

ELECTRICAL PRECIPITATION

Historical Sketch

BY F. G. COTTRELL

MY FUNCTION this morning, I feel, is more that of an enunciator than anything else. What you really want to see, I know, is the experimental side of this matter and the pictures, both in motion and in still life, that the other speakers have to show you. All I shall try to do is to outline briefly a few of the steps that have led up to this work and give it a little better setting in time and space.

If we go back to the earliest reference we can find on the subject of the application of electrical forces to precipitation, we find, as far as I know, that Hohlfeld, in 1824, was the first to point definitely to the phenomena as a practical matter. Hohlfeld was a teacher of mathematics in the Thomas School of Leipzig, and published a brief article on this subject, which is filed in Kastner's archives in Nuremberg, Vol. 2, page 205-6, 1824. I had rather hoped perhaps we could show this morning an exact duplicate of this described experiment, but it was not convenient or possible to get a static machine here in time for the work. What he did was simply to take a piece of paper, form a funnel out of it, settling the lower tip into a bottle, and lighting the edge of the paper until the smoke filled the bottle, and then placing a wire through the stopper of the bottle, within about three inches of the bottom, and connecting to it an old fashioned friction machine, which separated the smoke in the bottom of the flask or bottle. One of the experiments which Mr. Bradley is to show you later is substantially a duplicate of that experiment of Hohlfeld's. It is perhaps worth while to note the words in which Hohlfeld expressed his ideas on this subject: "It is, of course, well known that during a thunder storm, after almost every bolt of lightning, there comes a heavier fall of rain, and this occurs, indeed, just such a period after the bolt as would be required for the water to reach the earth from the clouds. It has also been observed that follow-

ing a thunderclap, and especially the heavier ones, sleet and hail often fall and in increasing amounts with repetition of the discharges. Finally, it has been noted that so-called cloud-bursts are usually preceded by strong and rapidly repeated discharges. The first of these phenomena may easily be illustrated with the use of artificial electricity." Then he describes the experiment of which I have spoken. That constitutes our first historical reference to this subject, so far as known.

This fell, apparently, on deaf ears; no one appreciated its importance. The machinery available was, of course, inadequate for commercial work, and nothing again appears until 1850, when C. F. Guitard mentioned a similar phenomenon which he independently observed on tobacco smoke in a bottle in much the same way. His article appears in the *Mechanics Magazine* of London for 1850, a quarter of a century after Hohlfeld published his article. Then, the matter seems to have rested without further notice, with an occasional incidental reference. For example, there was a reference in 1878 in *Vuo Am.* by Robert Norwald, on the dispensing of dust inside an electrometer he was working on, showing that the conductors of the air disappeared when the charge was left on, and he ascribed it to this cleaning effect of the air. That is merely incidental, and not anything that was put into practical effect.

The first attempt actually to apply this principle in practise, in technical methods, seems to have come almost simultaneously in Germany and in England. We find a record of a patent issued in England to A. O. Walker, the superintendent or manager of one of the large lead works in England, who obtained a patent dated August 9, 1884. We also find a record of a German patent issued to Dr. Karl Moeller, this patent being dated October 2, 1884, the same year, and only a few months later. Both of these seem to be entirely independent of the earlier work, and seem to be again practically rediscoveries of the fact. All we can learn about Walker's process is through the outgrowth of some experiments and observations made by Oliver Lodge about that time, although Lodge's only publication of the fact was somewhat later than the date of this patent, evidently indicating they had been working together on it, probably before Lodge's publication took place. Lodge's own publication of the facts in a scientific form in the journals is comprised in his paper read before the Royal Institution, in May, 1886, nearly two years after Walker's patent;

and later in the same year, 1886, in November, he read a paper before the Society of Chemical Industry, in which he considerably elaborated on the phenomenon as applied to practical purposes. It is interesting to note in the paper presented by Lodge to the Royal Institute, that what he was after in that case was primarily, not electrical precipitation, which came as an accident of an independent investigation, but he was studying the effect of hot and cold bodies on dusty matter. It had been known for a long time that if you took a hot iron rod and held it in a cloud of smoke or dust and looked along the edge of the rod you could see dark spaces between the rod and surrounding cloud of smoke and dust, and considerable work had been done by various investigators to explain the theory of this dust or cloud. It was assumed to be the combustion of the particles surrounding the rod. It was afterwards shown that this would occur with a rod not only hotter than the space surrounding it, but colder than the space, and so finally it was run down to a question of molecular bombardment. Lodge employed that and tried to determine whether any electrical phenomenon were associated with this purely thermal phenomenon, and he connected them up to voltages of 100 and more, and found no effect, but when he went to thousands of volts he was surprised to find that a manifest activity was produced, that the smoke or dust was driven away from the rod, and rapidly deposited, and it was at that time his attention was first called to the phenomenon independently of his earlier work. It was only years after that he came across these references, and he published a brief historical note citing them and giving credit to the investigators for their work, although they were unknown to him at the time of his discoveries.

These two papers which Lodge prepared and delivered at these two institutions, the Royal Institution and the Society of Chemical Industry, served to bring the subject clearly before the public, and I think it is fair to say that the place of precipitation in our modern lines of thought dates from that time. The work of Moeller, while following somewhat the same line, did not appear to produce the same effect on the public, because it only appeared at that time as a patent, and was not followed by any further general publicity, and disappeared from public attention.

While we find Lodge's work referred to in various journals from that time down to comparatively recent years, and a

number of patents covering details of proposed methods occurring here and there in the literature, there was nothing that could fairly be considered as a permanent contribution to the technical developments of the subject until quite recently.

Just following Lodge's original patent, there was an installation put in at the works of Mr. Walker—whose name appears as the patentee—at the Dee Bank Lead Works in England, which consisted of two Wimshurst machines. The Wimshurst machine had just come out at that time, and the descriptions of this plant seem rather amusing today on account of the terms in which they describe the Wimshurst machine. It was so vastly superior to the friction machines in use before that time that they considered it a commercial piece of apparatus, a thing which would revolutionize the industry, and static electricity would come into practical use, which we know has not been the case in any such measure as was anticipated.

Current from these two large Wimshurst machines, which were six feet in diameter and were driven by a small steam engine, was carried to the flues of the lead smelter and pointed rods were placed in the flues, with barbed ends distributed over them, and the discharge from the machines was distributed to the gases from these discharge terminals, the other terminal of the machine being grounded. The descriptions we have are simply of the installation while under construction, and after that we hear nothing about it, so it evidently proved impracticable to maintain in continuous operation and disappeared entirely from the technique.

About 1905 some of us happened to be interested in these same problems, the clearing of fumes from acid works in California, and experiments were undertaken by the University of California to determine the practicability of these general methods, using more modern apparatus, or substituting the modern high-tension transformer with the auxiliary apparatus necessary, for the friction machine, and working out a more efficient system of electric distribution.

Out of that grew some interesting laboratory experiments which looked promising, and these attracted the attention of some of the local manufacturing concerns who were willing to carry the work further. It was very soon found that the work would have to be put on a more stable basis than it was possible for us to do as we were working, several of us at the University, members of the staff, and carrying the thing to a practical issue.

That led eventually, to the organization of the present institution which is handling the work. It was realized that it would be difficult for the University itself to undertake this work, and there was a distinct prejudice against purely commercializing the matter in the hands of any one commercial organization. A scheme was worked out to develop the patents under the guidance and backing of local capital, with the understanding that when the money invested had been returned, and a reasonable profit secured, that then the patents, as a whole, should go over to some holding organization to be used for the benefit of scientific research. It was expected then that would be the University of California, because all of us who were working on the problem at that time were alumni of that institution. As the work developed it took more time and money than was anticipated, and by the time it had become stabilized, which was a matter of five years, the work had spread over such a large territory that it seemed impracticable to attempt to administer it from a single institution, and we foresaw the danger, if it became successful and was handled from one institution, that the same sort of procedure would naturally be emulated elsewhere, and before we knew it the very thing which we wanted to prevent, that is the commercializing of educational laboratories, would be really brought about by the fact that we would be competing among one another on a commercial basis, and that would be the most dangerous effect we could bring about. To obviate that, we looked for a solution in finding some national, or better, international, body, to undertake the administration of these rights, and any others that might come in the same way, which would include all research work of this character; an organization in which all the different academic activities, universities and colleges could co-operate.

To make a long story short, after considerable search for the best means of carrying this out, the patents were turned over to the Smithsonian Institution, and by it transferred to a corporation which was formed under its auspices—its guidance at least—called the Research Corporation, organized as a business organization with directors and a charter, to conduct the business of the research work, and to turn over the property to the Smithsonian Institution and other national institutions, and it is under the guidance of that organization, the Research Corporation, with which probably many of you are familiar,

that the work which immediately grew out of our activities in California has developed.

Right alongside of that came also a similar development here in the East. About the time I was starting out on the final consolidation of this work under the Smithsonian and the Research Corporation, in the East Prof. Robert Kennedy Duncan was busy with his plan. I presume most of you are familiar with the fact that they have a system of Industrial Fellowships at the University of Kansas and University of Pittsburgh, and one of the problems he undertook as a part of this general program was the same problem of smoke and dust elimination. We worked in the West particularly on smelter smoke, and in the East he worked on black smoke, and his specific problem was the City of Pittsburgh. In this particular field, he was supported and financed by Mr. R. B. Mellon, of Pittsburgh, and out of this work, together with the other work on fellowships which was in his hands at that time, finally grew the Mellon Institute of Industrial Research, and School of Specific Industries, located in Pittsburgh, and the beautiful new building, which is to be dedicated this coming week, stands as a splendid monument to Dr. Duncan's energy and devotion to that work. He was, as you know, most unfortunately taken from us last year by death, at the time when he was just getting into his full energy and usefulness in this work, but, as he told me shortly before his death, he felt that the work had come to such a point that it would stand. He said, "I have been nervous about this whole matter, for fear I could not carry it far enough to have it thoroughly stabilized when I might be taken. I have carried it now to such a point in the last few months that that fear is eliminated, and I have it now in hands I am confident of," speaking particularly of his associate, who has since succeeded him, Dr. Raymond F. Bacon.

The work of the Mellon Institute in smoke abatement was wider than our work; it covered not only electrical precipitation, but other fields; electrical precipitation was, however, a prominent part of it, and from the first, even before the details of his plan were taken up, Dr. Duncan and I discussed the work and arranged a tentative plan of co-operation. At that time both the work of the Research Corporation and the work under his direction was still in such a formative period, that it did not seem wise to try to make a formal arrangement or co-operative plan between them, but to work it out as we went

along, and see how the work came out, and it was agreed we should do it in that way until such time as the work was sufficiently stabilized to bring them together.

That has pretty well transpired, and the two institutions are now working together to a common patent situation, the patents developed by each are being now, to a very considerable extent, consolidated and operated in a co-operative manner. The Mellon Institute is working for the same general aims as the Research Corporation, the methods of its administration varying somewhat from those which we have adopted in the organization, but they are each mutually supporting the other. The field of the Mellon Institute in the precipitation work will be more particularly in the immediate vicinity of Pittsburgh, for the immediate future, at least, and the Research Corporation working co-operatively with them, but also over the rest of the country, where work is already started.

These details will be understood better from the papers to be presented. Prof. Nesbit and Dr. Strong will speak for the work of the Mellon Institute, and Mr. Bradley will speak for the work of the Research Corporation this morning, and they will give you an idea of the relationship of these organizations. It has been a great satisfaction to me to see this come about, because I knew Dr. Duncan and valued his friendship very highly, and we both had this same view in mind, of thorough co-operation between the two organizations. I know there is nothing that would please him more than a knowledge of the results as they are now working out, and I feel sure the same applies to Mr. Mellon, who has so generously endowed the work of that Institute. So much for retrospect.

As regards the actual work, the apparatus will tell the story better than I can. I will not attempt to speak on that side of the work. I might say just one or two words in regard to the future, and some of the things we hope to see worked out, and perhaps one of the most interesting of these comes up in the form of the question—How far can this work go beyond what has actually been done and will be brought to your attention this morning in the papers? In Lodge's original plans there were two distinctly separate conceptions, two separate lines of attack, one was directed at the precipitation or collection of industrial fumes and smoke, house smoke and volatile matter, or at least man-made fumes. The other was directed to the question of fog precipitation in the open, the clearing

of routes for navigation, the clearing up, particularly, (as the idea existed in Lodge's mind) of the fogs of London. That in one sense looks like a greater task, like a more hopeless task, than those we have undertaken. In another sense, perhaps it is in some ways the easier of the two. It is a field we have kept thinking of, up to the present, feeling that we had our hands full enough with direct commercial developments of fume precipitation. The latter were matters in connection with which we could get direct financial support from individual firms, and the clearing of fog in the open is a thing which it will be harder to interest business interests in. It is a matter of public interest, and we felt that we could best attack that after the other work had been established. Also it is a matter which will require probably more powerful apparatus, and the small installations that have grown up to the present time have been a forerunner of that. Having developed the apparatus up to the point where we are now, it is time to make a start on the other work.

Another reason I personally had in rather discouraging an attempt to undertake that work in the earlier history, was that I thought that was particularly a problem of Sir Oliver Lodge's, and I had hoped that our work would reach a sufficient basis in practical success so that by the time that came to be taken up we could make a real effort to have Sir Oliver associated directly with the work in a way that would illustrate and emphasize the credit that is due him as the father of this whole art. I thought I saw an opportunity for that in the coming of the International Exposition at San Francisco, and particularly in the International Electrical Congress which was planned, and efforts were made to get such a program under way to carry out some of these experiments in their application to fog work at that time. These plans were thoroughly well under way, when the outbreak of the war, as you know, made the holding of the International Electrical Congress impracticable, and then the matter came back for consideration as to whether it was worth while to carry it out anyway. In the meantime, it was found that the amount of apparatus which would be necessary had been pledged, and that there would be facilities for handling extremely high-tension, high powered apparatus at the exposition. There will be a good deal of it there and available for such experiments, and the local electrical departments of the two Universities have taken an active part in

that and preparations are being made to do what can be done at the time of the Exposition to carry on some of these experiments.

While I have been here in the East on my present trip, I have been very much pleased to find the interest which is being taken in this matter. The possibilities of such experiments are growing right along among the shipping and other interests, to whom the fog question is a vital one, and I think it is entirely probable that within the next few months a direct start may be made in experiments along this line. Lodge has carried out some very startling experiments in this direction already. On the top of the building in his old headquarters at the Physics Laboratory at Liverpool, before he left there to go to Birmingham, he erected a mast, practically a flagpole, carried a wire up that to the top, and charged that simply from a series of large electrostatic machines available at the laboratory, and was able to gradually dissipate the fog for several hundred feet around the wire. This, at least, represents what may be hoped for. My own feeling is that the line of attack we will take up here, not to mislead you in what I have said, would be somewhat different from that. I think with the development we already have in precipitation, that the method would probably lie more in finding a means of carrying an electrode system, slightly modified, but in general not so different from those at present in use in the flue systems, in front of the vessel that was to be protected from fog dangers. Aerial problems are coming fairly and distinctly in the field of the engineer, and in talking with aeronautic people they do not see why there should be any question of carrying at a couple of boat lengths ahead of the vessel, from each side, an apparatus such as the electrode system, and by connecting that to high-tension apparatus, such as you will see operating here, on shipboard through the cable that is supporting this motor, you would have a broom, so to speak, sweeping a path clear in front of that vessel, that would give you room enough to stop if you were about to run into anything. If you wreck the helicopter, that does not amount to much, and that can be deflected in a shorter time than that in which you can stop a large vessel. I do not put this forward as a definite program or finished piece of work. It is offered purely as a suggestion as to the line of work that may be taken up, but I wish to say it is not half as big a jump into the dark as it would, perhaps, seem at first sight. Some of the steamship

people in New York have indicated their willingness to go forward with the work and supply the necessary means to carry on preliminary tests to see what may be done in the matter, and it is hoped some such development may take place in the near future; and I bespeak your cordial help and co-operation as members of the electrical profession in helping this good work along, because it certainly should add very materially to the safety and comfort of people traveling at sea, as well as materially assist in solving many problems on land.
