From Immigrant to Inventor

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X.—THE FIRST PERIOD OF MY ACADEMIC CAREER AT COLUMBIA UNIVERSITY

THE new "Department of Electrical Engineering in the School of Mines of Columbia College" had announced its courses of instruction quite a number of months before I arrived in New York. The late Francis Bacon Crocker, at that time the newly appointed instructor in electrical engineering and my future colleague and lifelong friend, had been consulted with regard to these courses, and he was most liberal to the theoretical side, which was to be my share of the instruction. He attached much importance to the fundamental theory, although he was a practical engineer. The new department was to be independent from the other scientific departments. We had some difficulty, however, in maintaining that independence; the older departments of engineering showed a disposition to claim some right of guardianship over the new infant department. For instance, many chemists thought that electrical engineering was largely chemistry on account of the storage batteries, the galvanic cells, and the electrochemical processes which formed an important part of the electrical operations in the early history of applied electricity. Others claimed that, since mechanical engineering attended to the design and the construction of electromagnetic generators and to the power plant which furnished the driving power, electrical engineering was, therefore, largely mechanical engineering.

Crocker and I maintained that there was an electrical science which is the real soul of electrical engineering, and that every other abstract science or its application was an incident only in electrical engineering. We won out in spite of the fact that at other institutions of higher learning in the United States electrical engineering was taught in the departments of physics or of mechanical engineering. But it was not an easy matter in those days to persuade people that the electrical science with its applications was then, or that it ever would be, big enough to need a department of its own, like, for instance, civil engineering.

A small brick shed, a temporary structure, had been built at Columbia College to accommodate the new department. The students called it the "cowshed," and the boy who invented the name did not indulge in any stretching of his imagination. It certainly looked like a cowshed. The laboratory equipment consisted of a dynamo, a motor, and an alternator, with some so-called practical measuring instruments. When I compared the facilities of the new "Department of Electrical Engineering at Columbia College" with that of the Polytechnic School in Berlin, I felt somewhat humbled, but not discouraged. I said to Crocker: "Our guns are small and few in number; the men behind the guns will have to expand much beyond their present size if this department is to make any impression upon the electrical art." "Pupin," said Crocker, "you have no idea how rapidly a young fellow grows when he tries to teach a new subject to poorly prepared beginners."

Crocker and I were given to understand that any additional equipment during the first year would have to be bought from contributions outside of the university. We raised some money by giving a course of twelve popular lectures for which we charged ten dollars per person. Each lecture lasted two hours; we were somewhat dubious about their quality, and so we provided a generous quantity. We raised in this manner three hundred dollars and bought additional equipment, but no two young scientists ever worked harder to earn three hundred dollars. The experience, however, was worth many times that amount. Our audience consisted of business men and lawyers, who were either interested in the electrical industries, or intended to become interested. They had hardly any previous scientific training. It took much judgment and skill to talk science to these people without shooting much above their heads. Every one of them believed that the electrical science was in its infancy, and that most of its useful applications were obtained empirically by a rule of the thumb. When we told them that the electrical science was one of the most exact of all physical sciences some shook their heads and exhibited considerable scepticism. One of them asked me: "Doctor, do you know what electricity is?" "No," said I, and he added another question: "Then how can you have an exact science of electricity when you do not even know what electricity is?" To this I retorted: "Do you know what matter is? Of course you do not, nor does anybody else know it, and yet who will deny that there are exact sciences relating to material things? Do you deny that astronomy is an exact science?" It is a difficult thing to make unscientific people understand that science studies first and foremost the activities of things and not their ultimate nature.

In that first course of public lectures I found it necessary to devote much of my exposition to the correction of erroneous notions which were lodged in the minds of my audience. When I told that audience that no electrical generator generates electricity, because electricity was made by God and, according to Faraday, its quantity in the universe is constant, and that for every positive charge there is an equal negative one, most members of my audience were inclined to think that I was talking metaphysics. "Then what does it generate?" asked one of my hearers. I answered: "It generates motion of electricity, and by that motion it furnishes us with means of doing useful work like telegraphy, telephony, and electrical lighting." Then I added: "The electrical science studies the forces which make electricity move against the reactions of the bodies through which it moves; in the overcoming of these reactions the moving electricity does useful work." Illustrations from dynamics of material bodies did not help very much, because my audience had hardly any knowledge of even the elements of Newton's great work, although Newton considered these elements as obvious truths. All they knew about Newton was that he had "discovered gravitation." When I told them that Newton had discovered the law of gravitational action and not gravitation itself, they thought that I was splitting hairs. I was never quite sure that those good people had carried away much knowledge from my lectures, but I was quite sure that they had left much knowledge with me. In trying to straighten out their notions I straightened out my own very considerably. Crocker was right when he said: "You have no idea how rapidly a young fellow grows when he tries to teach a new subject to poorly prepared beginners." That was the real profit from our first course of public lectures.

Every cultured person is expected to have an intelligent view of literature, of the fine arts, and of the social sciences, which is as it ought to be. But who has ever thought of suggesting that culture demands an intelligent view of the primary concepts in fundamental sciences? If cultured people had it, there would be no need to renew periodically the tiresome topic of the alleged clash between science and religion, and there would be much more straight thinking about things in general. Every child in the public schools should be made perfectly familiar with the simple experiments which illustrate the fundamental elements of Newton's divine philosophy, as Milton calls science. Barnard, Joseph Henry, Andrew White, and the other leaders of scientific thought in the United States, who started the great movement in favor of higher scientific research and of a better scientific education, had a difficult up-hill pull, because people in high places lacked an intelligent view of science. A famous lawyer, a trustee of a great educational institution, looked surprised when I told him, over thirty years ago, that one cannot teach science without laboratories both for the elementary and for the advanced instruction. He actually believed that graduate schools in science needed only a lot of blackboards, chalk, and sponges, and a lecturer who could prepare his lectures by reading books. He believed what he thought would suit him best, namely, that a university should be built on the top of a heap of chalk, sponges, and books. These instrumentalities are cheaper than laboratories, and that appeals to many university trustees. The teacher who can lecture from books and not from his experience in the laboratory is also much cheaper. But heaven help the country which trusts its destiny to cheap men operating with cheap instrumentalities. I gave that trustee a lecture by reciting the sermon which Tyndall preached in the summary and conclusions of his famous lecture of 1872–1873. I was bold enough to deliver several of these lectures to men in high places. Some liked them and some did not, but they all agreed that I had my own opinions upon the subject and was not afraid to express them.

The American Institute of Electrical Engineers had heard of my somewhat novel opinions regarding the teaching of the electrical science in its bearing upon electrical engineering, and it invited me to give an address upon the subject at its annual meeting in Boston, in the summer of 1890. The address was entitled "Practical Aspects of the Alternating Current Theory." It was a eulogy of the electrical science, and particularly of Faraday, Maxwell, and Joseph Henry on the purely scientific side, and of the technical men who were developing the system of electrical-power distribution by alternating-electrical forces. I noticed that my audience was divided into two distinct groups; one group was cordial and appreciative, but the other was as cold as ice. The famous electrical engineer and inventor, Elihu Thomson, was in the friendly group, and he looked me up after the address and congratulated me cordially. That was a great encouragement and I felt happy. Another man, a well-known physicist and engineer, also looked me up, and asked me whether I really expected that students of electrical engineering could ever be trusted to swallow and digest all the mathematical stuff which I had presented in my address. The "mathematical stuff" to which he referred was a very elementary theoretical illustration. I thought of my chums, the tripos youngsters at Cambridge, and of their wonderful capacity for swallowing and digesting "mathematical stuff," but said nothing; the man who was addressing me was one of those people who had a small opinion of the capacity and willingness of our American boys to "swallow and digest" just as much "mathematical stuff" as their English cousins do.

A short time prior to my return to Columbia College, in 1889, a bitter polemic was carried on in the New York newspapers concerning the two methods of electrical-power distribution, the *direct* and the *alternating* current method. The New York interests favored the first, and another group, including the Westinghouse Company, supported the alternating-current method. The opponents of the last method called it the "deadly alternating current," and did their best to discredit it. They actually succeeded, I was told, in persuading the State authorities to install an alternating-current machine at the Sing Sing prison, to be used in electrocution. When in my address at Boston I recited my eulogy of the alternating-current system I did not know of this bitter polemic, but when I heard of it I understood the chilliness among a part of my audience.

In the following autumn I was given to understand that my address in Boston had made a bad impression, and that it had offended the feelings of some *big* men who were interested in the electrical industries. I could not help seeing the glaring hint that the new "Department of Electrical Engineering at Columbia College" was expected to suffer from the fact that one of its two instructors was accused of an unpardonable "electrical heresy." The great and mighty person who broached this matter to me suggested that perhaps the easiest way out of this difficulty was my resignation. "Very well," said I, "I will certainly resign if the trustees of Columbia College, who appointed me, find me guilty of a scientific heresy." The trustees never heard of this incident, but my colleague Crocker did, and he said in his characteristic manner: "There are many people to-day who would not hesitate to burn the witch of Salem, but no people of that kind are on the board of trustees of Columbia College." Crocker was a Cape Cod man and he had a very soft spot for the witch of Salem.

The notion among many captains of industries that the electrical science was in its infancy, and that it worked by the rule of the thumb, made it possible to launch an opposition of that kind against the introduction of the alternating-current system of electrical distribution of power. Tesla's alternating-current motor and Bradley's rotary transformer for changing alternating currents into direct were available at that time. The electrical art was ready to do many things which it is doing to-day so well, if it had not been for the opposition of the people who were afraid that they would have to scrap some of their direct-current apparatus and of the plants for manufacturing it, if the alternating-current system were given any chance. A most un-American mental attitude! It was clear to every impartial and intelligent expert that the two systems supplemented each other in a most admirable manner, and that the advancement of one would also advance the other. Men like Elihu Thomson and my colleague Crocker knew that, but ignorance and false notions prevailed in the early nineties, because the captains of electrical industries paid small attention to highly trained electrical scientists. That explains why in those days the barbarous steel cables were still employed to drag cars along Third Avenue, New York, and in 1893 I saw the preparatory work on Columbus Avenue, New York, for installing additional barbarous steel ropes to drag street-cars. But fortunately these were never installed; electrical traction came to the rescue of Columbus Avenue.

During the summer of 1893 I had the good fortune to meet, quite often, William Barclay Parsons, the distinguished engineer, the future builder of the first New York subway. He passed the summer vacation at Atlantic Highlands, and I at Monmouth Beach, and we used the same steamboat in our occasional trips to New York. His head was full of schemes for the solution of the New York rapid-transit problem, but I observed that his ideas were not quite clear on the question of the electrical power transmission to be employed. A very few years later his ideas had cleared wonderfully. He had visited Buda Pest in 1894 and had seen a subway there operated electrically and most satisfactorily. It was a most instructive object-lesson, but how humiliating it was to the engineering pride of the great United States to ask little Hungary to instruct it in electrical engineering! The electrical power transmission system employed to-day in the New York subways is practically the same which had been proposed to and accepted by Parsons, the chief engineer, not so many years after our trips to New York, in 1893; it is the electrical power transmission consisting of a combination of the alternating and direct current systems. No fundamentally novel methods were employed which did not exist at the time when the alternating-current machine was installed at Sing Sing for the purpose of electrocuting people by the "deadly alternating current." In less than five years a radical change in people's notions had taken place about a matter which was well understood from the very first by men of higher scientific training. How was it brought about?

Four historical events, very important in the annals of the electrical science in the United States, had happened in rapid succession between 1890 and 1894. The first was the successful electrical transmission of power between Lauffen and Frankfurt, in Germany, in 1891; it employed the alternating-current system. The second was the decision of the Niagara Falls Power & Construction Company to employ the alternating-current system for the transmission of its electrical power. Professor Henry Augustus Rowland, of Johns Hopkins University, as consulting expert of the company, favored this system; another consulting scientific expert was the famous Lord Kelvin, and he favored the direct-current system. The third historical event was the consolidation of the Edison General Electric Company with the Thomson-Houston Company of Lynn, Massachusetts. This consolidation meant the end of the opposition to the alternating-current system on the part of people who were most influential in the electrical industries. No such opposition could exist in an electrical corporation where Elihu Thomson's expert opinion was the guiding star. The fourth historical event was the Electrical Congress at the World Exposition in Chicago, in 1893. Helmholtz came over as an official delegate of the German Empire, and was elected honorary president of the congress. The subjects discussed at that congress, and the men who discussed them showed that the electrical science was not in its infancy, and that electrical things were not done by the rule of the thumb.

Once I asked Professor Rowland whether anybody ever suggested to him resigning from Johns Hopkins University on the ground that in favoring the alternating-current system for the Niagara Falls Power Transmission Plant he had made himself liable to being charged with heresy. "Heresy?" said he; "I thought that my heresy was worth a big fee, and when the company attempted to cut it down the courts sustained my claim." An interesting bit of history is attached to this. When the Niagara Power & Construction Company objected to the size of the fee which Rowland charged for his services as scientific adviser, and asked for a reduction, the matter was referred to the court. During Rowland's cross-examination the defendant's lawyer, the late Joseph Choate, asked him the question: "Who, in your opinion, is the greatest physicist in the United States?" Rowland answered without a moment's hesitation: "I am." The judge smiled, but agreed with the witness, and his agreement was in harmony with the opinion of all scientific men. Rowland justified his apparently egotistical answer by the fact that as a witness on the stand he was under oath to speak the truth; he certainly spoke the truth when he testified that he was the first physicist in the United States.

Rowland's interest in the electrical science and its technical applications helped much to dissipate the notion, entertained by many, that it was empirical and still in its infancy. Bogus inventors always encouraged this superstition. The attention which Rowland and his former pupil, the late Doctor Louis Duncan, devoted to electrical engineering at Johns Hopkins University helped much to raise the status of electrical engineering. When the new General Electric Company was organized by the consolidation of the Edison General Electric Company and the Thomson-Houston Company, Elihu Thomson became the chief technical adviser of the new corporation, and its highest court of appeals in technical matters. I remember telling my colleague, Crocker, that, if the Thomson-Houston Company had contributed nothing else than Elihu Thomson to the new corporation, it would have contributed more than enough. Thomson was the American Siemens, and Rowland the American Helmholtz, of the new era in the history of American industries, the era of close co-operation between abstract science and engineering. With these two men at the head of the electrical science and industry in the United States, the senseless opposition to the alternating-current system of power distribution began to disappear. It vanished quickly after the Electrical Congress of 1893. The first visible result of the co-operation between abstract science and the technical arts was the splendid power plant at Niagara Falls, and later the electrical power distribution system in the New York subways, in which the alternating and the direct current systems supplemented each other most admirably. The late Professor Duncan of Johns Hopkins, and Doctor Cary T. Hutchinson, both pupils of Rowland, were the consulting electrical engineers of the Rapid Transit Commission of this city.

The scientific spirit of Rowland's laboratory and lecture-room was felt everywhere in the electrical industries; it was also felt in our educational institutions. His and his students' researches in solar spectra and in other problems of higher physics made that spirit the dominating influence among the rising generation of physical sciences in America. It was universally acknowledged that Johns Hopkins was a real university. The intellectual movement in favor of higher scientific research, first inaugurated by Joseph Henry, President Barnard of Columbia College, and Doctor John Draper, in the early seventies, was marching on steadily under the leadership of Rowland when I started my academic career at Columbia, thirty-four years ago, and he led on like a "doughty knight of Troy," as Maxwell used to call him. It was the spirit of Johns Hopkins which inspired the generation of the early nineties in its encouragement of the movement for the development of the American university. Some enthusiasts at Columbia College went even so far as to advocate the abolition of the college curriculum and the substitution of a Columbia University for Columbia College; I was not among these enthusiasts, because I knew only too well the historical value of Columbia College and of other American colleges. What would the University of Cambridge be without its ancient colleges? College lays the foundation for higher citizenship; the university lays the foundation for higher learning.

Speaking for physical sciences I can say that in those days there was no lack of trained scientists who could have easily extended the work of the American college and added to it a field of advanced work resembling closely the activity of the European universities. Most of these men had received their higher academic training in European universities, and quite a number of them came from Johns Hopkins. But there were two obstacles: first, lack of experimental-research facilities; second, lack of leisure for scientific research. Rowland and his followers recognized the existence of these obstacles and demanded reform. Most of the energy of the teachers of physical sciences was consumed in the lecture-room; they were pedagogues, "pouring information into passive recipients," as Barnard described it. My own case was a typical one. How could I do any research as long as I had at my disposal a dynamo, a motor, an alternator, and a few crude measuring instruments only, all intended to be used every day for the instruction of electrical-engineering students? When the professor of engineering died, in the summer of 1891, a part of his work, theory of heat and hydraulics, was assigned to me. The professor of dynamics died a little later, and his work was also transferred to me. I was to carry the additional load of lecture-room work temporarily, but was relieved from it, in part only, after several years. As a reward my title was advanced to adjunct professor, with an advance of salary to two thousand five hundred dollars per annum. But in return for this *royal* salary I had to lecture three to four hours each forenoon, and besides help in the electrical laboratory instruction in the afternoons. While this pedagogic load was on my back scientific research could not be seriously thought of. My young colleagues in other colleges were similarly situated. This overloading of young scientists with pedagogic work threatened to stunt, and often did stunt, their growth and also the growth of the rising American university. "Let chairs be founded, sufficiently but not luxuriously endowed, which shall have original research for their main object and ambition," was the historical warning which Tyndall addressed to the American people in 1873, but in 1893 there was little evidence that it was heeded anywhere outside of Johns Hopkins University. But there they had Rowland and a number of other stars of the first magnitude who succeeded Joseph Henry, Barnard, and Draper as leaders of the great movement in favor of higher scientific research. In 1883 Rowland delivered a memorable address as vice-president of one of the sections of the American Association for the Advancement of Science. It was entitled, "A Plea for Pure Science," and described the spirit not only of Johns Hopkins of those days but also of all friends of higher learning in science. That spirit was advocated by Tyndall in 1872–1873, and under Rowland's leadership it was bound to win our battle for higher ideals in science. The people of the United States owe a great debt of gratitude to Johns Hopkins for the leadership in that great movement which, as we see to-day, has produced a most remarkable intellectual advancement in this country. Nearly thirty years ago I heard Rowland say in a public address: "They always say in Baltimore that no man in that city should die without leaving something to Johns Hopkins." When he said it he knew that Johns Hopkins was very poor. It is poorer to-day than ever, and no rich man in the United States should die without leaving something to Johns Hopkins, the pioneer university of the United States.

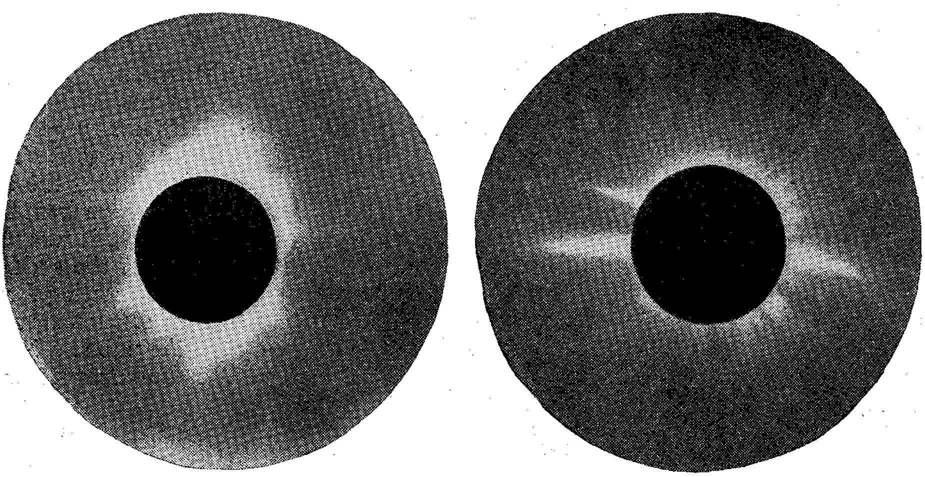
Rowland said once that lack of experimental facilities and of time are not a valid excuse for neglecting entirely scientific research. I agreed with that opinion; neglect breeds indifference, and indifference degenerates into atrophy of the spirit of inquiry. The alternating-current machine of the electrical engineering laboratory at Columbia was free in the evenings, and so was my time; that is, if my wife should not object, and, being a noble and unselfish woman, she did not object. With the assistance of several enthusiastic students, among them Gano Dunn, to-day one of the most distinguished engineers in the United States, I started investigating the passage of electricity through various gases at low pressures, and published two papers in the *American Journal of Science.* I soon discovered that most of my results were anticipated by Professor J. J. Thomson, of Cambridge, who, in all probability, had received his inspiration from the same source from which I had received it. He not only had anticipated me but, moreover, he showed a much better grasp of the subject than I had, and had much better experimental facilities. I decided to leave the field to him, and to watch his beautiful work from the outside. It was a wise decision, because it prepared me to understand the epoch-making discoveries in this field which were soon to be announced, one in Germany and one in France. I turned my attention to another field.

I must mention, however, one of the results which Thomson had not anticipated and which created quite an impression among astronomers. I noticed a peculiar appearance in the electrical discharge proceeding from a small metal sphere which was located in the centre of a large glass sphere containing air at low pressure. The discharges looked very much like the luminous corona of the sun which astronomers observe during eclipses. It was always a mysterious puzzle in solar physics. Pasting a tin-foil disk on the glass sphere, so as to hide the metal sphere and see only the discharge proceeding from it, I photographed the appearance of the discharge and obtained the pictures given opposite. The resemblance of these photographs to those of the two types of the solar corona is most striking. This is what I said about it at that time:

"The bearing which these experimental results may have upon the theory of the solar corona I prefer to leave to others to decide. That they may prove a suggestive guide in the study of solar phenomena seems not unreasonable to expect."

In a communication read later before the New York Academy of Sciences I was much bolder, having previously discussed the subject with my friends at Johns Hopkins and with the late Professor Young, the famous astronomer at Princeton. I soon found myself advocating strongly the electro-magnetic theory of solar phenomena. A German professor, Ebert by name, a well-known authority on electrical discharges in gases, took me very seriously indeed, which was very flattering, but he claimed priority. I had no difficulty in establishing my priority through the columns of the periodical *Astronomy and Astro-Physics*, one of whose editors was George Ellery Hale, to-day the distinguished director of Mount Wilson Observatory. I was very fortunate to make his acquaintance during that period when both he and I were very young men. His influence prevented me from running wild with my electromagnetic theory of solar phenomena. Thanks to the splendid astrophysical researches at the Mount Wilson Observatory in California under Doctor Hale's direction, we know to-day that enormous electrical currents circulate on the surface of the sun, and we also know from other researches that negative electricity is shot out from all hot bodies, even from those not nearly as hot as the sun, and that the solar corona is, in all probability, closely related to this electrical activity on the sun.

After giving up the subject of electrical discharges in gases I looked around for another problem of research which I could manage with my meagre laboratory facilities. Rowland had found distortions in an alternating current when that current was magnetizing iron in electrical power apparatus. This distortion consisted of the addition of higher harmonics to the normal harmonic changes in the current. This reminded me of harmonics in musical instruments and in the human voice. Helmholtz was the first to analyze the vowels in human speech by studying the harmonics which they contained. The vowel *u*, for instance, sung at a given pitch, contains in addition to its fundamental pitch—say one hundred vibrations per second—other vibrations the frequencies of which are integral multiples of one hundred, that is two, three, four, . . . hundred vibrations per second. These higher vibrations are called harmonics of the fundamental. Helmholtz detected these harmonics by the employment of acoustical resonators; it was an epoch-making research. I proceeded to search for a similar procedure for the analysis of Rowland's distorted alternating currents, and I found it. I constructed electrical resonators based upon dynamical principles similar to those in the acoustical resonators employed by Helmholtz. These electrical resonators play a most important part in the radio art of to-day, and a few words regarding their operation seem desirable. In fact, there is to-day a cry from the Atlantic to the Pacific on the part of millions of people who wish to know what they are really doing when they are turning a knob on their radio-receiving sets, in order to find the correct wave length for a certain broadcasting station. I am responsible for the operation, and I owe them an explanation of it.



Electrical discharges representing two types of solar coronæ.

The mass and form of an elastic body, say a tuning-fork, and its stiffness determine the pitch, the so-called *frequency* of vibration. When a periodically varying force, say a wave of sound, acts upon the tuning-fork the maximum motion of the prongs will be produced when the pitch or frequency of the moving force is equal to the frequency of the tuning-fork. The two are said then to be in resonance, that is, the motion of the fork resonates to or synchronizes with the action of the force. Every elastic structure has a frequency of its own. The column of air in an organ-pipe has a frequency of its own, so has the string of a piano. One can excite the motion of each by singing a note of the same frequency; a note of a considerably different frequency excites practically no motion at all. Acoustical resonance phenomena are too well known to need here any further comment. There are also electrical resonance phenomena very similar to those of acoustical resonance. If you understand one of them there is no difficulty in understanding the other.

If an electrical conductor, say a copper wire, is coiled up so as to form a coil of many turns, and its terminals are connected to a condenser, that is to conducting plates which are separated from each other by insulating material, then the motion of electricity in that conducting circuit is subject to the same laws as the motion of the prongs of a tuning-fork. Every motion, whether of electricity or of matter, is determined completely by the force which produces the motion, and by the forces with which the moving object reacts against the motion. If the law of action of these several forces is the same in the case of moving matter as in the case of moving electricity, then their motions will also be the same. The moving forces are called the *action* and the opposing forces are called the *reaction*, and Newton's third law of motion says: *Action is equal to the opposing reaction.* I always considered this the most fundamental law in all physical sciences. It is applicable to all motions no matter what the thing is which moves, whether ponderable matter or imponderable electricity. Twenty-six years ago a student of mine, Albert R. Gallatin, brother of the present park commissioner of New York, presented a large induction coil to the electrical laboratory at Columbia College, in recognition of my services to him, because, he said, this formulation of the fundamental law in the electrical science, which I have just given, made everything very clear to him. This was most encouraging to a young professor, and it goes without saying that ever since that time he and I have been warm friends. He is a banker and I am still a professor, but the interest in the fundamental principles in physical sciences are a strong bond of union between us.

The electrical force which moves the electricity in the circuit, just described, experiences two principal reactions. One reaction is due to the lines of electrical force which, attached to the electrical charge on the condenser plates, are crowded into the insulating space between these plates. This reaction corresponds to the elastic reaction of the prongs of the tuning-fork, and follows the same law. In the case of the tuning-fork the *elastic reaction is proportional to the displacement* of the prongs from their normal position; in the electrical case the reacting force is proportional to the electrical charges which have been pulled apart, the negative from the positive, and driven to the plates of the condenser. Call this separation *electrical displacement*, and the law can be given the same form as above, namely: *The reacting force is proportional to the electrical displacement.* The greater the distance between the plates, and the smaller their surface, the greater is the reaction for a given electrical displacement. By varying these two quantities we can vary the electrical yielding, the so-called capacity, of the electrical condenser. This is what you do when you turn the knob and vary the capacity of the condenser in your receiving set.

The moving prongs have a momentum, and a change in the momentum opposes a reacting force, the so-called inertia reaction, which is equal to the rate of this change. This was discovered by Galileo over three hundred years ago. We experience the operation of this law every time we bump against a moving object. The Irish sailor who, after describing the accident which made him fall down from the mast, assured his friends that it was not the fall which hurt him but the sudden stop, appreciated fully the reacting force due to a rapid change of momentum. Every boy and girl in the public schools should know Galileo's fundamental law, and they would know it if by a few simple experiments it were taught to them. But how many teachers really teach it? How many of my readers really know that law? Just think of it, what an impeachment it is of our modern system of education to have so many intelligent men and women, boys and girls, ignorant of so fundamental a law as that which Galileo discovered so long ago!

The moving electricity has a momentum. The magnetic force produced by this motion is a measure of this momentum. Its change is opposed by a reacting force equal to the rate of this change. This was discovered by Faraday nearly a hundred years ago. The larger the number of turns in the coil of wire the larger will be the momentum for a given electrical motion, that is, for a given electrical current. But how can anybody understand very clearly this beautiful law, discovered by Faraday, who does not understand Galileo's simpler discovery? The fact that electricity just like matter has inertia and that both obey the same law of inertia is one of the most beautiful discoveries in science. Whenever I thought that so many intelligent and cultured people knew nothing about it I rebelled against the educational system of modern civilization.

The motion of electricity in the conductor described above overcomes reacting forces which follow the same laws as the motion of the elastic prongs of the tuning-fork. The motion of one has, therefore, an analogy in the motion of the other. In an electrical circuit having a coil and a condenser the moving electricity has a definite inertia and a definite electrical stiffness, hence it will have a definite pitch or frequency for its vibratory motion, just like a tuning-fork; it will act as a resonator. It is obvious, therefore, that an electrical resonator, the pitch of which can be adjusted by adjusting its coil or its condenser or both, is a perfect analogy to the acoustical resonator. By means of an electrical resonator of this kind, having an adjustable coil and an adjustable condenser, I succeeded in detecting every one of the harmonics in Rowland's distorted alternating currents, in the same manner in which Helmholtz detected the harmonics in the vowel sounds, but with much greater ease, because the pitch of an electrical resonator can be very easily and accurately changed by adjusting its coil and condenser. There are millions of people to-day who are doing that very thing when they are turning the knobs on their radio receiving sets, adjusting it to the wave-length of the transmitting station. The expression, "adjusting it to the pitch or frequency of the transmitting station," is much better, because it reminds the operator of the analogy existing between acoustical and electrical resonance. The procedure was inaugurated thirty years ago in the "cowshed" of old Columbia College. I called it "electrical tuning" and the name has stuck to it down to the present time. The word "tuning" was suggested by the operation which the Serbian bagpiper performed when he tuned up his bagpipes, and which I watched with a lively interest in my boyhood days. Those early impressions had made acoustical and electrical resonance appear to me later as obvious things.

The results of this research were published in the *American Journal of Science* and also in the *Transactions of the American Institute of Electrical Engineers for 1894*. They, I was told, had never been anticipated, and they confirmed fully Rowland's views concerning the magnetic reaction of iron when subjected to the magnetic action of an alternating current. When Helmholtz visited this country in 1893, I showed him my electrical resonators and the research which I was conducting with their assistance. He was quite impressed by the striking similarity between his acoustical resonance analysis and my electrical resonance analysis and urged me to push on the work and repeat his early experiments in acoustical resonance, because my electrical method was much more convenient than his acoustical method.

Helmholtz was always interested in the analysis as well as in the synthesis of vibrations corresponding to articulate speech. The telephone and the phonograph were two inventions which always enjoyed his admiring attention. During his visit in America he looked forward with much pleasure to meeting Graham Bell and Edison. The simplicity of their inventions astonished him, because one would have hardly expected that a simple disk could vibrate so as to reproduce faithfully all the complex variations which are necessary for articulation. He spent a Sunday afternoon as my guest at Monmouth Beach and in the course of conversation I told him what impression the telephone had made upon me when I first listened through it. It happened during the period when I was serving my apprenticeship as greenhorn, and when I was trying hard to master the articulation of the English language. The telephone plate repeated perfectly everything spoken at the other end, and I said to myself: "These Americans are too clever for me; they can make a plain steel plate articulate much better than I can ever expect to do it with all my speaking organs. I had better return to Idvor and become a herdsman again." Helmholtz laughed heartily and assured me that the articulating telephone plate made a similar impression upon him, although he had spent several years of his life studying the theory of articulation. "The phonograph disk is just as clever," said Helmholtz, "as the telephone disk, perhaps even more so, because it has to dig hard while it is busily talking."

My scientific friends in New York saw in the construction of my electrical resonator and in its employment for selective detection of alternating currents of definite frequency a very suitable means for practising harmonic telegraphy, first suggested by Graham Bell, the inventor of the telephone. They finally persuaded me to apply for a patent and I did it. I often regretted it, because it involved me in a most expensive and otherwise annoying legal contest. Two other inventors had applied for a patent on the same invention. One of them was an American, and the other a French inventor, and each of them was backed by a powerful industrial corporation. A college professor with a salary of two thousand and five hundred dollars per annum cannot stand a long legal contest when opposed by two powerful corporations, but it is a curious psychological fact that when one's claim to an invention is disputed he will fight for it just as a tigress would fight for her cub. The fight lasted nearly eight years and I won it. I was declared to be the inventor, and the patent for it was granted to me. But a patent is a piece of paper worth nothing until somebody needs the invention. I waited a long time before that somebody came, and when he finally showed up I had almost forgotten that I had made the invention. In the meantime I had nothing but a piece of paper for all my pains, which nearly wrecked me financially.

Just about that time the newspapers reported that a young Italian student by the name of Marconi, while experimenting with Hertzian waves, had demonstrated that a Hertzian oscillator will send out electrical waves which will penetrate much longer distances when one of its sides is connected to earth. "Of course it will," said I, "the grounded oscillator takes the earth into closer partnership." When as a herdsman's assistant on the pasturelands of my native Idvor I stuck my knife into the ground and struck its wooden handle I knew perfectly well that the ground was a part of the vibrating system and that the sound-producing stroke was taken up by the ground much better than when I struck the knife-handle without sticking the knife into the ground. But I also knew that unless the boy who was listening pressed his ear against the ground he would not hear very much. It was, therefore, quite obvious to me that the best detector for a Hertzian oscillator which is grounded must be another Hertzian oscillator which is also connected to the ground. Grounding of the sending and of the receiving Hertzian oscillators was in fact the fundamental claim of the Marconi invention. Marconi, in my opinion, was unwittingly imitating the young herdsmen of Idvor when, figuratively speaking, he stuck his electrical knives into the ground for the purpose of transmitting and receiving electrical vibrations, but the imitation was a very clever one; very obvious indeed as soon as it was pointed out, like all clever things.

Every now and then we are told that wireless signals might be sent some day to the planet Mars. The judgment of a former herdsman of Idvor considers these suggestions as unscientific for the simple reason that we cannot get a ground on the planet Mars and, therefore, cannot take it into close partnership with our Hertzian oscillators. Without that partnership there is no prospect of covering great distances. A very simple experiment will illustrate this. Scratch the wood of a pencil and ask your friends who are sitting around a table whether they hear the scratching. They will say "No." Put the pencil on the table and scratch it again; your friends will tell you that they can hear it faintly. Ask them to press their ears against the table and they will tell you that the scratching sound is very loud. In the third case the pencil, the table, and the ears of your friends are all one closely interconnected vibratory system. Every herdsman of Idvor would interpret correctly the physical meaning of this experiment. "If Marconi had waited just a little longer I would have done his trick myself," I said jokingly to Crocker, and then I temporarily dismissed the matter from my mind as if nothing had happened. But I was fairly confident that my electrical resonators would some day find a useful application in this new method of signalling, and Crocker was even more hopeful than I was. I turned my attention to another problem and would have completed its solution, if my work had not been interrupted by the announcement of a most remarkable discovery made in Germany, I mean the discovery of the Roentgen rays.

I cannot describe the effects of this epoch-making discovery without referring again to great Helmholtz. It was due to his initiative that Hertz took up the research of electrical oscillations, which suggested to Marconi their technical application. This started a new technical art, wireless telegraphy, which developed into the radio art. Without Helmholtz, not only the experimental verification of the Faraday-Maxwell electro-magnetic theory but also the radio art might have been delayed quite a long time. I shall point out now that the great discovery of the Roentgen rays was also due in a great measure to the initiative of Helmholtz.

While in Berlin I was conducting a research upon vapor pressures of salt solutions. For this purpose I needed the assistance of a clever glass-blower. A Herr Mueller was recommended to me by the people of the Physical Institute. I paid frequent visits to him, not only because I liked to watch his wonderful skill in glass-blowing, but also because he knew and entertained me often with the history of a remarkable physical research which had been carried out by Doctor Goldstein, a Berlin physicist, under the auspices of the German Academy of Sciences, Herr Mueller, the glass-blowing artist, assisting.

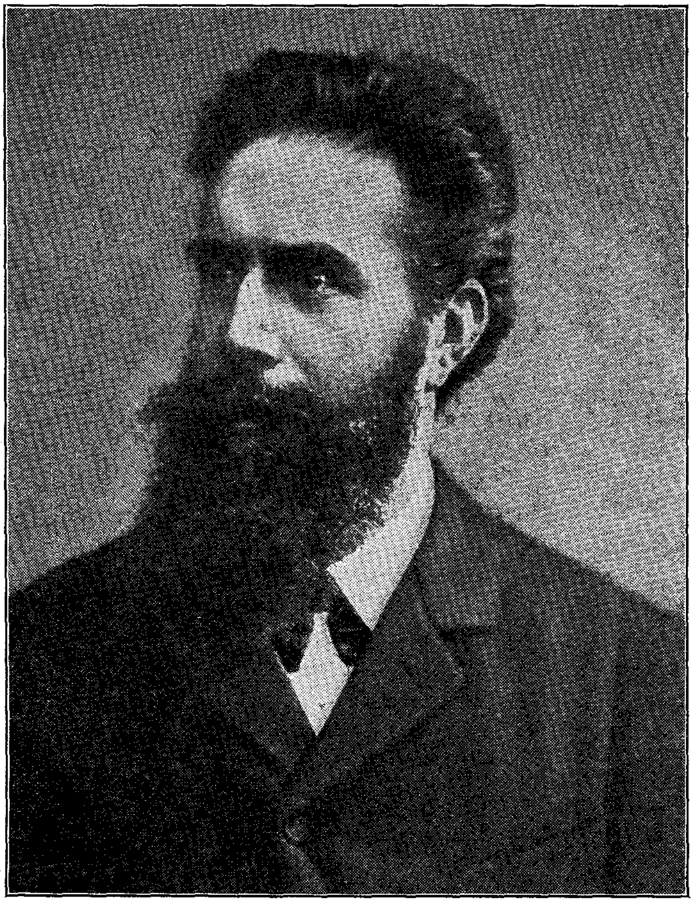
The motion of electricity through rarefied gases was first extensively studied in Germany in the fifties and sixties by several investigators. Hittorf was one of them, and I mention him here for reasons given later. The English physicists took up the subject a little later, and among them Crookes did the most distinguished work. His tubes with a very high vacuum gave brilliant cathode rays, first discovered by Hittorf, which produced among other things the well-known phosphorescence in vacuum tubes made of uranium glass. In spite of the surpassing beauty of the electrical phenomena in vacuum tubes revealed by Crookes's experiments, no final and definite conclusions could be drawn from them toward the end of the seventies. But he was undoubtedly the first who correctly inferred that the cathode rays were small electrified particles moving with high velocity. This inference proved to be of very great importance. In 1893 Lord Kelvin said: "If the first step toward understanding the relations between ether and ponderable matter is to be made, it seems to me that the most hopeful foundation for it is knowledge derived from experiment on electricity in high vacuum." This was the very opinion which Helmholtz had formulated fifteen years earlier, and he persuaded the German Academy of Sciences to make a special grant for a thorough experimental review of the whole field of research relating to electrical motions in high vacua. Doctor Goldstein was selected to carry out this work. Mueller was his glass-blower. The most important result of this work was the discovery of the so-called *Canal Rays*, that is, motion of positive electricity in the direction opposite to the motion of negative electricity, the latter being the cause of the *cathode rays*. To get that result Mueller had to make innumerable vacuum tubes of all sorts of shapes. He told me that if all these tubes could be resurrected they would fill the house in which his shop was located. "But the grand result was worth all the trouble, and I am proud that I did all the glass-blowing," said Mueller, with a triumphant light in his eyes, and his beaming countenance testified that he felt what he said. He was an artisan who loved his craft, and, judging from his remarkable knowledge of all the vacuum-tube researches which had been conducted up to the time of his co-operation with Doctor Goldstein, I inferred that he was a unique combination of the science and the art involved in the job which he was doing for Doctor Goldstein. Mueller was the first to arouse my interest in the results of vacuum-tube researches, and I always considered him as one of my distinguished teachers in Berlin. New knowledge is not confined to the lecture-rooms of a great university; it can be often found in most humble shops, treasured by humble people who are quite unconscious that they are the guardians of a precious treasure. Mueller was one of these humble guardians.

The importance of Goldstein's work was due principally to the fact that it brought into the field three other German physicists of great acumen. The first one was Hertz. Several years after he had completed his splendid experimental verification of the Faraday-Maxwell electromagnetic theory, he showed that the cathode rays penetrated easily through thin films of metal, like gold and aluminum foil, although these films were perfectly opaque to ordinary light. It was a novel and most important contribution to our knowledge of cathode rays, and would have been followed up by more additional knowledge if Hertz had not died on January 1, 1894, at the age of thirty-six. Helmholtz died several months later. Science never suffered a greater loss in so short an interval of time. Helmholtz met with an accident on the ship on his return trip from the United States in 1893. He never completely recovered, although he lectured at the University of Berlin until a few days before his sudden death in the midsummer of 1894. Autopsy revealed that one side of his brain was and had been in a pathological state for a long time, but nobody had ever observed that his intellectual power had shown any signs of decay. It is a pity that he did not live another two years; he would have seen what he told me during his visit here he longed to see, and that is an electrified body moving at a very high velocity suddenly reversing its motion. That, he thought, might furnish a direct experimental test of the mobility of ether. The discovery described below furnished such a body.

Hertz's work was continued and greatly extended by Professor Lenard of the University of Kiel. He would have undoubtedly reached the final goal if Roentgen had not announced, in December, 1895, that he, experimenting with Lenard vacuum tubes, had discovered the X-rays. This discovery marked the last step in the survey which Goldstein, under the initiative of Helmholtz, had undertaken some fifteen years before Roentgen had entered the field of electrical discharges in high vacua. It was a great truimph for German science. The science of electrical discharges in rarefied gases was started in Germany and in less than forty years it had reached there its highest point. It is a science which may justly be said to have been "made in Germany," just as the science of radiation. It started a new and most remarkable era in physical sciences by extending the meaning of the Faraday-Maxwell electromagnetic theory.

No other discovery within my lifetime had ever aroused the interest of the world as did the discovery of the X-rays. Every physicist dropped his own research problems and rushed headlong into the research of the X-rays. The physicists of the United States had paid only small attention to vacuum-tube discharges. To the best of my knowledge and belief I was at that time the only physicist here who had any laboratory experience with vacuum-tube research, and I got it by overtime work in the electrical-engineering laboratory of Columbia College. I undertook it because my intercourse with Mueller, the glass-blower of Berlin, directed my attention to this field of research, and particularly because I did not see that with the equipment of that laboratory I could do anything else. I decided, as mentioned above, to leave the field to Professor J. J. Thomson, of Cambridge, and to watch his work. When, therefore, Roentgen's discovery was first announced I was, it seems, better prepared than anybody else in this country to repeat his experiments and succeeded, therefore, sooner than anybody else on this side of the Atlantic. I obtained the first X-ray photograph in America on January 2, 1896, two weeks after the discovery was announced in Germany.

Many interesting stories have been told about the rush to the West during the gold-fever period, caused by the discovery of gold in the far West. The rush into X-ray experimentation was very similar, and I also caught the fever badly. Newspaper reporters and physicians heard of it, and I had to lock myself up in my laboratory, in the cellar of President Low's official residence at Columbia College, in order to protect myself from continuous interruptions. The physicians brought all kinds of cripples for the purpose of having their bones photographed or examined by means of the fluorescent screen. The famous surgeon, the late Doctor Bull of New York, sent me a patient with nearly a hundred small shot in his left hand. His name was Prescott Hall Butler, a well-known lawyer of New York, who, while shooting grouse in Scotland, met with an accident and received in his left hand the full charge of his shotgun. He was in agony; he and I had mutual friends who begged me to make an X-ray photograph of his hand and thus enable Doctor Bull to locate the numerous shot and extract them. The first attempts were unsuccessful, because the patient was too weak and too nervous to stand a photographic exposure of nearly an hour. My good friend, Thomas Edison, had sent me several most excellent fluorescent screens, and by their fluorescence I could see the numerous little shot and so could my patient. The combination of the screen and the eyes was evidently much more sensitive than the photographic plate. I decided to try a combination of Edison's fluorescent screen and the photographic plate. The fluorescent screen was placed on the photographic plate and the patient's hand was placed upon the screen. The X-rays acted upon the screen first and the screen by its fluorescent light acted upon the plate. The combination succeeded, even better than I had expected. A beautiful photograph was obtained with an exposure of a few seconds. The photographic plate showed the numerous shot as if they had been drawn with pen and ink. Doctor Bull operated and extracted every one



Wilhelm Konrad Roentgen.
1845-1923.

of them in the course of a short and easy surgical operation. Prescott Hall Butler was well again. That was the first X-ray picture obtained by that process during the first part of February, 1896, and it was also the first surgical operation performed in America under the guidance of an X-ray picture. This process of shortening the time of exposure is now universally used, but nobody gives me any credit for the discovery, although I described it in the journal *Electricity*, of February 12, 1896, before anybody else had even thought of it. Prescottt Hall Butler was much more appreciative and he actually proposed, when other offers to reward me for my efforts were refused, to establish a fellowship for me at the Century Club, the fellowship to entitle me to two toddies daily for the rest of my life. This offer was also refused. On March 2, 1896, Professor Arthur Gordon Webster, of Clark University, Worcester, Massachusetts, addressed a letter to *Worcester Gazette*, from which I quote:

> Sunday morning I went with Professor Pupin to his laboratory to try the effect of a fluorescent screen in front of the plate. I placed my hand under the bulb and in five minutes the current was stopped. . . . The result was the best plate that I had yet seen. . . . One who has tried the experiments and seen how long it takes to obtain a good result can judge of an improvement. I think that Doctor Pupin should enjoy the credit of having actually . . . shortened the time of exposure ten and twenty times.

A description of the improvement, which I published in final form in *Electricity*, of April 15, 1896, ends with the following sentence:

> My only object in working on the improvement of the Roentgen ray photography was for the purpose of widening its scope of application to surgical diagnosis. I think that I have succeeded completely and I wish full credit for the work done.

My friends suggested that I apply for a patent on the procedure and enforce recognition that way, but I was having one expensive experience in the patent office with my electrical resonators and did not care to add another.

The question of reflection and refraction of the X-rays had to be answered and several strange claims were brought forward by investigators. My investigations of this matter, aided by Thomas Edison's most efficient fluorescent screen, resulted in a discovery, which, in a communication to the New York Academy of Sciences, on April 6, 1896, I summed up as follows: "*Every substance when subjected to the action of X-rays becomes a radiator of these rays.*" The communication was published in several scientific journals, like *Science* and *Electricity*, and no statement can claim the discovery of the now well-known secondary X-ray radiation more clearly than the one given above. But of this matter I shall speak a little later.

Looking up some data lately I found that I had finished writing out these communications relating to my X-ray research on April 14, 1896. I also found a reprint of an address delivered before the New York Academy of Sciences in April, 1895, and published in *Science* of December 28, 1895, at the very time when the X-ray fever broke out. It was entitled: "Tendencies of Modern Electrical Research." But the X-ray fever prevented me from reading it when it was published. I saw it three months later, but never again since that time, and I had forgotten that I had ever composed it. I find now that the picture which I drew then of the growth of the electromagnetic theory is in every detail the same as that which I have given in this narrative. Both of them are due to the lasting impressions received in Cambridge and in Berlin. Evidently these impressions are just as strong to-day as they were twenty-eight years ago, proving that the tablets of memory have a mysterious process of preserving their records. I remember that on April 14, 1896, I did not go to the laboratory, but stayed at home and reflected, and read my address mentioned above. I took an inventory of what I had done during my six years' activity at Columbia and I closed the books satisfied with the results. My wife, who had helped me, writing out my reports, lectures, and scientific communications, and who knew and watched every bit of the work which I was doing, was also satisfied and congratulated me. My colleague Crocker, I knew, was satisfied, and so were all my scientific friends, and that was a source of much satisfaction. But nothing makes one as happy as his own honest belief that he has done his best.

(To be continued.)

His Creed

BY WOLCOTT LeCLEAR BEARD

ILLUSTRATIONS (FRONTISPIECE) BY HARVEY DUNN

THE "wop! wop! wop! woppety-wop-wop!" of distant rifle fire died away somewhere in old Mexico, over the edge of a yellow prairie that met the southern horizon. Thomas Harvey Crofton grinned with sincere appreciation of his own indifference to those vanishing sounds.

Tommy Crofton sat on the veranda of the big adobe house in which he was born and which, in the course of years, had taken on something the appearance of a mediæval fortress stung with ambitions to become a modern villa. He winked at his small son Peter, aged four, who passed with Anita, the fat Mexican nurse, and grinned again when Peter winked back at him. Tommy was supremely contented; contented and happy. He thought he had every reason to be.

He owned the low, spring-fed hill upon which he lived and which made an island of vivid green in a motionless sea of parched grass; upon which grass, nevertheless, his cattle of the "Pitchfork" brand lived and throve amazingly. He owned the little village which nestled under the hill. He had caused his heritage to grow, and to such a degree that he felt himself justified in having run away with Edith and with having married her. Moreover, Edith's father, Philadelphian and—which means the same thing—conservative though he was, of late had shown a tendency to overlook the fact that Tommy had been born elsewhere.

Finally, Tommy was happy because he lived in the United States. Formerly he had accepted this fact as a matter of course; now it was an additional cause for rejoicing. This was owing to one Heradura, who was a Mexican—*and* a patriot—of sorts.

Originally a bandit in a much smaller way of business, Heradura had taken the adjacent portion of northern Mexico and established a precarious "government" there. Among other officials he had appointed an *intendente*. Tommy had disappointed him.

With many flowers of speech, but also with a heat that fairly wilted them, the official in question had contended that the Pitchfork headquarters lay south of the Mexican line, and so were subject to Heradura's exuberant taxes. Tommy's contention had been that he didn't care a profanely small amount what the *intendente* or any one else said; he, Tommy, lived in the United States and nowhere else. Then, with the aid of an ordnance map and an engineer's transit, Tommy proceeded to prove his point, and the concrete evidence of that proof, just completed, was before him. It consisted of a low railing of peeled cottonwood poles, colored red, white, and blue with paint which still glistened stickily, and further adorned with small calico editions of the Stars and Stripes, which Tommy had been at much pains to procure.

This fence was the line. Well within it, a circular trench enclosed house and outbuildings. Not for a moment did Tommy believe it ever would be necessary to man this trench; Heradura, whose ambition was to be recognized by Uncle Sam, would hardly care to invade Uncle Sam's territory. Still, if Heradura *should* come, the trench was there, and Tommy thought he could depend upon his Mexicans, who liked him and hated Heradura. They hated Heradura not so much because he was a bandit as because he came from another part of the country.

This was a characteristically Mexican reason for hating a man. Tommy chuckled aloud as it recurred to his mind, so that Edith, who just then emerged from the house, smiled in sympathy.