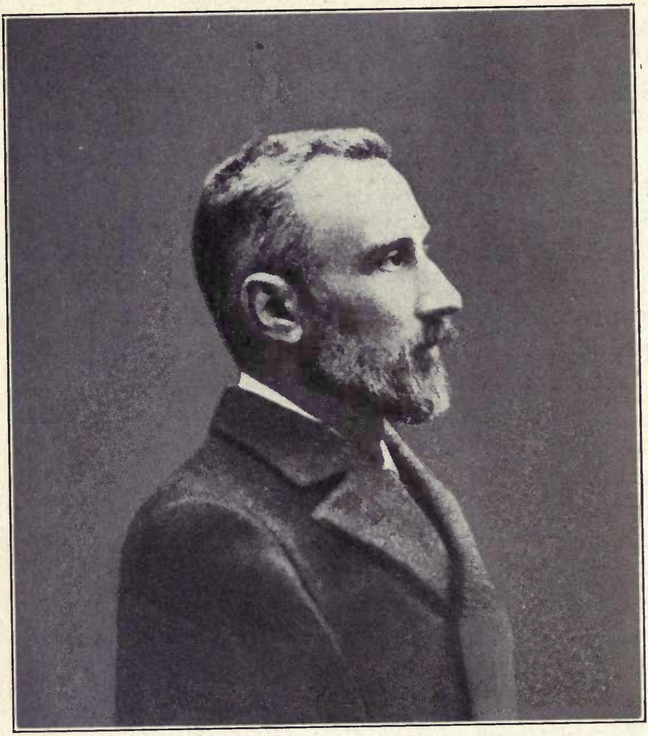
This thesis, unlike Arrhenius's, was received with acclamation. The reason for this is not hard to seek. Arrhenius proposed a novel theory which very few were prepared to understand. Madame Curie, on the other hand, presented the results of experiments on a subject which was engaging the attention of some of the best minds in Europe. The world was prepared for it; the world was not prepared for the theory of electrolytic dissociation.

In the history of doctor's dissertations Madame Curie's easily takes first place for importance of contribution, with Arrhenius's as a close second; many of the others—including even van't Hoff's and Ramsay's—have unnecessarily taxed the shelf capacity of our libraries.

With a bound Mme. Curie leaped from complete obscurity to the center of the world's stage. Unlike most scientific theories or discoveries, radium lent itself freely to sensational newspaper "write-ups," so that this modest little woman was discussed in parallel columns with the prominent politician and the stage beauty. Since natural repugnance for the limelight made it impossible for reporters to get interviews, the imagination came into free play, and a halo of romance and mystery was thrown over her. In the middle ages she might have been a sorceress; now she was a wizard in science.

In the same year—that is, 1903—Madame Curie and her husband came over to London at the express invitation of Lord Kelvin, and Monsieur Curie delivered an address on radium at the Royal Institution. The Curies were presented with the Davy Medal of the Royal Society.

How little known the Curies were until about 1903 is shown by the following account, due to Mrs. Hertha Ayrton, herself a distinguished English physicist: "I



P. Curie

was chatting in the laboratory [in London] one day about the year 1900, when a stranger entered, a Monsieur Becquerel, whom I had known previously, and he announced that he had with him a new element, 'radium.' He produced a little packet containing a substance which he said was radium bromide. He subjected the substance to a chemical test for our information. Someone asked him who discovered it. He replied, 'Madame Curie of Paris.' This was the first time I had heard of Madame Curie."

Within the next few months the Nobel Prize, the highest mark of distinction that can come to any scientist, was divided between the Curies and Becquerel.

In the following year Madame Curie was appointed *Chef de Travaux*, or chief of the laboratory, in the department at the Sorbonne that was especially created for her husband.

For two more years were M. and Mme. Curie to live together, loving and working, and living as happily as any man and woman ever have lived. Then one day, early in 1906, after having lunched and chatted with his intimate friend, Professor Perrin, Pierre Curie left him and crossed the Rue Dauphine in Paris "whilst that thoroughfare was, apparently, crowded with vehicles." He was knocked over by one of these vehicles and instantly killed.

This terrible accident well nigh resulted in Madame Curie's death. For months her state was such that her friends gave up all hope of any recovery. Slowly she found herself again. Her two children and her science had saved her, and to these she consecrated her life.

Langevin, their friend, has this to say of M. and Mme. Curie's marriage: "Cette époque marque un changement profond dans son [Pierre Curie's] existence par son mariage avee Mme. Marie Sklodowska. . . . Il est difficile, en effet, d'imaginer une union plus intime que celle, plus étroite chaque jour, où ils eurent tous deux la joie de vivre onze ans. Avec la clarté de son esprit sincère, Curie avait senti ne pouvoir réaliser entierement sa vie que grâce à une femme qui fût en même temps sa collaboratrice. Ce serait une belle chose à laquelle je n'ose croire, écrivait-il quand il êut trouvé celle qu'il espérait de passer la vie l'un près de l'autre hypnotisés dans nos rêves."

Henri Poincaré, as president of the *Académie des Sciences*, delivered an address on Pierre Curie's life and work in which the following reference was made to the widow: "Dans le deuil où nous sommes tous plongés, notre pensée va à cette femme admirable qui ne fut pas seulement pour lui une compagne dévouée, mais une précieuse collaboratrice."

Madame Curie's work on radium has continued without a break. In 1910 she, in conjunction with her assistant, Debierne, succeeded in isolating and determining the properties of the metal itself, and radium in the chemical sense was shown to have properties resembling closely those of calcium. In the same year she published her *Traité de Radioactivité*, which covers over a thousand pages, and is the most exhaustive and authoritative work on radium that has thus far been published. With no little pride could Mme. Curie say in the preface! "La Radioactivité constitue aujourd'hui une branche importante et independante des sciences physico-chimiques." And this "important and independent branch of physical chemistry" was originated and developed within the space of fourteen years!

In 1911 Madame Curie was again the recipient of the Nobel Prize, the prize for literature going to Maeterlinck. So far Madame Curie is the only individual who has received the award more than once; this in itself speaks volumes as to her standing in the eyes of her fellow-scientists. Prof. E. W. Dahlgren, the president of the Swedish Royal Academy, had this to say in presenting Mme. Curie for the award: "This year the Academy has decided to award you the prize for chemistry for the eminent services you have rendered the science by your discovery of radium and polonium, and by your study of the properties of radium and its isolation in the metallic state. . . . Since the inception of the Nobel Prize twelve years ago it is the first time that this distinction has been accorded to a laureate who has already once received the prize. I want you to see, Madame, by this circumstance a proof of the importance which our Academy attaches to your discoveries. . . ."

In this same year the French Institute dishonored itself by refusing to elect Madame Curie to membership. To the honor of the Academy of Sciences, which is one of the five academies of the French Institute, the representatives of this body placed Mme. Curie at the head of their list of final candidates. This gave rise to a lively discussion on the eligibility of women for membership when Mme. Curie's name was brought before the one hundred and fifty Academicians at the quarterly meeting of the five academies. The motion to admit women was finally rejected by 90 to 52, and this august body went on record to the effect that whilst they did not wish to dictate to the separate academies, there was "an immutable tradition against the election of women, which it seemed eminently wise to respect." Science in its search for truth has thrown tradition overboard on innumerable occasions. But it is one thing to defy the "immutable tradition" of man's origin, and another to deny civil rights to his own flesh because of this same "immutable tradition." Such logic diplomatists might envy, and some newspapers applaud, but it can hardly stand the test of that scientific criticism which these Academicians apply with such telling effect to their scientific work.

Shortly before the outbreak of the world war the University of Paris undertook the creation of a radium institute for research in radioactivity. This has since been completed and Madame Curie has been placed at its head. The Institute is divided into two departments, the Curie Laboratory, devoted to research in the physics and chemistry of the radioactive elements, and the Pasteur Laboratory, devoted to the application of radioactive substances to medicine. The street has been appropriately renamed the "Rue Pierre Curie." Even during the war this institute was the headquarters for all work in radiology at the French military hospitals, supplying not only the necessary materials, but training apprentices in the methods of application. The French government placed Mme. Curie in absolute charge of all such work.

Just now Mme. Curie is supervising the construction of a radium institute in her native city of Warsaw. If Paris is her father Warsaw is her mother.

Even after her marriage Madame Curie's struggles were not ended. As late as 1904 the joint income of the Curies was such as to make the simplest life not particularly easy. At that time, we are told, the "dismal Boulevard Kellerman" was not the safest of neighborhoods, and the Curies, who lived there, were in a section of Paris "inhabited by a class of Russian students of both sexes, who are never favored with invitations to their embassy." The furniture in the modest little house was of the simplest, with all ideas of the aesthetic sacrificed for the useful. Later, when circumstances improved, the Curies acquired a small estate at Fontenay-aux-Roses, near Paris, and here

Mme. Curie, together with her two children and old Dr. Curie (her late husband's father) lives.

"In outward appearance," writes Mrs. Cunningham, "she is tall, just above middle height, broad shouldered and graceful. Her brow is splendid; her lovely grey eyes full of sadness. Her mass of fair hair is wavy, like Paderewski's hair. There is a suggestion of squareness in her face, very firm mouth and chin, but there is gentleness withal. Her voice is musical, and to her intimate friends she can sometimes be persuaded to recite poetry, which she does, using the tones of her voice with charming inflections. . . . In manner she is perfectly simple and unaffected. Like so many Polish women, she has a magnetic personality and an intense love of beauty, for beauty in nature and art. Seeing her one May morning in the classic hall of the Sorbonne, with her long trailing diaphanous draperies, she suggested strongly to me a similarity to the old Greek statue of Demetes, the goddess whose face suggests strength and sadness. I would that Rodin thought so too and gave expression to that thought."

This description probably reflects a somewhat overabundant enthusiasm. At any rate, years of grief and ill-health have left their impress upon Mme. Curie. A representative of the *Figaro* speaks with something nearer the truth when he describes her as "like something washed out, the color gone, the fire extinguished. . . . One is tempted to say her eyes are grey until a closer inspection brings out a trace of blue; but in the end the hue of these frigid orbs relapses into a sheer neutrality."

Her complexion, we are told, is neither pale, nor red, nor sallow, but faded; her hair is neither auburn, nor brown, nor grey, but neutral. The prominence of the cheek bones bespeaks Polish origin. "Madame

Curie looks like a person in need of the sun, a person who would benefit from more fresh air."

Her voice is low and free from theatricality. Her manner is decidedly cold; in fact her coldness "suggests the passionless spirit of pure science"—a view hardly supported by the few who are her intimates.

As a lecturer Mme. Curie is unsurpassed in lucidity of expression, and from the tricks of political oratory she is quite free. Her voice is hardly ever raised beyond the regulated academic level, and her arms, which are long, slender and graceful, are rarely called into play, even when emphasis is sought. Her accent betrays her Polish origin, but she expresses every idea in perfectly idiomatic French.

In 1907, one year after her husband's tragic death, and after she had succeeded to the chair which her husband had held at the Sorbonne, Mme. Curie delivered a discourse on polonium, which is still remembered even in fashionable Paris circles of to-day. Lord Kelvin, Sir William Ramsay and Sir Oliver Lodge made a special trip from London to hear this great little woman. Even the unfortunate King Carlos of Portugal was attracted. President and Mrs. Fallières headed a crowd which was representative of the wealth, fashion and cosmopolitanism of the gay capital of France. "On the stroke of three an insignificant little black-robed woman[1] stepped in, and the vast and brilliant throng rose with a thrill of homage and respect. The next moment a roar of applause burst forth. The timid little figure was visibly distressed, and raised a trembling hand in mute appeal. Then you could have heard a pin drop, and she began to speak."

Mme. Curie may be the great scientist, but she has many of the traits of feminity and motherhood which

[1] Mrs. Cunningham, saturated with a reporter's romance, describes Mme. Curie as "tall, just above middle height."

most men of all ages have admired. Aside from her work, her attention is devoted almost exclusively to the welfare of her two daughters, Irene and Eve, seventeen and thirteen years old respectively. Irene cares little for science, but much for music and the arts, but little Eve is all for laboratory work. Already to-day she assists her mother in much the same way that Madame Curie, years ago, assisted her father in Warsaw.

When the two children were younger Mme. Curie made all their dresses, and washed and ironed the more delicate pieces of lingerie. In so far as she herself is concerned, Mme. Curie gives little thought to her own appearance. She is excessively neat, as becomes the nature of her work, but her dress is of the simplest, which changes not when fashion changes. The first and only time that "Madame" indulged in *a décolletée* silk dress was when she was invited to dinner by President and Mrs. Loubet. Gossip has it that this "fancy" dress has served as useful a purpose as the young lady's customary wedding gown.

Mme. Curie's sister, Dr. Dluska,[2] has charge of a sanitarium at Zakopane, a famous retreat in the Carpathians, and there, in days gone by, Sienkiewicz, Paderewski and Mme. Curie spent their summers, dreaming of the rebirth of a nation. *Jescza Polska nie zginela* (Poland is not yet lost) runs the first line of Poland's national song. Mme. Curie continues to spend her summers at Zakopana; one of the other two is dead; and the third has just retired from the presidency of

[2] Mme. Curie also has a brother, Dr. Sklodowski, who practices medicine in Warsaw. Lest the reader be somewhat confused, we hasten to add that "ski" is the masculine, and "ska" the feminine ending in Polish; hence Mlle. Marie Sklodowska. The rumors that the Sklodowski family is of Jewish origin are not true.

the old-new country whose chief glory is that it has given birth to Marie Sklodowska Curie.

References

Part of the material for this biography has been obtained from private sources. Miss Cunningham's account (1) has good personal touches but is quite worthless scientifically. The same may be said of the articles by Emily Crawford (2) and W. G. Fitzgerald (3). Some sidelights on Madame Curie are given by Paul Langevin (4) in his account of Pierre Curie. For a layman desirous of an intelligent description of radium and its significance, Soddy's *Matter and Energy* (5) stands alone in the English language. A more technical account may be found in Rutherford's article prepared for the 11th edition of the *Britanica* (6). The beginner in inorganic chemistry can hardly do better than consult Smith's *Introduction* (7). The more comprehensive works of Soddy (8), Rutherford (9), and Curie (10) are the standard reference books.

1. Marian Cunningham: Madame Curie (Sklodowska) and the Story of Radium (Saint Catherine Press, London).
2. Emily Crawford: The Curies at Home. *The World To-day*, *6*, 490 (1904).
3. W. G. Fitzgerald: Madame Curie and her Work. *Harper's Bazaar*, *42*, 233 (1908).
4. Paul Langevin: Piere Curie. *La Revue du Mois*, *2*, 5 (1906).
5. F. Soddy: Matter and Energy (Henry Holt and Co.).
6. Ernest Rutherford: Radioactivity [Encycl. Britannica, *22*, 794 (1911)].
7. Alexander Smith: Introduction to Inorganic Chemistry (Century Co. 1917).
8. F. Soddy: The Chemistry of the Radio-Elements (Longmans, Green, and Co. 1915).
9. Ernest Rutherford: Radioactive Substances and their Radiations (Cambridge University Press. 1913).
10. Marie Curie: Traité de Radioactivité (Gauthier-Villars, Imprimeur-Libraire, Paris. 1910).

VICTOR MEYER

VICTOR MEYER belongs to the school of pure organic chemists—to the period when organic chemistry was in its ascendency. He easily takes his place among the foremost pioneers in this phase of the science. He began work when the superstructure of organic chemistry had yet to be built up, and in this building process few can claim the share he can. When the beauty and symmetry of the building was all but apparent Meyer passed away. The man of forty-nine (he had reached that age when he took his own life), with the rare mind that was his, could still have accomplished much.

Meyer was born in Berlin on September 8, 1848. His father, a prosperous Jewish merchant and a man of high intelligence, surrounded himself with the *élite* of the intellectual element of the city. The chemist Sonnenschein, then a *privat-docent* at the University; Bernstein, the founder and editor of the *Volkszeitung;* Franz Duncker, Love-Kalbe, Major Beitzke (author of the "Thirty Years' War"), Schulze-Delitzsch and Berthold Auerbach were frequent visitors to the house. It was in such an atmosphere that Victor Meyer was brought up.

Together with his brother, Victor received his earliest instruction from his mother. Later a private tutor prepared the children for the gymnasium, and this Victor entered when he was ten years old.

During these early years at the gymnasium, Meyer's leanings were rather towards literature than science. The drama especially had a strong attraction for him.

Indeed, at fifteen, the boy had quite made up his mind to become an actor. To his father's remonstrances, who watched these developments with much perturbation, Victor replied: "Never can I become anything else—never! I feel it. In any other profession I shall remain a good-for-nothing the rest of my life."

However, in the meantime the lad continued his academic studies, and in the spring of 1865 he passed his matriculation examination (*Abiturientenexamen*). Hoping against hope that possibly the university atmosphere would tend to direct Victor's thoughts in another direction, the family persuaded the youth to proceed to Heidelberg, there to attend some lectures in the company of his elder brother. What the incessant arguments of the parents and friends had failed to do, the chemical lectures of one of the professors easily accomplished. In Bunsen the young man encountered one of those rare minds who can see and demonstrate the beauty and poetry of anything they happen to be engaged in. From the lips of Bunsen chemistry issued forth as a song to nature, and as a song to nature Meyer caught the refrain.

Small, and quite childish in appearance, the seventeen-year-old boy enrolled as a student of the university. During the first semester he attended Hofmann's lectures in Berlin, so as to be near his parents. After that he took up his abode in Heidelberg. Here he followed Kirchhoff's lectures on physics, Kopp's on theoretical chemistry, Helmholtz's on physiology, Erlenmeyer's on organic chemistry, and Bunsen's on general chemistry—truly as illustrious a band of scholars as could be found anywhere.

Under the same roof there lived Julius Bernstein (the son of the family's old friend), who was at that time one of Helmholtz's assistants, and who, as professor of physiology at Halle, has since risen to be one of Germany's great physiologists. Bernstein and the Meyers fraternized much together. To this trio there was later added a fourth—Paul du Bois Reymond, then *privat-docent* in mathematics.

Meyer's work at the university was brilliant in the extreme: he headed the lists in every course. In May, 1867, when but nineteen years old, he received the doctor's degree *summa cum laude*—which is given on but rare occasions. Bunsen immediately appointed him to an assistantship, and here he chiefly busied himself with analyses of various spring waters by methods initiated or improved by Bunsen and his pupils.

In addition to his work at the laboratory, Meyer was much in demand as a coach for the doctor's examination. Yet he found time to cultivate his artistic tastes in many ways. From his earliest days he played the violin; now he began to take lessons in piano playing. The classics he assiduously cultivated, and never missed an opportunity of attending the more notable performances at Mannheim. His week ends were usually spent wandering near Heidelberg. Julius Bernstein, who often accompanied him on these excursions, tells of a pretty little incident that occurred to them on one occasion: "Towards evening, tired and weary after a day's tramping, we entered a wine cellar, and there sat down at one of the tables. A young peasant who happened to come in came up to us and asked permission to sit at our table. As we were chatting with him he fixed his eyes on Victor, stared at him for some time, and then exclaimed, 'See here, never in my life have I seen such a handsome fellow as you are.' Just quite in this way Victor was hardly ever addressed again, but it is a fact that the ladies were all more or less in love with him."

In the late sixties Baeyer had already established a reputation such as to attract students from all parts of the world, and it was to Baeyer's laboratory in Berlin (at the *Gewerbeakademie*) that Meyer proceeded in 1868. And what a busy and profitable place this proved to be! Baeyer himself had already begun his classic researches on indigo blue. Graebe and Liebermann had just produced alizarin artificially—the first instance of the synthesis of a plant-coloring matter. S. Marasse, B. Jaffe, E. Ludwig and W. A. van Dorp were all helping to make the laboratory famous.

The young Meyer made more than a favorable impression, according to Liebermann's testimony: "Meyer's remarkable ability could hardly pass unnoticed. His congenial personality added but to the esteem in which he was held. He seemed to have read everything, and his memory was simply phenomenal. . . . Many obscure references that at that time were rather difficult to locate could easily be traced by consulting Meyer. He could usually tell you not merely the volume but the very page."

During the three years that Meyer remained here he published several important papers, among which may be mentioned his contributions to the constitution of camphor, of chloral hydrate and of the benzene ring.

Towards the end of 1870, at Baeyer's recommendation, Meyer was appointed professor extraordinary at the Stuttgart Polytechnik, of the chemical laboratory of which H. v. Fehling was the director. Here the twenty-three-year-old professor, who had never been *privatdocent*, was put in charge of the organic chemistry department.

Stuttgart proved an incentive to renewed activity. Here he announced his discovery of the nitro compounds of the aliphatic series—his first really lasting contribution to the advancement of the science.

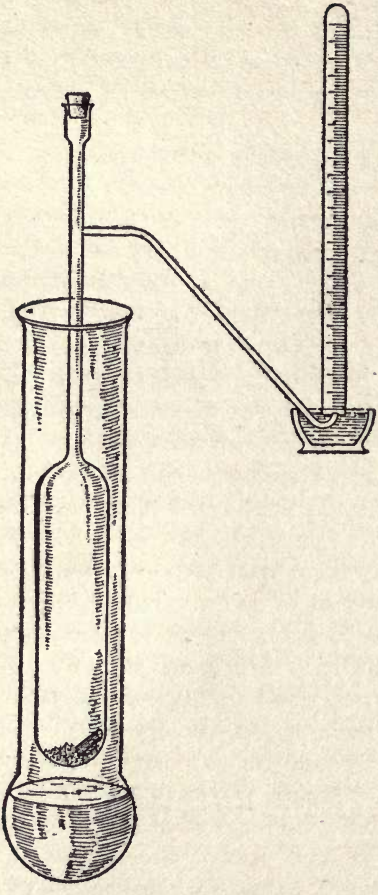
Though little burdened with routine at Stuttgart, Meyer was sorely tempted to accept a first assistantship at the University of Strassburg, offered him by Baeyer, who was about to take charge of the chemical institute there. On the one hand, there was the opportunity of once again coming in contact with the great master mind; on the other hand, he was to be put in charge of the analytical department, and this meant running around the laboratory and attending to the wants of the students the greater part of the day. In Stuttgart he therefore remained—till one day President Kappeler, of the Zürich Polytechnik, chanced to walk into his lecture-room. Kappeler was so impressed with the young man's ability that he immediately offered Meyer the vacant professorship of chemistry at Zürich. And so at twenty-four Victor became a full-fledged professor *ordinarius!*

This appointment Meyer celebrated in a highly appropriate way: he became engaged to the companion of his youth, Fraulein Hedwig Davidson.

The Zürich laboratory was divided into two parts, the analytical and the technical, and of the former Meyer had charge. His predecessor was Wislecenus, who had accepted a call to Leipzig. Bolley had control of the technicological side. With Bolley, as well as with Eduard Schär, the professor of pharmacy, and Ernst Schulze, the professor of agricultural chemistry, the newly-appointed instructor fraternized much. The researches that had been started at Stuttgart were now renewed with the utmost vigor. In the beginning all did not go well. A mercury compound of nitromethane which Rilliet, his private assistant, had prepared, exploded, with serious injury to Rilliet. Wurster was brought from Stuttgart to replace him, and Meyer found him a competent substitute. "I have given him rooms in the laboratory," he writes; "this is of the utmost importance, as thereby he can do twice as much work. He is very conscientious—so much so, that I think I shall send for another one of my Stuttgart pupils."

Satisfied as he was with the assistants he imported, Meyer was far from satisfied with the assistants he found, or with the cool reception accorded him by the students. In Stuttgart he was the idol of his pupils; here the men had little sympathy with one so much taken up with the theoretical aspect of the subject. "One single publication on some cheese preparation makes one far more celebrated in Switzerland than one thousand discoveries in the field of pure organic chemistry," he writes bitterly. But the day was to come when the Swiss were to venerate him, and the day was also to come when Meyer would love his Zürich students and the Zürich atmosphere.

From the very first he had his hands full. "I am very busy," he writes, "as you can conclude from the following: I devote eight hours to lectures in organic chemistry, two to lectures on analytical chemistry, two to metallurgy (in place of Kopp, who is in Vienna), and besides this I have to superintend Kopp's as well as my own laboratory." But this did not prevent him from pursuing his research work. In the month of July he records the synthesis of diphenyl-methane from benzoyl alcohol and benzene. This compound, which melts at 26° C., Meyer placed on his writing table, and used it in place of a thermometer. At ten in the morning, if the substance was in a molten state, the Herr Professor would announce that weather conditions made it impracticable to pursue any work in the laboratory; and then professor and students would go bathing. On one



V. Meyer's apparatus for determining the vapor density, a factor of extreme importance in deducing the constitution of compounds. [Reproduced from the *Berichte der deutschen chemischen Gesellschaft.*]

of these occasions Meyer rescued one of his assistants, Michler, from drowning.

But recreation played but a small part in the Zürich life. Apart from the regular students there were (in 1876) twelve men working for their doctorate, in addition to Meyer's four assistants, who had already passed that stage, but who were busier than any of the candidates creating new compounds. The nitro compounds of the aliphatic series, the first piece of classical research with which the name of Meyer is associated, were engaging the attention of the youthful professor; but even at that time he made excursions into the realm of indigo chemistry (the artificial production of which he hoped to solve in one week!) and discussed van't Hoff's views on optical activity and the asymmetry of the carbon atom.

With Baeyer, the great master, and with Graebe and Liebermann, Meyer carried on a brisk correspondence, the letters dealing chiefly with views on current scientific topics. In 1876 his elder brother obtained a position near Zürich and Victor's delight knew no bounds. Gustav Cohn, the economist, and Eduard Hitzig, the psychiatrist, were about this time appointed professors at the University. Graebe himself, who had been in delicate health, resigned from his Königsberg position and came to Zürich to join the happy crowd. But for a rather unpleasant polemic with Ladenburg (Meyer later dubbed this episode the *Ladenburg-Fieber*) which tended to undermine Meyer's delicate constitution, there was nothing at this time to mar the even tenor of the young man's life. He had just begun his second classical work: his method of determining vapor density. We find him writing to Baeyer asking for some methyl anthracene, a substance which by analysis can hardly be differentiated from ordinary anthracene, but which can easily be identified by the vapor density method.

In the spring of 1876 Meyer received a call from the Königsberg authorities, but by this time he had come to like Zürich and was loath to leave it. As an inducement to remain, and in appreciation of his services, Kappeler had Meyer's salary increased by 1500 francs a year. Not so very long after this a vacancy occurred in Erlangen. The rumor had gone forth that Meyer would be offered the position, and this came to the ears of the president. Without waiting to hear from Meyer, Kappeler took the initiative by informing him that the wish of the governing body to have him remain in Zürich was so earnest that they were willing to make his position tenable for life, provided he would decide to stay (Meyer held it on a ten-year contract), and that they would further increase his salary by 1000 francs. "As I had no desire to go to Erlangen," Meyer writes to Baeyer, "I gave him the assurance with pleasure."

The miscarriage of one of his experiments before the student class made him hit upon what is conceded to be his most brilliant discovery—thiophene. "The analyses," he writes to Baeyer, "have shown the compound to have the formula C_4H_4S. It boils at 84° C. How should it be named? Kindly help me. I do not like such a name as thiofurfuran. . . . How about indogen? . . . or indophenin? or thiochrom, krytan, kryptophan? I would like to get hold of a name that would please you, too. Possibly the Frau Professor would like to take part in this." Thiophene was the name finally selected, and this became the mother substance of a group of compounds almost as extensive as benzene itself, which the genius of Meyer introduced into organic chemistry.

In January, 1884, in the company of Professor Bluntschli, the architect, Meyer undertook a journey through Austria-Hungary, with the view to examining the various chemical laboratories there. Their journey lay over Munich, and here the first stop was made. "We have already been in Munich and Graetz," he writes "and in both places we had a most delightful time. In Munich I spent a lovely time with Baeyer, Otto Fischer and König, and one delightful musical afternoon with the Heyses." (Here he refers to Heyse, the poet and novelist.) Again: "The new buildings in Vienna defy description. The Parliament, the Guildhall, the University and the Hofburg Theatre constitute a section beside which the Place de la Concorde in Paris fades into insignificance. In addition, they have the recently-constructed museums by Semper, which are the finest examples of Renaissance architecture. I witnessed a performance of the *Walküre* and the second part of *Faust*. I also saw my old flame, the actress Lucca. You can imagine how happy I was to see her again after thirteen years of absence. She is as beautiful as ever, time not seeming to have altered her."

In July, 1884, Hübner, the Göttingen professor, died. Meyer's friend Klein, who informed him of this, also told him that he was a likely candidate. The thought of having to leave Zürich was quite unbearable. What had he not accomplished during these thirteen never-to-be forgotten years! But, then, to step into the world-famed Göttingen school—that had also to be considered.

Meyer had not yet reached his thirty-sixth year. He had to regard the call to Wöhler's old establishment as the highest compliment that could be paid to him. Indeed, the compliment proved a higher one than even he expected, for none others were even to be considered.

During the last days of the year 1889 Meyer proceeded to Bonn to undergo an energetic cure: a sort of massage and electrical treatment combined. He writes: "For fourteen days I lived in the strictest incognito, going under the name of Professor Meyer, of Berlin. Since a week ago I have given this up and am now with Wallach and Kekulé daily. To see Kekulé once again and to speak to him does one's heart good. You will not consider me vain when I tell you that it was delightful to hear him say to me that he considered me the foremost among the chemists of the younger generation. Wallach is a splendid type of fellow. He visits me daily. He has no easy life of it. What a pity that he cannot go to Zürich! I suppose you have heard that Hantzsch has been nominated to succeed me. I am glad to see that both Kekulé and Wallach approve Kappeler's choice. Wallach has completed a wonderful piece of work on the terpenes which must surely become epoch-making."

Meyer left Bonn in indifferent health and after a short stay in Zürich proceeded to the Riviera with his parents. Here he felt himself slightly better, but not very much so. "Italy and the Riviera are very nice, but only for the one who is in a position to enjoy her beauties," he writes. "In my case, where I dare not go beyond one-half hour's distance from the house, the mountains call in vain."

In this condition Meyer proceeded to Göttingen. He was comforted to a large extent in that his excellent assistant, Sandmeyer, accompanied him for the summer semester. Sandmeyer, one of Meyer's "discoveries," is to-day known wherever chemistry flourishes. He started as a mechanic in Meyer's laboratory, but soon gave this up to devote all his time to chemistry.

Meyer left Zürich without being able to take leave of his students, but some months later he returned to attend the seventieth birthday of Kappeler. At the *Kommers*, which was given in the old man's honor, Meyer was among the speakers. Professor Goldschmidt thus describes the scene: "I see him (Meyer) even now before me as he spoke to the students at the *Kommers* in the evening. The 'Züricher Polytechnikers' have, as a rule, but little opportunity of knowing the professors outside their special faculty, and have therefore but little interest in those who are not their own teachers. As Victor Meyer's slender form appeared on the platform, and as his bright blue eyes glanced around the assembly, there broke forth a shout of welcome from all—engineers, machinists, architects, as well as from his own students, the chemists—to be ended in a whirlwind of applause at the close of a speech, sparkling and witty as ever."

Meyer's reception in Göttingen was all that could be desired. His inaugural lecture created a furore ("es war zum Brechen voll," he writes), and he was well pleased with so auspicious a beginning. Besides, the other men on the staff were such as any head of a department could well be proud of. C. Polstorff, K. Buchka, R. Leuckardt, P. Jannasch, and L. Gattermann were among the regular forces. Then there was the old attendant Mahlmann, whom the students of Wöhler still remembered as a marvel in glass blowing. And, finally, Sandmeyer, Stadler, and several other Zürich men completed the list.

The scientific work inaugurated here was in the main a continuation of what had previously been started elsewhere. That wonderful thiophene, which seemed to be the starting point for as many derivatives as benzene itself, was still a keen subject for study in his laboratory. The material along these lines accumulated to such an extent that Meyer found himself warranted in publishing a book on these sulphur compounds. Vapor density determinations—a subject which had agitated him even early in his Zürich career—were being followed up with unslackened zeal.

But Meyer was never so engrossed with his own work as not to keep abreast of the work which others in the field were doing. Thus we find him engaging in a friendly polemic with Baeyer on the latter's views as to the constitution of benzene. Stereoisomerism—a term coined by Meyer—dealing with configuration in space, a subject then in its infancy, also engaged his attention; and he early applied van't Hoff's views to explain several perplexities, such as the configuration of hydroxylamine and isomeric oximes of unsymmetrical ketones. Here we see the Professor no less proficient in the field of speculation than in that of experimentation.

Feeling the need of a comprehensive treatise on organic chemistry, which neither the German nor any other language supplied, Meyer, in collaboration with his assistant Jacobson, started his famous text-book. To this day it has not a peer. Those who have had occasion to do any extensive work in this branch of the science know well enough how indispensable a part of their equipment this book is. Unfortunately the senior author did not live long enough to see the work in its completed form (it ultimately appeared—still incomplete—in two bulky volumes).

Much as the nature and extent of the research work adds to the renown of an institution, certain other factors tend to have no small influence. When Meyer came to Göttingen the size and equipment of the laboratories were far from what could be desired, and one of his stipulations was that this state of affairs would soon be altered. With a willingness which could result only from the esteem in which Meyer was held, the authorities appropriated a sum sufficient to build a new laboratory, and gave him complete charge of supervising its construction. Of course, this took up much time, but as the laboratory was to prove the tools of the carpenter, and realizing how much the finished product is dependent upon the quality of the tools employed, Meyer threw himself into it with a wholeheartedness which was characteristic of everything he undertook.

Another step in the direction of increasing efficiency was the formation of the Göttingen Chemical Society. The number of research men had risen to such a height—at this time there were 105—that Meyer readily foresaw the advantage of organizing a club where these men could congregate and discuss current topics. At these meetings the students would give accounts of the progress of their latest investigations, and professors and students would engage in friendly criticism. The *esprit de corps* thus created was little short of wonderful.

The one source of great worry to Meyer as well as to his dear friends was the state of his health, which at best was but indifferent. Here in Göttingen he had formed a very intimate friendship with Ebstein, a well-known professor in the medical faculty, and, fortunately for him, Ebstein was untiring in his efforts. In 1888, when Meyer suffered a bad attack of diphtheria, only his friend's constant attention saved him. Ebstein prescribed no end of rest cures. These were well enough in themselves, but, as they so often clashed with work in the laboratory, Meyer fretted not a little. However, feeling that it was a question of life and death, he usually yielded.

It was on one of these recuperation tours that Meyer revisited his old Zürich. His reception by faculty and students left no doubt as to the way they regarded their old professor. But he had already had a proof of this shortly after he came to Göttingen. Then his Zürich scholars sent him an address which he described as "so etwas schönes habe ich noch nicht gelesen und auch noch nicht gesehen!"

The summer vacations were usually spent in Heligoland by the sea. Here, in company with his friends, Liebermann, Tollens, Ebstein, and occasionally Kirchhoff, the weeks were passed in recuperation and interchange of views.

In the fall of 1888 his quiet life gave place to days of great agitation.

On November 11 he writes to his brother: "Confidential! Yesterday I received an official communication from the ministry offering me the professorship in Heidelberg in succession to Bunsen. They are ready to do anything I want them to do. But not a soul must know of this till next Thursday. On that day the new chemical building will be officially opened, and were this news to leak out then, it would cause a great scandal. What shall I do, unlucky man that I am! The greatest piece of good fortune in the world, and yet here I am—a most dissatisfied beggar." To Baeyer he writes: "I must write to you in the very first place. I am not far wrong when I surmise that you have had a great deal to do with the honor that has come to me. My debt of gratitude to you is forever on the increase. The Minister of Education writes that the Faculty and Senate have nominated me *unico loco*, and that Bunsen was particularly desirous of seeing me succeed him."

In Berlin, where negotiations were begun, Althoff, the minister, was as bent upon retaining Meyer—at least in Prussia—as the Heidelberg authorities were bent upon getting him. He held out the assurance that Meyer would be the logical successor to Hofmann in Berlin, as Helmholtz and the majority of the faculty there had declared themselves in his favor. "I brushed all this aside," writes Meyer, "and told Althoff that I hoped Hofmann would write a nice obituary notice of me in the *Berichte*." Not even the title of *Geheimrat*, which was bestowed upon him at this time, could influence him. "On the envelope you address me as *Geheimrat*," he writes to his brother. "That, of course doesn't matter, and yet it troubles me. I have strictly forbidden any of my assistants to apply that title to me. 'Professor' is far more to my liking, and that they shall call me, as they have hitherto done."

Urged by Bunsen, Meyer finally decided for Heidelberg. "I am the happiest and yet the most wretched of men," he writes.

Before proceeding to assume his duties in Heidelberg he spent several delightful days in Bordighera. Here were Baeyer, Emil Fischer, Wallach and Quincke, "the masters of them that know" in chemistry.

To Heidelberg Meyer took as his assistants Jannasch, Gattermann, Jacobson, Auwers and Demuth. At this day when one reads these names one cannot but help admiring Meyer's wonderful judgment of men. Every one of these five has since made an enviable name for himself.

"I saw him in Heidelberg in the spring of 1891," writes Thorpe, "when he was busy with the enlargement of the old laboratory, and it was with a glance of pride—a pardonable pride—that he pointed out the places where he and I had worked with 'Papa' Bunsen. . . . It was strange, too, to hear the sound of children's voices and their laughter, and the bustle of servants in what was formerly the silent, half-deserted rooms overlooking the Wredeplatz; and stranger still to me was it, as we together called upon Bunsen, sitting solitarily in his rooms overlooking the Bunsenstrasse, to behold the meeting and to listen to the greeting of these two men—the memory of whose names and fame Heidelberg will cherish so long as Heidelberg exists."

At forty-one Meyer found himself head of—what then was—the most famous chemical school in the world. For many years Bunsen had been looked upon as the Nestor of the science. The most promising students all flocked to Heidelberg to sit at the feet of the great master. Almost every university chair of chemistry of any pretensions was filled by one of Bunsen's pupils. Yet of all of them Bunsen looked upon Meyer as the most brilliant, and it was because of that that he was so eager to have Meyer succeed him.

As in Göttingen so in Heidelberg, Meyer continued researches long before begun. These were, however, supplemented by one important addition: a study of conditions determining both the gradual and explosive combustion of gaseous mixtures, and this new phase of his labors may be regarded as the outstanding feature of his Heidelberg tenure of office.

All would have been well but for his physical sufferings. These re-commenced soon after he came to Heidelberg, and they scarcely left him till the day of his death. Early in the morning of August 8, 1897, he took his own life by swallowing some prussic acid. On the table he left this message: "Geliebte Frau! Geliebte Kinder! Lebt wohl! Meine Nerven sind zerstört, ich kann nicht mehr." At the early age of forty-nine, when in the full bloom of his powers, this remarkably gifted man passed away.

. . . .

From the reports which have come to us it would seem that Meyer's qualities as a teacher were rivalled only by his powers as an investigator. Mention has already been made of his histrionic talents; these were put to effective use in later days as professor. His extraordinary command of language, spoken in a wellmodulated voice, and coupled with a well-nigh unrivalled knowledge of his subject, went far to assure success. In addition, Meyer's laboratory technique, one of his precious assets, stood him in excellent stead when experimentally illustrating his lectures—and his lectures were always copiously illustrated by experiments, in the preparation of which no pains were spared.[1]

Nor as a man did he fall short. Sympathetic by nature, generous almost to a fault, always eager to acknowledge the labor of others, with not a taint of jealousy in his make-up, full of a hearty optimism which made him a congenial companion, a splendid *raconteur*, an excellent after-dinner speaker, a violin-player of no mean calibre—these qualities endeared him to all. His friends, Bunsen, Kopp, Erlenmeyer, Baeyer, Graebe, Kekulé, Liebermann, Fischer, etc., respected him not only as an eminent colleague, but loved him as a man of worth.[2] His house was a centre not merely for scientific, but literary and artistic notables. At these gatherings his

[1] "I well recollect that the word most frequently used in Zürich in defining the opinions of Victor Meyer's students of his lectures was 'brilliant!' (Watson Smith). "What particularly struck me about his lectures was their finished style. He made fairly constant use of notes, speaking with great rapidity. Yet his treatment of the subject was very clear, and his language perfect. The experiments were always well prepared and exceptionally successful. Indeed, his lectures were most popular. . . ." (John I. Watts.)

[2] "Ich muss Euch doch sagen, wie entzückt ich wider von allem bin: Berlin, Halle, München. In München war es ganz herrlich mit Baeyers, Fischers, und dem anderen. Baeyer ergriff einmal bei Tische das Glas um mit Emil Fishcer und mir Schmollis zu machen, denkt nur, der liebe Mann! Es brachte uns momentan in förmliche Verlegenheit, denn natürlich brauchten wir mehrere Tage, bis wir uns daran gewöhnen konnten, ihn ungeniert Du zu nennen." (Victor Meyer, in a letter to his brother, October 17, 1883.)

charming wife and four daughters did much to contribute towards a delightful evening.[3]

Meyer was not one of those professors who shrink from popularizing their science. He frequently wrote for the *Naturforcher*, *Naturwissenschaftliche Rundschau*, *Deutsche Revue*, *Deutsche Wörte*. Even in Harden's *Zukunft* we find an article on Pasteur in which the attempt is made to explain the asymmetry of the carbon atom to a lay public. Nor were his activities strictly confined to scientific subjects. In pure *belles-lettres* he published *Wanderblättern und Skizzen Aus Natur und Wissenschaft* and *Märtztage im Kanarischen Archipel*.

At the time of his death Meyer was president of the German chemical society, Emil Fischer being the vice-president. In 1888, when the new building at Göttingen was finished, the title of Geheimrath was bestowed on him. He was also a member of the *Akademien der Wissenschaften zu Berlin*, *München; die Gesellschaft der Wissenschaften zu Upsala*, and *Göttinger Gelehrte Gesellschaft*. From the Royal Society of London he received the Davy Medal, and the University of Königsberg granted him the degree M.D. (Hon.).

[3] "Die jugendliche Gestallt, der fein geschnittene, geistreiche Kopf, das seelenvolle blaue Auge, der Wohlklang der Stimme nahmen schon äusserlich Jeden für ihn ein." (Liebermann.)

"Young, handsome, well dressed—for a German professor—with a quick wit and a genial manner, he was a welcome addition to any gathering." (John I. Watts.)

"No one was more popular at these gatherings (the Chemical Society at Heidelberg) than Meyer. His nimble mind and retentive memory, his gift of ready speech, his sense of humor, and genial manner combined to make it pleasant to listen to him, no matter whether he was, in accordance with the rules of the society, called upon to give an account of some work which had just been published, or whether he was discussing and criticising a communication from a fellow-member." (Thorpe.)

References

For much of my material I am indebted to Richard Meyer's life of his brother (1). Carl Liebermann's memorial lecture (2) delivered to the members of the German chemical society is a beautiful homage to a departed friend. Prof. E. Thorpe in his Essays on Historical Chemistry (3) has an interesting article on Victor Meyer. A detailed accound of Meyer's work will be found in Dr. Harrow's article (4).

1. Richard Meyer: Victor Meyer. *Berichte der deutchen chemischen Gesellschaft* (Berlin), *41*, 4505 (1908).
2. Carl Liebermann: Victor Meyer. *Berichte der deutchen chemischen Gesellschaft* (Berlin), *30*, 2157 (1897).
3. E. Thorpe: Essays on Historical Chemistry (Macmillan and Co. 1911).
4. Benjamin Harrow: Victor Meyer—His Life and Work. *Journal of the Franklin Institute*, Sept. (1916), p. 377.

IRA REMSEN

CHEMISTRY in America is a very young product. It probably received its impetus from the Englishman, Priestly, the discoverer of oxygen, who came to these shores towards the close of the eighteenth century, and from Robert Hare, the inventor of the oxy-hydrogen blowpipe. Indirectly, the illustrious Benjamin Franklin also had a share in laying foundations.

The flame was kept a-burning by a number of well-known teachers at various university centers in the country, such as Wolcott Gibbs (1822–1908) and J. S. Cooke (1827–94) of Harvard, S. W. Johnson (1830–1909) of Yale, and J. W. Mallet (1832–1912), of Virginia. The more modern period was ushered in by Charles Eliot in Boston, Frederick Chandler, at Columbia E. F. Smith at Pennsylvania, and Ira Remsen at Johns Hopkins.

From small beginnings, the science has enlarged a thousand fold. The American Chemical Society has a membership of 13,000. It publishes an erudite journal, devoted to recording the results of research by its members; a chemical abstracts, embracing a digest of the world's chemical literature; and a journal of industrial chemistry which, in the last four or five years, has become one of the best in the world.

Remsen was the first professor of chemistry at the first institution ever established in America for post-graduate work—Johns Hopkins. He was the founder of the *American Chemical Journal*, the first of its kind in America. As teacher, as research worker and as writer, he is probably more directly responsible for the remarkable development of the science in the United States than any other man living.

Remsen was born in New York City on February 10, 1846. His father, James Vanderbilt Remsen, was descended from one of the earliest Dutch settlers of Long Island. His mother, Rosanna Secor Remsen, could also trace her descent from early Dutch settlers and French Hugenots. Her grandfather was the Rev. James D. Demarest of the Dutch Reformed Church, who had married Eliza Haring, daughter of John Haring, a man of some distinction in Revolutionary times.

In the house of the Rev. Demarest, where Remsen spent part of his childhood, both Dutch and English were spoken; the clergyman, in fact, preached in both these languages. The atmosphere was a deeply religious one. There were morning and evening prayers and reading of the scriptures, and rather long grace before and after each meal. Before he was twelve Remsen had read the Bible several times, and fervently believed every line written in the holy book.

To improve his wife's health, Remsen senior bought a farm in Rockland County, New York, and Ira was brought here when some eight years old. The next two years were spent in the country, giving the boy an opportunity to come into close contact with nature—a most valuable education for any boy. Trees and birds and fruits and flowers and animals and various aspects of farming, all came under his survey.

After his mother's death young Remsen and the rest of the family returned to New York. The smattering of knowledge which the boy had received in rural schools was now augmented by first sending him to the public school, and later, when fourteen old, to the Free Academy, now the College of the City of New York.

With the exception of history, Remsen excelled in all subjects at the College, particularly in mathematics. The highly suggestive way of teaching history was to cram dates down your throat: if they refused to stick, you were a poor student of history. Remsen had no memory for dates, and so he was adjudged a poor student of history.

Latin and Greek were also pumped into his poor little system, to which, strangely enough, Remsen took very kindly. Of science there was precious little. Dr. Ogden Doremus embraced the whole of science,—anatomy, physiology, geology, astronomy, etc.,—in a course of lectures given once a week during the year. Prof. Wolcott Gibbs, later at Harvard, did give a few lectures on chemistry, but these made no impression upon Remsen. What helped considerably were Doremus's popular lectures on physics and chemistry, given in the large lecture hall of the Cooper Institute. Doremus never spared experiments, and thereby he aroused interest in many of his hearers, among them Remsen.

Remsen never graduated from the Free Academy. His father had decided that the lad should study medicine, and in the opinion of this good man, as well as in that of the family physician, the earlier Ira was started upon his medical career, the better. That the boy had shown no aptitude along this line mattered little. In those days parents did not consult children, and children were obedient.

Remsen was apprenticed to a medical man who taught chemistry in the homeopathic medical college. That worthy man gave the boy a text-book of chemistry, and said, "Read!" So read he did. But it was Greek to him—worse than Greek, for he knew something of that language. Years later, in one among his many addresses which never failed to interest, Remsen recalled this period:

"While reading a text-book of chemistry I came upon the statement, 'nitric acid acts upon copper.' I was getting tired of reading such absurd stuff and I determined to see what this meant. Copper was more or less familiar to me, for copper cents were then in use. I had seen a bottle marked 'nitric acid' on a table in the doctor's office where I was then 'doing time!' I did not know its peculiarities, but I was getting on and likely to learn. The spirit of adventure was upon me.

"Having nitric acid and copper, I had only to learn what the words 'acts upon' meant. Then the statement, 'nitric acid acts upon copper' would be something more than mere words. All was still. In the interest of knowledge I was even willing to sacrifice one of the few copper cents then in my possession.

"I put one of them on the table; opened the bottle marked 'nitric acid'; poured some of the liquid on the copper; and prepared to take an observation. But what was this wonderful thing I beheld? The cent was already changed, and it was no small change either. A greenish blue liquid foamed and fumed over the cent and over the table. The air in the neighborhood of the performance became colored dark red. A great colored cloud arose. This was disagreeable and suffocating. How should I stop this?

"I tried to get rid of the objectionable mess by picking it up and throwing it out of the window, which I had meanwhile opened. I learnt another fact—nitric acid not only acts upon copper but it acts upon fingers. The pain led to another unpremeditated experiment. I drew my fingers across my trousers and another fact was discovered. Nitric acid acts upon trousers.

"Taking everything into consideration, that was the most impressive experiment, and, relatively, probably the most costly I have ever performed. I tell of it even now with interest. It was a revelation to me. It resulted in a desire on my part to learn more about that remarkable kind of action. Plainly the only way to learn about it was to see its results, to experiment, to work in a laboratory."

The boy tasted experiment, and he liked it well; he tasted it again, and he liked it better. Plainly, chemistry had something to it provided you could handle things and see things.

Without any instruction beyond what he could get from the text-book and his own independent investigations, Remsen was next asked to act as lecture-assistant to the professor who had so well undertaken to develop the young man's chemical knowledge. Remsen was required to prepare experiments which he himself had never performed, and had never seen; the results can be imagined. He was further requested to form a "quiz" class in chemistry—a request asking "the blind man to lead the blind." Success again was unavoidable, was it not? Here we get our first glimpse of science teaching in America in the sixties. Only by comparing its status then with what it is now can we form an opinion of the enormous change that sixty years have wrought.

Remsen was pretty well disgusted with the teacher, but not with chemistry. But chemistry could not yet be taken up. His father said that he was to be a physician, and a physician he had to be; but if a medical man, he was at least going to some college with a better reputation. The father mildly protested, and so did the professor, but nevertheless Remsen entered his name as a student of the College of Physicians and Surgeons of Columbia University.

In 1867, at the age of 21, Remsen graduated as doctor of medicine. For a thesis, which was required of every member of the graduating class, he selected a subject dealing with the fatty degeneration of the liver. Addressing the Medical Faculty of Maryland in 1878, Remsen referred to this thesis as follows:

" Eleven years ago, in company with 99 others, I was proclaimed fit to enter upon the career of a medical man. My erudition in medical matters was exhibited in a thesis on the *Fatty Degeneration of the Liver*, a subject on which I was and am profoundly ignorant. I had in fact never seen a liver which had undergone fatty degeneration, nor a patient who possessed, or was supposed to possess one; nor, I may add have I had that pleasure up to this day."

And yet Remsen got one of the two prizes offered for the best theses! The College of Physicians and Surgeons, since grown into the well-known " P. and S." school, was then perhaps a little better than the worst of its type, but very, very far from acceptable. There *were* no acceptable medical colleges in the United States. Johns Hopkins had not yet shown the way.

What was Remsen to do now? True, his precepter, the " professor," offered him a partnership in his lucrative practise; but aside from any repugnance in going forth to kill when he could do that but clumsily, he really did not like medicine at all. The little experiment with copper and nitric acid still lingered in his mind.

But if a chemist, where was he to go to get his instruction? The big chemical laboratories at Harvard, at Chicago, at California, at Illinois, at Columbia, familiar to the student of to-day, were yet to be born. Harvard was a possibility, but small in comparison with research centers on the continent. Remsen had read Liebig's *Chemical Letters.* Liebig was the great chemist of Germany, with but one rival, Wöhler. Everybody spoke of Liebig; even the child in the street had heard of Liebig's beef extract.

We are not told how well Remsen's father received the young man's proposed change of program. Whether well or otherwise, the younger man triumphed. Towards the end of the summer in 1867 the *M.D.* set out for Munich.

Arriving in Munich, Remsen had his first hopes dashed to the ground by being told that Liebig no longer received students. All he did at this time was to give a lecture course in inorganic chemistry. The young foreigner then was forced to turn to the most promising *privat-docent* in Liebig's laboratory, who happened to be Jacob Volhard. In Volhard's laboratory Remsen received his first systematic instruction in chemistry. Up to that time he had never made the simplest analysis; he had only performed the crudest experiments for lecture purposes.

He spent two semesters in Munich, from October 1867 to August 1868, working in Volhard's laboratory. The *privat-docent* had few students—sometimes Remsen was the only one in the laboratory. This was an extremely fortunate circumstance for the American; he received private instructions from one of the best laboratory manipulators of the day. Remsen also attended Liebig's course of lectures.

At the end of the year Volhard advised him to go to a larger laboratory and suggested Göttingen. Fortunately, Wöhler, the professor at Göttingen, was then in Munich, on a visit to his old friend Liebig. Through Volhard Remsen secured an introduction to Wöhler, who told him that he would be very welcome in Göttingen.

Wöhler kept his promise; he even procured a nice lodging for the young man.

Remsen came to work directly under Fittig, then professor *extraordinarius* at Göttingen. In due time the undergraduate became a research worker, with the oxidation of xylene (a compound closely allied to benzene) as a subject to work upon. The outcome of this research was sufficiently promising to warrant Fittig suggesting another line of work, this time connected with a method of synthesis which Fittig had inaugurated, and which still bears his name. This was not so successful.

To complete his requirements for the Ph.D., Remsen undertook another investigation,—one dealing with piperic acid. The results of this work were embodied in his dissertation presented to the faculty of the university in partial fulfilment of the requirements for the degree of doctor of philosophy, and later published in the *Annalen der Chemie*. Early in 1870 he received the doctor's degree.

Remsen was about to return home when Fittig received a call to Tübingen to succeed Strecker, whereupon Fittig suggested that Remsen should accompany him to Tübingen as an assistant. To this Remsen gladly assented. In Tübingen he remained for two years, acting as lecturer and laboratory assistant, and utilized his spare time in carrying on investigations of his own.

In Tübingen, also, Remsen made the acquaintance of William Ramsay,—then a young undergraduate but recently arrived from England—under somewhat dramatic circumstances. "Ramsay appeared in the laboratory for the first time. Ringing for a long time at the door he was finally answered by a young man in overalls. 'Können sie mir sagen wo ist die Vorlesungszimmer?'

queried Ramsay. This was shocking German, but he had done the best he could with his phrase book." The "young man in overalls," who was none other than Remsen, looked at the stranger, paused, and then said, "Oh! I guess you want the lecture-room!"

Remsen and Ramsay became great chums. Around them they gathered most of the English, Scotch and American students in Göttingen. A baseball club was formed, in which the English (including the present Lord Milner) and Scotch took part, but not the Germans. Then there was skating on the ice winter afternoons, and—sometimes—dinner parties in the evening, when Ramsay entertained the company with "A fine Old English Gentleman," to his own accompaniment.

In 1872 Remsen returned to the United States after having spent nearly five years in Germany. He was now a university man, appreciated university life, and could conduct research. But what opening was there for such a man?

He wandered to Philadelphia, and there completed a translation of Wöhler's *Organische Chemie* which he had begun in Tübingen, and which H. C. Lea and Company had promised to publish. But what next? At times he lost faith and became despondent. He had given up one profession, prepared himself for the practise of another, and apparently every position was filled and every opportunity had been seized by some-one else. His long absence from the country and his change of pursuit had left him with practically no one to look to for help and advise.

After some months of fruitless endeavour to get some-thing, he received an offer from the University of Georgia, and close upon this offer came another, from Williams College. Offers, like sorrows, come not in single file, but in battalions.

Remsen accepted the appointment at Williams College as professor of physics and chemistry. When he got there he found the cupboard bare—Williams College possessed no laboratory! A mild request for one received the following answer from the president: "You will please keep in mind that this is a college and not a technical school. The students who come here are not to be trained as chemists or geologists or physicists. They are to be taught the great fundamental truths of all sciences. The object aimed at is culture, not practical knowledge." With which immortal discourse the great man dismissed the subject. At the end of a year, the board of trustees did, however, build Remsen a small laboratory for his own use, and here, amid such discouragement, he prosecuted research on the action of ozone on carbon monoxide, on phosphorus trichloride, and on derivatives of benzoic acid. The results were published in the *American Journal of Science* and in the *Berichte der deutschen chemischen Gesellschaft*.

"I remember," writes Remsen, "that once after the appearance of one of my articles in the *American Journal of Science*, we had a faculty meeting in the college library. Someone picked up the number of the journal containing my article, and some good-natured fun was poked at me when an attempt was made to read the title aloud. I felt that in the eyes of my colleagues I was rather a ridiculous subject." Remsen was only 27 then, and over-sensitive.

So four years were passed. In the meantime, a book on *Theoretical Chemistry*, which Remsen had written during his many despondent hours, proved an extraordinary success. The novel method of presentation, the systematic arrangement, a rare clearness and simplicity in style, afforded it a welcome among all scientific workers. It passed through five editions, and was translated into German and Russian.

Later, when at Johns Hopkins, Remsen wrote a number of books on inorganic and organic chemistry, with almost unvarying success. Had his reputation to rest on nothing more than author of such text-books, he would find no inconspicuous place in the history of chemistry in America.

Then in 1876 came that great change in universities in the United States with the establishment of a graduate school at Johns Hopkins, in Baltimore. Huxley, then in this country, very appropriately ushered in the new era by an address of welcome. Gildersleeve, the Greek scholar, Rowland, the physicist, and Sylvester, the mathematician, were appointed to form a nucleus of promising scholars. To this trio was added Ira Remsen as professor of chemistry. He was then thirty years old.

The position could not have been more ideal. Emphasis was to be placed upon advanced, graduate work, the professors were expected to do research, and the necessary facilities were to be provided to the extent that money could provide them. There were no petty restrictions of any kind. " Do your best work and do it in your own way." That was the only advice President Gilman had to offer.

In May, 1877, Remsen delivered his first lecture on advanced organic chemistry to a small group of students huddled together in a room which has since become a storeroom for odds and ends. Research was begun immediately. Regular weekly meetings to discuss current topics were also introduced. " . . . nowhere else [in America], so far as I know, had the advanced students been taken in and given an opportunity to acquire the habit of familiarizing themselves with the current progress of the science and of perfecting them selves in the art of giving concise and lucid expression to the information acquired in the course of their reading."[1]

The extensive series of researches begun in 1877 and carried on without a break well into the twentieth century dealt with various phases of organic chemistry. Perhaps the most interesting outcome from a practical standpoint was the preparation of *orthobenzoic sulphinide*, or *saccharin*, in 1879. This substance, obtained from toluene, a product of coal tar, is unique in being five hundred times as sweet as sugar. In spite of the more than 100,000 carbon compounds that have been prepared, no substance similar to it in sweetness has ever been unearthed. And the wonder increases when we remember that, chemically, saccharin and sugar have nothing in common.

At first Remsen sent his contributions to Prof. J. D. Dana for the *American Journal of Science*, but soon the amount of matter grew to such proportions, that it frightened poor Dana. The work was of such a specialised character; perhaps it would be more desirable to send such contributions to foreign journals? queried Dana.

Remsen felt that the time had come to found a chemical journal in America. With this in view, he got into touch with the leaders of science. Most of them discouraged the plan; very few had anything to say in favor of it. Despite this cold reception, he started the *American Chemical Journal* in 1879. It proved a success from the start. Workers from all over the country began to flood the publication with contributions. As a stimulant to research in chemistry at various scientific centers, the Journal stood in the same relation as John

[1] Prof. H. N. Morse, Director of the Johns Hopkins Dept. of Chemistry.

Hopkins University did towards the other universities of the country.

For many years, and long after influential scientific centers had sprung up in the United States, the *American Chemical Journal* continued to be the sole medium for the publication of American chemical research. In the beginning of the twentieth century the *Journal of the American Chemical Society*, the official organ of the American Chemical Society, came to the forefront, and in 1914, Remsen's journal, its purpose served, was discontinued.

In the last number of the *American Chemical Journal* Remsen says: "The American Chemical Society has grown to great importance and is amply prepared to provide for the publication of all articles on chemical subjects likely to be prepared in this country. . . . Taking everything into consideration it now seems best to the editor to place the control of his journal in the hands of the society. It is needless for him to say that after 35 years of editorial work he does not now withdraw from it without a feeling of deep regret. His earnest hope is that the step may prove wise."

During the absence of President Gilman in Europe in 1889–90 Remsen served as acting president of Johns Hopkins, and in 1901, when President Gilman retired from office, he was elected as Gilman's successor. This office he held with marked distinction until 1912, when he resigned.

During his tenure of the presidency what distinguished it particularly was the perfect freedom he allowed professors. He realized that "every man does his best work when he is allowed to do it in his own way." "The many criticisms that in recent times have been directed toward this [the president's] office in our American institutions are certainly not applicable to him.

He never abused the power placed in his hands, there has been no autocratic interference with the autonomy of the individual departments, and above all there has been no suspicion of indirection in his dealings with his staff. We have had implicit confidence in his motives. . . . We have been very contented, happy, and prosperous under his administration." [1]

It has been pointed out how, first as writer, then as investigator, and finally as editor, Remsen's influence upon chemical research in America has been profound; as teacher, it was no less so. "I will only say, as many others have said before me in effect, that I have never seen his equal as a master of simple and lucid exposition . . . as a teacher of many other teachers, his influence, direct and remote, has been and will continue to be of incalculable value to American students of chemistry." [2]

His former students are some of our very best chemists to-day: Orndorff of Cornell; (the late) H. C. Jones of Johns Hopkins; W. A. Noyes, Illinois; Kohler, Harvard; C. H. Herty, editor of the *Journal of Industrial and Engineering Chemistry*; J. F. Norris, Mass. Inst. of Technology; S. R. McKee, Columbia; E. E. Reed, of Johns Hopkins; and Burton and Gray, superintendent and chief chemist respectively of the chemical department of the Standard Oil Company.

Several attempts to induce Remsen to leave Baltimore for other and more lucrative positions, proved futile. The University of Chicago made a particularly tempting offer, but Remsen remained true to Johns Hopkins. "This is my birth for life," he said in an address to the students.

When Remsen went to Williams as a very young man the students "had it in for him," so some of them con-

[1] W. H. Howell, prof. of physiology at Johns Hopkins.

[2] Prof. H. N. Morse.

fessed quite frankly later. With time the students' desire to make it " hot " for the teacher gave place to a desire to please. Remsen with his simplicity, his humor, his interesting methods of presenting the subject, made himself very much liked. At Johns Hopkins he was extremely popular because, in addition to sound scholarship, he had so much of the milk of human kindness; he forgave much.

One point, however, about which he was very particular was punctuality. A story is told of him in this respect. While engaged in a lecture upon some of the chemical elements, he was in the act of describing some attributes of sulphur. As he uttered the first syllable, " sul—," the door in the back of the room opened and a young man noted for his habitual lateness entered. The instructor stopped short and stood with the word half uttered while the abashed student, in the midst of an awful and soul-oppressing silence, made his hasty way to a seat. Then with a tone of strong relief, and with the interest of each student intensified upon him, Remsen suddenly gave expression to the concluding syllable of his word—" phur! "

At the request of the National Board of Health of Baltimore, Remsen, in 1881, undertook an investigation into the organic matter in the air, and a study of the impurities in the air of rooms heated by hot air furances and by stoves. Similar work was done for the city of Boston. In 1882 he became a member of the National Academy of Sciences, and in 1884 served on a committee appointed to investigate the glucose industry of the United States. Another committee upon which he served dealt with the question of the processes employed in denaturing alcohol.

In 1909 President Roosevelt appointed Remsen chairman of a board of consulting scientific experts to aid the Secretary of Agriculture in matters pertaining to the administration of the pure food law. The other members of this board were Dr. R. H. Chittendon, Director of the Sheffield Scientific School; Dr. J. H. Long, Professor of Chemistry and Director of the Chemical Laboratories in Northwestern University; Dr. C. A. Herter, Professor of Pharmacology and Therapeutics, Columbia University; Dr. A. E. Taylor, Professor of Pathology and head of the Department, University of California; now Professor of Physiological Chemistry in the University of Pennsylvania. Dr. Herter died in December, 1910, and Dr. Theobald Smith, Professor of Comparative Pathology in the Harvard Medical School, was appointed to fill his place. The Board was generally known as the "Remsen Board."

Dr. Wiley, chief chemist of the U. S. Department of Agriculture, selected a number of men as subjects for investigation on the assimilation of benzoate of soda. These men came to be known as the "poison squad." Dr. Wiley declared that in experiments which had lasted some twenty days, a number of the men had become ill. The maximum amount of the sodium benzoate given to any one man, and distributed over the twenty days was one and two-thirds ounces.

Dr. Wiley's conclusion did not pass unchallenged. Some authorities declared that the fever of the young men was due to nothing more than an epidemic of grip which was then raging. Neither were the experiments themselves considered very satisfactory. The majority of the individuals had been used in previous experiments where they had been made ill; and the sodium benzoate, instead of being distributed in the food—just as it is when used as a preservative—was given to the patients in capsules.

The members of the "Remsen Board" repeated Wiley's experiments, working quite independently of one another. The assistants took from one-third of a gram to six grams (1/5 oz.) daily, and in no instance were any ill-effects noticed. Now the law allowed no more than 0.3 gram of sodium benzoate for one pound of beef, which was only one-twentieth of what the assistants had received.

In 1914 the "Remsen Board" reported on the use of alum in baking powders; this they found to be non-injurious, provided too large quantities were not used. Large amounts provoke catharsis, due to the sodium sulphate which results from the reaction. The general conclusion drawn was that alum baking powder was no more harmful than any other baking powder; but possible secondary effects due to chemical reactions between the ingredients made it seem advisable to recommend that food leavened with alum baking powder should be used in moderate quantities only.

Remsen has been the recipient of many honors. The LL.D. was conferred upon him by Columbia in 1893; Princeton, 1896; Yale, 1901; Toronto, 1902; Harvard, 1909; and Pennsylvania, 1910. In 1898 he was elected a Foreign Fellow of the London Chemical Society, and in 1911, a Foreign Member of the French Chemical Society. In 1902 he was elected to the presidency of the American Chemical Society, and in the following year to that of the American Association for the Advancement of Science.

From 1907–1913 Remsen was President of the National Academy of Sciences—the highest American scientific distinction. The president preceding Remsen had been Alexander Agassiz. In 1908 he was awarded the Gold Medal of the Society of Chemical Industry (England), and two years later became its president. In 1914 he received the Willard Gibbs Medal of the Chicago Section of the American Chemical Society.

Remsen was married in 1875 to Elizabeth H. Mallory, a daughter of a New York merchant, who with his family spent his summers in Williamstown. They have two sons, Ira M. who is an artist, and Charles M., a surgeon, practicing in Atlanta, Ga.

As President of Johns Hopkins, Remsen's time for research was very limited. One of his reasons for retiring from the presidency was a desire to return to the love of his younger days, and this "return to the fold" made him happy again. "The transformation from university president to chemist is complete, and I rejoice."

References

Part of the information comes from private sources. Remsen's address before the Chicago section of the American Chemical Society, delivered in 1914 (1) contains much of biographical interest. For details regarding the Tübingen days, Tilden's *Sir William Ramsay* (2) has been of service. Other articles that were found useful were 3, 4, 5, 6, 7 and 8.

Remsen's celebrated article on saccharin was published in the *American Chemical Journal* (9). He is also the author of a number of well-known texts, references to some of these being given (10, 11, 12, 13, 14).

1. Ira Remsen: The Development of Chemical Research in America. *Journal of the American Chemical Society*, *37*, 1 (1915).
2. Sir W. A. Tilden: Sir William Ramsay (Macmillan and Co. 1918).
3. Anon.: Referee Board Reports on Alum Foods. *American Food Journal*, May, 1914, p. 188.
4. Anon.: A Vindication of Benzoate of Soda from the attacks of Dr. Wiley. *Current Literature*, *52*, 304 (1912).

5. Marcus Benjamin: Prof. Ira Remsen, President of the American Association for the Advancement of Science. *Scientific American*, *88*, 19 (1903).
6. Anon.: Johns Hopkins' New President. *Baltimore Sunday Herald*, Oct. 13, 1901.
7. Marcus Benjamin: Development of Chemistry in America. *The Star*, May 25, 1890.
8. Anon.: The Resignation of President Remsen. *The Johns Hopkins University Circular*, No. 10, 1912.
9. Ira Remsen and C. Fahlberg: On the Oxidation of Substitution Products of Aromatic Hydrocarbons. IV. On the Oxidation of Orthotoluenesulphamide. *American Chemical Journal*, *1*, 426 (1879).
10. Ira Remsen: Principles of Theoretical Chemistry (H. C. Lea's Son and Co., Philadelphia. 1883).
11. Ira Remsen: An Introduction to the Study of the Compounds of Carbon (D. C. Heath and Co., Boston. 1906).
12. Ira Remsen: Elements of Chemistry (Macmillan and Co. 1887).
13. Ira Remsen: Inorganic Chemistry (Macmillan and Co. 1889).
14. Ira Remsen: A College Text-Book of Chemistry (Macmillan and Co. 1908).

EMIL FISCHER

THE news has reached us that Emil Fischer is no more. Since the fateful August, 1914, Germany has lost her Ehrlich, her Buchner and her Baeyer; England, her Ramsay, Crookes and Moseley. Deaths occur, wars or no wars; yet Buchner might have lived had not a shell cut short his existence; and young Moseley had barely started along his brilliant career when he, like the promising Rupert Brooke, laid down his life for his beloved England. Ramsay's end, we know, was hastened by manifold war duties. To what extent Fischer was a victim of the war is still unknown to us; but we were told, from time to time, of his violent pan-Germanism, doubtless encouraged by the exalted position he held under the crown. The magnitude of Germany's *dêbâcle* would have crushed a spirit less proud than *Geheimer-Regierungsrat* Fischer.

Whatever opinions we may have regarding Fischer's political affiliations, there can be no question of his position in the history of chemistry. His bitterest enemies are the first to pay tribute. He easily takes his place as the greatest organic chemist of our generation.

To appreciate his work a little more, we must look into the state of the science when Fischer began his labors. In those days—in the seventies—organic chemistry, or the chemistry of the compounds of carbon, was a field for the most fruitful research. The addition of carbon and hydrogen and oxygen atoms, and the various rearrangements within a molecule, could be accomplished with such relative ease, that candidates wishing to get a doctor's degree in the shortest time were readily attracted to this branch of the science. New compounds of carbon were being daily manufactured by the score in Germany, England and France.

In many cases these compounds have remained of interest to the writers of reference books only. A number, however, found wider application in the dye and drug industry.

That animal and vegetable life were largely made up of carbon compounds, that the food we eat could be largely divided into fat, proteins and carbohydrates,—all this was known. If, then, a knowledge of the composition of these substances, as truly belonging to organic chemistry as marsh gas or benzene, was vague and wholly unsatisfactory, this was due to the complexity of their make-up. Chevreul and Berthollet had cleared the situation in so far as the fats were concerned, but the chemistry of the carbohydrates, and particularly that of the proteins, remained as mysterious as ever. The three foodstuffs were the borderland where chemistry ended and biology began; the lack of a solution of the composition of at least two of these foodstuffs left the finishing touches of the edifice of organic chemistry still undone, and gave a wholly unsatisfactory foundation for the science of physiology.

To the solution of this problem Fischer pledged his life while still a student, and brilliantly did he fulfil his life's task. With an imagination tempered only by a splendid scientific training, an originality of mind which made a lasting impress upon every piece of work with which he was associated, and a rare skill in devising apparatus, he, first by his own labors, and later, as director-general of an army of aspiring students, gradually unfolded the mysteries that had enshrined the most complex chemical substances known to man. Like all great contributions, his has added not only to our chemical knowledge, but has shed a flood of light on cognate sciences, such as botany, zoology and physiology.

Fischer was born in Euskirchen, Rhenish Prussia, on October 9, 1852. His father, Lorenz Fischer, was a successful merchant whose success in business must have made a deep impression upon his son, for Emil, after matriculating the gymnasium in Bonn, joined his father's concern at the age of seventeen.

This enthusiasm for the commercial world, however, was short lived. Within two years he had abandoned all thoughts of high finance, and has inscribed himself as a student at Bonn University. Kukulé, one of van't Hoff's teachers, was the professor of chemistry, and Engelbach and Zincke were his active assistants. Fischer came in contact with all three.

The ill-omened Franco-German war had barely terminated when the German government decided to found a university at Strassburg. To this place, in the autumn of 1817, Fischer, true to the German student's traditions, came to spend part of his *wanderjahre*. The initial training for a chemist required a sound course in inorganic chemistry, particularly of an analytical kind. Under Rose, Fischer was made acquainted with Bunsen's methods for the analysis of water, an experience which was of use when the young man undertook to do analytical work for the town of Colmar.

By the end of a year Fischer was ready for the next step in the training of a chemist—a course in organic chemistry. This brought him in contact with Adolf von Baeyer, the professor of the subject.

Baeyer, a man of eighty, died recently in Munich. He was the connecting link between Liebig and Wöhler on the one hand, and his own pupils who so brilliantly carried on the best traditions of the great school of organic chemistry which Liebig and Wöhler had built. To him, even when at the small *Gewerbeakademie* in Berlin, came Graebe and Liebermann, whose synthesis of alizarin has already been discussed (see Perkin); and Victor Meyer, the conquering hero among chemists. Fischer now came to pay homage. At a later date Willstätter joined the little band of Baeyer's scholars. Fischer and Baeyer are no more, but Willstätter, the chlorophyll wizard, who has recently been appointed to Baeyer's chair in Munich, bids fair to equal, if not outstrip his master in quality and originality of work.

Fischer immediately came under the spell of Baeyer. The professor was rapidly reaching the height of his intellectual output. His amazing mastery of every phase of the subject, the keen criticism to which every piece of work was subjected, the fertility of his ideas, combined with the fatherly care he took of his "children," the students, made Baeyer very popular with his assistants and research workers, not least of all with Fischer.

In July, 1874, Fischer completed an investigation on the coloring matters fluorescein and orcin-phthalein, for which he received his Ph.D. His immediate appointment to an assistantship was evidence that he had already made an impression upon Baeyer, whose faculty for detecting promising material was not the least of his gifts.

In less than a year Fischer, with his discovery of phenylhydrazine, forged to the very front rank of organic chemists. Later this substance in his hands proved the most effective tool in synthesising the sugars, which are typical members of the carbohydrate family. To-day the *osazone* test for sugars, a test depending upon the use of this same phenylhydrazine, is among the commonest and the most effective methods used by the chemist, the physiologist and the clinician for the isolation and detection of the sugars.

Little wonder, then, that when Baeyer in this same year was selected to succeed Liebig in Munich, he was desirous that young Fischer should accompany him. This, of course, was just what Fischer wanted.

For the next three years Fischer held no official position at the University of Munich. As events proved, this was the most fortunate thing that could have happened. He had no students to instruct, no laboratory work to supervise; the entire time could be devoted to research.

And how well did Fischer make use of this time! With phenylhydrazine as the starting point, the various derivatives of this parent substance were investigated, and its relationship to a group of substances that act as "intermediates" in the manufacture of dyes—the *diazo* compounds, was clearly established. The ease with which phenylhydrazine combines with other substances gave rise to an almost endless series of new compounds. To us of particular interest is its combination with two important classes of organic compounds known as the *aldehydes* and *ketones*—a discovery which found direct application in the chemistry of the sugars. Victor Meyer, by the use of hydroxylamine, a substance closely related to ammonia, had also shown how the aldehydes and ketones could be recognized. Starting from two different angles, Meyer and Fischer, who became the closest of friends, and whom Baeyer regarded as his two most talented pupils, met on common ground. Between them they opened up two vast chapters in organic chemistry.

At the same time, Fischer, in collaboration with his cousin Otto Fischer, began an investigation of the rosaniline dyestuffs—the magenta of Perkin—which terminated in the brilliant discovery that these dyes were all derivatives of a base *triphenylmethane*.

The importance of this work may be gauged when we reflect that Otto Fischer owed his appointment as professor at Erlangen to this investigation, and its possibilities are such that all of Otto Fischer's subsequent contributions have largely centered around the pioneer work in which his cousin played such a leading part.

Genius will out, and recognition came quickly. Fischer was made *privat-docent* in 1878, and at the end of the year was promoted to the extraordinary professorship and given entire charge of the analytical department in Baeyer's laboratory.

Then began those classical investigations into the active constituents of coffee and tea, caffeine and theobromine, and their relationship to xanthine and guanine—decomposition products obtained from the protein in the nucleus of cells—which ultimately opened up an entirely new chapter in plant and animal chemistry.

In the Easter of 1882 Fischer accepted a call as full professor (*ordinarius*) to Erlangen, and three years later he exchanged this chair for one in Würzburg.

Fischer was not much over thirty when he assumed charge in Würzburg, yet the ten years which had passed since he had received the doctor's degree had been put to such good use that he already belonged to the four or five leading chemists of Germany.

Thus far his work had been carried out with little assistance, but now, as an *ordinarius*, research students were not wanting, particularly in view of Fischer's eminence. Under his supervision a fine new laboratory was built, and with his active co-operation his students continued work on indol, uric acid and the sugars.

After many weary trials, Fischer managed to synthesise the most important sugars—among them fruit and grape sugar—and also to prepare many new ones artificially. It was in the course of this intricate and laborious work that he had occasion to put van't Hoff and Le Bel's theory of the asymmetric carbon atom to exhaustive tests, with results which established the theory more firmly than ever.

This work on the sugars threw some light on the method by which carbohydrates are formed in the plant. We know that the carbon dioxide and the moisture are taken up from the air by the plant and, in the presence of chlorophyll, are first probably converted to glucose, then to starch and fat and, in the presence of nitrogen obtained from the soil, partly to protein. Baeyer's theory of the first part of the reaction is that the carbon dioxide and moisture combine to form formaldehyde ("formalin"), liberating oxygen, and that by polymerization, or a method of coalescing, the formaldehyde molecules condense to form a molecule of sugar.

This theory received its first experimental support when Butleroff showed that formaldehyde in the presence of lime water yielded a sugar-like mixture. It was left, however, for Fischer to prove that this sugar-like mixture contained a small quantity of a substance, α-acrose, which he was able to transform into glucose. Fenton completed the cycle by his success in converting carbon dioxide into formaldehyde at a low temperature.

Thus the initial chemical processs in the plant were in a measure duplicated in the chemist's laboratory. Even the conditions of normal temperature under which these reactions proceed in the plant were fulfilled. But the well-nigh 100 per cent efficiency of the plant could not be even distantly approached.

The mechanism of the reverse process, by which such a substance as glucose is oxidised in the body to carbon dioxide and water, is hardly better known. We do know that oxidising ferments facilitate the reaction at body temperature, and the work of Dakin and Lusk in this country has made it seem probable that a glycerin-like substance or substances, and lactic acid, are important intermediate products.

Thus, as in simpler chemical reactions, the beginning and end of the reaction are clear, but again like any chemical reaction, the intermediate steps are very difficult to elucidate.

It was in the course of these epoch-making experiments on the sugars, when phenylhydrazine was constantly used, that Fischer began to suffer with chronic poisoning, due to the inhalation of the vapors of this substance. Its effects he never got rid of, and from then on he was more or less of a semi-invalid. This might perhaps explain why in after years students found him somewhat of a " grouch " and quite unapproachable. The testimony of some of his students at Würzburg seems to bear conclusive witness to the fact that in those days, at least, he was not only an inspiring leader and lecturer, but took a very active interest in his research men. It was no uncommon thing to see him spend a couple of hours at the desk of one of his students, not only discussing the problem and offering suggestions, but actually illustrating experimental methods of procedure. Such illustrations were simply priceless in value to the young *kandidat*, for Fischer was a master manipulator as well as a master thinker.

Like Victor Meyer and Ramsay and van't Hoff, the appointment to a full professorship made feasible his marriage to the lady he had long courted, *Fraulein* Agnes Gerlach. The two made a striking pair. Both were tall and handsome, with intellect and wit a-plenty. Their son, Hermann, has faithfully followed in his father's footsteps.

In 1892 came the crowning event of his career. A. W. Hoffmann, who had been professor at the Royal School of Chemistry in London for some years, and had there taught such men as Crookes and Perkin, and had then been appointed to the chair of chemistry at Berlin University, died, and Fischer was selected to succeed him. This was a signal honor, for the Prussian Ministry of Education left no stone unturned to make Berlin the foremost center of learning and research in the Empire, and only men whose standing in the world of scholarship was universally conceded, were at all considered.

Fischer stipulated that he would accept the position only on condition that a new laboratory would be built for him. He had in mind his splendidly-equipped laboratory in Würzburg, where the authorities provided him with ample facilities and gave him unrestricted freedom to equip the chemistry building with the best and the latest innovations. The Berlin authorities promised the new laboratory, and so Fischer moved to his new home. Four years, however, were to pass before the foundation-stone for the new structure was to be laid. This was due to the bad financial condition of the university.

In Berlin Fischer continued his work on the sugars. The fact that many of these bring about fermentation led Fischer to fruitful studies on the possible constitution of ferments and their relationship to the substance they act upon. This subject of ferments, or *enzymes*, is of such tremendous significance in the activity of all life-processes, that it merits a somewhat detailed discussion.

The word enzyme comes from a Greek word meaning "in yeast." Perhaps the most acceptable definition in the light of recent scientific research is to say that it is a substance showing the properties of a catalyst and produced as a result of cellular activity.

But what is a catalyst? The reader may recall his first very simple experiment in the preparation of oxygen. Here the instructor tells the bewildered youth that if you put a little potassium chlorate in a test tube and heat this very strongly, a gas is evolved which can be identified as oxygen. Now by merely adding a small quantity of a dirty black-looking powder, called manganese dioxide, to the potassium chlorate, the oxygen is evolved much more rapidly and at a much lower temperature. But this is not all. A careful examination at the end of the reaction shows that the manganese dioxide has not changed in any way: we have the same substance, and the same amount, at the end of the reaction as at the beginning. Many such substances are known to chemists. They all have this peculiarity: that they *accelerate* chemical reactions,[1] and that a relatively small, at times insignificant quantity of the substance suffices to bring about the chemical change.

In cells we find substances of this type, but thus far these cellular "catalysts," unlike the manganese dioxide, and like proteins, have never been produced outside of the cell.

When we consider that life is possible only because of continued cellular activity, and when we bear in mind that this activity is largely the result of chemical changes brought about by these enzymes, the paramount importance of these substances becomes manifest.

Alcoholic fermentation with yeast, the souring of milk, processes of putrefaction, and various other examples of changes in organic materials with, often enough, the accompanying liberation of bubbles of gas, had long been known. The epoch-making researches of Pasteur had shown that fermentations and putrefactions were inaugurated by the presence of living

[1] Cases are known where they *retard* chemical reactions.

organisms. Then extracts from the saliva and the gastric mucosa of the stomach were obtained which also had the power of bringing about chemical changes in carbohydrates and proteins. This led to the classification of ferments into those which, like yeast and certain bacteria, acted because of certain vital processes (organised ferments), and those which, like the extracts from the saliva and stomach, were presumably "non-living unorganized substances of a chemical nature" (unorganised ferments) Kühne designated the latter *enzymes*. This classification was generally accepted, and the "vitalists" held absolute sway until 1897, when Emil Buchner, fired by Fischer's work, overthrew the whole theory by a series of researches which, in their influence, were only second in importance to those of Pasteur in an earlier generation.

One of Buchner's classical experiments consisted in grinding yeast cells with sand and infusorial earth, and then subjecting the finely pulverized material to a pressure of 300 atmospheres—a pressure far more than enough to destroy yeast, or any other cells. The liquid so obtained had all the fermentative properties of the living yeast cell. Obviously, then, the living cell could not be responsible for the fermentation. On the other hand, this experiment did suggest that cellular activity gave rise to some substance which, once produced, exerts its influence whether the cell is alive or dead. All subsequent experiments have but strengthened the conviction that cells do produce these substances, and that the chemical changes are due *not* to the living organisms, but to the *lifeless* substances (enzymes) to which the seorganisms give rise.

Minute in quantity, and tenaciously adhering to substances present, particularly protein, the isolation of an enzyme in the pure state has become one of the most difficult problems in physiological chemistry. Yet any elementary student in the subject finds little difficulty in performing simple experiments which convince him either of the presence or the absence of the enzyme.

The method consists essentially in making use of the so-called "specificity" of enzymes, a conception for which Fischer is largely responsible.

Fischer's synthetic work in the sugar series, particularly his studies into the configuration of cane sugar, maltose and lactose, received a great impetus from the success which attended his efforts in preparing glucosides—combinations of glucose and one or more other substances—artificially. By the study of emulsin, and other enzymes in yeast, on such glucosides, Fischer found that the slightest change in the configuration of the glucoside inhibited the action of the enzyme. Zymase, another enzyme in yeast, which is directly responsible for the conversion of glucose into alcohol, behaved similarly. This led him to the conclusion that a close chemical relationship exists between the enzyme and the substance on which it acts—a view which led to his famous analogy of the lock and key relationship. Just as one key fits one lock, so any one enzyme will act on only a certain type of substance.

Take, for example, the enzyme found in saliva, ptyalin; it readily acts on the carbohydrate, starch, but has no action on protein. Again take the pepsin of the stomach: this enzyme breaks down proteins, but is without result on carbohydrates. These instances may be multiplied indefinitely.

Some enzymes show their specificity to an even more marked degree. Fischer's work has given us beautiful illustrations. Even in the yeast cell we find one, *sucrase*, which acts only on cane sugar (sucrose), but on no other sugar or any carbohydrate.

In the winter of 1894 Fischer resumed his earlier work on uric acid and caffeine. After three years he succeeded in synthetically producing every constituent of the group, and traced them all to a mother substance to which he gave the name of *purin* (a word suggested by the phrase *purum uricum*).

The chemist, the physiologist and the pathologist can but wonder at such genius. Here are the most complex and the most important class of protein bodies, the so-called nucleoproteins, which as their name implies, are found in the nucleus of the cell, and which, in the course of their chemical decomposition in the body, give rise to *xanthine*, *hypoxanthine*, *adanine*, *guanine*, etc.—all typical purines; here are these purines which, in their further travels in the body, come to the liver, where a large percentage of them are oxidised to uric acid—another member of the purine family. This same uric acid is a never-failing constituent of the urine, and its quantity gives valuable data regarding nucleoprotein metabolism in the body,—of paramount importance in such a disease as gout. The interrelationship of these complex purines, as well as their relationship to plant analogues, such as caffeine and theobromine, have been as thoroughly probed by Fischer as the composition of water or that of air. He has gone even further. Having found relationships, and having traced the substances to one mother substance, he has succeeded in building them all up from this mother substance—a piece of work which, with but one exception, finds no equal in synthetic chemistry.

The one exception is Fischer's crowning series of researches on the proteins. No work approaching this had ever been done before.

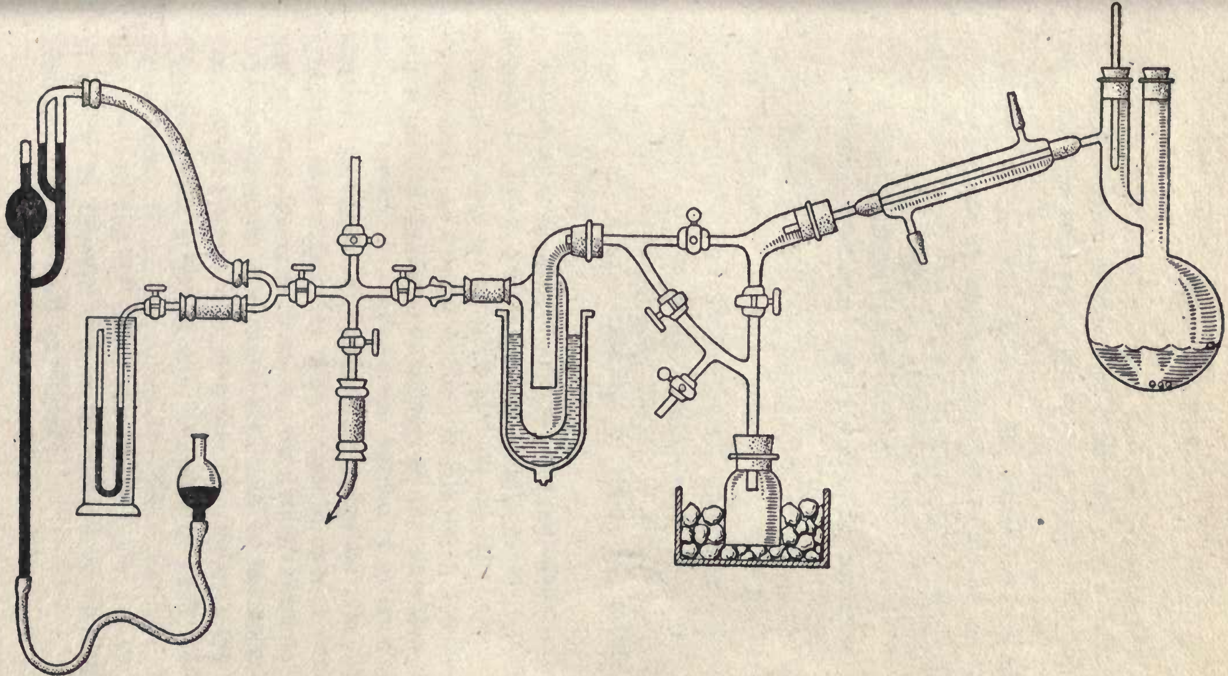
The proteins are the most important of the three classes of foodstuffs. Without them cellular growth and repair would be impossible. The belief has been general that the elucidation of their constitution would open up the key to some of life's great mysteries.

Fischer was not the first to tackle this problem of problems, but he was the first to give the lead in the right direction.

As a result of nearly a century's labor by many chemists and physiologists, the proteins have been shown to be made up of combinations of much simpler substances, the *amino-acids*, the first and simplest of which, glycine, was synthesised years ago by Perkin. The process by which these amino-acids are obtained from proteins is known as hydrolysis, because water plays an indispensable part in the reaction; and this hydrolysis can be brought about either by the use of acids, alkalies or such enzymes as pepsin and trypsin, which are found in the stomach and pancreas respectively. The changes that the protein undergoes in the stomach and the small intestine can be duplicated in the laboratory, and it is then shown that this hydrolysis proceeds in stages, giving us metaproteins, primary proteoses, secondary proteoses, peptones, polypeptids and amino acids—all more or less well-defined substances, whose chemical complexity is greatest at the protein end, and simplest at the amino-acid end.

The crude physical methods of classifying proteins have pointed to the fact that there are some 40 to 50 in number. All of these, when hydrolysed, give a large percentage of the 19 amino-acids which are common to most proteins; the differences among proteins is most marked in the amount of the various amino-acids which they yield when hydrolysed.

Due in no small part to the labors of Fischer and his co-workers, most of these nineteen amino-acids have been synthesised from simpler bodies.



Apparatus used by Fischer in his researches on proteins. [Reproduced from the *Berichte der deutschen chemischen Gesselschaft.*]

If the hydrolysis of proteins, and the investigation of the decomposition products so produced was a difficult task, what are we to say of the reverse process, whereby, by starting with amino-acids, we build up proteins?

Yet that is what Fischer did. He succeeded in working out methods by which amino-acids could be chemically joined on to one another in some such way as the links of a chain. He has given the name *polypeptids* to such combinations of amino-acids.

In his most celebrated experiment in the synthesis of proteins, Fischer succeeded in combining eighteen amino-acids—an octadecapeptid—which is one of the most complicated artificial substances that has ever been produced, and which shows some very striking resemblances to the natural proteins, not the least of which is the way trypsin, the pancreatic enzyme, breaks it up into the amino-acids out of which the artificial protein was built.

The enzymes, as the reader may remember, are specific in their reaction. The trypsin is an enzyme which acts only on proteins and on no other class of substances; hence its action on Fischer's octadecapeptid is good evidence in support of the view that the artificial product is really of the nature of at least the simpler proteins. The starting materials for this synthesis cost $250; "so that," says Fischer, "it has not yet made its appearance on the dining table!"

These glorious researches were still in full blast in 1902 when Fischer was awarded the Nobel prize in Chemistry, the prizes in physics going to van't Hoff's countrymen, H. A. Lorentz and Pieter Zeeman; in medicine, to Ronald Ross, the malaria hero; and in literature, to Theodor Mommsen, the Roman historian. Fischer's diploma reads as follows:

CHIMIE

L'Académie Royale des Sciences de Suide dans sa séance du 11 novembre 1902, a décidé conformément aux prescriptions du testament d'Alfred Nobel en date du 27 novembre 1895, de remettre le prix décerné cette année " à celui qui aura fait la découverte ou l'invention le plus importante dans la domain de la physique " a

EMIL FISCHER

en reconnaissance des mérites eminents dont il a fait preuve par ses travaux synthétiques dans les groupes du sucre et de la purine.

Stockholm, le 10 decémbre 1902.

Hj. Théel

CHR. AURIVILLIUS

If the sugars and the purines deserved the Nobel prize, no prize yet founded is big enough and important enough as a reward for Fischer's protein studies.

In 1907 the Faraday medal of the English Chemical Society was presented to Fischer. This entailed a trip to England to deliver the Faraday lecture—an invitation which had been extended once before in 1895, but which ill-health at the time prevented from accepting.

The historic lecture, largely taken up with a discussion of the chemistry and significance of the three great classes of foodstuffs, was delivered in the theatre of the Royal Institution, on October 18th of that year, with Sir William Ramsay, president of the Society, in the chair. In presenting the medal Ramsay remarked that it was awarded " as a testimony of our great regard for you as our foreign member and of our affection for you as a man." Within seven years a bloody war was to twist affection into the deepest hatred.

Sir Henry Roscoe, a star pupil of Bunsen in Heidelberg, and for years professor of chemistry at Manchester University, had this to say in proposing a vote of thanks to the Faraday Medallist: "I have had the good fortune to hear many Faraday Lectures. I remember with pleasure the eloquence of Dumas; the charm of Wurtz; and the thought and beautiful diction of Helmholtz; but, Mr. President, I do not think that any of our Faraday Lecturers have awakened greater interest than the one to which we have just listened; and this, not only because Emil Fischer is a master of his subject, and because he has laid before us work mainly accomplished by his own inventive brain and his own able hands, but also because the subject of the application of synthetical chemistry to biology, which the lecturer has so ably brought before us, is one which at the present moment is exceeded in interest and importance by no other branch of the science, not even—if I may be allowed, in the presence of the President, to say so—by that of radioactivity. . . . When some years ago we learnt that Emil Fischer had synthesised the sugars, all chemists were loud in their expressions of satisfaction and admiration.[1] How much greater will these expressions be now when we learn what success has attended the apparently almost insoluble problem of the synthesis of proteins. . . ."

Since the time of Fischer's work various phases of protein chemistry and protein metabolism have been pursued with much success by such men as Folin, Levene, Dakin, Jones, Osborne, Van Slyke and T. B. Johnson, in this country, Hopkins, E. F. Armstrong and Plimmer in England, and Kossel and Abderhalden in Germany.

[1] "His (Fischer's) name," said Roscoe on the occasion of the Perkin Jubilee, "has the sweetest of tastes in the mouth of every chemist."

The significance of individual amino-acids in diet has been eloquently expounded by Abderhalden, and Mendel and Osborne, and the additional "vitamine" factors in diet—a distantly related topic, but not to be confused with the amino-acid factor,—have been put on a firm foundation by the labors of Funk, Hopkins and McCollum.

There seems to be some foundation for the fact that the opening up of the Rockefeller Institute in New York City gave German scientists some very unpleasant moments. They were afraid that an institute, devoted entirely to research, and manned by talent second to none, would soon outstrip any university, where of necessity teaching, aside from research, required much attention. This led Ostwald, Nernst and Fischer to start an agitation for the endowment of some similar institute in Germany. The Kaiser gave the full weight of his authority to the scheme, and by his exertions managed to get considerable sums from wealthy Germans. The Research Institute at Berlin—Dahlem was the result.

The initial meeting to celebrate the formation of the *Kaiser Wilhelm-Gesellschaft zur Förderung der Wissenschaften* was held at the offices of the Ministry of Education in Berlin, on Jan. 11, 1911.

The principal address, *Recent Advances and Problems in Chemistry*, was delivered by Prof. Fischer.

With a graceful tribute to the far-sighted policy of the Germans in encouraging science, Fischer proceeded to show that such encouragement brought its own reward. Up to 1911 sixty percent of the total number of Nobel prizes in chemistry had gone to Germans.[1]

[1] It needs perhaps to be emphasized here that, as Fischer himself admits, this excellent German showing is not the result of superior German intelligence, but purely the result of far greater

Fischer next briefly reviewed the important contributions of the chemist to our knowledge of the three classes of foodstuffs, the development of the dye industry, the methods of extracting nitrogen from the air for use as fertilisers, and the manufacture of artificial indigo, india-rubber, camphor and "baekalite."[1] "The beakers and flasks of the scientific investigator," added Fischer, with a twinkle which always delighted his students, "are minute when compared with the vats employed by the chemical manufacturer. This relative difference in size is also borne out by the comparative wealth of these two classes of men."

Turning to plant and pharmaceutical products, Fischer proceeded to exhibit a sample of pure chlorophyll,—the work of Willstätter—and drugs such as veronal and caffeine—both the products of Fischer's genius. Then came this characteristic comment: "One tenth of this quantity [of veronal] would suffice to send this entire gathering into a peaceful slumber. But should the mere demonstration of this soporific—coupled with this lecture of mine—take effect on any susceptible persons present, there is no better remedy than the cup of tea which we are to enjoy later, for tea—and coffee—contains a chemical substance [caffeine] which stimulates the heart and nervous system."

government encouragement than is given elsewhere. In England, France, and to a large extent, in our own country, the chemist—and the scientist generally—received no attention from statesmen until the outbreak of the present war. The disgraceful remuneration offered at colleges, and, with few exceptions, the poor facilities offered for research, have retarded every effort, and have resulted in the loss to universities of some of their best minds. This was before the war. Perhaps things will change now. Perhaps.

[1] This last is the discovery of Dr. Baekeland of New York. The "baekalite," as is now well known, resembles amber, and is used for such articles as necklaces, combs, cigar-holders, etc.

"Caffeine," proceeded Fischer, "was now obtained largely from uric acid, which, in its turn is a constituent of guano.[1] The chemist may apply to such substances the remark made by the Emperor Vespasian concerning the tax-money which came to him from an unclean source: *non elet* (it does not smell)."

A sample of adrenalin, the active constituent of the suprarenal glands, which plays such an important part in the regulation of blood pressure, was also exhibited and its value discussed, and with characteristic German egotism, its isolation, chemical composition, as well as its synthetic production, were claimed for Germans. Not a word was said of Abel, of Johns Hopkins, the pioneer in this field, nor, while touching on the fascinating chapter of "hormones," or body regulators, was any mention made of the two immortals and inseparables, Bayliss and Starling, of University College, London. However, what followed smacks of the now celebrated "2 and 75 percent." "A skin surface well charged with blood—as for instance a red nose—is instantly rendered quite pale on painting it with such a solution." "Unfortunately," proceeded Fischer, amid the shrieks of the audience, "it does not last."

Next, and the last among the list of drugs, came the "606," or salvarsan, the great discovery of Ehrlich, who, by the way, composed one of the audience at this lecture.

The final phase of the discourse dwelt upon the remarkable development of the synthetic scents, which, even in 1911, gave rise to a production of over ten million dollars' worth, and which is now a serious competitor of natural flowers. A sample of *ionone*, the artificial violet scent, contained enough material, we

[1] Uric acid is as important and characteristic an excrement of birds as is urea of man.

are told, "to envelop the entire avenue, *Unter den Linden*,[1] in an atmosphere of violet perfume." Samples showing scents of lily-of-the-valley, mock-orange, lilac, and, the greatest achievement of all, synthetic attar of roses, were also displayed. This last was truly a triumph of the chemist's skill. The natural oil from roses contains no less than twenty different substances. These were all isolated, then synthesised, and finally reunited in just those proportions which give us the pleasant odor of the much-prized rose.

Fischer's researches into the carbohydrates, purines and proteins, is of such enormous importance that, at the repeated requests of the scientific public, they were published in book form in three bulky volumes, the first, *Untersuchungen über Amino-Sauren*, *Polypeptide und Proteine* (1899–1906), dealing with the proteins, the second, *Untersuchungen in der Purin Gruppe* (1882–1906), with the purines, and the third, *Untersuchungen über Kohlenhydrate und Fermente* (1884–1908), with the carbohydrates and enzymes. It is certain that in organic chemistry no three volumes of such far-reaching influence have ever before been published.

Fischer's most recent work dealt much with the tannins, substances that play an important part in leather manufacture.

Fischer's work, his influence as teacher and inspirer of men, raised the Berlin chemical laboratory to the first position among the chemical laboratories of the world. His fame attracted students from every quarter of the globe, and these flocked in such numbers to him that they soon counted in the hundreds, and special *privat-docenten* had to be appointed to take care of them. It thus came about that many of the men who

[1] Berlin's principal thoroughfare.

had gone to Berlin to work under Fischer in reality worked under some of Fischer's *privat-docenten*, and, outside of the lectures, probably did not see Fischer himself more than two or three times during their three or four years' stay in the German capital. At one time or another H. Gideon Wells, the excellent pathologist of Chicago University, T. B. Osborne, of the Connecticut Experimental Station, and the foremost authority on vegetable proteins, and P. A. Levene, D. D. Van Slyke, and W. A. Jacobs, the well-known physiological chemists of the Rockefeller Institute, were his students. Of his many pupils Fischer considered Emil Abderhalden, now professor of physiology at Halle University, a Swiss by birth, the most gifted.

Fischer's death is an irreparable loss to science. He is so much of our generation that one hesitates to use superlatives, but one is sorely tempted to speak of him as the greatest organic chemist of all times.

References

Part of the material has been obtained from private sources. The account of Fischer in the Nobel volume (1) has been of great service. Fischer's work on purines, carbohydrates and proteins has been published in book form (2, 3, 4). His address to the members of the English chemical society (5) contains much of interest. See also 6. A summary of Fischer's work on tannins has appeared in English (7). Enzymes are discussed in Dr. Harrow's article (8).

1. Anon.: Hermann Emil Fischer. *Les Prix Nobel* (Stockholm), 1902, p. 58.
2. Emil Fischer: Untersuchungen in der Puringruppe, 1882–1906 (Julius Springer, Berlin. 1907).
3. Emil Fischer: Untersuchungen über Kohlenhydrate und Fermente, 1884–1908 (Julius Springer, Berlin. 1909).

4. Emil Fischer: Untersuchungen über Aminosauren, Polypeptide und Proteine, 1899–1906 (Julius Springer, Berlin. 1906).
5. Emil Fischer: Synthetical Chemistry in its Relation to Biology. *Journal of the Chemical Society* (London), *91*, 1749 (1907).
6. Emil Fischer: Recent Advances and Problems in Chemistry. *Nature* (London), *85*, 558 (1911).
7. Emil Fischer: Synthesis of Depsides, Lichin-Substances and Tannins. *Journal of the American Chemical Society*, *36*, 1170 (1914).
8. Benjamin Harrow: What are Enzymes? *Scientific Monthly*, March (1918), p. 253.

INDEX

Names of persons are printed in *italics*.

INDEX