Discharge Through Gases

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IT IS PERHAPS TIMELY THAT "DISCHARGE Through Gases" should be presented before the Institution, since 1947 marks closely the 50th anniversary of the proper scientific beginning of this field of study. It is particularly appropriate that it be presented before the Institution of Electrical Engineers, since it is from the research laboratories of England that most of the early significant advances have come. The subject has a peculiar and important interest for the electrical engineer, in that we live and operate in a gaseous atmosphere. Thus, it is not surprising that limitations set by the electrical properties of the atmosphere and, in particular, its electrical breakdown are in this electrical age of considerable practical importance. When it is recognized, furthermore, that the peculiar properties of electrically conducting gases furnish important elements in modern technical advance in the form of illuminating, rectifying, and welding agencies, and agencies for electrical counting, electrical switching, and the amplifying of electrical currents, the importance of this field to the engineer is adequately established.

With the discovery of X-rays late in 1895 there came to hand a medium of producing sufficient conductivity in gases that proper study could be initiated, and in the next 10 years significant advances were made by that brilliant international group of young physicists working under Sir J. J. Thomson at the Cavendish Laboratory. As time went on, these investigators moved elsewhere and carried further the researches begun at Cambridge.

In these first 10 years most of the fundamental properties of gaseous conduction were delineated, and a general understanding of the subject was achieved. At the end of this period the lure of the exciting developments in the fields of atomic structure attracted the attention of many of these brilliant workers and drew them into other channels of endeavor. The loss of active workers, however, does not entirely account for the period of relatively slow development of the subject which followed; for it must be clear that the initial studies had gone as far as the existing knowledge and techniques of the period permitted. The later development of the Bohr atom, the discovery of the elastic electron impacts, excitation and ionization functions, inelastic impacts of the second class, and various mechanisms of secondary electron emission were essential to proper interpretation. Likewise, the vast improvements in physical techniques such as high vacua and outgassing, achievement of gaseous purity, measurement, the achievement of electrical pulses of short time intervals, and the development of high-frequency oscillographs, were perhaps even more essential to further advance.

It is accordingly not unexpected to discover that the mechanisms derived from earlier study, while correct in outline, were in many cases seriously wrong in detail and that in general the simple pictures which have now become fixed in the textbooks need radical revision. It is thus my purpose to sketch the significant advances in the various aspects of the subject and—perhaps more important—to indicate the tasks which remain to be done.

In the early studies much interest was focused on the nature and mobility of the positive and negative ions in gases, with particular emphasis on the question of whether the ion was molecular or consisted of a large cluster of molecules. Inadequate techniques and lack of gaseous purity led to completely confusing and contradictory results. When it was realized that ions could change charge, had specific affinities for certain atomic or molecular species, and that in one second of time an ion could collide with some hundred millions of molecules, the confusing results became comprehensible. Hence, in order to study ions of a known constitution with the controlled gaseous purity obtainable, it was required that ions that did not change charge be observed in short periods of time. Beginning simultaneously in the laboratories of Loeb (*45*) in Berkeley and Tyndall (*88*) in Bristol, the recognition of these difficulties led to a satisfactory solution of the problem. Particular credit must go to Tyndall and his school of able investigators who, over many years of tedious study, developed the techniques which ultimately clarified the problem (*86*). This work together with the recognition that dielectric polarization and electrostatic attraction play a role in giving a fictitious collision cross-section, independently of the size of the ion cluster, has now satisfactorily brought the theory of ionic mobilities into agreement with experiment (*48*). Notable among these theories is the one published in 1905 by Langevin (*41*) and not recognized until rediscovered by Hassé (*25*) in Bristol. Clustering of a limited and often very specific sort does occur, but is not of the extensive character originally

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assumed (*87*). While much in detail yet remains to be done, the resulting information will be of value primarily to the physicist and physical chemist interested in atomic and ionic force fields and structure. It will be of no great importance in engineering applications. What the engineer must, however, recognize is that gaseous ion mobilities with ions of more than 10^{-4} second of age, except in a few special cases, can have significance unique to the particular gas conditions of a given measurement only. Thus, most values given in tables are not of general applicability even in the supposedly same gases, but may be used in roughly predicting ionic velocities in order of magnitude in practical application.

The coefficients of diffusion of ions are related to their mobilities by the classical expression, $k/D = Ne/P$, where k is the mobility; D, the coefficient of diffusion; N, the number of molecules per unit volume; e, the charge; and P, the pressure. Thus, what has been said about mobilities likewise can be said of the coefficients of diffusion with little modification.

One of the most important problems in the study of gaseous conduction and breakdown hinges on the values of the velocities of electrons in electrical fields in gases and, more in particular, on their energy distribution and their average energies. As early as 1910 it began to be recognized by Kovarick (*38*) and Franck (*21*) that in certain gases electrons generated by an ionizing agency remained free, and that even in air at lower pressures the electrons did not at once attach to molecules to yield negative ions. By 1913 Townsend (*84*) and his pupils had devised a means for studying the mobilities of electrons in gases and evaluating some sort of an average value of their energies, using the ratio of mobility to diffusion noted above. Today it is the only existing method of even roughly evaluating the energies of the electrons and relating them to the experimental parameter, X/p, the ratio of field strength to pressure, which is the determinative quantity in most measurements of gaseous behavior. In 1920 Loeb (*44*) began investigations on the conditions both of attachment of electrons to molecules in gases and of their velocities, extending the velocity measurements to higher pressures and lower X/p values, and on somewhat purer gases than Townsend. This work has been continued over the years by Loeb's students, notably Wahlin (*90*), Cravath (*12*), and later by Nielsen (*61*) and Bradbury (*8*). Today it can be said that the problem of negative ion formation is fairly satisfactorily solved, as will later be indicated, while satisfactory experimental data on electron velocities over a wide range of pressures in some standard gases now exist. This is due largely to Bradbury and Nielsen's work, using Loeb's electron filter, redesigned by them as an electrical shutter. With techniques available since World War II the velocity measurements could easily be extended in range and accuracy if required.

Stimulated by the work in the early 1920's, together with the new observations concerning the mean free paths of electrons in gases determined by Ramsauer (*66*) and later by Brode (*9*), the way was open for a theoretical study of electron velocities. This was initiated by Compton (*11*), who extended his earlier approach with classical kinetic theory procedures to give a fairly satisfactory theory of velocities of electrons of energies of below the threshold for excitation and ionization. In thus proceeding, it had been assumed that electrons moving in a field in a gas had the Maxwellian energy distribution characteristic of thermal equilibrium. Following a study of Hertz (*26*), Druyvesteyn (*17*), of the remarkable team of physicists active at the research laboratories of the Phillips Company in Holland, derived the energy distribution for electrons in gases below excitation energies with *constant* electron mean free paths. He showed that the energy distributions did not exactly follow the Maxwell law, although the law which he derived was not very radically different. This was independently rederived, using more general procedures in kinetic theory, by Cravath (*13*), Davydov (*15*), and Morse, Allis, and Lamar (*59*). Experimentally, however, the point at which Bradbury and Nielsen's values of the velocities began to deviate from the theoretical values given by Compton for different gases indicated to these workers that the departures of the energy distribution from the Maxwellian, or even Druyvesteyn form, was much more radical than anticipated. They ascribed this to the fact that the Ramsauer electron free paths for some gases increased or decreased markedly with electron energy in the regions studied and roughly indicated how it operated. The rigorous calculation up to ionizing energies was carried out by Allis and Allen (*1*) to include variable Ramsauer free paths, using a method initiated by Pidduck (*63*) and further developed by Morse, Allis, and Lamar.

The fact that the departures from the conventional distribution law are so radical and that the distribution varies extensively over the energy ranges experimentally covered makes it impossible either to interpret the meaning of the Townsend evaluation of average electron energy in terms of X/p or to apply these to an analysis of the ionization and excitation functions. Thus, probably the most important fundamental problem outstanding today is in the evaluation of electron energy distribution functions in various gases over extensive energy ranges and the relation of these to the experimental parameter, X/p. At present I see no method of approach to the solution of this problem except through the laborious and intricate method of calculation initiated by Smit in Holland (*74*). Smit has shown how to derive these functions and has calculated them fairly rigorously for helium for only four values of X/p. This work must be extended unless experimental techniques can be devised.

The increase in our knowledge of atomic structure, together with improved techniques, led to the solution

of the problem of the formation of negative ions from free electrons and molecules. The work initiated by Loeb and Wahlin in 1920 and carried further by Loeb in 1923 indicated the essential soundness of the theory for this process, suggested by Thomson in 1916 (77). This work led to an estimate of the values of the probability of attachment of an electron to a molecule to make an ion for some gases. In 1925 V. A. Bailey (3), then at Oxford, showed experimentally that the probability of attachment in air decreased as the electron energy in the gas increased—a condition not recognized by Thomson. With the invention of the electron filter by Loeb, Cravath and later, more successfully, Bradbury (7) were able to measure the character of the process of the formation of negative ions. This was found to be a diversified and, in some cases, a complicated process. In only two molecular gases, O_2 and SO_2, do electrons directly attach. In these the process is one in which the electron attaches to the molecule, the low heat of ion formation going to vibrational energy of the molecular system. Unless in a relatively short time the molecule can lose this energy in impacts with other molecules, the electron will reabsorb its energy and leave the ion. The problem has been successfully attacked wave mechanically by Bloch and Bradbury (5), and the theory agrees with Loeb's findings (46) as to the energy of attachment. In other gases the energy of attachment to one of the atoms of the molecule, together with the energy of the electrons, suffices to dissociate the molecule and form the ion in a three-body process, the residual energy being taken up as kinetic energy of separation. While radiative capture of an electron yielding negative ion formation is possible, the probability is so small that ion formation takes place, if at all, only by three-body processes where the excess energy is removed in the kinetic form. As shown by Massey (55), certain spectroscopic states of atoms and molecules have negative heats of ion formation, and in these no electron attachment occurs. Notable among them are the $^1S_0$ and $^1\Sigma$ states (7). In such gases electrons remain permanently free. While much work still remains to be done, the data to be gained are of value primarily to those interested in atomic and molecular structure, and the present information is adequate for the engineer.

While the process of recombination of negative ions or electrons with positive ions, to annihilate the charges in the gas, was one of the first subjects studied in 1896, it has proved to be one of the most difficult and stubborn of all the problems to interpret. The basic equation for recombination reads that the loss of ions by recombinations dn in a time dt out of n ions of each sign per cm.^{3} is $dn = -\alpha n^2 dt$. Its solution indicates that, starting with n_0 positive and negative ions per cm.^{3} at $t = 0$, there will be $n = \frac{n_0}{1 + n_0 \alpha t}$ ions remaining after a time t. The quantity α is called the coefficient of recombination of ions. Early work by Thomson and Rutherford (79), Townsend (80), and especially Langevin (40) derived the order of magnitude of α as 10^{-6} for ions in most gases. An early theory for the value of α, deduced by Langevin, was based on assumptions which were erroneous as applied to the usual conditions met with in a gas. This, together with the complicated nature of the processes at work and the experimental limitations, imposed thwarted interpretation for many years. In general there are four possible steps in the recombination process: a diffusive approach, an active electrostatic attraction, a mutual orbital encounter, and the transfer of charge. Depending on the density of the ions, the nature of the carriers, the pressure, the temperature, and the energy of negative ion formation, some of these four processes either are the determinative factor fixing α or may even be absent. Furthermore, measurement does not yield the concentrations needed directly but gives the quantity of ions, q, in a given gross volume, V. Use of the values of concentration n determined by the ratio q/V requires, then, a random but isotropic distribution of ions in space. With some forms of ionizing agencies used such as β particles or γ rays and X-rays, this is not achieved, and n changes in time by diffusion irrespective of the recombination loss, thus complicating theory. This latter aspect is also affected by the speed of attachment of electrons to molecules to give negative ions. Finally, recombination is a slow process, and the time required to get measurable changes in concentration n by recombination is of the order of 10^{-2} second or more long. Thus, loss by diffusion of ions and especially of electrons to the walls causes trouble. The worst difficulty, however, lies in the fact that in all but the purest gases made of single types of atoms the ions change their size and character during the 10^{-2} second of measurement, thus altering α continually. As a result of a long series of investigations beginning in 1925, using the most modern techniques by Marshall (53), Luhr (52), and finally Gardner (22) in Loeb's laboratory, fairly good results were obtained in one gas only (O_2) by Gardner in 1938. Similar techniques were used nearly simultaneously by Sayers (69) for air, and while the data are good, the values cannot be compared with theory because of the heterogeneous nature of air. Even more than for ionic mobilities are the values of the coefficient of recombination dependent on ionizing conditions, gaseous purity, and composition, so that practically all data given in the literature are unreliable. For purposes of calculation the values of the coefficient given by Gardner and Sayers may be used as orienting magnitudes for ion-forming gases. The coefficient of recombination for *electrons* and positive ions cannot be evaluated in gases under normal conditions because of its low value in contrast to rapid electron diffusion. Kenty (35) and Mohler (58) and associates have evaluated it for arc plasma and obtained values of the order of 2×10^{-10}.

which is higher than wave mechanical theory yields but very small compared to the values of about 2×10^{-6} for air and O_2. Later work of Sayers on arcs and of Craggs on spark channels have confirmed and extended these findings.

In theory the mechanism of recombination depends on the relative importance of the four steps indicated. It thus depends on the distribution of ion pairs in space, the nature of the carriers, the pressure, the temperature, and the density of ionization in a complicated fashion. Thus, much confusion has existed for a long time. Lately Jaffé (*31*) has studied the general problem and indicates that for very high gaseous densities the original Langevin theory applies. From some 10 atmospheres down to 2 atmospheres, a diffusion theory of Harper (*24*) which, except for an undefined constant, is in form the same as Langevin's equation, is valid. Thomson's (*78*) equation is applicable and has been experimentally established to hold from about 1 atmosphere down to 10^{-2} mm. pressure. Below this, a simple theory proposed by Loeb (*47*) is indicated. For recombination under α-particle ionization, the theory developed by Jaffé (*32*) must be used. No adequate quantitative theory has yet been derived for initial recombination with X-ray ionization for time intervals shorter than 0.1 second.

In the study of gaseous discharges such as arcs or glows with ion concentrations in the order of 10^8 ions/cm.^{3}, much information of value can be obtained by the use of electrical probes. With such electron and ion densities and greater ones, the Maxwellian energy distribution again begins to appear, despite the action of the field. Thus, good approximate values of ion and electron densities, ion and electron energies, space, and wall potentials can be derived. These techniques were initially developed by Langmuir and Mott-Smith (*43*), Compton (*42*), Schottky (*71*), and others. In recent years advances enabling the measurements to be analyzed to give electron energy distributions of non-Maxwellian form have been introduced by Druyvesteyn (*16*). An excellent alternating current method of study of the energy distribution has been developed by Sloane and McGregor (*73*) in the laboratory of Emmeléus at Belfast.

Probably the greatest contribution to the fundamental analysis of electrical discharge mechanisms is due to the studies of Townsend (*81*) and his group, which began in 1899 to 1903 on the two ionization coefficients. Townsend observed that if one started a current of i_0 electrons from the cathode in a uniform field plane parallel gap of length x and placed a high field, X, at a pressure, p, across the gap, the current of electrons and ions, i, measured after the initial electrons had crossed the gap was given by $i = i_0 e^{\alpha x}$. Here α is a constant dependent of the nature of the gas and the value of X/p. It gives the average number of new electrons produced per unit distance traversed in the field direction by one electron through ionization by collision of the gas atoms or molecules. Townsend further found that this quantity, α, now designated as the first Townsend coefficient, varied such that one could write $\alpha/P = f(X/p)$. The $f(X/p)$ is a complicated slanting S-shaped curve in which the *initial* rise was shown by Masch (*54*) in Aachen and Sanders (*68*) in Loeb's laboratory independently to be of the form $e^{BX/p}$, while the top of the curve has a form varying about as $(X/p)^{1/2}$, as shown by Posin (*64*). Although earlier evaluations suffer from deficient control of gaseous purity, the later results of Penning and Kruithoff (*39*) in Eindhoven on the inert gases and of Bowls (*6*) and Hale (*23*) in Loeb's laboratory for pure N_2 and H_2 probably begin to represent the proper values. The work of both Penning and Loeb's group demonstrates the effectiveness of the actions of certain impurities present to 1 part in 10^5 or 10^6. While earlier theoretical attempts to evaluate α before a proper appreciation of interaction between electrons and atoms or molecules was at hand are faulty, we now have a proper procedure outlined in the theory first suggested by Holm (*27*) but finally developed by Emmeléus, Lunt, and Meek (*18*). It cannot today be applied to calculations of α/p except in limited regions where the electrons have the Maxwellian energy distribution, in that it depends critically on the form of the distribution law. One can then rest assured that the pioneering work done by Townsend is basically correct and feel quite confident in the applications of the first function. Aside from the ignorance of the correct electron energy distribution, the information most urgently needed today by the engineer is a reliable set of experimentally determined values of α/p over large ranges of X/p in important gases of recognized purity. The need for such data is evident when it is noted that we have as yet no values for α/p in pure mercury-free air from X/p 20 to X/p 1,000, the region covering most discharge phenomena in air. Values in mercury-contaminated air may well be 25 per cent off at high values of X/p.

Townsend's second (*83*) great contribution in the study of discharges lay in the analysis of the curves for i at higher values of X/p, in air above X/p = 100, at which point i increases more rapidly than according to $i = i_0 e^{\alpha x}$. This is seen at once by plotting $\log \frac{i}{i_0}$ against x. The resulting linear graphs, whose slope gives α, are replaced by lines that are initially straight but curve upward at higher values of x. This upcurving was ascribed by Townsend to the creation of β new electrons, by impact of *positive* ions on molecules, per unit distance of advance from anode to cathode. Both Townsend and Thomson (*76*) in about 1902 independently recognized, however, that the effect of this β mechanism could also be produced if each positive ion striking the cathode had a chance, γ, of liberating a new electron from the cathode.

While Townsend has for a long time adhered to his

original assumption that positive ions can ionize in the gas, the extensive experimental data and theoretical study have shown that, with values of X/p existing in the gas, positive ions cannot ionize (*89*). Meanwhile it had been shown by Brode and Neumann (*10*) that other secondary mechanisms beside the electron liberation can be effective in producing the observed increase in the curves of Townsend. These mechanisms are photoelectric liberation at the cathode originally proposed by Taylor (*75*), Holst (*29*), and others, the action of metastable atoms on the cathode (*36*), and photoelectric action in the gas, which plays important roles in gaseous breakdown at higher pressure (*14*) but is probably of little significance under Townsend's experimental conditions. Aside from the quantity, β, then, at least three processes, electron liberation by positive ion bombardment, photoelectric liberation from the cathode, and, in appropriate gases, action of metastable atoms at the cathode, are known by experiment to give adequate numbers of electrons to yield a satisfactory process. It now happens that the extent of the curved portions of Townsend's curves are not adequate to enable one to differentiate the processes active, and Brode and Neumann in 1928 showed that within the limits of uncertainty each of these processes would yield an equation for i, with appropriate constants that would fit the observed curves. In consequence we will designate what Townsend originally called his coefficient β as the second Townsend coefficient without implying any specific mechanism. It is conventionally ascribed today to a liberation of electrons by positive ion impact on the cathode and designated by the Greek letter γ. The resulting current, i, for high values of X/p is thus given by $i = \frac{i_0 e^{\alpha x}}{1-\gamma e^{\alpha x}}$. If the other processes are active, equations of analogous form with appropriately altered constants can be used.

The essential validity of these conclusions is again indicated by the results obtained by Bowls and Hale in pure N_2 and H_2, using different cathode surfaces, once mercury contamination had been eliminated, as well as by the work of Huxford and Engstrom (*20*).

While the recent advances have clarified the situation and indicated the solution, the state of knowledge is woefully deficient, for in the interpretations of most discharge phenomena the values of these various secondary mechanisms are indispensable. The current needs in this direction will now be indicated.

While the beautiful studies of Oliphant (*62*), the 37th Kelvin lecturer, and others (*33*) on the secondary electron liberation by positive ion impact gave data for ions of higher velocities, nothing is known about the efficiency of different ions on various surfaces for ions below 20 volts of energy. Nor is the relation between the work function of the cathode and this liberation known. Such defects in knowledge extend as well to the liberation of electrons by metastable atoms. Likewise, while the work of Taylor and others has indicated the order of magnitude of the photoelectric yields from the cathode by photons produced in a Townsend gap, there is absolutely no proper quantitative data either on the actual yields for different metals under different conditions of gaseous content or on the changes in work functions of such surfaces with different gas coatings. There is also little information as to the efficiency of the various wave lengths in the ultraviolet below 1,000 A. either at surfaces or in gases, or of the absorption coefficients of such photons in the gases used.

Finally, we are in complete ignorance of the yield of photons accompanying the electron avalanches caused by Townsend's α in their passage to the anode at various electron energies in fields near those of breakdown. This deficiency is now more serious, since the photon production is essential for the streamer process which characterizes all breakdown at higher pressures and gap lengths. Hence, intensive study of photon production, absorption, and photoionization in standard gases constitutes one of the most urgent fields of investigation. It is believed that the postwar experimental techniques are probably now adequate for the tasks indicated above.

The original criterion for the breakdown of a plane parallel gap to yield a spark, which Townsend (*82*) took from the equation $i = \frac{i_0 e^{\alpha x}}{1-\gamma e^{\alpha x}}$ by setting $1 - \gamma e^{\alpha x} = 0$, has been substantiated in fact. It is now established that Townsend's use of the equation above for this purpose was unwarranted, but Holst (*29*) has indicated that the condition fixes the *threshold* for a spark breakdown on other considerations. This correct interpretation, as more recently indicated by Loeb (*49*), at once leads to an understanding of the statistical fluctuation in the values of the potential at which individual sparks appear and further correlates well with recent time-lag studies. Experimental confirmation of the Townsend-Holst mechansim for sparks at relatively low pressure and gap length has been achieved by the time-lag studies of Schade (*70*). As, however, Meek (*57*) has indicated in his excellent paper before the Institution, after 1927 many lines of evidence converged to show that the mechanism as pictured by Townsend and Schade for the plane parallel gaps at lower pressures cannot apply at pressures approaching atmospheric and gaps in excess of a few millimeters in length. The work of Loeb (*85*) and his students on positive corona, beginning in 1936, inevitably led to the development of the streamer theory of the spark by 1940. For this Meek (*56*) provided the quantitative criteria. Slightly later and from quite a different line of investigation using a C. T. R. Wilson Cloud Chamber, Raether (*65*) in Jena arrived at the streamer mechanism of breakdown and actually photographed the streamers. Quite independently of Meek, he arrived also at the same quantitative criterion, al-

though he did not carry his investigations very far. The logical extensions of this new mechanism were carried as far as speculation and the war permitted by Meek and Loeb in their book (*51*) on the mechanism of the spark. This analysis leads to countless unanswered questions, and the problems awaiting solution are too many to mention. The famous Paschen law that the sparking potential is a function of the product of pressure and gap length is not obeyed by Meek's theory, and the recent measurements of Howell (*30*) indicate that the discrepancies are very large at higher pressures. There are no continuous, consistent sets of data on sparking potentials in mercury-free dry air with pressure varying and gap constant, and vice versa, over a sufficient range of pressure and gap length values to permit of a test of the theory. Such an investigation by one laboratory is urgently required but is an exceedingly laborious and perhaps costly undertaking, as high potentials and fairly long gaps free from electrical field distortion are needed. Another urgent study requires that the points of transition from the Townsend mechanism to the streamer mechansim at different pressures be determined. Present estimates range from a value of 200 mm. pressure × cm. gap length for a 1-cm. gap in air to 1,000 mm. × cm. Other problems involve the study of the streamer mechanism of breakdown which proceeds from a negative point, and the question of the sparking potential for very long gaps.

The use of the simultaneous application of all available methods of study as initiated by Loeb's (*50*) group and the development of new, fast oscillographs have led to notable advances in the understanding of the positive and negative corona mechanisms and their ultimate breakdown to a spark. Much still remains to be done, but progress appears to be satisfactory. The study of spark breakdown with alternating potentials, particularly as functions of frequency, has shown that the threshold is lowered some 17 per cent as frequency is increased from 10^4 to some 4×10^5 cycles in short gaps, doubtless owing to positive space charge accumulations (*67*). As frequencies go up to the radar range of 10^{10} to 3×10^{10} cycles, the potential rises some 25 per cent above the d-c breakdown. In this case the electrons travel only short distances between reversals of the field, and the whole mechanism is little understood. The investigation of the whole range of frequencies and a study of the mechanisms at work present a series of fascinating problems for an interested observer.

Considerable advance in the understanding of the vacuum spark, so important in modern industrial application, has resulted from Bennett's (*4*) theory of the self-focusing beam. This theory materially assisted the successful study of the mechanism of the vacuum switch by Koller (*37*) in Loeb's laboratory.

One or two comments are in order with respect to glow discharge and the arc. Thanks to the development of probe studies, more is known about the glow than any other form of gaseous discharge. Thus, wherever probe studies are possible, gradients, potentials, electron energies, and, thanks to Druyvesteyn (*16*) and Sloane and McGregor (*73*), their energy distributions can be measured. There are, however, two regions in which such methods are not too successful. These are the cathode dark space, where ionization is insufficient, and the region of the anode fall of potential. It is desirable that much more data be gathered concerning the anode fall where the electrons have high energies, a negative space charge sheath exists, and in which the positive ions are created. The vital element in any quantitative treatment of the glow discharge lies in a knowledge of the cathode fall of potential. Since probes fail in this region, only one proper survey has been made in one discharge type by the late F. W. Aston (*2*), using the deflection of a fine cathode-ray beam. It is urgent that further studies be carried out in other gases over a range of pressures. With modern improvements in techniques, such as electron multipliers and cathode-ray oscillographs, it is expected that more information can be obtained by electron beam measurements. Recent investigations by Morton (*60*) and later work by Johnson (*34*) in Loeb's laboratory have shown that in fields which vary as rapidly with distance as do the dark space fields, one cannot apply the first Townsend coefficient as measured in uniform fields for calculating the ionization produced. Not only is the ionization from 2 to 6 or more times greater than given by the integration of such coefficients over the cathode fall, but the spatial distribution of ionization about the cathode is just the inverse of that given by the use of Townsend's functions. This recent investigation explains the striking characteristic structure of dark space and glow always observed and previously at variance with theoretical prediction on Townsend's function Thus, past attempts at calculation of the glow discharge using the condition of Holst and Oosterhuis (*28*), Seeliger (*72*), and v. Engel and Steenbeck (*19*), that $\gamma e^{\int \alpha x} = 1$, must be modified by replacing $e^{\int \alpha dx}$ by the evaluations of G. W. Johnson for the conditions found by Morton. Johnson's work covers these functions for pure H_2 and mercury-free air from 0.1 to 760 mm. pressure. More data such as Johnson's on other standard gases are urgently needed, and the investigation should be extended to spherical electiode symmetry instead of cylindrical.

With regard to the mechanisms of arcs, the high temperatures and microscopic scales of distance involved in the critical areas preclude study, and much remains to be done. While the refractory electrode arcs are amenable to some measure of theoretical treatment, the mechanism of the low-melting-point metal vapor arc is not clearly understood and is not amenable to solution. There seems little doubt but that the source of

ions in such arcs is in the plasma of the gas adjacent to the cathode and previous speculations such as those invoking field emission are inadequate to account for the phenomena. Equally illusory are the attempts to account for the mechanism by the glib term, "temperature ionization." Granted that the region near the cathode is at a high temperature, conditions are so far removed from equilibrium that one cannot apply M N. Saha's equation. It appears that we are confronted in this study with a situation presently also being encountered near the surface of particles moving with supersonic velocities, in which we cannot apply the familiar procedures of thermodynamics and kinetic theory. In these cases one has in microscopic regions large numbers of atoms or ions moving to the cathode or a surface, interchanging energy with solid or liquid surfaces, and leaving those surfaces with considerable directed momentum and energy. In these regions there are thus large numbers of particles of energy many times those of thermal equilibrium with directed velocities. The conditions for ionization are present, but a new mathematical physics is needed to calculate the consequences which depart widely from equilibrium conditions.

In presenting this summary I have attempted to include the advances and accomplishments in the last 50 years. What I consider more important, however, in this brief summary is not so much a glorification of the past but an indication of the problems still to be solved. It is my hope that the indications of just a few of these will arouse an interest in this presently neglected field, inspiring some few individuals, at least, to aid physicist and engineer in the work which remains to be done.

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The Truman Veto

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SCIENTISTS WILL UNDOUBTEDLY BE tempted to condemn the President for vetoing the National Science Foundation Act of 1947 (S. 526), and some have already yielded to this temptation. It may be hoped that the rest will apply the same processes of analytical thought to the reasons behind the veto as they conscientiously apply to their own professional problems.

Although the vetoed Smith bill has evolved a long way from the Magnuson bill, which was introduced into the 79th Congress during the summer of 1945, it is basically the same type of legislation. It has made some political concessions on patent provisions; it has prescribed a feeble measure of geographical distribution for scholarships; and it has liberalized the divisional structure of the Foundation. However, it strictly adheres to administration by a part-time board of eminent scientists, and it commits the legislative blunder of creating an Inter-Departmental Committee on Science whose chairman is an appointee of the part-time board, notwithstanding the fact that his powers affect many of the executive departments of the Government. For the President to have no say in naming a committee chairman who, in regard to scientific matters, outranks his own Cabinet officers in the executive branch of the Government is little short of preposterous. As originally drawn and finally passed, the bill still precludes reasonable geographic distribution of funds for research. It is, in short, the Bush bill, based upon the Bush report, *Science, the endless frontier*. It was, moreover, engineered through committee by the man who, in 1945 and 1946, personally represented Dr. Bush.

In 1946 the Administration made it perfectly clear that, if such a bill reached the President's desk, it would be vetoed, and the reasons given were substantially the same as those contained in the veto message. That the Administration's views were shared by many scientists and other educators was evident from the size and vigor of the committee formed by Harlow Shapley and Harold C. Urey. Furthermore, that the President had not changed his mind in 1947 must have been clear to Dr. Bush and the other supporters of the bill. It is reported that Senator Smith had even more authoritative information regarding the President's attitude.

In the face of the President's forthright statements, S. 526 was drafted and introduced. Little regard was paid to the carefully considered recommendations of the Inter-Society Committee, sponsored by the American Association for the Advancement of Science. The Morse Amendment, which liberalized the geographic distribution of research funds and which might possibly have saved the bill from veto, was defeated in the House. So the Bush bill went to the President, and the President kept his word. If the proponents found no reason to change the bill, certainly the President had no reason to change his mind. Indeed, it was virtually necessary to veto a piece of legislation which assigned powers in the executive branch of the Government to a director whom the President could neither name nor remove.

Scientists must look at facts squarely and honestly. There are many who favor S. 526 because they have faith in administration by scientists who have demonstrated ability in science and in administration. But there are as many more who feel that, however capable the administration of a scientific board may be, the Board is bound to consist of men who, from temperament and experience, will administer Government-supported scientific research in the same way as industrial and national defense research. At present fewer than 50 institutions are receiving well over 90 per cent of all research financing from industry and from the Army and Navy. There is no complaint about this allotment of funds, but there is serious objection to the disproportionate award of additional grants to the same institutions. Not only will it widen the gap between the large and the small universities and technical schools; it will literally dwarf the latter by drawing Foundation-subsidized students and underpaid but competent instructors to the former.

Two provisions of the bill make such a trend inevitable: (1) the qualifications set for members of the Board, who will necessarily be mainly big-name men from big-name institutions; and (2) the stipulation that research grants