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ELECTRICAL PRECIPITATION

Historical Sketch

BY F. G. COTTRELL

MY FUNCTION this morning is merely to present a brief introduction to the real speakers and their topics. What you are chiefly interested in at this time, I know, is the experimental side of this matter, including the lantern slides and motion pictures, which these speakers have to show you. All I shall try to do is to outline a few of the steps that have led up to this work and thus attempt to give it a little more definite setting in time and space.

Going back to the earliest reference on the application of electrical forces to precipitation, we find that M. Hohlfeld, in 1824, was the first, as far as I know, to point definitely to the phenomena as of practical significance. Hohlfeld was a teacher of mathematics in the Thomas School of Leipzig, and published a brief article[1] descriptive of an experiment he had made on this subject. What he did was simply to form a funnel out of a piece of paper, set the lower tip into a bottle, and light the edge of the paper. When the smoke filled the bottle, the stopper was inserted with a wire through it reaching to within about three inches of the bottom. This wire was then connected to an old-fashioned frictional electric machine, and upon operating the latter the smoke in the bottle quickly disappeared by deposition on the inner surface. One of the experiments which Mr. Bradley is to show you later is substantially a duplicate of this experiment of Hohlfeld's. It is perhaps worth while to note the words in which Hohlfeld expressed his ideas on the significance of this phenomenon. He writes:

1. Kastner's *Archiv für die Gesammte Naturlehre*, Nürnberg, 2, 205-206 (1824).

" 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 following 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 cloudbursts 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. This constitutes our first historical reference to this subject, so far as I know.

This fell, apparently, on deaf ears, no one appreciating its importance. The machinery at the time available was, of course, inadequate for commercial application, and nothing again appears until a quarter of a century later, when C. F. Guitard mentioned[2] a similar phenomenon, which he independently observed on tobacco smoke in a bottle in much the same way. Then, the matter seems to have again rested for a still longer interval without further notice, save for an occasional incidental reference. For example, in 1878 Robert Norwald cites[3] the dispersing of dust inside an electrometer he was working on, when the instrument was left charged, which he ascribed to this cleaning effect on the air. The reference, however, is merely incidental, and represents nothing that was put to practical use.

The first attempt actually to apply this principle to practical affairs seems to have come almost simultaneously in Germany and in England. We find under date of August 9, 1884, an English patent on electrical precipitation issued to A. O. Walker, one of the proprietors of a large English lead works, and also a German patent, dated October 2, 1884, issued to Dr. Karl Moeller, covering much the same general ideas. Both of these seem to be entirely independent of the earlier work, and to represent, practically, rediscoveries of the phenomena of electrical precipitation. Walker's patent was directly inspired[4] by

2. *Mechanics Magazine*, London, Nov. 1850.

3. *Wied. Ann.*, *5*, 460-499.

4. "A New Application of Electricity," editorial in *Engineering*, London, *39*, 627-628; June 5, 1885.

experiments and observations made by Prof. Oliver Lodge and presented by him about that time in several lectures.[5]

It is interesting to note in these papers presented by Lodge, that the phenomenon which he was primarily studying was not electrical precipitation at all, but that this only came up as an accident of an independent investigation, he having set out originally to study the effect of hot and cold bodies on dusty air. It had been known for a long time that if a hot iron rod were held in a cloud of smoke or dust and one looked along the edge of the rod he could see a dark space between the rod and the surrounding cloud of smoke or dust, and already considerable work had been done by various investigators to explain this phenomenon. It was at first assumed to be due to the combustion of the particles surrounding the rod. This was later disproved and the effect finally was run down to a question of molecular bombardment. Lodge, in attempting to determine whether any electrical forces were associated with this purely thermal phenomenon, tried the effect of electrifying the heated rod. Up to potentials of a hundred volts or so he found no appreciable effect, but on going 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. It was only later that he came across references to the observations of others, and published a brief historical note[6] citing them and giving credit to these early investigators for their work, which was unknown to him at the time of his own discoveries.

These lectures and papers by Lodge 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 thought dates from that time. The work of Moeller, while following somewhat the same line, does not seem to have produced the same effect on the public, probably because it only appeared at that time as a patent, and was not followed by any further general publicity, and thus largely escaped public attention.

5. Lodge preliminary letter, *Nature*, *28*, 297; July 26, 1883.
Lodge and Clark, *Proc. Roy. Soc.*, Feb. 1884.
Nature, Apr. 24, 1884. Lecture by Lodge before Roy. Dublin Soc.
Nature, Jan. 22, 1885. Lecture by Lodge before Brit. Ass'n. Advc. Sci., Montreal Meeting.
Proc. Roy. Institution of Gr. Br., *11*, 520-529. Lecture by Lodge before Roy. Inst. May 28, 1886.
Jour. Soc. Chem. Ind., *5*, 572-576 (1886). Lecture by Lodge before Liverpool Section of Soc. Chem. Ind., Nov. 3, 1886.

6. *Nature*, *71*, 582 (1905).

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 actual industrial development of the subject until quite recently.

Just following Walker's original patent, an installation was made[7] at his plant, the Dee Bank Lead Works in England, in which two large Wimshurst influence machines were relied upon to supply the electricity. The Wimshurst machine had just come out at that time, and the descriptions of this plant are especially interesting 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 it was considered a commercial piece of apparatus, a thing which would revolutionize the industry, and bring static electricity 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, whose rotating disks were five ft. (1.5 m.) in diameter and were driven by a small steam engine, was carried to the flues of the lead works and there discharged into the gases from an insulated system of rods studded with sharp points placed in the flues, the other terminal of the machine being grounded. The installation, however, proved impracticable to maintain in commercial operation and was eventually abandoned.[8]

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

Out of that grew some interesting laboratory experiments which looked promising, and these attracted the attention of the local manufacturing concerns who were willing to carry the

7. W. M. Hutchings, *Berg- und Huttenmännische Zeitung*, *44*, 253-254 (1885).

8. "The Condensation of Fume by Static Electricity," Malvern M. Iles. *School of Mines Quarterly*, N. Y., Vol. *16*, No. 4, July 1895; and Vol. *17*, No. 2, Jan. 1896.

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 then working.

This was what led, eventually, to the organization of the Research Corporation. It was realized that it would be difficult for the University itself to undertake this work, and there was a distinct prejudice against permanently commercializing the matter in the hands of any one private industrial organization. A plan was therefore 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, then the patents as a whole, or at least a large portion of them, should go over to some holding organization to be used for the benefit of scientific research. It was expected then that the University of California would probably become the recipient, 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 inexpedient to attempt to administer it from a single local institution. We also foresaw the danger, if it became successful and was handled from one such institution, that the same sort of procedure would naturally be emulated elsewhere, and before one knew it the very thing which we wished above everything to prevent, that is, the commercializing of educational laboratories, would be really brought about by the fact that they would be competing among themselves on a commercial basis, the most dangerous effect that could be brought about. To obviate this, we looked for a solution in finding some national, and better, international, body, to undertake the administration of these rights, and any others that might come in the same way—an organization with and through which all the different academic activities, universities and colleges could cooperate.

To make a long story short, after considerable search for the best means of carrying this out, the patents were offered to the Smithsonian Institution, and by it transferred to a Board of Trustees which was formed under its auspices and guidance, and called the Research Corporation.[9] This was organized as a business corporation with directors and a charter, to conduct the commercial development of the patents, and to turn over the profits to the Smithsonian Institution to be used at the latter's

9. *Jour. Ind. and Eng. Chem.*, *4*, 864-867, Dec. 1912.

discretion in aiding and encouraging scientific investigation throughout the institutions of learning of the world, for which it acts as a sort of clearing house, and it is under the guidance of the Research Corporation, with which probably many of you are already familiar, that the work which grew out of the activities in California has developed.

Right alongside of the latter came also a somewhat similar development here in the East. About the time we were starting out on the final consolidation of this work under the Smithsonian and the Research Corporation, Professor Robert Kennedy Duncan was crystalizing his work on industrial fellowships at the University of Pittsburgh.[10] I presume most of you are familiar with the system of industrial fellowships at the Universities of Kansas and Pittsburgh. One of the problems which Prof. Duncan undertook as a part of this general program in Pittsburgh was that of smoke and dust elimination. We worked in the West particularly on smelter smoke, and in the East he and his co-workers concentrated their efforts more on coal smoke, their more specific field being the city of Pittsburgh.

Out of this, together with the work on other fellowships which was in Prof. Duncan's 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 and fitting monument to Dr. Duncan's energy and devotion to that work. He was, most lamentably, taken from us last year by death, just at the time when his long-cherished dreams were blossoming into substantial realities, but, as he told me shortly before his death, he felt that the work had then come to such a point that it would stand. He fully realized the uncertainty of human life and said he had until lately been apprehensive for fear he might not be able to carry the work far enough to have it thoroughly stabilized before he should be taken, but that it had been carried to such a point in the last few months that that fear was eliminated, and at last it had reached a stage and was in hands which left his mind at ease, referring particularly to his associate, Dr. Raymond F. Bacon, who has since succeeded him as director of the Institute.

The original program of the Mellon Institute in smoke abatement was wider than that of the Research Corporation. It

10. *Jour. Ind. and Eng. Chem.*, *3*, 177-186, Mar. 1911.

covered not only electrical precipitation, but other fields, such as improved furnace construction and the like. Electrical precipitation played, however, a prominent part in it, and from the first, even before the details of his campaign were taken up, Dr. Duncan and I discussed the work and arranged a tentative plan of cooperation. At that time both the work of the Research Corporation and the work under his direction were still in such a formative state that it did not seem wise to try to make a formal arrangement between them, but rather to work it out as we went along, and it was decided we should follow this procedure until such time as the work on both sides was sufficiently developed to bring them into more definite interrelation.

That has since then in large measure been brought about, and the two institutions are now working together to a common patent situation—the patents developed by each are now, to a very considerable extent, being consolidated and utilized in a cooperative manner. The Mellon Institute and the Research Corporation are working for the same general aims, although their methods of administration and conduct of work vary somewhat, but they are mutually sympathetic and trying earnestly to extend to each other all the moral support possible. The field of the Mellon Institute in the precipitation work is expected to be more particularly in the vicinity of Pittsburgh, for the immediate future, at least, with the Research Corporation working cooperatively with them there, but also over the rest of the country, where work is already started.

These details will be understood better from the papers which are to follow. This morning Professor Nesbit and Dr. Strong will speak for the work of the Mellon Institute, and Mr. Bradley for that of the Research Corporation. It has been a great satisfaction to me to see this close cooperation come about, not only because of the greater practical efficiency for useful effort which it represents, but because I knew Dr. Duncan and valued his friendship very highly, and we both had this same view in mind, of thorough cooperation 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 the harmony of purpose and action between the two organizations, and I feel sure the same applies to Mr. Mellon, who has so generously endowed the work of the Institute. So much for the retrospect.

As regards the actual work, the speakers to follow and the apparatus will tell the story, so I will not attempt here to speak

on that side of the subject, but I might perhaps say a few words in regard to our dreams of the future; in other words, as to 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 rather distinctly separate projects, two separate lines of attack. One was directed toward the precipitation or collection of industrial fumes and smoke—man-made fumes. The other was directed toward fog precipitation in the open, the clearing of routes for navigation, of congested railway terminals and crossings and even of metropolitan streets. It is natural to think of the famous fogs of London as probably a powerful stimulus to Lodge's imagination and interest in this matter. This, in one sense, looks like a greater task, a more hopeless undertaking, than those we have already to some extent accomplished. Still, it is a matter of such general public interest as to justify the most earnest effort in its attempted solution, and after all, there would seem to be nothing necessarily inherent in the engineering difficulties to be anticipated which would put it beyond the reach of present mechanical developments. It is a service which will probably require powerful apparatus, and the small installations that have served as stepping stones up to the present time have been a natural forerunner. Having developed the apparatus up to the point we have reached now, it is perhaps time to make a start, at least, on this new work.

Aside from these practical considerations, I must confess to having had a personal feeling of reticence in undertaking the fog problems proper, as I have always associated them particularly with Sir Oliver Lodge's own personality, and I had hoped that the work over here would eventually reach a sufficient basis in practical success so that by the time the real fog problems came to be taken up it might be possible to secure Sir Oliver's association directly with the work in such a way as would adequately indicate and recognize the credit that is due him as the father of this whole art. I had hoped that at last such an opportunity was about to present itself in the coming of the International Exposition at San Francisco, and particularly in the International Electrical Congress which was planned. Accordingly, efforts were made to arrange a program to carry out some of these experiments in their application to fog work at that time. These plans were well under way, when the outbreak of the war, as you know, made the holding of the International Electrical

Congress impracticable, and the matter then came back for consideration as to whether it was expedient to attempt anything at all. In the meantime, however, it was found that much of the apparatus desired had been pledged, and that there would probably be facilities for handling extremely high-tension, high-powered circuits at the exposition. The electrical departments of the two neighboring universities have taken an active interest in the matter and preparations are being made to do what can be done at the time of the exposition to carry on some, at least, of these experiments in general high-tension technique, and it may still be possible to get in a little work on the fog problems.

While here in the East on my present trip, it has been most pleasing 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. Sir Oliver Lodge, it is understood, has carried out some very striking experiments in this direction already. On top of the Physics Laboratory in Liverpool, just before he left there to take his present post at Birmingham, he mounted an electrical conductor and charged it from a series of large electrostatic machines with the result, it is reported, that he was able gradually to dissipate the fog for a considerable distance around the conductor. This would seem, at least, to represent what may be hoped for where one has to do with quiet air and stationary objects. My own feeling is that the line of attack most directly connected with established practise in this country may probably lie in finding a means of carrying an electrode system, slightly modified from those now used in flue systems, out in front of the vessel to be protected from fog dangers. Aeronautical problems are coming fairly and distinctly into the field of the engineer, and in talking with aeronautic engineers I find they do not see why there should be any insuperable difficulty in carrying at a couple of boat lengths ahead of the vessel, from each side, an apparatus such as the electrode system, and connecting it to high-tension apparatus on shipboard through the cable that is supplying the helicopter's or dirigible's motor. You would thus have a broom, so to speak, sweeping a path clear in front of the vessel, that would give room enough to stop or turn aside as objects ahead were sighted. The light electrode system and its supporting mechanism can of course be deflected in a shorter

time than that in which you can stop a large vessel. This picture is not put forward as a definite program or detailed project, but mentioned purely by way of suggestion as to one of the lines of work that may be taken up, and to indicate perhaps, to some, how it may not, after all, be as blind a jump into the dark as it would seem at first sight. Some of the steamship people in New York, and elsewhere, have expressed their willingness to go forward with the work should the preliminary tests prove encouraging, and it is hoped some such development may take place in the near future. In closing, I bespeak your cordial help and cooperation as members of the electrical profession in helping this work along, for 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.