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RECENT DEVELOPMENTS IN PHOTOELECTRICITY[1]

By Professor **C. E. MENDENHALL**

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THE general quantum theory may be said to have been born of a black body and sired by the photoelectric effect. Consequently, since we are all, willy-nilly, quantists, every one has at least a genealogical interest in photoelectricity. For some, this interest is enhanced by a certain fascination of the phenomenon itself, and just at present the relation of experiment to statistical and wave-mechanical theory is attracting particular attention. While it is my purpose to discuss mainly recent aspects of the subject, though with no attempt at completeness, it will be useful for the sake of clarity to restate very briefly certain of the earlier generalizations, in order that the later material may be placed in proper relation to them.

The term photoelectricity covers what are, from the standpoint of technique at least, two distinct fields, the "external" and "internal" effects, the latter being sometimes more descriptively called "photo-conductivity," and it is with the former only that we shall be concerned. Furthermore, the "external effect" itself involves two sets of phenomena, which are experimentally and theoretically quite distinct, according as the matter from which electrons are being removed is in the one case a vapor or in the other case a solid or liquid.

The case of a vapor is obviously the simplest, but it has been the last to be developed because of experimental difficulties. The Bohr theory led one to expect that if the energy of the light quantum equalled or exceeded the ionization energy of the atom, or molecule, ionization would result, and experiments of Williamson, Lawrence and others have verified this and given values of the ionization potential consistent with other determinations. On account of the difficulty of working with sufficiently short wave-lengths,

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[1] Address of the retiring vice-president of Section B—Physics, American Association for the Advancement of Science, Cleveland, December 31, 1930.

photoelectric measurements of ionizing potentials have been carried out only with the vapors of the alkali metals. However, a repetition of some early work of Steubing's carried out with very considerable improvements by Rouse and Giddings has led to a considerable study of what might be called secondary phenomena, namely, ionization in what were thought to be monatomic vapors by light whose quantum value is *less* than the energy necessary for ionization —in some cases not half the necessary amount. In the case of mercury and cadmium the atomic absorption of the resonance line, resulting in excited atoms, is the primary action, and *two* such excited atoms are necessary for ionization. In the case of the alkali metal vapors, as studied by Foote, Lawrence and Edlefson, and Mohler and his coworkers, not only does absorption in the atomic series lines produce ionization, but there is a continuous background of absorption of wave-lengths longer than the ionization limit, which also results in ionization. The most probable explanation of all such cases involves a suggestion of Franck's that the ions observed are molecular, though no direct confirmation of the presence of such ions has yet been made by measurements of $\frac{e}{m}$. In the case of mercury and cadmium the molecules are probably formed by the combination of two excited atoms. In the case of caesium, Mohler and Boeckner conclude that neutral molecules are initially present either in the normal or in some vibrational state—the range of vibrational states present accounting for the range of wave-lengths longer than that corresponding to the ionization of the normal molecule, which will actually produce ionization. This type of photoelectric effect thus leads one at once into the theory of molecular structure and of impacts between molecules and atoms. In the hands of Mohler and his coworkers it has proved a very sensitive means for studying the distances involved in such collisions as dependent on the excited states of the atoms, as well as the mean life of these states.

Turning now to the photoelectric discharge from solids and liquids, the problem is of course entirely different and molecular and atomic theory has not yet come to play a part in it, though there are some very puzzling empirical correlations between photoelectric quantities and atomic constants. The earlier generalizations can best be summarized by Einstein's two famous equations, which not only correlated the facts so beautifully, but gave to the young quantum theory an aspect of much greater physical reality. These equations are:

$$U = 1/2\ m \cdot v^2_{max} = h\nu - e\varphi = h(\nu - \nu_o)$$

$$N = \frac{Q}{h\nu} = \frac{Q\lambda}{hc}$$

where U is the *maximum* energy of the electrons after emission due to light of frequency, ν, ν_o is the low frequency limit, N is the number of electrons emitted by the *absorbed* light energy Q, λ is the wave-length corresponding to ν v_{max} is the maximum electron velocity, and h is the Planck constant of action. The process according to these equations is very simple. Each quantum gives up *all* its energy to a single electron (thereby distinguishing the photoelectric from the Compton and Raman effects later discovered) and if that energy is greater than $e\varphi$, the work necessary to get the electron through the surface, the electron is emitted.

In determining the maximum velocity of emission by direct experiment it is usual to apply an electron-retarding field between the illuminated metal and a surrounding receiver. If the potential difference between the metal and receiver is adjusted until the fastest photo-electrons are just prevented from reaching the receiver, this potential difference is called the stopping potential, V_s, and we have the relation

$$1/2\ mv^2_{max} = (V_s + V_c)e$$

where V_c is the Volta contact potential difference between the observed metal and the receiver and $(V_s + V_c)$ is the actual potential difference between these two. Furthermore, energy considerations led Einstein to the relation

$$(V_c)_{12} = \varphi_1 - \varphi_2$$

that is, the contact potential difference between two metals is equal to the difference of their surface work functions, having proper regard to sign.

These four equations express all the results of classical and early quantum theory with respect to the photoelectric effect, and we must now briefly consider the results of experiment in relation to them. The first Einstein equation expresses a linear relation between the maximum energy of emitted electrons and the frequency of the light, and the slope of this line should be h. The most consistent determination of h by this method is that of Lukirsky and Prilezaev (1928), and the photoelectric method is one of the best available for the determination of h. The determination of h does not involve any specific characteristics of either the illuminated or receiving surface, but does of course require that these characteristics should remain fixed during any one determination. No matter how contaminated the surfaces are, if they remain constant, the correct value of h should result.

Early measurements of long wave limits for various metals were extremely discordant, and it only gradually came to be realized that the surface and volume conditions of the metal altered the limit and the related value of φ in a very marked way. Attempts to clean the surface produced such large changes that for a time a considerable group of experimenters held the view that for a really clean metal surface there would be no photoelectric effect whatever. It is only in the last few years that it has been possible for different observers to get concordant results. That these observers, even with their extremely painstaking methods, are really dealing with perfectly clean gas-free surfaces is very difficult to prove. I am inclined to think, from indirect evidence, that in many cases they are, and to hope that the values of ν_0 (or φ) now being secured can later be correlated to other fundamental properties of the metals in a systematic way. However this may be, recent work shows a good agreement between the photoelectric and the thermionic work functions, if proper allowance is made for temperature variations, and the photoelectric and thermionic measurements are made on the same specimen. In a similar way, recent work confirms the predicted relation between V_c and φ, if again observations are made on the same specimen. Agreements between observations of V_c by the observer and the corresponding φs by another must be considered accidental unless the metals have in both cases been very carefully cleaned. But no matter how contaminated the surface, if V_c and $\varphi_1\varphi_2$ are measured under the same conditions, they are found to bear the theoretical relation to each other. The cleaning process, granted the metals are sufficiently free from metallic impurities, is largely concerned with the removal of gas from the surface and body of the metal, and for this only two methods have been successfully used, distillation and extreme heating in the highest possible vacuum. During the process of removing the gas, the long wave limit for a given metal may shift by as much as 1,000 Å. In some cases the shift is at first in one direction and then in the other, but such complicated effects are probably due not alone to the removal of gas but to other changes brought about simultaneously by the heat treatment. In the end (and this may be only after hundreds of hours of heating, or prolonged redistillation) surface conditions are reached which are almost completely stable as regards further treatment, and which change so slowly in the high vacua at room temperatures that accurate observations may be carried out upon them. It is such surfaces, if any, which may be said to be gas free.

The expression for the photoelectric current-density may be written

$$i_\nu = f(\nu)\, I \quad d\nu$$

where I_ν is the intensity of incident radiation of frequency ν, and $f(\nu)$ gives the dependence of photocurrent on frequency for unit incident intensity. Obviously $f(\nu)$ would be expected to depend on a number of factors, namely: the reflecting power of the surface for light; the index of absorption of the metal for light, as determining the penetration of the light into the metal; what is sometimes called the "quantum efficiency" or the probability that an available quantum will excite a photoelectron; the absorption coefficient for photoelectrons in the metal, and an internal reflection coefficient for such electrons at the metal surface. In spite of the complicated possibilities which these considerations suggest, it is found that for most metals $f(\nu)$ runs a simple course from zero at the long wave limit to continuously increasing values at higher frequencies. For the alkali and alkaline earth metals $f(\nu)$ usually shows a pronounced maximum which only appears, if the surface is specular, when observed at oblique incidence with the parallel ("dig-in") component of the electric vector (E). An effort has been made to separate these effects into two independent types of "selectivity," one the difference in the effectiveness of the perpendicular and parallel components of E, the other the occurrence of a maximum in the $f(\nu)$ curve. The situation is much more complicated than at first appeared; but, though the experimental evidence is in many cases conflicting, from the early work of Millikan and Souder and the recent work of Fleischer and Dember, Ives, and Suhrman and Theissing, it is possible to draw certain conclusions, which though not finally established seem most probable.

(1) The two selectivities, vectorial and spectral, are probably aspects of one phenomenon, the former being largely determined by the roughness of the surface. (2) Even with the alkali metals, the usual selectivity is absent when the metal surfaces are in what one might call their simplest condition if the observations are expressed as current per unit of *penetrating* light intensity. (3) The selectivity does not result merely from the adsorption of gas on such a simple surface, but as Ives suggested, from the development of an invisible surface structure, which may take weeks to appear. (4) The effect of this surface structure is not merely to alter the absorption characteristics for light, but also to change the intrinsic photoelectric properties. (5) These effects are much influenced by the thickness of the layer of alkaline metal which is being studied, but are relatively insensitive to gas contamination.

If we may hope to learn something about the nature of this surface structure and how it operates, then there is much more work to be done, involving dispersed illumination, simultaneous measurement of reflecting power and photoelectric characteristics, and X-ray study of the structure of the surface; though it may well be that the structure we are interested in is not deep enough to show by X-ray examination. There is needed either great elaboration of technique, or a considerable and clarifying innovation.

Recalling again the various factors which determine the normal run of f(v), one might hope to separate these, which affect the velocity distribution as well as the number of emitted electrons, by a study of metal films of varying thickness, but such studies as carried out by Compton and Ross, Goldschmidt and Dember, Lukirsky and Prilezaev and Ives, have led to rather discordant results. This is not surprising when one considers not only the difficulty of measuring the thickness of very thin films, but also the difficulty of producing films which differ in thickness but are in other respects alike. From work with thin films the "mean free path" of the photo-excited electrons in metals is estimated at from 1 to 5×10^{-6} mm, while the thickness of the "active layer" or depth from which measurable numbers of photoelectrons can escape is in some cases put at 1×10^{-5} mm and in others as greater than 1×10^{-4} mm. In the nature of the case it is very difficult to free such films, once formed, from gas, and this is undoubtedly another reason for the discordant results. On the whole, work with thin films has been more successful in raising new questions than in answering old ones.

As directly observed, f(v) is in terms of unit incident energy, and one would like to eliminate the effect of the optical properties of the metal in order to get nearer to the quantum efficiency or probability of excitation. With massive metals—*i.e.*, much thicker than the "active layer"—the outgassing develops a surface crystalline structure, and it is doubtful if the optical constants R and κ measured on polished surfaces are applicable to the multi-crystalline surface. Only a few attempts have been made to measure simultaneously the optical and the photoelectric properties. It is not surprising then that observations of f(v) are discordant, and that no correlation between the f(v) for various metals has been obtained.

There have been some experimental advances in recent years which, though somewhat qualitative in nature, are of decided interest. Of these consider first the influence of temperature, which might conceivably alter the long wave limit, the various factors determining f(v), and the velocity distribution of the photoelectrons. It is important also to analyze the results from the standpoint of the probable cause, and it is usual to limit the term "temperature effect" to such as are *not* due to a change in a gas layer, or a definite allotropic change in the metal, though it is frequently not easy to eliminate changing gas conditions. In all, about a dozen metals have been studied with sufficient care so that some conclusion can be drawn concerning temperature variation, though for only a few metals do the results at all approach completeness. Generally, though not in all cases, the long wave limit moves toward longer wave-lengths with increasing temperature. In general also f(v) changes —sometimes without any measurable change in the long wave limit. The most complete studies have been made of tantalum, gold, and silver by Messrs. Cardwell, Morris and Winch, and from their unpublished results I quote the most systematic example of temperature variation which has been found. If one plots the photo-current per unit incident light energy as a function of temperature for a series of discrete wave-lengths, then for all three of these metals one finds that for wave-lengths near the long wave limit there is a marked *increase* in current, while for those roughly 200 Å or more shorter than the long wave limit there is a less marked but definite *decrease* in current with rising temperature. Put in another way, the current-frequency curve f(v) for high temperatures (600 to 800° C.) crosses that for room temperatures, the "toe" of the high temperature curve being more pronounced and the long wave limit less sharp and definite. Part of this systematic variation is foreshadowed in some earlier work of Ives with potassium and other alkali metals. That these systematic variations are real and not artifacts due to spectral impurity and the extremely rapid variation of sensitivity with wave-length near the limit, seems much more probable if one considers that two different dispersing systems—one double and one single—were used in the above work. As for interpretation, the *increase* in emission near the long wave limit and the extension of the "toe" of the curve is exactly what would be expected from the standpoint of increased kinetic energy of the conduction electrons from which the photoelectrons probably originate, but the *decrease* in emission for shorter wave-lengths would remain unaccounted for. Probably more complicated considerations are necessary, but in any event it appears to be a rather significant experimental result.

The study of the effect of temperature has brought out the effect of change in structure. The most marked changes are observed with iron, in which the $\alpha \rightarrow \beta$ and more particularly the $\beta \rightarrow \gamma$ transformations greatly alter the photoelectric characteristics. Cobalt shows at 850° C. changes very like those accompanying the $\beta \rightarrow \gamma$ transformation in iron, but the situation is more complicated, for resistance measurements indicate no transformation point at 850° C. but do indicate one at 450° C. which, however, does not appear photoelectrically. X-ray observations so far as they have been carried out agree with the photoelectric results in indicating a change in crystal structure at 850° C. Evidently the photoelectric effect and conduction are differently "structure-sensitive," to use Goetz's term. Goetz has shown that the long wave limit increases progressively from β(2,740 Å) to γ(2,820 Å) to liquid (2,925) tin, but found *no* temperature change in photoelectric properties not associated with a change in structure.

All the work we have just been discussing has been done with multi-crystalline specimens, the study of single crystals, much to be desired, having been delayed by experimental difficulties. High melting point single crystals are difficult to produce, while low melting point single crystals would be either melted or recrystallized by the usual outgassing treatment. Unless this matter is very carefully considered, mistakes are likely to be made, and in particular it seems quite probable that different crystal faces might appear to be the same simply because the underlying characteristics are entirely concealed by gas contamination. On the other hand, positive evidence of a difference in the photoelectric behavior of different faces of a zinc single crystal, such as obtained by Linder, would appear to mean something even though outgassing precautions were not very carefully attended to. We have some preliminary evidence of a dependence of the long wave limit upon the crystal face, also in the case of zinc. In this case for the first time the crystals were grown in a very high vacuum, but were subsequently exposed to air for a short time. This is all that is known about the behavior of single crystals.

There is a general similarity in the behavior of metals during the outgassing heat treatment, to which attention may be called. Beginning with a surface condition resulting from rolling, annealing and mechanical cleaning by fine emery, the effect of the first heating is to cause a very considerable increase in general sensitivity, perhaps a hundredfold, followed by a more gradual decrease. During these changes the long wave limit shifts correspondingly, that is, toward long wave-lengths while the sensitivity is increasing, and toward short waves while the sensitivity is decreasing, indicating that at least the major part of the sensitivity changes are due to shifts in the long wave limit. During this part of the process, large amounts of gas are given off by the specimen. With most metals the final stage is reached by a gradual decrease in sensitivity to a steady value, the long wave limit becoming fixed at the same time. With iron and molybdenum, on the other hand, the final stage is reached by a further *increase* in sensitivity to a stable value. If air is admitted to the tube at any stage, the characteristics of the surface are changed to pretty closely their initial values. After such exposure a repetition of the heat treatment will, in a much shorter time, bring back values of long wave limit, etc., which had before been attained; but as far as our experience goes, the final condition is reached without passing through the original series of intermediate states. In other words, the heat treatment has produced a permanent change which is *not* reversed by mere exposure to gas. Whether this change is nothing more than the growth of fairly large crystal grains which are quite obvious to the eye, or whether it has to do with the removal of vaporizable metal impurities, or of gas from the body of the metal (the initial volume content of gas not having been reproduced by the relatively short re-exposure to gas which has been used) can not now be said. Probably all three factors—structure change, volume gas change and removal of impurities—have something to do with it. Work with single crystals should help to clear this up, and if once clean crystal faces can be obtained, a study of the effect of exposure to different gases should be of particular interest from the standpoint of the nature of the gas layers which are formed.

During the past few years there has been a decided revival of interest and activity in the electron theory of metals. Started by Sommerfeld, it has been carried on by Houstoun, Eckart, Fowler, Nordheim, Wentzel, and as regards our particular problem most recently by Frölich. The new theory, by the adoption of the Pauli exclusion principle (which may be described as social legislation to prevent overcrowding of the electrons) removes *a priori* the great difficulty with the older forms of the electron theory—namely, the specific heat paradox. According to the new picture, the valence electrons of the metal atoms become free in the solid state, but classical equipartition is given up and instead the energy distribution is given by the Pauli-Fermi-Dirac statistics, according to which the average electron energy is practically independent of temperature except at very high temperatures of the order of 10,000° C. At the absolute zero there would be a perfectly definite maximum electron kinetic energy (w_i), while at ordinary temperatures this maximum becomes less sharp and there is an approximately Maxwellian distribution over a short range period beyond w_i. For a metal having one free electron per atom, this maximum zero point energy corresponds to 7 or 8 volts. In dealing with the emission of electrons, the metal is treated as a *potential box,* and since the electrons are now given relatively large kinetic energies, the potential wall, w_a, of the box must be correspondingly higher than in the old theory, so that the difference w_a-w_i will agree with the experimentally determined surface work function, φ. From the standpoint of the electrons we have played on them a typical protectionist trick—wages have been increased, but prices have gone up correspondingly, so that at first sight it might appear that we were no better off than before. There are however certain advantages, aside from the fundamentally important one of avoiding the specific heat paradox. The existence of velocities beyond the zero point maximum, the distribution curve rising with increasing temperature, leads to the conclusion that the long wave limit would be absolutely sharp only at the absolute zero, and that the *effective* or observed limit would shift toward long wave-lengths and become less sharp at higher temperatures. As we have seen, this is just what we have observed for tantalum, gold and silver, and it would be exceedingly interesting if these changes could be definitely connected with the predictions of the new statistics. At present we can not say whether the observed effects can be attributed solely to the temperature change in the electron velocity distribution.

The theory proceeds to consider the electrons in the potential box from the standpoint of wave mechanics, the effective minimum wave length of the electrons being of the order of a few Ångstroms. One immediate result is the computation of a *transmission coefficient* for electrons striking the potential wall, which depends upon the electron velocity and the height and form of the wall—that is, whether the potential change is sharp or gradual, whether the wall is flat or has a parapet, etc. For a given wall, and given *total* (kinetic plus potential) energy of the electrons, the transmission coefficient for internal electrons is theoretically the same as that for electrons approaching the surface from the outside. This gives an interesting opportunity to see whether transmission coefficients measured for electrons incident externally can be usefully applied to photoelectric and thermionic observations. Unfortunately, the theoretical transmission coefficient should differ from zero or unity only for a very narrow range of internal kinetic energies about equal to the height of the potential wall, and the kinetic energy of the corresponding external electrons would be of the order of a fraction of a volt. The determination of reflecting power for such slow external electrons is very difficult and up to the present there are no measurements available. If experimental values of the transmission coefficient could be obtained, then it might be possible to draw conclusions as to the form of the potential wall in specific cases, and this would be a most interesting result.

Of particular interest in relation to the surface electrostatic forces are the recent results of Suhrman, Becker and Mueller and Lawrence and Linford on the effect of external electric fields upon photoelectric characteristics. Working with the alkali metals it has been shown that relatively moderate external electric fields shift the long wave limit by considerable amounts, the effective value of φ changing by as much as 0.2 volt. Not only this, but the entire $f(\nu)$ curve is shifted toward longer wave-lengths, without change of form. This is quite in harmony with the theoretical view-point, according to which the form and effective height of the potential wall can be changed by the superposition of a sufficient external field. Furthermore, while a change in the potential wall should in general change the form of the $f(\nu)$ curve, the computed magnitude of this change turns out to be too small to detect under the conditions of Lawrence and Linford's experiment.

The more detailed wave mechanical theory has arrived at two further conclusions which are very general and perhaps capable of experimental test. The first is Wentzel's deduction that $f(\nu)$, giving the photo-sensitivity for unit penetrating radiation as a function of frequency, should in *all cases* have a maximum, and the theoretically determined position of this maximum agrees fairly well with the position of the selective maxima for some of the alkali metals. At first thought this seems an extremely significant agreement, but there are two considerations which decidedly lessen our satisfaction. The first is the accumulation of evidence, which we have already discussed, which strongly supports the idea that the selective maxima of the alkali metals are *not* characteristic of the metals themselves but are due to little understood surface conditions, and the second is the fact that with several of the alkali metals the selective maxima are followed on the ultra-violet side by a rising sensitivity curve which is *not* predicted by theory. For other metals having their long wave limits farther in the ultra-violet, the theoretical selective maxima come at such short wave-lengths that it is not surprising that they have not yet been observed. It may be that the general predictions of the theory are correct, but that it is a mistake to attempt to correlate them with the commonly observed selective maxima of the alkali metals, and the most interesting test of this will be to push observations with the heavier metals farther into the ultra-violet to see if any evidence of the existence of a maximum sensitivity can be obtained.

More successful is the correlation of the theoretical velocity distribution of photoelectrons with the observations of Lukirsky and Prilezaev on thin films of silver. With decreasing film thickness the observed velocity distribution curve becomes qualitatively quite similar to the theoretical curve, showing a preponderant number of electrons having nearly the maximum velocity. Since the theory has been worked out only for a thin layer, neglecting the absorption of light and electrons in the metal, this agreement appears significant.

As Wentzel is careful to say, the theory so far is so idealized that one must be cautious in attempting to correlate it with experiment. In simplifying the problem from the theoretical standpoint, several factors have been neglected which are just those which the experimenter can not, or at least has not, eliminated. Most important of these is the structure of the surface, which has theoretically been assumed to be *perfectly smooth.* It is doubtful whether experiment can ever deal with a surface approximating this condition. Furthermore, until values of the internal absorption coefficient for both light and electrons are available, either from experiment or from more fundamental theory, the present type of wave mechanical theory will be limited in its application to *thin films.* As we have seen, experimental work with thin films has its own peculiar difficulties, and the results in many cases show new complications rather than the simplifications which one would hope for in order to compare with theory.

The present situation then is that while experiment is providing continually more complicated results, though to be sure they are undoubtedly more reliable and more reproducible, theory naturally asks for simple characteristics obtained under idealized conditions. Perhaps with better controlled experiments and more elaborated theory, we can reach not only an understanding of the fundamental photoelectric process, but also, what is of equal interest and importance, a better picture of the structure of a metal surface and of the gas layers which form on it.

PSYCHOLOGY'S FAMILY RELATIONS AMONG THE SCIENCES[1]

By Professor **MADISON BENTLEY**

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SCIENCES, like families, have their lines of descent. In some the ancestral strains are easily to be traced; in others the derivation is clouded by uncertainty or complicated by strange infusions. Some of the older sciences derive with great directness and simplicity from remote cosmogonies and philosophies; but the younger members often branch widely in process of generation, going back to disparate sources and interlacing with many other lines. For the contrast you have only to compare chemistry and biochemistry, physics and endocrinology.

Like the average family, again, the science possesses a large number of collateral relations. It has its brethren in other sciences, its cousins and nephews among the professions, and numerous legal kin acquired by solemn union with the arts and vocations. No occasion in the year so inevitably brings out these collateral relations as does the great winter pilgrimage of our tribes of the Triple-A to the common Mecca of the Faithful. Here we find many evidences of our familial ties and of our close fraternal dependencies; our intersectional interests, our passionate allegiance to the virginal mother, our implicit trust in the beneficent guidance of the Council of the Elders, our frigid intersectional shuttling from door to door in wintry blasts, our hybrid conferences, and our embracing symposia.

But the individual subject may itself be aptly used to exemplify the same sort of horizontal membering. Take psychology. It is not easy to enclose within a single central area all that is named by that name. And when we look beyond the more immediate boundaries, we observe a large number of widely radiating lines leading first toward a number of psychological specialties and ultimately to other collateral subjects each with its own family seat but each maintaining an intimate relation with psychology. These lines run outward from our own central domain toward general biology, zoology and ecology; toward physiology, neurology and endocrinology; toward anthropology, ethnology and sociology; toward business, vocation and industry; toward medicine, criminology and hygiene; toward physical and cosmological theories and doctrines of mind and matter; toward heredity, embryology and genetics; and, finally (if the long list may be completed), to education and human betterment. Add a multitude of cross-threads running helter-skelter throughout the figure and you have a gross representation of the great psychological family as it greets the New Year of 1931.

Now it is necessary that the inner circle of psychology be drawn wide enough to make room upon its convexity for all these centrifugal connections. But it is obvious upon inspection that the wide diameter has not been arbitrarily chosen. It has of necessity to embrace the existing schools and basal varieties.

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[1] Address of the retiring vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Cleveland, January 2, 1931.