NATIONAL ACADEMY OF SCIENCES

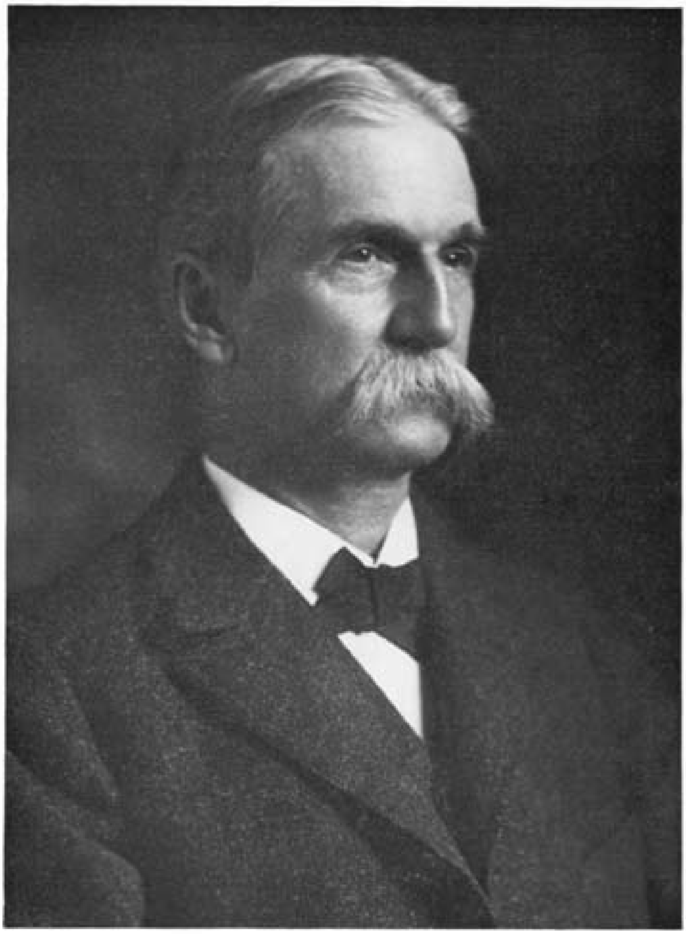
CHARLES LORING JACKSON

1847—1935

A Biographical Memoir by

GEORGE SHANNON FORBES

Biographical Memoir



Charles Loring Jackson

CHARLES LORING JACKSON

April 4, 1847—October 31, 1935

BY GEORGE SHANNON FORBES

CHARLES LORING JACKSON, a pioneer in American chemical education and research, was born in Boston, April 4, 1847. His father, Patrick Tracy Jackson II, had a strong taste for scientific work; to help Jackson's grandfather, temporarily embarrassed, he entered business upon graduation from Harvard College in 1838. The grandfather, Patrick Tracy Jackson, was prominent in early New England cotton manufacture; he also built the Boston and Lowell railroad. Jackson's mother, Susan Mary (Loring), was a woman of marked intellectual ability who devoted much time to the education of her children. Her father, Charles G. Loring, was an eminent lawyer who often locked horns with Webster and Choate.

Jackson's scientific interests were fostered by both parents from his early years. Although his preparation for college was hampered by illness, he entered Harvard College with the class of 1867, and was graduated in that year. His studies included qualitative analysis, physiology, and botany; he was not, however, a high-ranking scholar.

The Archives Department of Harvard University kindly allows me to quote at length from Jackson's autobiographical notes. These are of the utmost value for the purposes of this memoir because of their historical interest and also because at this late date it would be impossible to obtain a sufficient amount of authentic information from other sources.

Excerpt from Jackson's autobiographical notes:

When I graduated from Harvard College in 1867, Professor Cooke asked me to be his lecture assistant. So I began my career with a most meagre equipment. I had had an excellent course of lectures on inorganic chemistry from Professor Cooke, a year's study of his Chemical Physics . . . and the elective in qualitative analysis. My salary was to be free tuition, but at first my duties took all my time, and later an attack of appendicitis (it was not discovered, as it was called enteritis) prevented me from getting any teaching.

On my twenty-first birthday, April 4, 1868, I was appointed regular assistant, as my predecessor had to leave because of ill health. My salary was $600, and Professor Cooke was to teach me to be a chemist, but this arrangement was no more effective than before, as Cooke was so overwhelmed by his regular work, and my place was no sinecure, for in addition to running, and, for that matter, doing most of the teaching of the elective, I did all the storekeeper's work (giving out the apparatus and keeping the accounts), and part of the janitor's (making the reagents, filling the bottles and keeping them in order), so I was able to snatch only occasional half hours—more usually quarters—in which to teach myself qualitative analysis.

I did, however, get a full course in mineralogy by taking the students' course and supplementing it with a study of the whole mineral collection.

At the beginning of 1873 I had a slight attack of rheumatic fever, and when I got back to work, Professor Cooke advised me to take a year's leave of absence and study in Germany.

ROBERT BUNSEN

After a short trip in Switzerland with Professor Trowbridge and some weeks studying German in Dresden, the beginning of the semester saw me established in Heidelberg to study inorganic chemistry under the great Bunsen. He was a big burly man with a round ruddy face clean-shaved but valanced with whiskers and beard below his cheeks and chin. As some one said, he looked more like a carpenter than a professor. But his expression was charming since his beautiful character shone through it. It was a wonderful combination of the most placid good nature with farseeing wisdom.

We frequently complain justly of the poor accommodations in our laboratory but they are paradise compared with Bunsen's. My desk was covered with such a sticky paint that if you put a beaker down on it the chances were that it left its bottom sticking to it when you took it up. To remedy this defect one got a large piece of brown paper and some thumb tacks from the janitor, covered one's desk with this, and worked on paper! I was warned at once not to use any of the reagents on my desk as they were made so badly that they were worse than useless. There was only one set of reagents for the forty-odd students and as it was in the other room I had to walk twenty to thirty feet whenever I needed a reagent and then often found some one else using it. The one good thing in the laboratory was a set of overelaborate waterbaths. All of which impressed on my mind that it is the man that counts, not the laboratory.

Bunsen began his day with an hour's lecture at nine o'clock. His experiments were on a smaller scale than ours but numerous and exceedingly good. His style was a model for academic lecturing, clear, concise, going straight to the point with no sensationalism. After his lecture he wandered about the laboratory nearly all day with a two-inch stump of a cigar in one corner of his mouth. I never saw it longer or shorter. Sometimes, when he was teaching you, he would stop in such an impressive way that you were sure something of the first importance was coming; then he would take the stump from his mouth, hold it in your lamp until it was lighted, return it to his mouth, where it promptly went out, and go on with the thread of his talk.

I could not approve of the style of his teaching. In the first place he was entirely unsystematic. Instead of going to each desk in regular order he went to any one who could catch him, and the result was that he was always pursued by a number of students—sometimes as many as five waiting to grab him. It was a wonderful sight to see him, with these men quivering to get at him, go on with the man he was teaching as calmly as if there were no one in a hundred miles. This wasted a great deal of time for the student, and shy men were apt to get passed over, but the cheeky got more attention than in a laboratory run systematically. It is needless to remark that I belonged to the cheeky class.

Then his teaching was decidedly mechanical. He gave you most particular directions for the next step in your process as, "Then you make it into a dish and evaporate it very precociously and get me to see what has happened." "Precociously" puzzled me for a long time, then I saw it meant precautiously. These were the only mistakes he made in talking English. The difficulty with his teaching was that he left nothing to the student. It was most careful and detailed but did not encourage initiative or even reading on the part of the student. I asked him for a book and he discouraged me but, when I pressed him, said reluctantly I might get Rose. He despised the bible of quantitative analysis Fresenius. With all these defects in principle, the effect was very good. Even a poor teacher would have produced fine results with such devotion to his students and when it was a perfect teacher according to his lights and in addition a very great man, working under him was one of the highest privileges a chemist could have.

He put me first on the analysis of potassic dichromate, which I told him I thought too easy, but he said he wanted me to learn how to manage the Bunsen pump that he had recently invented, and when I did the analysis I found how wise he was, as I not only learnt about the pump but also a host of other things most necessary for me. Among them the use of the little finger which he had discovered is a Heaven-sent swab far superior to the clumsy contraption of glass and rubber then and still in use, as the sheeplike character of the chemist, who must follow a bell-wether no matter how stupid, has prevented the general adoption of the little finger. I learnt also many important points in the principles of quantitative analysis. One came to me in a startling way. He was showing me how to transfer my potassic chloride from the evaporating dish as large as a dessert-plate over which it was spread to my crucible, using only a thimble-full of water and yet not leaving the slightest trace of it behind, and was using his little finger (about the size of my thumb), when I saw the water running down black as ink from the dirt washed off of his finger. . . .

My next analysis was copper pyrites, then rhyolite (a rock), chrome iron and triphiline (a mineral phosphate). This last was suggested to me by Arthur Michael, who proposed we should do it together, so we started at the same time. He spoilt his work so often that he had to begin again seven times, but worked so fast that he got a good result at the same time that I plodded to the end of my single analysis.

I reached this point by the middle of the half year and then felt I had mastered quantitative analysis sufficiently and took up gas analysis by the lengthy Bunsen methods, as Hempel had not yet done his work on this subject. I also prepared salts of the principal platinum metals from one of the residues the Russian government had given Bunsen. When I had finished this work there were only a few weeks left in the semester and Bunsen had nothing to suggest for these that especially interested me, but I took up ash analysis with little enthusiasm. Still later he suggested a little research to see if lead could be determined as the dioxide. I made one successful determination but as there was no time to make more it went for nothing.

I was well satisfied with this first half year. I had sufficiently mastered quantitative and gas analysis, had got some idea of inorganic preparation, and had greatly increased my knowledge of inorganic chemistry by Bunsen's lectures and my own reading....

When I went to bid him goodbye, he expressed a good deal of disappointment that I was going. . . . I told him that I had only one year, and felt that I ought to get a start in organic chemistry, and so was going to Hofmann. He approved highly, and said Hofmann was an excellent man, thus showing his greatness of mind, as most German professors had only abuse for Hofmann because he held the first chemical prize, the Berlin professorship.

AUGUST WILHELM VON HOFMANN

When I came abroad I expected to get a full year of work and was a good deal disappointed when I found that with vacations and holidays I should get a bare seven months, but there was no use in grumbling, so I went to Italy with Hodges for the winter vacation, and then to Berlin for the short (three months) semester, which I hoped would be enough for the smattering of organic chemistry—all I needed to help me in my inorganic work. But alas for my plans! As soon as I fairly tasted blood in organic chemistry that plan went by the board and I lost my heart to organic and have been faithful to my love ever since.

This was not at first, however, as I was set to learn to make organic analyses—a type of work entirely new to me. The fellows advised me to go to Bauermeister the janitor, who made all their analyses, but I felt it undignified for a professor to be taught by a janitor and so went to the assistant whose duty it was, but it was far from his pleasure, as he thought me a complete nuisance interfering with his researches; he did as little for me as he could and that little badly. The result was perhaps the most unhappy month of my life. I thought chemistry was slavishly obeying directions no matter how stupid, and this view had been encouraged by Bunsen's method of teaching; so if there was any fool mistake I did not make, I do not know it. I behaved as if I were absolutely brainless, and did not make a single good analysis. It was heartbreaking! Suicide seemed very attractive and the brightest prospect was resigning my professorship and finding some occupation contemptible enough for my meagre abilities....

At the end of this month I decided that I could do the additional work needed to make me an analyst in Cambridge and took up research. If Hofmann had known I had done no work in organic chemistry he probably would not have let me do it, but fortunately he did not, as in this way I got exactly the teaching I needed most.

As I now came under Hofmann's direct teaching, I will describe him. He was a complete contrast to Bunsen. Under the middle height, so that I wondered how he could have the physique to do such an enormous amount of work, but on looking more carefully at him I saw his body was extraordinarily big and massive.

He wore a white mustache and a little pointed beard which usually curled up at the end. In the laboratory he wore a not over-new tall hat and, if it was winter, a white knit comforter around his throat.

He gave three lectures a week but each was two hours long. One hour of such a lecture is as much as most men can struggle through, and it was a constant wonder to me that he could give two and, as if that were not enough, spend the whole morning teaching in the laboratory. I did not like his type so well as Bunsen's, as it had much of the popular rather than the college lecturer, but it was very effective. His experiments also were numerous and good, although on a smaller scale than ours.

But it was in the laboratory that he was the most unlike Bunsen. He bustled into the room, ran up to the first man, and went through the desks in regular order. His first question was, "We will try a pair of experiments." Then he made a circle with thumb and forefinger which meant a watchglass, stirred with his right hand which meant a rod, and then began experiments on experiments, all as instructive as possible since they showed you how he attacked and solved chemical problems. Soon your desk would be covered with watchglasses and if after he had used two dozen or more you had no clean one he shook his legs at you—the most alarming gesture of impatience I have ever encountered. He kept himself and you on the clean jump. When he gave you a new research his first question the next day was, "What has been done on this subject?" And woe to you if you had not looked it up thoroughly.

He encouraged you to think for yourself and received new ideas proposed by you most kindly, advising you to try them instead of saying, "Poo! That will not work." In short, just the sort of inspiring teacher I needed.

My first research was to repeat some work done by a man named Brakebusch and then expand the work in an interesting way. But, alas, I could not get any of his results, so I began to think I was as ineffective in research as in analysis. At this point Hofmann said, "I will give you something I am sure will work." But before I take this up I will say that the next year, when I had found my feet in research, I took up the Brakebusch experiments, repeated them on a large scale, and proved they did not give his results. It then came out that he had left the Berlin Laboratory (where he had been an assistant to Hofmann) in very bad odor, had gone to Hamburg and asked an apothecary to let him marry his daughter and inherit the shop. The apothecary answered, "You must get your Ph.D. first," whereupon in a month or two he had presented these experiments as a thesis at Goettingen and got his Ph.D. with the desired results. Every one who knew him thought it a complete fraud even before I had proved it one. It was beautifully executed with melting points and analyses about as far out as real ones would be. I never heard whether my exposure of him resulted in a divorce.

To return to my second subject: It was the preparation of some new organic compounds of selenium—an excellent one, as on account of their terrible smell (perhaps the worst in the world) they had been little studied, and in fact in a very short time I discovered my first new compound, but when Bauermeister the janitor analyzed it for me, it turned out not to be the one we were expecting. Here came the turning point in my life. My brains came up out of my neck, for when I showed the analysis to Hofmann the next day I was able to tell him what the substance was, why we had not got the benzylselenide we were expecting but the diselenide, and how we could make it. In short, I began to be a chemist.

The study of my diselenide kept me busy all the rest of that semester. The determinations of carbon and hydrogen made by the janitor were enough to settle the three new compounds obtained, but I wanted also selenium determinations and could get no good ones by any of the methods known (possibly because of my clumsiness as an analyst), so I worked out a new method—another landmark in my history, as it was my first attempt at original work of my own. I submitted it to Hofmann, who said, "Try it." So I mixed my substance with baric nitrate and heated it in a glass tube. I kept my eyes as close to the tube as I could without burning my eyebrows and was much interested in seeing little flashes of flame play along the tube. Next I heard the loudest sound and saw the brightest light in my experience and found myself at the other side of the room. On opening my eyes, I found I could see out of both, but everything looked strangely red and putting up my hands I found they were full of blood. The explosion had thrown a handful of splinters of redhot glass into my face from a distance of six inches.

The next day was a dreary one, as the doctor had put atropine into my eyes so that I had to be in complete darkness, and I saw no one but my landlady when she brought my meals, I was not certain that I had two eyes, and all this so far from home. The next day the Americans and English came to the rescue finely. Pitman, whom I knew best, put off starting for home three days to take care of me. Strangely enough, there was no serious injury and as soon as I could travel I started for home, as a few weeks before

I had very unwillingly decided to stay a second year in Germany but wisely passed my summer vacation in Beverly. . . .

After I went back to Berlin I again took up my selenium work with vigor, made the benzyl selenide and some of its derivatives, and extended it to the methyl compounds, correcting some mistakes of my predecessors. This work was described in three preliminary papers and one long one covering all the work. Another subject Hofmann gave me, however (making a croton alcohol), gave no result whatever,

One day about the beginning of this semester Hofmann asked me if I would correct the English of a lecture on Liebig he was writing as the Faraday Lecture of the London Chemical Society. After living twenty years in England he spoke the language well, but now he had grown rusty and needed my help. I jumped at this piece of good luck, as it gave me a chance to grow intimate with Hofmann, and to learn a great deal about writing English and also about Liebig. So Hofmann gave me a bundle of manuscript, and that evening I tackled it. It was not in German script, but his writing was so bad that after working at it for half an hour I had not read a word. This was terrible, for if I had to tell Hofmann I could not read his writing, instead of growing intimate with me he would certainly be down on me. So I set to work again in a grim do-or-die spirit and found to my great relief that my half hour of work had made me able to decipher it slowly, and before the end of the evening I could read it almost easily. . . .

The week before he went to England to deliver the lecture I worked fifteen hours a day (eight in the laboratory and seven on the lecture) but I did not feel troubled as I was eating twice as much as usual. On the last day my appetite dropped and I said, "I must stop." Fortunately the work stopped then too. . . .

I must now go back to describe my last semester in Europe. When Hofmann heard I meant to pass it in Berlin he said, "Then you must come as my guest." So when I got back from my winter vacation in Paris I began work without any formalities. . . .

Hofmann gave me one or two new researches and Oppenheim, the ausserordentlich (assistant) professor, and the assistant Biedermann asked me to take up researches with them. The one with Oppenheim gave results which were published in a short paper.

One day near the end of the year Hofmann asked me what I was going to do next. I answered that I meant to stop work for the year. "Ah! You will continue your work in America." . . .

I took no degree in Germany as I decided (wisely) to devote my whole time to research, which I could not study to advantage at home, instead of spending a large part of it studying chemistry and physics from books for a degree which I could do as well alone.

I came home well satisfied with my two years abroad. I had built a sufficiently solid foundation of those parts of chemistry in which I did not specialize and in my specialty I had got some knowledge of organic chemistry and, best of all, had learned how to carry on an organic research. In short, I had become a chemist. . . .

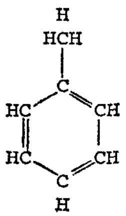
CHEMICAL RESEARCH AT HARVARD—THE FIRST NEW COMPOUND

When I came home after my two years' study in Germany I was confronted with a problem which caused me a great deal of anxiety. I had, to be sure, learnt how to carry on organic research under a teacher but should I be able to do it alone? . . .

While still in Berlin, I had selected the brombenzylbromides as my subject for research. My professor, A. W. Hofmann, asked what I meant to take up and, when I told him, said, "Beilstein has already pretty much covered that ground." But I had been looking up the subject and knew better, as the greater part of the field had not even been touched by him.

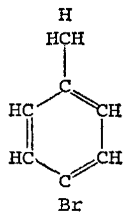
These brombenzylbromides were to be made from toluol, a liquid derived from the distillation of coal tar and looking much like gasoline, although not nearly related to it. Toluol is made up of seven atoms of carbon and eight of hydrogen—C_7H_8. Six of the carbon atoms are arranged in a hexagonal ring and five of the hydrogens are attached to five of these carbons. The sixth carries the seventh atom of carbon and to this the remaining three atoms of hydrogen are attached (1). The hexagon of carbon atoms is called the ring, the seventh atom of carbon with the atoms of hydrogen attached to it the side chain.

1



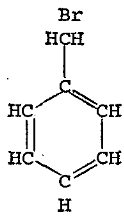
Toluol

2



Parabromtoluol

3



Benzylbromide

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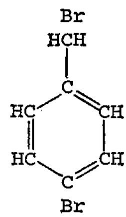
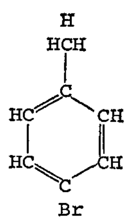


Brombenzylbromide

If toluol is treated with bromine in the cold, an atom of hydrogen is removed from the ring (combining with bromine to form hydrobromic acid) and its place is taken by an atom of bromine forming a new compound called bromtoluol—a mild substance with a not unpleasant smell; but, if bromine is passed into boiling toluol, a different compound is formed, shown by its different smell and still more by its being a savage thing which attacks the eyes furiously. This is called benzylbromide. There two different substances contain the same atoms in the same proportions—seven of carbon, seven of hydrogen and one of bromine, so that the difference between them must depend on the arrangement of the atoms, and it has been proved that the difference consists in the fact that the bromine in the bromtoluol is attached to the ring (2), in the benzylbromide to the side chain (3), the more savage disposition of the latter being due to the fact that the bromine is more loosely attached than in the bromtoluol. The discoveries I have just described had been made before I began my work, and the problem I meant to tackle was to make a brombenzylbromide, that is, a substance combining the peculiarities of (2) and (3)—a thing that had never existed before (4). It was almost certain that such a substance could be made by treating hot bromtoluol with bromine. If it was not formed in this way it would be a very interesting observation but disappointing, because it would not lead to further work.

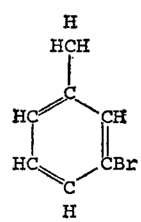
Obviously the first thing for me to do was to prepare a supply of bromtoluol. The simplest way to do this was to treat toluol with bromine in the cold, but this did not give a pure product, because two bromtoluols are formed by this process. There are three bromtoluols differing in the place in which the bromine is attached to the ring. If it is upon the carbon opposite the side chain it is called parabromtoluol (5). If next but one to the side chain, metabromtoluol (6). If next the side chain, orthobromtoluol (7).

5



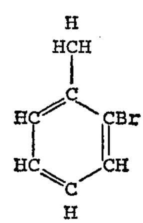
Parabromtoluol

6



Metabromtoluol

7



Orthobromtoluol

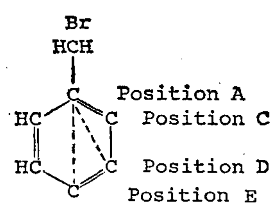
The product from bromine and cold toluol is a mixture of much para with a little orthobromtoluol, so if treated with bromine when hot this would give a mixture of two brombenzylbromides which it would be almost impossible to separate into the two pure bodies. . . .

Amid this disappointment I remembered that I had brought some parabromtoluol home with me from Germany. Why had I not remembered it before? There was very little, but enough for one experiment, which I quickly arranged. I have already explained that we felt the whole success of our lives depended on this momentous experiment, so Hill and I kept our eyes glued to the flask in which the bromtoluol was boiling and were delighted to see the brown color of the bromine disappear as it struck the vapor, and presently white clouds of hydrobromic acid appeared at the top of the apparatus, which looked as if all was going well. But when the experiment was finished and the product on cooling deposited white crystals which could be nothing but the expected parabrombenzylbromide, we fairly danced about the laboratory.

This was the first new organic compound made in the Harvard laboratory—the first child of a family so numerous that it is counted by hundreds—for all I know, now by thousands; and quite as important, it proved we could carry on researches and the whole field of organic chemistry lay open before us. . . .

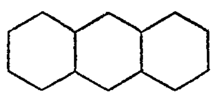
In Jackson's autobiography there is a section entitled "An Immortality that Failed." Here is his own story in condensed form.

By 1878 he had prepared the three isomeric brombenzylbromides in a state of high purity, but in rather small quantities, for a crucial experiment. It was already known that the side-chain bromine could be replaced by other atoms or groups in considerable variety—the acetate group, $C_2H_3O_2$, for instance. In a given time, treatment with sodium acetate displaced unequal amounts of bromine from equal weights of the three compounds. It was evident that the differences were in some way related to the distances between the side-chain bromine A and the bromine atoms attached to the ring at positions C, D, or E. Jackson represented these distances by the lines AC, AD, and AE on a regular hexagon, irrespective of the fact that carbon, not bromine, is the atom

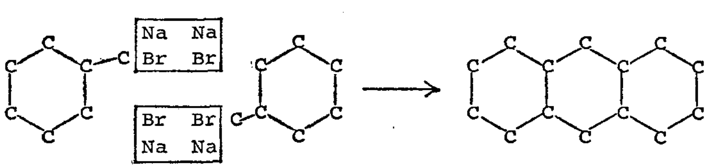


directly attached to the hexagon at A. Up to this time the picture of the benzene ring as a regular hexagon had been a pure assumption. He imagined that the displacement reactions were governed by the gravitational attractions between the heavy bromine atoms. If so, the square roots of the weights of bromine displaced ought to be proportional to the distances AC, AD, AE. His first results seemed to confirm the above hypothesis, and he exulted in the belief that he had established the regular hexagonal structure experimentally. A second set of experiments, with the relatively small quantities of material still available, gave different ratios, results which he thought due to the greater effect of experimental errors. A very guarded announcement was published, after which a great deal of inconclusive work was done on the problem. In the end, worry brought on an attack of nerve prostration, the effects of which persisted for years after. In consequence, he adopted a somewhat rigid schedule, adjusting the hours of work each day to safe limits and avoiding unnecessary exertion.

When he returned to his laboratory, his assistant J. F. White told him that his proposed synthesis of anthracene



by removing four atoms of bromine (by action of sodium) from two molecules of orthobrombenzyl bromide had been successful. This proved that the two middle carbon atoms in anthracene are connected with the benzene rings in the ortho position. Disregarding hydrogen atoms and double bonds, the reaction could be represented thus:



This genuine triumph had a magical effect on Jackson and restored his initiative. He soon proved that sodium acetate and also water removed bromine in the same ratios from the three brombenzylbromides, respectively. But then he found that the amount of bromine thus removed from the side chain was the same whether chlorine, bromine, or iodine was present at E, the para position. That convinced him that the distances in the hexagon could not easily be correlated with gravitational effects. This undesired outcome was a real contribution to the knowledge of intramolecular forces, even though the bid for "scientific immortality" had failed.

Throughout Jackson's life the preparation and properties of halogen substitution products of aromatic hydrocarbons—later of quinones—remained his major research interest. One of the most significant of his findings was that the introduction of nitro groups ($-NO_2$) into such compounds enhanced the reactivity of the halogen atoms previously attached. Owing to ill health, he did little work with his own hands after 1887, but he continued to inspire his graduate students and assistants, who turned out an imposing volume of scholarly and important results. The thorough and stimulating instruction which they received stood them in good stead during later life. Many of them became prominent in education, research, and industry, as a survey of Jackson's bibliography will show.

The autobiographical manuscript left by Jackson describes his early experiences as a teacher of elementary chemistry.

CHEMISTRY I AT HARVARD

In this part I shall describe only those of my methods that were unusual and therefore shall say nothing about my teaching of organic research in which I followed in the beaten tracks. In my teaching of inorganic chemistry to beginners, on the other hand, I introduced many innovations or at least what I supposed to be such, and the most heretical of these was the order in which I took up the elements. When I planned my lectures I tried to arrange them according to the periodic classification but soon got so entangled in the web of cross-relations that I gave it up and decided to be guided only by the convenience in teaching. The usual arrangement of the elements with the non-metallic first seemed to be about the worst that could be devised, as it puts most of the hardest facts at the beginning before the student is prepared to struggle with them to advantage. It seems strange that such a system should be used by any one. I suppose the reason is that most chemists, having been brought up under it, think of changing this order no more than they would of altering the order of the letters of the alphabet.

My aim was to begin with the simplest compounds and work up by degrees to the more complex. So after two lectures on theory, of which more later, I took up oxygen and hydrogen because of their importance and then began the systematic work with the metals of the alkalies, studying the salts of these with the common acids, a few words about which had made them something more than mere names. After the student had mastered salts of univalent metals we passed to those of bivalent metals of the alkaline earths, then the trivalent aluminum, after which I gave two lectures on the periodic classification, which were among my most exciting, as the men were now prepared for it, in fact, had begun to feel the want of it. Next chromium and its relations introduced the elements with more than one quantivalence, and then the remaining metals were spotted in, the position of each in the system being dwelt upon. Before the middle of the second term the arsenic group led us to the nonmetallic elements with phosphorus and nitrogen when the class was thoroughly prepared to grapple with their difficulties. . . .

Jackson viewed the laboratory work of Chemistry 1 as a training in inductive reasoning. He says:

The directions for laboratory experiments bristle with questions to be answered by reasoning from the work just done, and the teachers paid more attention to these questions than to anything else. . . . Once when I was pluming myself on it as my greatest discovery in teaching, I remembered that I had been anticipated in it by a man named Socrates.

Here is Experiment 10, from the manual which I used as a freshman (1898–1899). There were about seventy experiments, all told.

10. Test for H_2SO_4 or a Sulphate. To dilute $H_2SO_4 + Aq$ (test tube) add $BaCl_2$ (write reaction), then dilute $HCl = Aq$. Treat Na_2HPO_4 in the same way. Why is it necessary to add HCl in testing for H_2SO_4? To a few drops $BaCl_2$ add much strong HCl + Aq. Why must *dilute* HCl + Aq be used in testing for H_2SO_4?

Jackson believed that students should begin scientific investigation as soon as possible.

A device from which I expected a great deal consisted in giving problems in the laboratory during the second term. These were preparations, or separations of two elements of increasing difficulty, which were worked out from the experiments already tried on these substances.

Here are some of the problems assigned in the spring of 1899, as entered by me on the back of my laboratory manual:

Preparations	*Separations* Each compound was to be isolated in a "pure" state.	
Fe_2O_3 from Fe	$MgSO_4$, $ZnSO_4$	$CuSO_4$, Fe_2Cl_6
$MgCrO_4$	$CuSO_4$, $Ni(NO_3)_2$	NaCl, Na_2SO_4
HPO_3	$BaCl_2$, $Ca(NO_3)_2$	$CuSO_4$, $SnCl_4$
$(NH_4)_2S$ solution	$CaCl_2$, $MgCl_2$	$BaCl_2$, $CaCl_2$

My most vivid undergraduate recollection of Jackson pictures him seated in a dilapidated arm chair in the northeast corner of the basement laboratory of old Boylston Hall, as I drew near to submit my efforts for his kindly criticisms.

Jackson issued a printed syllabus for his lectures, but this failed to suggest the elegance and charm of his presentation. Here, for instance, is the section on barium:

Barium, Ba, 137.4.
Bologna Phosphorus, BaS, 17th century. Scheele, 1774.
Barite or heavy spar, $BaSO_4$. $BaCO_3$.
$BaCl_2$, $BaSO_4 + C =$? $BaS + HCl =$?
Test for H_2SO_4, $BaCl_2 + H_2SO_4 =$?
BaN_2O_6 like $BaCl_2$.
BaO_2H_2, $BaS + Cu + H_2O = BaO_2H_2 + CuS$.
$BaSO_4$. Solubility, color. Permanent white, paper collars.

Some enterprising character produced a printed book entitled *General Descriptive Chemistry,* which had eighty-seven large pages but no mention of an author or a publisher. (I have just discovered a copy, with the date 1896 written in, in our department library.) It was said to be for sale in a neighboring barber shop. It included copious notes on the lectures—with numerous errors, a complete write-up of all the laboratory experiments, and (worst of all) detailed answers to the laboratory problems. Jackson was, of course, exasperated by all this, so in his opening lectures he always denounced the book, and promised to expel from the course anyone who used it. In all probability no one was ever caught.

Jackson commented on the situation as follows: "This dishonesty was disheartening, but it must be remembered that many men think that the code of college morality is distinct from that of real morality." On the other hand, he was convinced that the great majority depended on their own efforts, and profited thereby.

The signal success of Chemistry 1 is attested by its heavy enrollment over the years and by the great number of students who continued their chemical studies in more advanced courses.

My last student contact with Jackson came with my examination in inorganic chemistry for the doctor's degree. His questions were always written on small scraps of paper—just the symbol of a single chemical element. This called for a three-hour written discussion of the preparation, properties, reactions, and uses of the element and of its compounds. Well, in 1903 and 1904 it had been Sn, so in 1905 some of our group felt that tin could safely be neglected in the "boning-up" process. But when we were assembled, Jackson entered the examination room with his usual quizzical smile, and, once more, each scrap of paper read Sn.

Chemistry 1 and the work of his research students were Jackson's paramount interests. He expended enormous labor on his publications and took great pride in them. An account of each research, as a whole or in part, appeared in two, three, or even four periodicals, which often included the organ of the principal German chemical society. One or two of these were usually preliminary or highly condensed and one or two in great detail. In each report of the Harvard Class of 1867 one finds a complete list of his papers which had appeared since the previous report. The preparation of a bibliography for this memoir was greatly facilitated by his own record (with his old-time system of abbreviation) available in the Harvard Archives, which covered the years 1875 to 1908. For greater clarity, papers of nearly equivalent content have been grouped together.

Jackson's eminence was widely recognized. He was Instructor in 1870 (?), Assistant Professor in 1871, and Professor 1881–1912, after which he became Emeritus. In 1897 he received the coveted Erving professorship. He belonged to the American and the German chemical societies and the American and the British Associations for the Advancement of Science, being a corresponding member of the latter. He was elected to the American Academy of Arts and Sciences and to the National Academy of Sciences (1883). From 1894 to 1903 he was Chairman of the Division of Chemistry. Due to his interest in the student body, he was for fifty years a proctor in the dormitories of the Harvard Yard—Grays 5 from 1868 to 1871 and Holworthy 11 from 1871 to 1918.

Jackson's regimen of severely controlled activity for nearly half a century enabled him to live and function beyond his eightyeighth birthday. But, as a result, his biography cannot be fashioned into a chronology of events having wide repercussions. Three members of the Harvard Faculty of Arts and Sciences, Gregory P. Baxter, Francis G. Peabody, and Edwin H. Hall, who perhaps knew him best, prepared a minute of his life and services, which closes with the following picture of Jackson outside of his laboratory.

"His interest in art dates from study in his youth, which, though it convinced him that he had little talent, developed a critical taste which was permanent. Amateur theatricals, at home where he wrote the plays for the benefit of his invalid mother, and in the Cambridge Dramatic Club, were for many years a source of pleasure. Fondness for the solving of problems of all sorts undoubtedly was at the bottom of his keen interest in chemical investigation. Even at eighty-five, when owing to a serious operation in 1928 he had been a semi-invalid for five years, he began an intense study of what he called "The Mysteries of Shakespeare" in order to discover the answers to such questions as: What parts did Shakespeare act in his own plays? Who took the various women's parts? and he had already prepared for publication two articles on these subjects. In *The Gold Point,* published in 1926, is collected a series of fanciful stories originally told to groups around the fireside.

"His personal likes and dislikes were pronounced, perhaps not always unaffected by bias, but to his friends he was the soul of loyalty and generosity. Family affection and pride were particularly strong. During most of his life he lived at the family residence, 383 Beacon Street. Summers, however, were always spent at Pride's Crossing, where gardening became the greatest of his hobbies. His friends will always remember more vividly their visits to his summer home. There, among his flowers, with the sea almost at his doorstep, he was the perfect host, entertaining, witty and sympathetic."

KEY TO ABBREVIATIONS

Ber. = Berichte der deutschen chemischen Gesellschaft
Am. Chem. J. = American Chemical Journal
Ann. Chem. = Annalen der Chemie
Am. J. Sci. = American Journal of Arts and Sciences
J. Am. Chem. Soc. = Journal of the American Chemical Society
Proc. Am. Acad. Arts Sci. = Proceedings of the American Academy of Arts and Sciences
Proc. Chem. Soc. = Proceedings of the Chemical Society of London

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